Revealed Comparative Disadvantage of Infants: Exposure to NAFTA and Birth Outcomes*

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Abstract

This paper examines the effects of trade liberalization under the North American Free Trade Agreement (NAFTA) on infants' health outcomes in the US. I explore this question by implementing event studies and difference-in-difference regressions that compare birth outcomes of infants born in different years relative to NAFTA and localities with differential exposure to import competition. Using more than 88M birth records of Natality data, I find significant negative effects on a wide range of birth outcomes. The adverse effects are much larger for infants at the lower tails of birth weight and gestational age distribution. The heterogeneity analysis suggests larger effects for low-educated mothers and female infants. I show that these effects are not driven by selective fertility and preexisting trends in birth outcomes. Additional analyses using a wide range of alternative data sources suggest several potential pathways, including reductions in income-employment, decreases in housing wealth, lower health care utilization, lower health insurance use, and lower-quality health insurance. Finally, I provide discussions on the policy implications of these findings.

Keywords: International Trade, NAFTA, Birth Outcomes, Health Utilization, Health Insurance

JEL Codes: F14, H75, I12, I13, I18, J13

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1. Introduction

One of the long-lasting, highly controversial, and intensely debated policies in the US international trade agreements has been the implementation of the North American Free Trade Agreement, known as NAFTA. Prior trade agreements of the 1980s had already facilitated bilateral trade between US and Canada. In 1990, the Mexican president requested a similar trade agreement with the US. Despite hot debates during the presidential election of 1992 over the costs and benefits of such trade liberalization, legislatures of the US, Canada, and Mexico ratified the proposal. NAFTA was signed into law in 1993 and became effective on January 1, 1994.

Since the implementation of NAFTA, studies in various settings have explored its local and aggregate effects on the economic and non-economic aspects of the three countries (Burfisher et al., 2001; Cherniwchan, 2017; Gómez-Ramírez & Padilla-Romo, 2022; Hakobyan & McLaren, 2017; Lee, 2021). Within the United States, studies on aggregate welfare effects suggest very small rises in net benefits (Caliendo & Parro, 2015; Romalis, 2007). However, the impacts were heterogeneous across different geographic locations and industries. Hakobyan & McLaren (2016) search for local labor market effects and find that blue-collar workers residing in trade-exposed vulnerable localities experience sharp reductions in wage growth post-NAFTA. Choi et al. (2021) document reductions in employment in local areas with higher exposure to import competition following NAFTA. These local employment and income shocks mirror the economic conditions of exposed subpopulations which can have spillover effects on a wide array of other non-economic outcomes.

It is now well documented that the prenatal development period is a critical period with strong influences on birth outcomes and hence a wide array of later-life outcomes (Almond & Currie, 2011; Barker, 1990, 1994; Currie, 2011; Currie & MacLeod, 2008). Based on the Fetal Origin Hypothesis, an external stressor changes the epigenetic programming with the sole purpose of survival of the fetus. For instance, a negative shock to nutrition intake causes the fetal programming to silence off some growth-related genes to help the fetus survive the hardship but results in an infant with lower birth weight (Almond & Currie, 2011). A strand of literature provides empirical evidence that such shocks to employment and income are associated with changes in infants' health outcomes (Almond et al., 2011; Amarante et al., 2016; Currie & Rossin-

Slater, 2013; Hoynes et al., 2015; Lindo, 2011; Mocan et al., 2015). Therefore, trade-induced worsening local economic conditions may adversely affect newborns' birth outcomes.

Given the extent of studies and debates on spillovers of trade liberalization in general and NAFTA in specific, it is surprising that no previous studies touch on the health effects of NAFTA in the US. This paper aims to fill this gap in the literature by investigating the effects of NAFTA-induced trade liberalization on infants' health outcomes. In so doing, I exploit the cross-county geographic variations in the baseline employment composition with differential exposure to Mexico import competition combined with post-NAFTA policy change.

The simple idea is that local areas with higher pre-NAFTA dependence on industries for which Mexico reveals comparative advantage experience higher import competition post-NAFTA. A priori, one expects larger reductions in employment and income in these areas. I show that this is the case. In a series of event studies and difference-in-difference regressions, I find large and long-lasting effects in counties with higher initial exposure. The results suggest that a one-percentage-point higher initial vulnerability to import competition from Mexico is associated with 67 basis-points lower overall employment (off a mean of 0.38), 17 basis-points lower employment in the manufacturing sector (off a mean of 0.06), and $1,800 reduction in income (off a mean of $42.7K). Moreover, the event study figures show that these effects last more than a decade.

The impacts of NAFTA may well go beyond labor market outcomes and employment-income profiles. I provide evidence that affected counties experienced significant and relatively large reductions in housing wealth as housing values fell sharply. In addition, I show that total per capita current transfer receipts from social insurance increased significantly in counties with higher exposure to trade liberalization. For instance, a one-percentage-point higher exposure (roughly the mean of the trade exposure index) is associated with a 9 percent reduction from the mean of the housing price index and about a 2 percent increase from the mean of transfer receipts. Moreover, these counties also reveal sharp increases in retirement income following NAFTA. While the negative effects on wealth, employment, and income may lead to improved birth outcomes, social insurance transfers and retirement income may have positive externalities on infants' health outcomes (Duflo, 2000; Hoynes et al., 2015; Noghanibehambari & Salari, 2020).

I start the analysis by exploring the reduced-form effects of NAFTA-induced trade exposure and the subsequent tariff reductions on infants' health outcomes. I employ the universe of birth records in the US over several years pre-post-NAFTA and implement event studies that compare birth outcomes of infants born in different years relative to NAFTA implementation in counties with different baseline vulnerability to import competition. The event study results show that treatment and control groups do not trend differently for several years prior to NAFTA and start to diverge roughly three years after the trade policy change. In line with the effects on employment and income, the negative effects on birth outcomes last for more than a decade and do not show any evidence of revival. The difference-in-difference results suggest that being born in the top versus bottom deciles of the trade vulnerability index after NAFTA versus before is associated with roughly 11 grams lower birth weight. The effects are more pronounced for infants at the lower tails of birth weight distribution. This comparison is also associated with 26 and 5.4 basis-points higher likelihood of low birth weight and very low birth weight, equivalent to 4.2 and 4.9 percent reduction from the mean of their respective outcomes. I also find small but significant and negative impacts on gestational age. Again, the effects are concentrated among infants at the bottom of the gestational age distribution. Additional heterogeneity analyses provide evidence that these effects are more pronounced among low-educated mothers. Furthermore, I implement some tests to show that these results are not artifacts of selective fertility of mothers in response to trade-induced changes in economic conditions.

Further analyses suggest that affected mothers were less likely to have had any prenatal visits or utilized any prenatal care during their pregnancy. Finally, I complement mechanism channels using data from the Current Population Survey (CPS) and decennial Censuses. I find that a higher vulnerability is associated with a lower likelihood of being insured, a higher likelihood of depending on public insurance, and a lower probability of having presumably better-quality private insurance. Moreover, using CPS and census data, I observe significant reductions in wage income, decreases in housing wealth, and increases in usage of public welfare income.

The findings of this paper have important policy implications. Trade liberalization imposes differential costs and benefits on different subpopulations and localities. This differential effect is captured among trade economists by trade diversion and trade creation (Burfisher et al., 2001; Caliendo & Parro, 2015; Winters et al., 2004). From the policymaker's perspective, an optimal tariff schedule depends on the net benefits extracted from welfare analyses. The results of this study uncover the costs of trade liberalization for a subpopulation that are usually overlooked in aggregate cost-benefit welfare analyses. In addition, the findings suggest the sensitivity of infants' health outcomes to local labor market conditions. This procyclical behavior calls for policies that enhance social insurance spending during periods of unemployment and job loss, specifically following trade policy changes. Since several studies point to the long-term and intergenerational effects of health endowment at birth, policies that aim at promoting public health may prioritize resources for unemployed and poorer families (Bharadwaj et al., 2018; Currie & Moretti, 2007; Noghanibehambari, 2022).

This paper makes several contributions to the literature. First, to the best of my knowledge, this is the first study to explore the local effects of NAFTA on infants' health outcomes. The importance of these outcomes is more pronounced when we look into the bulk of empirical evidence that links them to later-life cognitive development, educational attainments, labor market outcomes, health status, longevity, as well as other multigenerational effects (Behrman & Rosenzweig, 2004; Black et al., 2007; Kleven et al., 2022; Maruyama & Heinesen, 2020; Royer, 2009). Second, this paper adds to the literature on the costs and benefits of trade liberalization by providing evidence of its unnoticed negative externalities for infants. Third, this study also contributes to the ongoing literature on the Fetal Origin Hypothesis by documenting the influence of economic conditions during pregnancy on birth outcomes.

The rest of the paper is organized as follows. In section 2, I discuss the background of NAFTA and provide a literature review. In section 3, I go over the data sources and construction of variables. In section 4, I introduce and discuss the empirical method. In section 5, I review the main results of the paper. In section 6, I explore potential mechanism channels between trade exposure and birth outcomes. In section 7, I discuss the magnitude of the findings, compare them with other studies, and discuss their potential later-life consequences. Finally, I depart some concluding remarks in section 8.

2. Background

2.1. Background on NAFTA

One of the early appearances of the idea of a free trade zone was in the US presidential campaigns of Ronald Reagan in 1980. After being elected, he started discussions of bilateral trade agreements with Canada. Bilateral agreements were facilitated by the enactment of the Trade and Tariff Act in 1984 and later by the Free Trade Agreement (FTA) of 1989. The administration of

George H. W. Bush entered negotiations for expansions of these agreements to include Mexico. By 1992, an agreement had been prepared and sent for ratification in the US, Canada, and Mexico. In the US, the trilateral agreement proposal, later known as NAFTA, was passionately debated in the media, specifically around the presidential campaign of 1992. Opposing NAFTA was the bulletin of independent candidate Ross Perot, who succeeded in attaining roughly 20 percent of popular votes, a figure never gained by any third-party candidate post WWII and it was never to be repeated. However, Bill Clinton entered the White House in 1993 and, after legislatures' ratification, signed NAFTA into law in December 1993. NAFTA became effective in January 1994.

Many provisions of NAFTA included immediate and substantial tariff reductions. However, the framework of the agreement assured a gradual phase-in of liberalization. The tariff rates of some industries were scheduled to be eliminated in the coming years. Therefore, for some industries, NAFTA was an announcement of future tariff reductions. This gradual but substantial decrease in tariff rates and, in the meantime, large increases in imports from Mexico can be observed in Figure 1. For illustration purposes, I deflate variables by their respective values in 1993. The left y-axis represents imports, and the right y-axis represents the values of tariffs. The US-Mexico tariff rates drop by about 90 percent from 1993 to 2000, although gradually (green line). While the MFN tariff rates[3] also decline, these reductions represent smaller changes than Mexico's. Similarly, while imports from other countries go up, increases in imports from Mexico (blue line) are considerably larger, representing a total increase of about 240 percent.

2.2. Literature Background

Trade liberalization and increases in import competition may have significant and differential effects on the society of trading partners. In this section, I discuss several aspects of these changes and their relevance for the infants' health outcomes.

The primary channel of effect is reductions in income due to worsening local economic conditions and diminishing job prospects (Arbache et al., 2004; Autor et al., 2016; Breinlich, 2008; Carneiro & Kovak, 2015; Revenga, 1997). For instance, Hakobyan & McLaren (2016) and Choi

[3] Most-Favored Nation (MFN) tariff rates are tariffs that the United States imposes on other countries that are a member of World Trade Organization (WTO). These tariffs are also promised by each WTO member for all other member countries.

et al. (2021) show that localities with a higher vulnerability to NAFTA experience lower wage growth and fewer employment opportunities. Autor et al. (2013) explore the local labor market effects of trade liberalization with China in 2000. They show that local labor markets with a higher concentration of manufacturing employment, hence a higher exposure to import competition, reveal large reductions in labor force participation and wages. On the other end, income influences birth outcomes through two primary channels. First, it affects the consumption of materials that directly impact health, such as nutrition, an important determinant of birth outcomes (Abu-Saad & Fraser, 2010; Almond et al., 2011; Almond & Mazumder, 2011; Ga & Feng, 2012; Haeck & Lefebvre, 2016). For instance, Haeck & Lefebvre (2016) explore the effects of a prenatal nutrition program in Canada to help low-income families and improve health at birth. They find a treatment-on-treated effect of about 70 grams of additional birth weight. Almond & Mazumder (2011) explore the effects of fasting during Muslims' holy month of Ramadan on birth outcomes and find a reduction of nearly 20 grams for infants of Muslim women whose prenatal development overlaps Ramadan. Second, income affects the consumption of materials that indirectly affect infants' health, e.g., health insurance. Having health insurance and utilization of prenatal care can potentially influence birth outcomes, although the literature provides mixed evidence and inconclusive results (Camacho & Conover, 2013; Chou et al., 2014; Corman et al., 2019; Currie & MacLeod, 2008; Goodman-Bacon, 2018; Joyce, 1999; Kumar & Gonzalez, 2018; Ma & Simon, 2021; Palmer, 2020). In the same line with reductions in income, declines in wealth could also influence health at birth. Zabel (2012) finds a strong correlation between local labor market shocks and changes in house prices. Daysal et al. (2021) show that housing wealth increases fertility and reduces preterm birth and low birth weight outcomes.

Mocan et al. (2015) use Natality birth records to investigate the effects of mothers' earnings on birth outcomes. They employ two-sample two-stage least-square regressions to impute mothers' earnings from Current Population Survey data and find positive but small effects. Their findings suggest that doubling mothers' income is associated with an increase in birth weight of about 100 grams and an increase in gestational age of 0.7 weeks. De Cao et al. (2022) use data from England and employ mother fixed-effects to explore the effects of local labor market shocks on infants' health. They find evidence of procyclical health outcomes. Specifically, a one-percentage-points increase in the local area unemployment rate is associated with roughly 5.3 grams lower birth weight. In a similar study using data from Argentina, Bozzoli & Quintana-

Domeque (2014) also document the procyclicality of infants' health. Kyriopoulos et al. (2019) show that a deep recession in Greek negatively affects birth outcomes and the effects are more pronounced among families of lower sociodemographic status. Stearns (2015) documents that paid maternity leave reduces the share of infants with low birth weight by as much as 10 percent. Lindo (2011) explores the effects of parents' job loss on birth outcomes and finds that husbands' job loss reduces birth weight by about 5 percent. Clark et al. (2021) employ data from the UK to investigate the effect of negative self-reported economic shocks on birth outcomes. They find that experiencing a negative economic shock during the first half of pregnancy is associated with reductions in birth weight by as much as 70 grams.

Social insurance programs such as Supplemental Nutrition Assistance Program (SNAP), Women, Infants, and Children (WIC) program, Medicaid, and Unemployment Insurance (UI) programs may insulate households from negative employment and income shocks. Several studies provide suggestive evidence for their impacts on birth outcomes (Aizer et al., 2016; Almond et al., 2011; Amarante et al., 2016; Chung et al., 2016; Figlio et al., 2009; Hoynes et al., 2011, 2015; Markowitz et al., 2017; Sonchak, 2016). Figlio et al. (2009) show that prenatal participation in WIC has no effect on mean birth weight but significantly reduces low birth weight. Almond et al. (2011) explore the effects of the introduction of the Food Stamp program on birth outcomes. They find sizeable improvements in birth weight with the largest effects among black mothers. Hoynes et al. (2015) investigate the spillover effects of tax rebates under the Earned Income Tax Credit (EITC) program on infants' health outcomes. They document significant increases in birth weight and sizeable reductions in low birth weight. Their results suggest a $1,000 treatment-on-treated effect reduces low birth weight by 2-3 percent. Chung et al. (2016) examine the effects of the Alaska Permanent Fund Dividend (APFD), annual payments to Alaska residents to share revenues of natural resources, on birth outcomes. They find that a $1,000 increase in income reduces low birth weight by about 14 percent of the sample mean. Noghanibehambari & Salari (2020) show that payments under Unemployment Insurance (UI) programs have the potential to improve birth outcomes. They find that a $1,000 higher payment to unemployed eligible mothers increases birth weight by about 20 grams.

The worsening local economic conditions caused by rising trade competition can also adversely influence mothers' mental health (Bradford & Lastrapes, 2014; Colantone et al., 2019; Currie, Duque, et al., 2015; Cygan-Rehm et al., 2017; Marcus, 2013). Several studies show that poor mental health during antenatal development is associated with poor infants' health outcomes (Álvarez-Aranda et al., 2020; Camacho, 2008; Carlson, 2015; Duncan et al., 2017; Kim et al., 2017; Olafsson, 2016; Torche & Kleinhaus, 2012; Wadhwa et al., 2004). Carlson (2015) examines the effects of exposure to the announcement of mass layoffs on birth outcomes. He finds that distressing economic news leads to significant reductions in birth weight and increases in low birth weight. Carney (2021) exploits the staggered adoption of state-level mental health parity laws, a mandate requiring mental health care coverage, to explore the effect of mental health on birth outcomes. She finds that among women with private insurance, the introduction of the law reduces adverse birth outcomes.

Another channel between trade liberalization and health is environmental air quality. This channel could offer improvements in infants' health post-NAFTA. If trade causes the pollutant industries to move overseas or to countries with lower pollution-per-output, one would expect a cleaner environment post-trade. In the case of the US, several studies show that trade has the potential to benefit local environments (Beghin et al., 1995; Benarroch & Gaisford, 2014; Cherniwchan, 2017; Cole, 2003; Dean et al., 2002; Johnstone, 1995). Cherniwchan (2017) explores changes in local pollution levels following declines in the manufacturing sector induced by NAFTA. He finds that a large portion of reductions in pollutants during the 1990s can be attributable to NAFTA. A growing body of literature documents the adverse effects of pollution on birth outcomes (Altindag et al., 2017; Currie et al., 2009; Currie, Davis, et al., 2015; Currie & Neidell, 2005; Currie & Schmieder, 2009; Currie & Walker, 2011; DeCicca & Malak, 2020; Hill, 2018; Inoue et al., 2020; Shah & Balkhair, 2011). For instance, Currie & Walker (2011) exploit the introduction of electronic toll collection as a source of exogenous shock that substantially reduced congestion and vehicle emissions to explore the effects of pollution on birth outcomes. They find that among mothers residing near a toll plaza, the incidences of preterm birth and low birth weight reduces by 11 and 12 percent, respectively.

In addition, there are several indirect channels between trade liberalization and infants' health. For instance, there is evidence that trade liberalization causes income and wage inequality (Galiani & Sanguinetti, 2003; Reuveny & Li, 2016). It also could lead to a widening gender wage gap and gender inequality in the labor market (Besedeš et al., 2021; Hakobyan & McLaren, 2017; Kis-Katos et al., 2018; Pieters, 2018). Hakobyan & McLaren (2017) show that NAFTA affected the wage growth of married blue-collar women much more than other demographic groups. On the other end, there is evidence that increasing inequality and the diverging gender gap in the labor market are associated with adverse birth outcomes (Biggs et al., 2010; Cubbin et al., 2020; Mayer & Sarin, 2005; Mellor & Milyo, 2002; Olson et al., 2010; Pabayo et al., 2019; Rauscher & Rangel, 2020; Tacke & Waldmann, 2013; Wallace et al., 2016). Furthermore, trade liberalization and worsening local economic conditions may lead to lower neighborhood safety and increases in crime rates (Beach & Lopresti, 2019; Dix-Carneiro et al., 2018; Mocan & Bali, 2010; Ortega et al., 2021). A narrow strand of research shows that decreases in neighborhood safety and exposure to areas with higher crime rates are correlated with adverse infants' health outcomes (Brown, 2018; Foureaux Koppensteiner & Manacorda, 2016; Mark & Torrats-Espinosa, 2022; Masi et al., 2007; Matoba et al., 2019).

A small but recently growing literature explores health effects of trade liberalization (Agüero & Ramachandran, 2020; Barlow et al., 2022; Chiappini et al., 2022; Dai et al., 2021; Dean & Kimmel, 2019; Fan et al., 2020; Feng et al., 2021; Fernández Guerrico, 2021; Lang et al., 2019; Lin et al., 2021; Olper et al., 2018). This literature mostly focuses on adult people's health outcomes and finds inconclusive evidence. One exception is Navaei & Farnoud (2021), who show that trade liberalization with China reduces pollution and, through this channel, improves infants' health outcomes. Pierce & Schott (2020) show that counties in the US with higher exposure trade liberalization with China reveal higher fatal drug overdose rates. Lang et al. (2019) show that individuals residing in areas with higher vulnerability to increased import competition following trade liberalization with China reveal higher morbidity of poor mental health. Agüero & Ramachandran (2020) examine the impact of imports from China on infants' birth outcomes using data from 25 Sub-Saharan Africa. They show that a higher exposure to imports increases birth weight and have potential gains for infants' health.

3. Data, Sample, and Variables

3.1. Birth Records Data

This paper uses a wide range of data sources which are listed below. The primary source of data is county-identified restricted-access Vital Statistics Natality detailed files extracted from National Center for Health Statistics (NCHS). The NCHS data is the full-count census of birth records that contains information on birth outcomes, pregnancy complications, and limited information on parental sociodemographic characteristics. Specifically, the data records the child's sex, parity, birth order, birth weight (in grams), clinical estimation of gestational age (in weeks)[4], and Apgar score[5]. The NCHS data provides information on maternal prenatal care utilization and prenatal doctor visits. The data also provides information on maternal race, age, ethnicity, marital status, and education. There is similar paternal information but not for all years and states. Moreover, even for state-years that this information is available, not all records contain paternal covariates for various reasons. When building the matrix of covariates, I include a missing indicator for each parental control with missing data.

Since the main outcomes of interest are birth weight and gestational age, I exclude from the final sample those records that have missing information on these variables. Next, I restrict the sample to singleton births since birth outcomes of multiple births are complicated by factors unrelated to determinants of intrauterine growth (Thekkeveedu et al., 2021; Lemos et al., 2013). I restrict the sample to several years pre- and post-NAFTA, specifically the years 1986-2008. However, in Appendix G, I show that results are robust and quite similar in magnitude for shorter and longer observation windows. Since the empirical design requires geographic information, I also remove records for which county and state variables are unidentified. These restrictions leave the NCHS sample with roughly 88 million observations.

3.2. Birth Outcome Variables

Based on the three primary birth outcomes mentioned above, I construct several other measures, which I explain below. Birth weight and gestational age are highly correlated (Magnus et al., 2009).[6] External stressors may impact health at birth through changes in birth weight with no effect on gestational age, vice versa, or both. The first measure to account for these complications is fetal growth, which is the intrauterine weekly weight gain of the fetus. It is computed as birth weight divided by gestational age. Another measure is to look at gestational-age-induced changes in the birth weight by regressing birth weight on gestational age and then

[4] Gestational age is the length of the period between conception and birth. The date of conception can be estimated as early as first trimester of pregnancy. If the mother is unaware of the conception date and she has not employed any method to extract the date during prenatal period, there are various methods to estimate this post-natal. One simple rule is that the gestational age is the period between the start of the woman's last menstrual period and the time of birth.

[5] Apgar score is a 5-minute clinical test for examining Appearance, Pulse, Grimace, Activity, and Respiration. Each test lasts 1 minute and scores between 0-2. Therefore, Apgar score varies between 0-10

[6] Over the sample period, the correlation between birth weight and gestational age is 0.48.

using the predicted value of this amount (i.e., gestational-age adjusted birth weight). The next outcome is the birth weight of infants who reach maturity before birth. This measure removes those births with health issues related to preterm birth. This variable is called term birth weight and is birth weight conditional on the gestational age of at least 37 weeks.

I also use more conventional outcomes related to birth weight and gestational age. Specifically, low birth weight, very low birth weight, and extremely low birth weight are defined as dummies to indicate a birth weight of less than 2,500, 1,500, and 1,000 grams, respectively. In addition, preterm and very preterm birth are dummies to indicate a gestational age of less than 37 and 27 weeks, respectively. Finally, I construct a dummy to indicate the incidence of low Apgar score, that is, an Apgar score of less than 8.

3.3. County-Level Variables

The main source of county-level data used to build trade exposure and vulnerability measure (explained in section 3.5) is the County Business Pattern (CBP) data extracted from Eckert et al. (2020). The information on industry-specific imports and exports is extracted from Schott (2008). The tariff schedule data is taken from Feenstra et al. (2002). I use concordance files provided by Pierce & Schott (2009) to convert industry codes between CBP data and the import-export-tariff database. Measures of the China trade exposure index and Multifiber Arrangement (MFA) index are extracted from Pierce & Schott (2020). (Feenstra et al., 2002; Pierce & Schott, 2009, 2020)

The data on various measures of earnings and welfare receipts are extracted from the Bureau of Economic Analysis (BEA). Weekly and quarterly wage data are extracted from the Quarterly Census of Employment and Wages (QCEW) database. The county-level government revenues and expenditures data are taken from Pierson et al. (2015). The annual county-level housing price index is extracted from the Federal Housing Finance Agency. Measures of natural resource extraction are taken from Bartik et al. (2019). Pollution data by county and year are extracted from the Environmental Protection Agency. Finally, county-level arrest data is based on the FBI's Uniform Crime Reporting (UCR) data extracted from Kaplan (2019).

3.4. Other Data Sources

In the section on mechanisms, I also use decennial census data for the years 1990 and 2000 extracted from Ruggles et al. (2020). Moreover, I employ the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) data extracted from Flood et al. (2018).

3.5. Vulnerability Index

The construction of the local vulnerability index to trade liberalization is based on the partner country's relative advantage in specific industries and the local dependence on employment in those industries. This procedure in building exposure measures is borrowed from the literature that explores local effects of international trade (Autor et al., 2013; Cherniwchan, 2017; Choi et al., 2021; Hakobyan & McLaren, 2016; Pierce & Schott, 2020). Specifically, I follow Hakobyan & McLaren (2016) and Choi et al. (2021) to generate an industry-specific measure of Mexico's *Revealed Comparative Advantage* (RCA) measured in 1990. The comparative advantage of a specific industry is based on its efficiency in producing a particular volume of products versus other trading partners. This measure requires detailed and usually unobserved measures of industry-specific efficiency and plant-based cost data. While such measures are difficult to attain, the RCA is what the market informs outsiders about the industry-specific efficiency of a country. The RCA measures, for each industry, the amount of Mexican export to all other countries relative to the total export of all other countries:

$$RCA_i = \frac{\left(\frac{X_i^{mexico}}{X_i^{row}}\right)}{\left(\sum_i X_i^{mexico}\right) / \left(\sum_i X_i^{row}\right)} \tag{1}$$

Where i indexes industry and X export. The numerator is the share in exports of Mexico in industry i to the total export of the rest of the world in the same industry (both measured in 1990). The denominator is the share of total Mexican export relative to the total export of all other countries (again in 1990).

US counties with a higher pre-NAFTA reliance on industries for which Mexico's RCA is higher are more exposed to import competition and vice versa. Therefore, the county-level vulnerability measure is defined as follows:

$$Vulnerability_{c_{1990}} = \frac{\sum_{i=1}^{I} EMP_{ic}\tau_i RCA_i}{\sum_{i=1}^{I} EMP_{ic} RCA_i} \tag{2}$$

Where EMP represents the employed population in county c who work in industry i. The parameter τ represents ad-valorem equivalent tariff rates. All variables are measured in 1990 as the baseline pre-NAFTA year.

Figure 2 depicts boxplots of 1990 tariff rates on Mexican imports across several industries. Since tariff rates dropped to virtually zero by 2000, this figure's values also serve as a proxy for the observed drops in tariffs imposed on Mexico. The initial protection is primarily concentrated in agricultural, mining, and manufacturing industries. Figure 3 illustrates the statistical distribution of the vulnerability index. The top panel shows the density distribution for two sets of counties: below-median and above-median of the vulnerability index. The two bottom panels depict the box plots of the index for the two subsamples.

Between 1994 and 2000, tariff rates converged to zero (see Figure 1). Therefore, the changes in tariff rates are equivalent to the initial protection from import competition. Hence, *protection*, *exposure* to trade, and *vulnerability* refer to the same changes in trade competition, and I use them interchangeably. Furthermore, the focus of the empirical method and implementation of the vulnerability measure is to exploit the statutory changes in trade policy. This method is more preferred than just using changes in actual imports since import values are dependent on local demand, which is potentially endogenous and could be correlated with, for instance, county-level income (a confounder in birth outcome equations).

3.6. Summary Statistics

The top panel of Figure 4 depicts quartiles of vulnerability index across US counties. Counties in East-South-Central, South Atlantic, New England, and parts of Pacific census divisions are among highly exposed counties. The bottom panel of Figure 4 illustrates birth weight distribution. Counties in Midwest, West, and Northeast are at the top quartiles of birth weight distribution.

Summary statistics of the NCHS final sample are reported in Table 1 for mothers residing in counties with below- and above-median of the vulnerability index. The average birth weight is about 3,332 and 3,346 grams in below and above median vulnerability counties, respectively.

Other birth statistics are quite similar in both groups. About 6 and 1 percent of births are categorized as low birth weight and very low birth weight, respectively. The average Apgar score in both subsamples is 8.9. Roughly 48.8 percent of infants are female. Approximately 12 percent of births are to teenage mothers (below age 20). About 5 percent of mothers in the sample have less than high school education. The average vulnerability index in the sample of the below-median vulnerability index is 0.7 and in the above-median index is 1.7. In interpreting the main results, I sometimes use the difference between a county at the 90th percentile of vulnerability and a county at the 10th percentile as the benchmark shock, a difference of roughly 2 units. This difference is also equivalent to the difference between the average vulnerability of counties above the median of the index to a county with zero exposure.

During the study period, imports from China experienced a sharp rise and, in some cases, overlap with industries where NAFTA granted tariff reductions to Mexico. To account for this potential confounder, I follow Pierce & Schott (2020) and construct an index for local exposure to Chinese imports. As expected, this index is larger in the sample of above-median vulnerability (1.3) versus in the sample of below-median (0.9). In addition, I also report and discuss summary statistics of the county-level variables in Appendix A.

4. Empirical Methodology

I start the analysis by implementing a series of event studies that examine changes in birth outcomes of infants born in different years relative to NAFTA-implementation-year in counties with different exposure to trade liberalization. Specifically, I employ regressions of the following form:

$$y_{icst} = \alpha + Vul_{cs}^{1990} \times \{\sum_{k=\underline{T}}^{1993} \xi_t I(k=t) + \sum_{k=1995}^{\overline{T}} \zeta_t I(k=t)\} + \beta X_{icst} + \delta Z_{cst} + \lambda_c + \gamma_{st} + \varepsilon_{icst} \tag{3}$$

Where y is the birth outcome of infant i born in county c in state s and year t. The parameter Vul represents the vulnerability of county c in the year 1990. The parameters ξ and ζ represent pre-trend and post-trend coefficients, respectively. I should note that the coefficient of 1994 is dropped so that all marginal effects are compared with the values of 1994 birth cohorts.

$I(.)$ is a unit function that equals one if its argument is true and zero otherwise. In X, I include a series of dummies for infant and parental characteristics, including infant's gender, infant's birth order, maternal race, maternal ethnicity, maternal education, maternal age, maternal marital status, paternal age, paternal race, and paternal ethnicity. Since a considerable portion of the sample has missing values for parental characteristics, specifically for paternal information, I assign a missing indicator to each category of parental covariates. Specifically, I add a dummy that equals one if the specific category is missing for a record and zero otherwise. The parameter λ represents county fixed effects that absorb all time-invariant county characteristics. To control for all state policy changes and shocks common across counties within a state and year, I include state-by-year fixed effects (represented by γ). The matrix Z includes the county-level China trade exposure measure (extracted from Pierce & Schott (2020)) interacted with year fixed effects. Although I include controls for China exposure in the preferred models, I avoid including county controls as those are potential pathways and endogenous controls. Finally, ε is a disturbance term. Standard errors are clustered at the county level to account for serial correlations in error terms.[7]

In addition to the event study analyses, I implement difference-in-difference models in which the main independent variable is a dummy to capture post-versus-pre-NAFTA that is interacted with the vulnerability measure, as follows:

$$y_{icst} = \alpha + \varphi Vul_{cs}^{1990} \times Post_t + \beta X_{icst} + \delta Z_{cst} + \lambda_c + \gamma_{st} + \varepsilon_{icst} \tag{4}$$

Where all parameters are the same as in equation 3. $Post$ is a dummy that equals one for post-1994 years and zero otherwise. In this specification, φ is the parameter of interest that shows the changes in birth outcomes following NAFTA for a one-unit difference in vulnerability index.

For county-level analyses, I use a similar formulation, summarized below:

$$y_{cst} = \alpha + \varphi Vul_{cs}^{1990} \times Post_t + \delta Z_{cst} + \lambda_c + \gamma_{st} + \varepsilon_{cst} \tag{5}$$

[7] In Appendix K, I extensively explore the robustness of the standard errors to alternative methods of correction. I show that the statistical significance of the main results of the paper are robust to heteroscedastic-robust standard errors, clustering at the state-level, two-way clustering at the county and year level, and two-way clustering at the county and state-year. Moreover, several recent studies suggest that the standard errors in shift-share research designs (as in the current paper) could over-reject the statistical tests (Adão et al., 2019; Borusyak et al., 2022). In the same appendix, I also show that the results are robust to implementing standard error correction technique developed by Adão et al. (2019).

Where the outcomes are at the county by state by year level. All parameters contain the same information as equations 3 and 4.

5. Results

5.1. Selective Fertility

Changes in trade policy and its differential impacts across counties could lead to population inflow and outflow (Arends-Kuenning et al., 2018; Burgess et al., 2010; Greenland et al., 2019). Moreover, parents may respond to observed changes in local labor markets by changes in their childbearing decisions (Anukriti & Kumler, 2019; Gries & Grundmann, 2014; Li et al., 2022; Schaller, 2016). Both migration and selection into parenthood could confound the estimations of birth outcome equations if they are correlated with other infants' health determinants. For instance, if more white women decide to enter parenthood (either as a result of inflow-migration of whites or higher childbearing of already resident white mothers), then the marginal effects of equations 3 and 4 underestimate the true effects (assuming the effects are negative) as whites have, on average, better birth outcomes due to unobservable reasons that cannot simply be captured by the inclusion of race dummies. Similarly, we would observe overestimated effects if the share of low-educated birth-giving mothers rises due to worsening economic conditions following trade liberalization. To explore this potential endogenous fertility issue, I collapse the NCHS sample at the county and year level and implement regressions of the form introduced in equation 5, in which the outcomes are fertility and the share of birth to different subpopulations. The results are reported in Table 2. I start by examining birth counts and log birth counts as the outcome (columns 1-2). The results suggest small and insignificant changes following NAFTA for both outcomes. In addition, I explore the change in sex ratio that could confound the birth outcome equations as there are systematic differences between birth outcomes of females and males (Challis et al., 2013; Renzo et al., 2007). However, I do not find evidence for such endogenous sex ratio changes (column 3). The point estimate is small in magnitude and statistically insignificant.

Next, I explore the share of birth to parents of different subpopulations based on sociodemographic characteristics. I observe small increases in the share of white mothers (column 4). Although the point estimate is relatively small (1.4 percent change from the mean of the outcome), it is statistically significant. However, the results do not provide evidence of changes in the share of blacks, teenage mothers, low-educated mothers, white fathers, black fathers, and young fathers. There is evidence of reductions in the share of maternal education missing and increases in the share of paternal age information missing. Overall, these results do not point to a robust, consistent, and significant endogenous fertility pattern. Increases in the share of white mothers could actually lead to underestimating the negative impacts as whites have better outcomes. Reductions in the share of missing information on maternal education could also suggest the same direction as usually uneducated women do not respond to the question of education (Cheema, 2014; Cox et al., 2014). Therefore, the results presented in the following subsection are a lower bound of true effects.

5.2. Event Study Results

The event study results introduced in equation 3 are reported in the top and bottom panels of Figure 5 for birth weight and gestational age, respectively. For each event-time marginal effect, I show the coefficient for three specifications: First, the baseline model that includes county fixed effects, year fixed effects, and covariates; Second, a model that adds state-year fixed effects so that the variation comes from the comparison of within-state counties with different exposure to trade before and after the trade policy change; Third, a specification that adds to the baseline with state-year fixed effects a series of county-level exposure measures to China import interacted with year fixed effects. Pre-trend coefficients are close to zero and, in almost all cases, statistically insignificant. This pattern of coefficients rules out the concern that the results are driven by preexisting trends in birth outcomes or pre-NAFTA differences in health trends of counties with different vulnerability values. Post-reform coefficients are still indistinguishable from zero for up to two years. This fact is not unexpected for two reasons. First, for some industries, NAFTA implementation was an announcement of future tariff reductions, and one would expect a delay in the effects. Second, for industries with immediate changes in tariffs, the firm adjustments and labor market adjustments take time; hence the effects may be delayed (Dix-Carneiro, 2014; Dix-Carneiro et al., 2017). After roughly 2-3 years, the effects start to rise (in magnitude) and become indistinguishable from zero. The coefficients peak their magnitude around 2000. More noticeably, the effects do not reveal any revival for more than a decade after the reform. These negative effects on birth outcomes and a similar pattern of effects can also be observed in Figure 6. The effects of full-term birth weight are very similar to birth weight (top panel). As an adverse outcome, the effects of preterm birth do not reveal a pre-trend and start to rise in the years following NAFTA. I continue to show the event study analyses for other birth outcomes in Appendix B. I observe a quite similar pattern of effects for alternative measures of infants' health.

5.3. Difference-in-Difference Results

The difference-in-difference results of equation 4 are reported in Table 3 for specifications that include county fixed effects, state-year fixed effects, China exposure by year fixed effects, and a full parental control set. Comparing most- and least-exposed deciles of vulnerability index (roughly 2-units difference) after trade liberalization versus before, birth weight and gestational age reduce by 11 grams and 0.07 weeks, respectively. To check whether the effects are concentrated on one outcome and appear on the other as a repercussion of the first, I explore the effects on fetal growth, gestational age-adjusted birth weight, and full-term birth weight. The effect on fetal growth is negative and significant, suggesting a reduction of 0.14 grams per week of gestation (for a 2-units difference in vulnerability). For an average of 38 weeks of gestation, this effect adds up to about 5.2 grams, roughly half of the effect on birth weight, implying the negative effects operate through both birth weight and gestational age channels. This fact is also confirmed by smaller marginal effects of alternative measures of birth weight in columns 4-5 versus column 1.

The effects on adverse birth weight outcomes suggest larger impacts relative to the mean of outcomes. For example, for a 2-unit difference in vulnerability index post-trade, the probability of low birth weight, very low birth weight, and extremely low birth weight increases by 26, 5.4, and 3.4 basis-points, equivalent to 4.3, 4.9, and 6.9 percent changes from the mean of the outcomes, respectively. Although these outcomes are conventional definitions and widely used measures, they are based on arbitrary thresholds of birth weight distribution. In Figure 7, I depart from these thresholds and employ a series of low birth weight outcomes for which the thresholds flexibly varies between 1,000 and 3,000 grams. The top panel shows the results of marginal effects and 95 percent confidence intervals. The bottom panel reports, for each respective outcome, the percentage change from the mean implied by effects and intervals reported in the top panel. While the marginal effects diminish in size for lower threshold definitions, the percent change of effects reveals a virtually monotonously increasing pattern of larger effects for lower thresholds. These results suggest that the effects are larger for infants at the lower tails of birth weight distribution.

Returning to Table 3, I also observe a similar pattern for gestational age as the adverse gestational outcomes of columns 9-10 reveal. For a 2-unit difference in vulnerability index post-trade, preterm birth and very preterm birth increase by 49 and 2.4 basis-points, equivalent to a rise of 4.8 and 4.1 percent from the mean of their respective outcomes. Comparing these implied percent changes with those of gestational age in column 2, one can observe the largest negative impacts on infants at the lower tails of gestational age distribution.

As an alternative measure of health at birth, Apgar score is a clinically evaluated and more qualitative measure of infants' health. The results suggest significant reductions in Apgar score (column 11). Similar to birth weight and gestational age, the effect is considerably larger for low Apgar score, suggesting 36 basis-points rises off a mean of 0.029.

5.4. Heterogeneity across Subsamples

Several studies document that the effects of recent trade liberalizations in the US and specifically NAFTA have been much larger among blue-collar workers and those with lower skills and human capital (Autor et al., 2019; Autor et al., 2013; Gould, 2021; Hakobyan & McLaren, 2016, 2017; Pierce & Schott, 2020). Therefore, one would expect to observe relatively larger reduced-form effects on birth outcomes for these subpopulations. I use mother education as a proxy for mother and father human capital and focus on a subsample of mothers with less than high school education, those more likely to be blue-collar workers.[8] I replicate the main results for this subsample and report them in Table 4. The marginal effects and percentage changes from the mean of outcomes suggest larger effects than those of Table 3 for birth weight-related outcomes and adverse gestational age outcomes. For instance, comparing pre-post-NAFTA and the least- and most-trade-exposed records based on deciles of vulnerability, low birth weight and preterm birth increase by 5.5, 5.1, and 11.5 percent from the mean of their respective outcomes. These impacts are comparably larger than the average impacts for all mothers in Table 3. In addition, the marginal effects of birth weight and fetal growth imply 52 and 122 percent increases in size, respectively. However, marginal effects of gestational age and Apgar score are virtually unchanged.

[8] The effects are, probably in most parts, driven by the impacts on husbands' job prospects. While the NCHS data does provide father's education information, there are many cases with missing information. However, in a marriage market based on assortative matching, mother's education is a good proxy for father's human capital (Siow, 2015).

Studies that explore prenatal shocks and birth outcomes usually find differential impacts across infants' sex. However, these gender-heterogeneous findings depend on the type of shock, the outcome of interest, and the subpopulation of the study. As a result, some studies suggest larger impacts on females (Ae-Ngibise et al., 2019; Chen et al., 2020; Wang et al., 2017), while others find the opposite (Clark et al., 2021; Rosa et al., 2019; Weinberg et al., 2008). To search for this potential heterogeneity, I replicate the main results for subsamples based on gender. The results are reported in Table 5 and Table 6 for female and male infants, respectively. Comparing marginal effects and their implied percentage changes from the mean of outcomes offers mixed insight. While the effects are comparable among both groups, we find slightly larger effects among females for several outcomes such as term birth weight, Apgar score, and preterm birth. For instance, for low birth weight and low Apgar score of females and a one-unit difference in vulnerability, the effects suggest a 2.1 and 7 percent rise from the mean, respectively. On the other hand, the same shock suggests a 2.2 and 5.7 percent change among males, respectively.

5.5. Additional Analysis

In Appendix D, I continue to search for heterogeneity across subsamples. The effects are slightly larger among teenage mothers, whose partners (and themselves) are probably less experienced and so more affected by local labor market demand shocks (Biagi & Lucifora, 2008; Hahn, 2009). The results, however, do not offer a differential impact across races. Although in both groups, I find significant effects and coefficients of comparable size to the main results. Furthermore, I find much larger effects when restricting the sample to mothers in counties at the top-quartile vulnerability index.

In the main analyses of the text, I avoid including county-by-year controls as they are endogenous controls. Indeed, the effects could operate through any post-NAFTA changes in county characteristics. Since there is theoretical and empirical evidence for many of these aspects to drive birth outcomes (discussed extensively in section 2.2), I also avoid implementing an instrumental variable strategy with selected county characteristics as endogenous variables and prefer reduced-form analysis.[9] However, I add several groups of county controls in separate sets

[9] The potential correlation with other county characteristics could simply violate the exclusion restriction assumption. This reduced-form technique is widely implemented in studies that employ shift-share designs (Choi et al., 2021; Hakobyan & McLaren, 2016; Pierce & Schott, 2020).

of regressions in Appendix E to search for suggestive evidence of which facets of counties are more likely to be the first stage outcome. In the first two tables of this appendix, I control for various income indicators and various measures of industry-specific employment. The fact that marginal effects drop considerably relative to the main results of Table 3 suggests that, at least, parts of the impacts are driven by shocks to income and employment. Next, I control for a wide array of social spending and observe reductions in the magnitude of the effects. Therefore, social transfers offer some insulation against the negative impacts of local labor market shocks. However, I do not find a discernible change in the effects once I control for the share of people in different demographic groups. This fact is also in line with the results of Table 2 that there are no changes in the demographic composition of births following NAFTA. In line with these results, Choi et al. (2021) also do not find a change in the population as a result of NAFTA.

Multifiber Arrangement (MFA) was an international trade agreement for clothing and textile imported from developing nations to developed countries. In 1995, MFA was phased out and replaced by the Agreement on Textiles and Clothing. The phase-out of MFA could endogenously influence the effects of NAFTA since many industries covered by MFA were those highly impacted by NAFTA. To address this endogeneity concern, I use the MFA exposure index from Pierce & Schott (2020), interact it with year fixed-effects, and include them in the regressions. The results, reported and discussed in Appendix C, reveal very similar effects to those in Table 3.

In Appendix F, I employ two alternative measures of vulnerability. First, I replace tariff rates in the numerator of equation 2 with imports from Mexico. Second, I drop the agricultural sector in the computation of the RCA measure. The results of both methods are comparable to the main findings of the paper.

Finally, one may be concerned that spillover effects of adjacent counties' labor market shocks affect own county's outcomes and these effects confound the estimations of birth outcomes. However, as shown in Figure 4, vulnerability indices of adjacent counties in many cases within a given region are highly correlated. To search for this potential concern, I aggregate tariff and hence vulnerability index at a level above county and below state, namely Pubic-Use Microdata Area (PUMA) level. I replicate the main results using this aggregated measure of conspuma and add conspuma fixed effects (instead of county fixed effects). The estimated results, reported and discussed in Appendix J, are quite comparable with Table 3.

6. Potential Mechanisms

6.1. County-Level Analysis

In this section, I explore the effects of NAFTA on a wide range of county characteristics. I implement event studies and regressions similar to equation 5 that account for county fixed effects and state-by-year fixed effects. For the analyses of the text, I focus on event studies as they provide a cleaner experiment to check pre-trends and compare them with post-trend coefficients. I start with measures of income and illustrate the results in Figure 8. While several pre-trend coefficients are statistically significant, they are in most cases insignificant for the years closer to NAFTA. More importantly, the magnitude of the pre-trend coefficients is substantially smaller than those of post-trend marginal effects. Post-NAFTA, the effects start to rise in magnitude and become statistically significant. For comparison purposes, all measures are divided by population and then standardized. Earnings, dividend/interest/rent income, non-farm income, and proprietor income reveal substantial reductions of about 0.2-0.4 standard deviation from the mean. In the meantime, withdrawal from retirement income rises steadily, consistent with the fact that more people started to enter into retirement, in line with studies that show the association between negative labor demand shocks and early retirement (Dorn & Sousa-Poza, 2008; Foote et al., 2018).

The event study of employment outcomes is reported in Figure 9. Again, all values are per capita and standardized with respect to the mean and standard deviation of the sample. For industries such as mining and utility, for which the initial protection was small, the post-trend coefficients reveal no significant change. However, for industries with higher exposure, specifically apparel, textile, and several other manufacturing industries, the coefficients suggest significant reductions in employment. Noticeably, these reductions do not show any evidence of revival for about 14 years following NAFTA.

Another possible change in counties' landscape is the increased dependence of the population on welfare payments as a result of worsening job prospects. For instance, there is empirical evidence that negative labor demand shocks (specifically following trade liberalization) increases the number of application for Social Security Disability Insurance (SSDI) and Trade Adjustment Assistance (TAA) programs (Bratsberg et al., 2013; Choi et al., 2021; Maestas et al., 2015; Ramrattan & Szenberg, 2010). In Figure 10, I search for NAFTA-induced changes in social insurance. As expected by reductions in employment and income, per capita employer contribution to social insurance falls significantly in post-NAFTA years. I do not observe a change in Income Maintenance Benefit but Current Transfer Receipts (summation of all welfare receipts) show a robust rise in magnitude and statistical significance following NAFTA. Therefore, in line with previous studies, social spending rise in counties with higher exposure to trade competition.

In addition to income, another potential outcome that could be affected by import competition is wealth in general and housing wealth in specific. Plant closure, mass layoffs, and worsening labor demand conditions could be translated into lower wealth and lower demand for housing, which can be detected in the housing prices. The event study results for the housing price index are depicted in Figure 11. To ease the interpretation, I standardized the index. The pre-trend coefficients are, in almost all cases, very small and statistically insignificant. Up to two years after the implementation of NAFTA, the marginal effects are very similar to those of 1993-1994. However, with a delay, housing prices start to fall. The drop in the coefficients is quite large in magnitude and continues until 2006-2007. The drop in housing prices for a decade following NAFTA and for a 1-unit change in vulnerability index is about 0.3 standard deviation from the mean of the outcome.

In Appendix H, I show the difference-in-difference results for the outcomes studied in this section. Moreover, I evaluate three additional sets of outcomes. First, I show that exposure to trade is not associated with a significant change in health expenditure and education expenditure. Second, I find a significant increase in crime rates measured by per capita arrests. Therefore, if a higher crime rate has a causal impact on birth outcomes, I would expect that part of the effects could operate through decreases in neighborhood safety (Mark & Torrats-Espinosa, 2022; Masi et al., 2007). Finally, I do not find a change in the extraction of natural resources as measured by changes in total oil-gas production.

Cherniwchan (2017) provides suggestive evidence that exposure to NAFTA is associated with improved environmental air quality and reductions in criteria air pollutants. I explore pollution outcomes using difference-in-difference regressions and event study analyses related to the full specifications of equation 5. The results are reported and discussed in Appendix I. The findings, conditional on the set of county and state-year fixed effects, do not produce any evidence that pollution decreases following NAFTA. There could be two explanations for the observed difference between Cherniwchan (2017) and the results of Appendix I. First, Cherniwchan (2017) employs a different data source at the plant-level with much more detailed industry information. Second, while he uses log of emissions, I deflated values by county area as the same level of pollution has a different effect on larger counties than smaller ones. However, even if NAFTA improved air quality, it could positively impact birth outcomes (Currie & Schmieder, 2009; Currie & Schwandt, 2016). Therefore, the effects of this paper are a lower bound of true effects of negative income-employment shocks on birth outcomes.

6.2. NCHS Data Analysis

To further explore mechanisms of impact, I use the information on prenatal care on the NCHS data. First, I build two variables that indicate whether the mother had any prenatal doctor visits or had utilized any prenatal care during pregnancy. Second, I use difference-in-difference regressions of equation 4 and report the results in Table 7. Comparing top and bottom deciles of vulnerability before and after NAFTA, mothers are 24 and 23 basis-points less likely to have any doctor visits and utilize any prenatal care during pregnancy, respectively. The effects are statistically significant at 10 percent level. However, compared to the mean of the outcome, they suggest relatively small changes.

6.3. Census and ASEC-CPS Analysis

I supplement the analysis of mechanism channels by looking at two alternative data sources: Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) data for the years 1985-2010; and decennial census data for the years 1990 and 2000. I restrict both samples to women aged 15-45 to have a similar demographic sample as the NCHS analysis.

The advantage of these two data sources is additional information that the NCHS files lack. The disadvantage is that none of these data sources report the county of residence.[10] To address this issue, I employ two methods. First, for the ASEC-CPS data, I focus on the industry of occupation of the head of the household. Therefore, I can construct the exposure measure at the household level rather than the county-level. Second, for the census data, I follow Hakobyan & McLaren (2016) to exploit an alternative geographic variable: Consistent Public-Use Microdata

[10] The IPUMS-extracted data reports a county identifier. However, this is a de-identified variable that IPUMS predicts to be the county of residence based on other available geographic variables and population estimates. The de-identified counties add up to only about one-sixth of US counties.

Area (consistent-PUMA or simply conspuma).[11] The conspuma is a combination of counties that are economically tied together. To build the vulnerability measure in equation 2, I replace county with conspuma. The resulting vulnerability index has a quite similar distribution as the county-level vulnerability index of the NCHS analyses.

For the ASEC-CPS sample, I explore the effects on insurance, wage income, and welfare income outcomes. I implement regressions that include race dummies and state-year fixed effects. These results are reported in Table 8. The effects suggest significant reductions in the likelihood of having any insurance and presumably better-quality private insurance (columns 1-2). For instance, for a one-unit difference in vulnerability post-NAFTA, the likelihood of any health insurance drops by 90 basis-points, off a mean of 0.86. Similarly, they suggest a significant increase in reliance on (presumably lower-quality) public insurance (columns 3-4). While the literature on health insurance, health care access, and prenatal care on birth outcomes is mixed, several studies provide suggestive positive effects (Corman et al., 2019; Currie & Gruber, 1996).

The results on wage income suggest a decrease of $320 for a 1-unit difference in vulnerability, equivalent to about 1.5 percent from the mean (column 5). The effect on welfare income reveals an increase of $1, roughly a 4 percent rise from the mean.

The census sample suggests significant reductions in the occupational income score of the household's head, equivalent to a 0.7 percent drop from the mean for a 1-unit difference in the trade exposure index (column 7). Consistent with the results of Figure 11, I also observe a significant and sizeable reduction in housing value (column 8). For a difference of a 1-unit in the vulnerability measure post-NAFTA, housing wealth drops by, on average, roughly $7K, equivalent to a 5.6 percent change from the mean of the house value. There is no significant effect on the likelihood of being a house owner, though the coefficient is negative.

7. Discussion on the Magnitudes

In this section, I discuss the economic meaning of the implied magnitudes of the results reported in Table 3. I use the comparison of top-versus-bottom deciles of vulnerability index (a difference of 2-units) after the trade liberalization versus before as the default shock. The results

[11] Appendix J replicates the main results for a case in which the vulnerability index is constructed at the conspuma level and regressions include conspuma fixed effects instead of county fixed effects.

suggest a reduction in birth weight of 11 grams, an increase in the probability of low birth weight of about 13 basis-points, and an increase in the likelihood of preterm birth of roughly 25 basis-points.

The important aspect of these results is that they only report intent-to-treat (ITT) effects and provide a lower bound for the true effects. Although heterogeneity analyses of section 5.4 provide suggestive evidence of larger impacts among low-educated mothers, they are still based on exposure assignment for the whole population. For the default shock (discussed above), I find reductions in per-capita earnings of about $2,500 (in 2020 dollars), an 8.6 percent fall from the mean (Appendix E). Chung et al. (2016) explore the birth outcome effects of Alaska Permeant Fund payments and find that an increase of $2,700 (converted into 2020 dollars) is associated with 18 grams of additional birth weight. This effect is only slightly higher than the ITT effects on birth weight in the current study (assuming that income is the primary channel). Hoynes et al. (2015) explore the infants' health effects of income rises due to tax rebates under the Earned Income Tax Credit (EITC) program. They find that an increase in income by about $2,500 (converted into 2020 dollars) is associated with about 13 grams of additional birth weight. This effect is quite similar to the intent-to-treat effects reported in Table 3.

Lindo (2011) explores the effect of parental job loss on birth outcomes. He finds an effect size of 4.5 percent reduction in birth weight. This effect is equivalent to a reduction of 150 grams (assuming an average birth weight of 3,300 grams). Among the top-quartile of trade exposure counties, the above-mention shock leads to about 5 percentage-points reductions in employment per capita. If we assume a labor force participation rate of 60 percent, this effect is roughly 8.3 percentage-points on those in the labor force. I also assume that at least one parent remains in the labor force post-NAFTA. Moreover, I assume that the impacts of NAFTA operate solely through its labor market channels. Using these first-stage results in employment, the treatment-on-treated effect of parental job loss due to NAFTA implies 133 grams lower birth weight.[12] This effect is comparable to the treatment-on-treated findings of Lindo (2011) and confirms the fact that reductions in income and employment are probably the primary channels.

[12] In a regression of per capita employment on vulnerability using a sample of top-quartile exposure counties, the marginal effect is 0.023 (se=0.011). The treatment-on-treated value is calculated as follows: $\left(\frac{0.0233\times 2}{0.60}\right)^{-1} \times 11$.

Hoynes et al. (2011) investigate the effects of the Supplemental Program for Women Infants and Children (WIC) on birth outcomes. They find intent-to-treat effects of about 2 grams and calculate treatment-on-treated effects of about 18-29 grams. Almond et al. (2011) examine the impacts of the introduction of the Food Stamp program on birth outcomes. They find intent-to-treat effects of 2.2 grams additional birth weight for exposure to the program among whites and 1.6 grams among blacks. However, their treatment-on-treated calculations suggest much larger effects: 15-20 grams among whites and 13-42 grams among blacks. Based on these results, the intent-to-treat effects of NAFTA (in magnitude) are equivalent to 40-60 percent of the treatment-on-treated effects of WIC and 35-90 percent of the treatment-on-treated effects of the Food Stamp program on birth weight.

To understand the relevance of the magnitude of the findings, I rely on the literature on later-life consequences of health at birth. One later-life consequence of being born at the low tail of birth weight distribution is extra hospital discharge costs. The hospital discharge costs for low birth weight infants are higher than those with normal birth weight. Almond et al. (2005) use the twin strategy to calculate the hospital costs of being born with below-normal birth weight in excess of the costs for normal birth weight. I use their cost estimation results to have a rough estimate of the intent-to-treat effects of NAFTA. In the year 2000, low birth weight infants counted to 307,000 singleton births. The vulnerability index changes by about one unit between counties at the above-median and below-median of vulnerability. The NAFTA exposure (for a 1-units higher vulnerability) implies an increase of 2.1 percent from the mean. This effect is approximately the change in low birth weight of infants in counties at the above-versus-below-median of the vulnerability index. Therefore, it suggests an increase of roughly 6,591 incidences of low birth weight. Therefore, based on Almond et al. (2005), this effect leads to an annual extra hospital cost of about $86M related to low birth weight.[13,14]

The effects of birth endowment also surface during adulthood and in the labor market. Behrman and Rosenzweig (2004) use the twin strategy method and show that birth weight and

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13 The dollar figures are in year 2020 dollars.

14 This number is calculated using Table V in their paper. I calculate the share of each strata of birth weight in 2000 Natality files and compute the weighted average cost based on the costs associated with each strata of birth weight in their paper to get average excess cost of low birth weight of $13,112. Since the marginal effect of low birth weight for above-median of trade exposure counties (roughly half the births in the nation) points to a rise of 6,600 incidences in year 2000, one can obtain an extra cost of $86M.

fetal growth have a sizeable impact on education and earnings. They find that each additional 100 grams of birth weight is associated with a 1.7 percent rise in earnings. Using this number and the intent-to-treat effects of NAFTA, I obtain a reduction in income of about 0.2 percent. I should again highlight that this effect could be much larger for the treated population. For instance, if we believe that the effects are primarily driven by employment loss of parents, I argued that the treatment-on-treated effect could be as large as 145 grams, suggesting a reduction in adulthood income by 2.5 percent.

8. Conclusions

Trade liberalization changes the landscape of local areas differentially and in various aspects. Like any other transition, the transition from protection to liberalization could be beneficial for some groups and harmful for others. Such changes bring observed impacts and sometimes effects that are usually unnoticed. This paper revealed one aspect of trade liberalization: the impact of NAFTA on infants' health outcomes. The results of this paper have two important features useful for policy designs following trade liberalization. First, since infants are vulnerable and their health outcomes are associated with medium-term, long-term, and even intergenerational impacts, it is of policy relevance to recognize the effects on their health outcomes. Moreover, the answer also calls for policies that aim at helping pregnant mothers in trade-exposed areas following trade liberalization. Second, the results help to implement a more optimal cost-benefit welfare analysis of trade liberalization.

I started with an event study design to compare the birth outcomes of infants born in different years relative to the implementation of NAFTA in counties with different vulnerabilities to trade competition. The outcomes do not uncover a significant pre-trend for several years preceding NAFTA. However, post-NAFTA, the negative effects appear in birth outcomes and last for more than a decade without any evidence of revival. In addition, I show that these effects are not driven by selective fertility and endogenous migration.

The difference-in-difference results suggest significant effects on a wide range of birth outcomes with much larger impacts for infants at the lower tails of birth weight, gestational age, and Apgar score distribution. For instance, comparing mothers at the top-versus-bottom deciles of vulnerability distribution post-versus-pre-NAFTA, low birth weight, preterm birth, and low Apgar score increase by 4.2, 4.8, and 12.6 percent from the mean of their respective outcome. I also find that these effects are considerably larger among low-educated mothers.

I use a wide array of data sources to search for potential pathways. I find sizeable and significant reductions in various measures of earnings and income. On the contrary, I find increases in retirement income suggest more people are forced to exit the labor market. Additional analyses show that industries with a higher initial protection experience a larger drop in employment. Overall, employment measures suggest noticeable reductions in job prospects for highly affected counties. I also find significant increases in receipt of social insurance as the reliance of affected areas on welfare payments rises. Furthermore, relatively large reductions in the housing price index in the affected counties suggest a sharp drop in their housing wealth.

I use census and CPS data to search for additional mechanism channels. I find significant reductions in presumably better-quality private health insurance and increases in the use of public health insurance among women. Moreover, the results suggest that in higher trade-exposed areas, the household heads receive higher welfare payments, have lower wages, and have lower occupational income scores. In addition, I observe a significant and sizeable reduction in their housing wealth.

Finally, I provide a separate section to discuss the magnitude of the effects. I show that the point estimates, when put in the context, are quite similar to other studies. Finally, I relate the magnitude of the findings on low birth weight and birth weight to the extra hospital discharge costs associated with low birth weight and to adulthood labor market outcomes.

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Tables

Table 1 - Summary Statistics

	Below Median Vulnerability			Above Median Vulnerability		
	Observations	Mean	SD	Observations	Mean	SD
Birth Weight	44,507,425	3332.585	573.296	44,373,772	3346.642	574.741
Gestational Age	44,507,425	38.917	2.514	44,373,772	38.957	2.512
Fetal Growth	44,507,425	85.457	13.612	44,373,772	85.737	13.653
Gestation-Adjusted Birth Weight	44,507,425	3314.225	321.34	44,373,772	3317.661	318.169
Term Birth Weight	39,940,089	3412.02	479.11	39,918,013	3425.098	481.928
Low Birth Weight	44,507,425	0.061	0.24	44,373,772	0.06	0.237
Very Low Birth Weight	44,507,425	0.011	0.104	44,373,772	0.011	0.102
Extremely Low Birth Weight	44,507,425	0.005	0.074	44,373,772	0.005	0.073
Preterm Birth	44,507,425	0.103	0.303	44,373,772	0.1	0.301
Very Preterm Birth	44,507,425	0.006	0.077	44,373,772	0.006	0.076
Apgar Score	34,399,283	8.9	0.749	37,860,857	8.926	0.768
Low Apgar Score	34,399,283	0.029	0.168	37,860,857	0.029	0.168
Female	44,507,425	0.488	0.5	44,373,772	0.488	0.5
Mother White	44,507,425	0.77	0.421	44,373,772	0.813	0.39
Mother Black	44,507,425	0.164	0.371	44,373,772	0.139	0.346
Mother Other Races	44,507,425	0.064	0.245	44,373,772	0.047	0.212
Mother's Age<20	44,507,425	0.117	0.321	44,373,772	0.121	0.326
Mother's Age 20-24	44,507,425	0.25	0.433	44,373,772	0.264	0.441
Mother's Age 25-29	44,507,425	0.279	0.449	44,373,772	0.286	0.452
Mother's Age 30-34	44,507,425	0.229	0.42	44,373,772	0.217	0.412
Mother's Age 35-39	44,507,425	0.104	0.305	44,373,772	0.093	0.29
Mother's Age 40-45	44,507,425	0.021	0.144	44,373,772	0.018	0.135
Mother's Education less than HS	44,507,425	0.053	0.225	44,373,772	0.056	0.231
Mother's Education HS	44,507,425	0.149	0.356	44,373,772	0.156	0.363
Mother's Education HS Graduate	44,507,425	0.294	0.456	44,373,772	0.319	0.466
Mother's Education Some College	44,507,425	0.206	0.405	44,373,772	0.215	0.411
Mother's Education ≥ Bachelor	44,507,425	0.225	0.418	44,373,772	0.202	0.402
Mother's Education Missing	44,507,425	0.072	0.259	44,373,772	0.052	0.222
First-Time Mother	44,507,425	0.337	0.473	44,373,772	0.336	0.472
Mother is Married	44,507,425	0.669	0.471	44,373,772	0.666	0.472
Mother's Marital Status Missing	44,507,425	0.0001	0.003	44,373,772	0.0001	0.004
Father's Age < 25	44,507,425	0.18	0.384	44,373,772	0.191	0.393
Father's Age 25-34	44,507,425	0.461	0.498	44,373,772	0.47	0.499
Father's Age 35-44	44,507,425	0.191	0.393	44,373,772	0.176	0.38
Father's Age > 45	44,507,425	0.022	0.148	44,373,772	0.02	0.14
Father's Age Missing	44,507,425	0.146	0.353	44,373,772	0.143	0.35
Any Prenatal Visits	44,507,425	0.985	0.123	44,373,772	0.989	0.103
Any Prenatal Care	44,507,425	0.986	0.117	44,373,772	0.99	0.098
I(Year>1994)	44,507,425	0.637	0.481	44,373,772	0.635	0.481
Vulnerability Index	44,507,425	0.741	0.21	44,373,772	1.693	0.627
Exposure to China Imports Index	44,507,425	0.887	0.406	44,373,772	1.276	0.566

Notes. ***Birth weight*** is the infant's weight at birth and is measured in grams. ***Gestational age*** is the clinical estimation of the period between conception and birth, and is measured in weeks. ***Fetal growth*** is the weekly intrauterine weight gain of infant and is calculated as birth weight divided by gestational age. Gestational age-adjusted birth weight is computed from the predicted value of a regression of birth weight on gestational age. ***Term birth weight*** is the birth weight of infants who reach maturity, i.e., those with a gestational age of at least 37 weeks. ***Low birth weight*** is a dummy that equals one if birth weight is less than 2,500 grams. ***Very low birth weight*** is a dummy that equals one if birth weight is less than 1,500 grams. ***Extremely low birth weight*** is a dummy that equals one if birth weight is less than 1,000 grams. ***Preterm birth*** (premature birth) is a dummy that equals one if gestational age is less than 37 weeks. ***Very preterm birth*** is a dummy that equals one if gestational age is less than 27 weeks. ***Apgar score*** is a 5-minute clinical test for examining Appearance, Pulse, Grimace, Activity, and Respiration. It varies between 0-10. ***Low Apgar score*** is a dummy that equals one if Apgar score is less than 8.

Table 2 - Balancing Tests

	Outcomes:					
	Birth Counts	Log Birth Counts	Child Female	Mother White	Mother Black	Mother's Age <20
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	31.89586	0.00213	0.00031	0.01147***	-0.00127	0.00007
Vulnerability Index	(55.82162)	(0.00754)	(0.00021)	(0.00355)	(0.00317)	(0.00082)
Observations	76721	76721	76721	76721	76721	76721
R-squared	0.96906	0.98624	0.06081	0.9762	0.98605	0.9311
Mean DV	1242.075	5.838	0.488	0.793	0.150	0.117
%Change	2.568	0.036	0.064	1.446	-0.850	0.057
	Mother's Schooling <12	Mother's Education Missing	Father White	Father Black	Father's Age <25	Father's Age Missing
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	-0.00059	-0.00225**	0.00219	-0.00039	-0.00006	0.00468*
Vulnerability Index	(0.00332)	(0.00115)	(0.00426)	(0.00302)	(0.00171)	(0.00276)
Observations	76721	76721	76721	76721	76721	76721
R-squared	0.93375	0.99271	0.95695	0.95702	0.92727	0.93327
Mean DV	0.203	0.068	0.696	0.103	0.184	0.143
%Change	-0.293	-3.316	0.315	-0.381	-0.035	3.275

Notes. Standard errors, clustered at the county level, are reported in parentheses. Regressions are weighted using average birth counts in each cell. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1986-2010 and covers 3,074 counties.

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

Table 3 – Main Results

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.41496***	-0.03521***	-0.06952***	-3.94414***	-3.60594***	0.00131***
Vulnerability Index	(1.28387)	(0.00757)	(0.02425)	(0.8286)	(1.17918)	(0.00027)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02376	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.162	-0.090	-0.081	-0.119	-0.105	2.147
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00017***	0.00245***	0.00012*	-0.01967***	0.00182***
Vulnerability Index	(0.00007)	(0.00005)	(0.00059)	(0.00007)	(0.00662)	(0.00063)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03764	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.443	3.451	2.399	2.037	-0.221	6.281

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Table 4 – Heterogeneity Analysis across Subsamples: Effects on Low-Educated Mothers

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-8.20564***	-0.03009***	-0.15367***	-4.05667***	-5.59475**	0.00218***
Vulnerability Index	(2.39622)	(0.00976)	(0.05047)	(1.1675)	(2.35516)	(0.00051)
Observations	18432603	18432603	18432603	18432603	16073547	18432603
R-squared	0.05091	0.02158	0.04743	0.01952	0.05639	0.01489
Mean DV	3249.717	38.836	83.528	3291.616	3336.892	0.079
%Change	-0.253	-0.077	-0.184	-0.123	-0.168	2.761
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00043***	0.00017*	0.00324***	0.00023**	-0.01675***	0.00131*
Vulnerability Index	(0.00013)	(0.00009)	(0.00101)	(0.00011)	(0.00627)	(0.00075)
Observations	18432603	18432603	18432603	18432603	13645818	13645818
R-squared	0.00391	0.00236	0.01393	0.00335	0.03684	0.0093
Mean DV	0.013	0.006	0.128	0.008	8.892	0.033
%Change	3.279	2.757	2.529	2.864	-0.188	3.971

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Table 5 – Heterogeneity Analysis across Subsamples: Effects on Female Infants

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.21497***	-0.03475***	-0.06698***	-3.78472***	-3.57522***	0.00139***
Vulnerability Index	(1.27464)	(0.00785)	(0.02458)	(0.84753)	(1.14319)	(0.00032)
Observations	43368970	43368970	43368970	43368970	39195367	43368970
R-squared	0.05292	0.02714	0.04583	0.02501	0.05073	0.01771
Mean DV	3280.752	39.000	83.965	3323.303	3353.722	0.065
%Change	-0.159	-0.089	-0.080	-0.114	-0.107	2.134
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00023***	0.00018***	0.00233***	0.00012	-0.01995***	0.00189***
Vulnerability Index	(0.00009)	(0.00006)	(0.00059)	(0.00007)	(0.00659)	(0.00062)
Observations	43368970	43368970	43368970	43368970	35250013	35250013
R-squared	0.00516	0.00335	0.01634	0.00403	0.03925	0.00839
Mean DV	0.011	0.005	0.096	0.006	8.925	0.027
%Change	2.064	3.648	2.426	1.944	-0.224	7.003

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Table 6 – Heterogeneity Analysis across Subsamples: Effects on Male Infants

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.60381***	-0.03564***	-0.0719***	-4.0944***	-3.64076***	0.00124***
Vulnerability Index	(1.35962)	(0.00751)	(0.02598)	(0.84038)	(1.26717)	(0.00027)
Observations	45512227	45512227	45512227	45512227	40662735	45512227
R-squared	0.05461	0.02382	0.0494	0.02186	0.05575	0.01403
Mean DV	3395.682	38.877	87.151	3308.924	3481.052	0.056
%Change	-0.165	-0.092	-0.083	-0.124	-0.105	2.205
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00031***	0.00016**	0.00256***	0.00013*	-0.01939***	0.00176***
Vulnerability Index	(0.00009)	(0.00006)	(0.00063)	(0.00008)	(0.00668)	(0.00065)
Observations	45512227	45512227	45512227	45512227	37010127	37010127
R-squared	0.00481	.00334	0.01407	0.00413	0.03606	0.00876
Mean DV	0.011	0.005	0.107	0.006	8.902	0.031
%Change	2.803	3.249	2.392	2.119	-0.218	5.665

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Table 7 - Exploring Mechanism Channels

	Outcomes:	
	Any Prenatal Visits	Any Prenatal Care
	(1)	(2)
I(year>1994) × Vulnerability Index	-0.00121*	-0.00116*
	(0.0007)	(0.00062)
Observations	88881197	88881197
R-squared	0.02516	0.02537
Mean DV	0.987	0.988
%Change	-0.122	-0.117

Notes. Standard errors, clustered at the county level, are reported in parentheses. Regressions are weighted using average birth counts in each cell. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Table 8 - Exploring Mechanism Channels Using Current Population Survey and Census Data

	Outcomes and Data Source:								
	ASEC of Current Population Survey Data 1985-2010						Census 1990-2000 (5%)		
	Any Insurance	Any Private Insurance	Any Public Insurance	Any Insurance on Medicaid-SCHIP	Wage Income of Householder	Welfare Income of Householder	Occupational Income Score of Householder	House Value	House Owner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
I(year>1994) × Vulnerability Index	-0.00895***	-0.01186***	0.00306***	0.00347***	-319.49***	1.04581**	-0.16299***	-7.17242***	-0.00288
	(0.00304)	(0.00407)	(0.00102)	(0.00116)	(106.48)	(0.48646)	(0.03148)	(1.93544)	(0.00188)
Observations	156804	156804	156804	156804	182,945	182945	4504041	3322369	5030386
R-squared	0.04046	0.05028	0.02359	0.02593	.042	0.01475	0.03577	0.34841	0.07052
Mean DV	0.860	0.809	0.077	0.062	22,003.56	25.328	25.095	127.919	0.641
%Change	-1.040	-1.466	3.973	5.601	-1.45	4.129	-0.650	-5.607	-0.450

Notes. Standard errors are reported in parentheses. Standard errors in columns 1-6 are clustered at the state level. Standard errors in columns 7-9 are clustered at the conspuma level. Regressions are weighted using IPUMS person weights. All regressions include state-by-year fixed effects and China exposure index by year fixed effects. The regressions also control for individual race and age dummies. The data covers women aged 15-45.

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

Figures

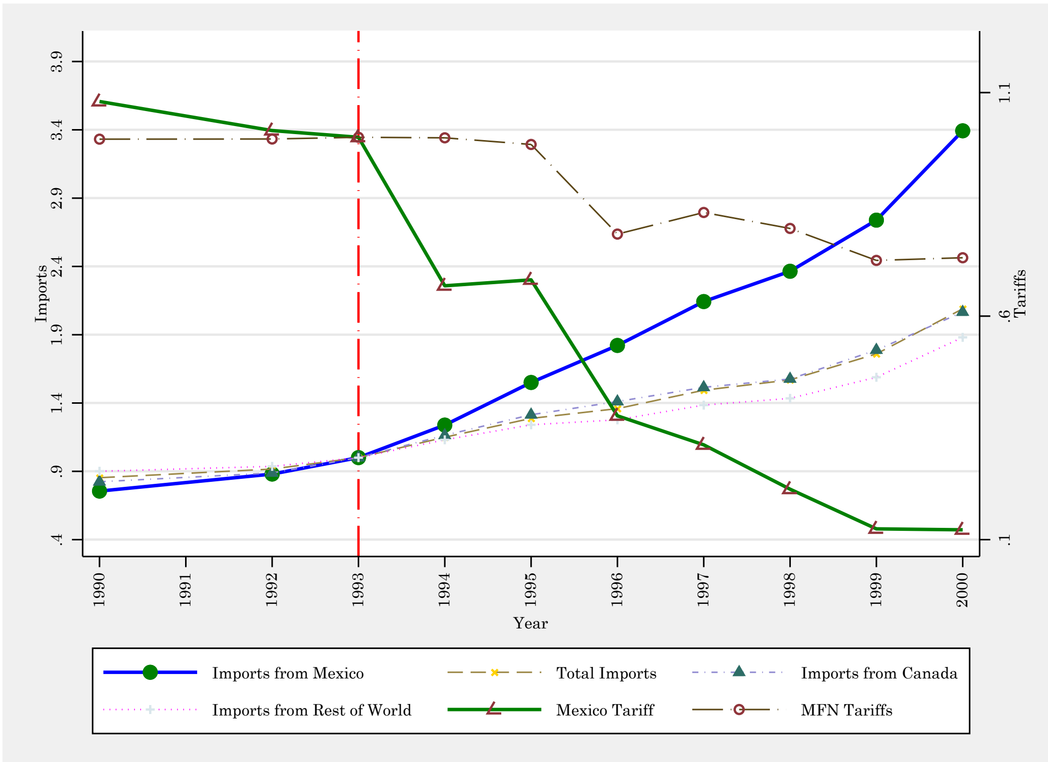


Figure 1 - Changes in Tariffs and Imports from Mexico, Canada, and the Rest of the World

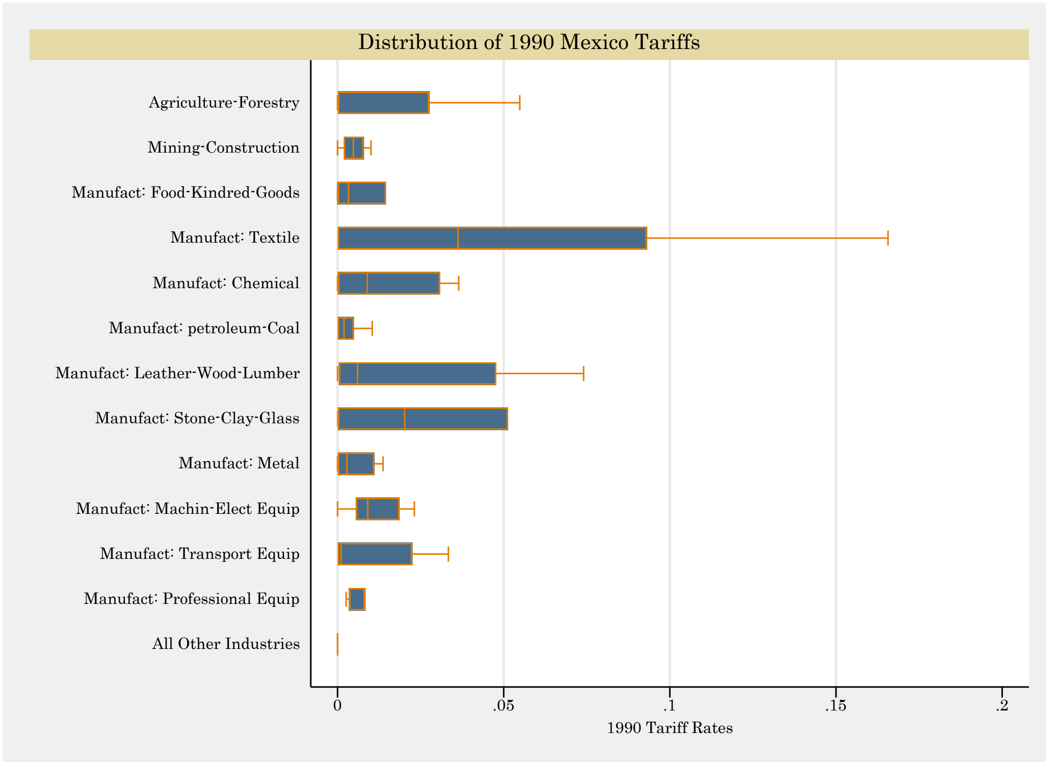


Figure 2 - Distribution of 1990 Mexico Tariff Rates across Industries

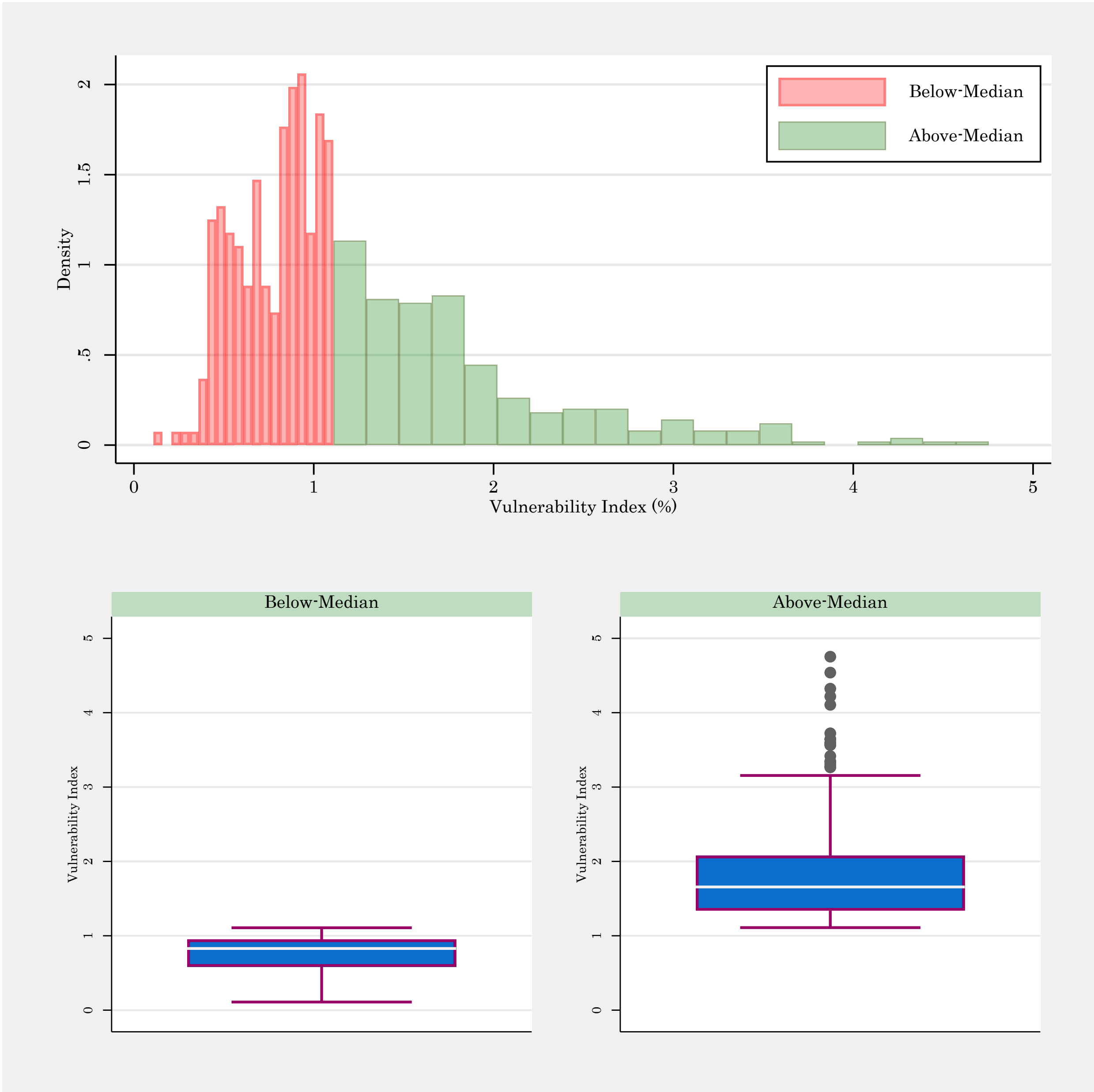


Figure 3 - Statistical Distribution of Vulnerability Index

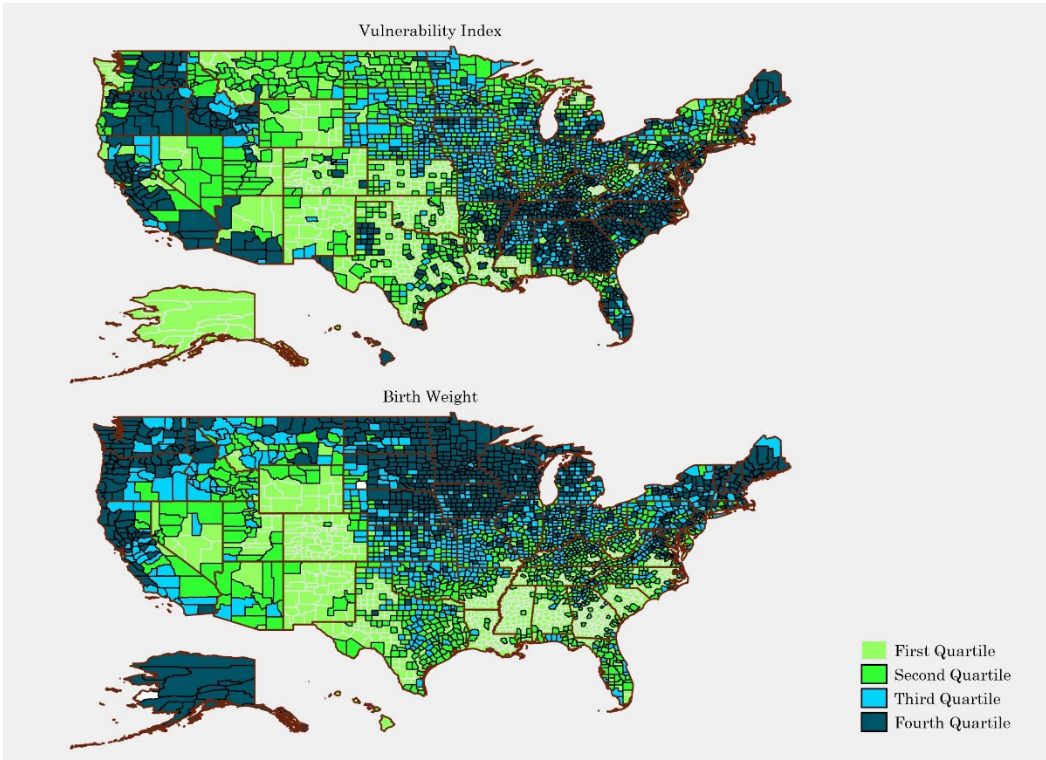


Figure 4 - Geographic Distribution of Vulnerability Index and Birth Weight across US Counties

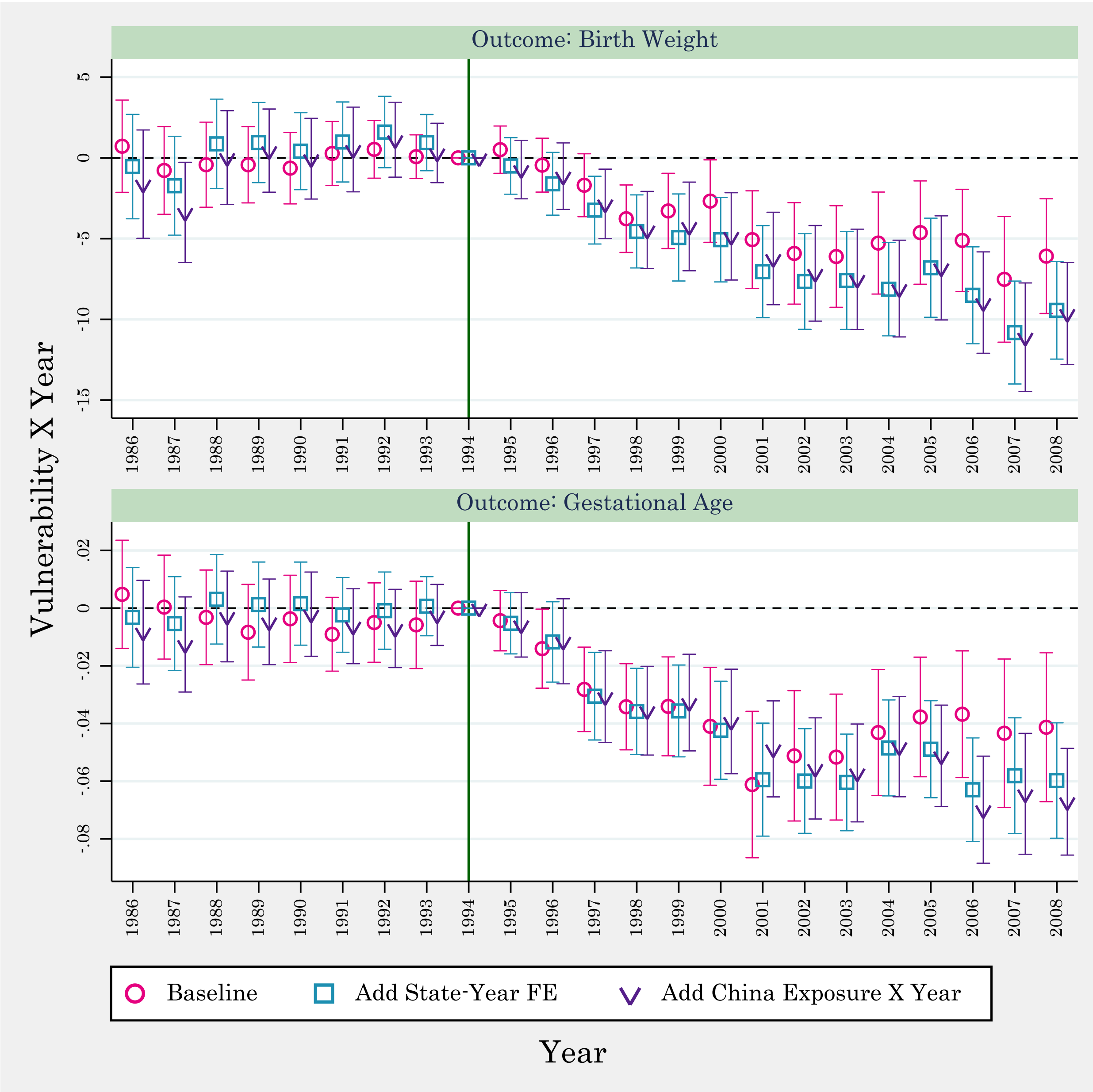


Figure 5 - Event-Study Analysis for the Effect of NAFTA on Birth Outcomes: Birth Weight and Gestational Age

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

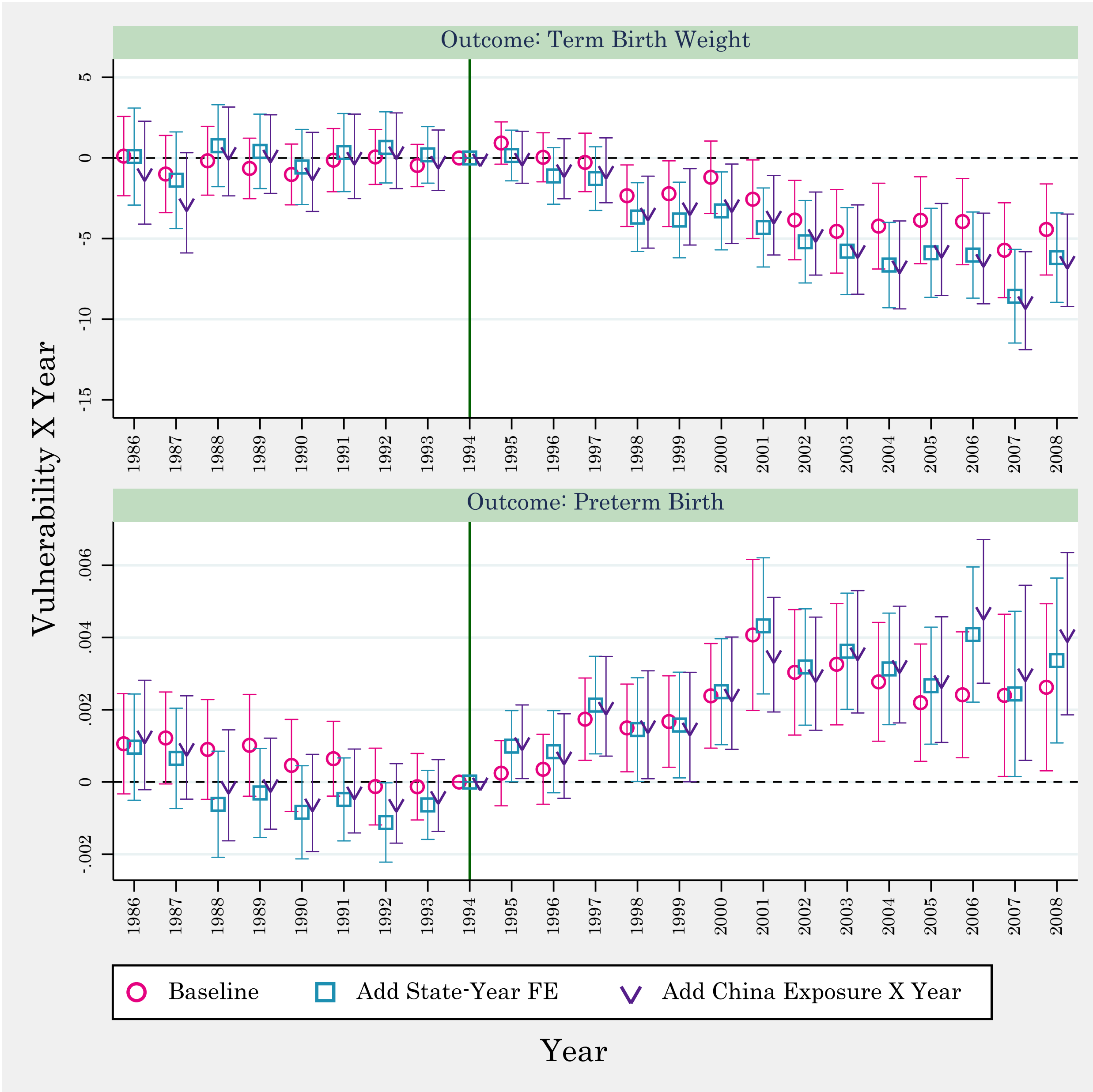


Figure 6 - Event-Study Analysis for the Effect of NAFTA on Birth Outcomes: Gestational-Age-Adjusted Birth Weight and Premature Birth

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

Figure 7 - Exploring the Effects and Percent Changes from the Mean for Various Thresholds of Low Birth Weight

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

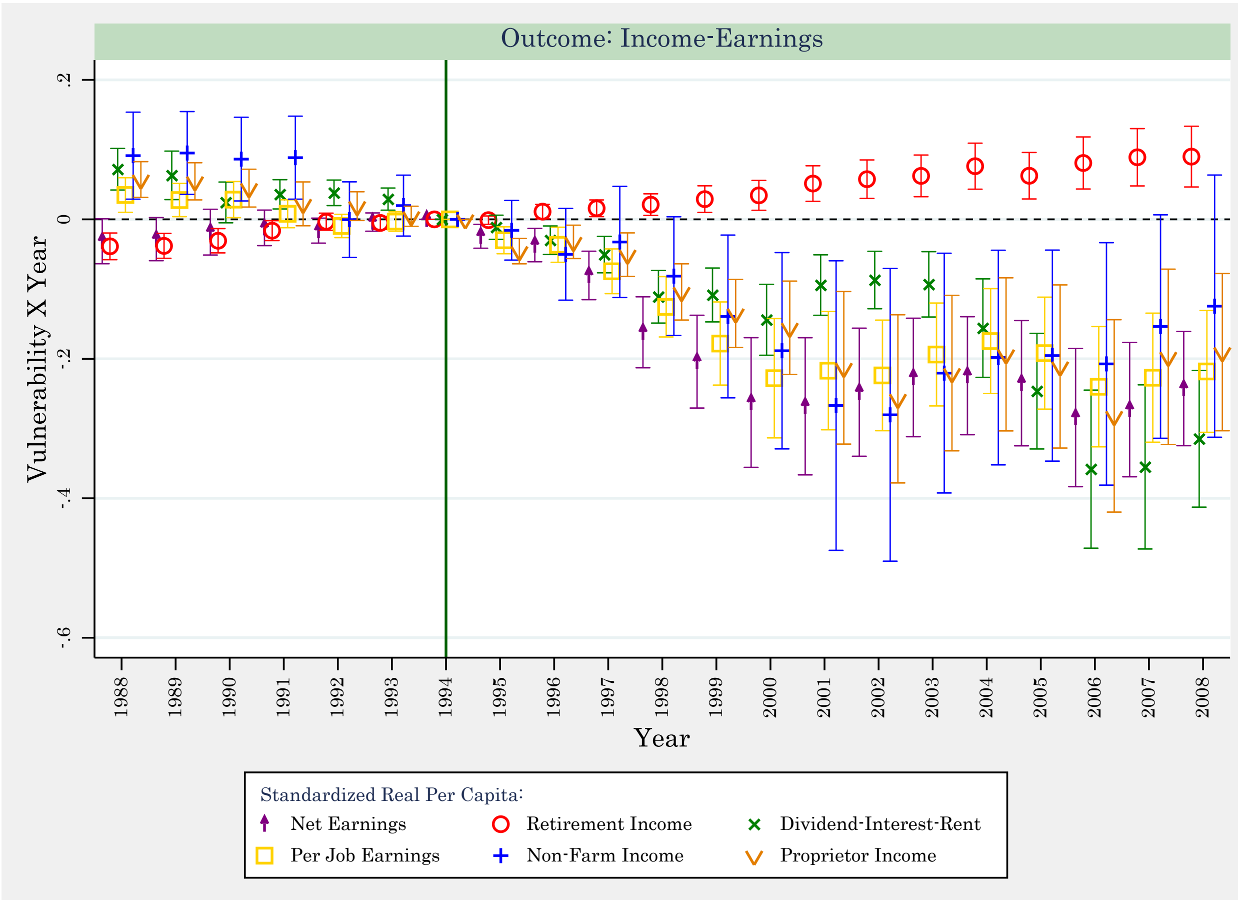


Figure 8 - Event-Study Analysis for the Effect of NAFTA on County Income

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties. All outcomes are standardized with respect to the sample mean and standard error.

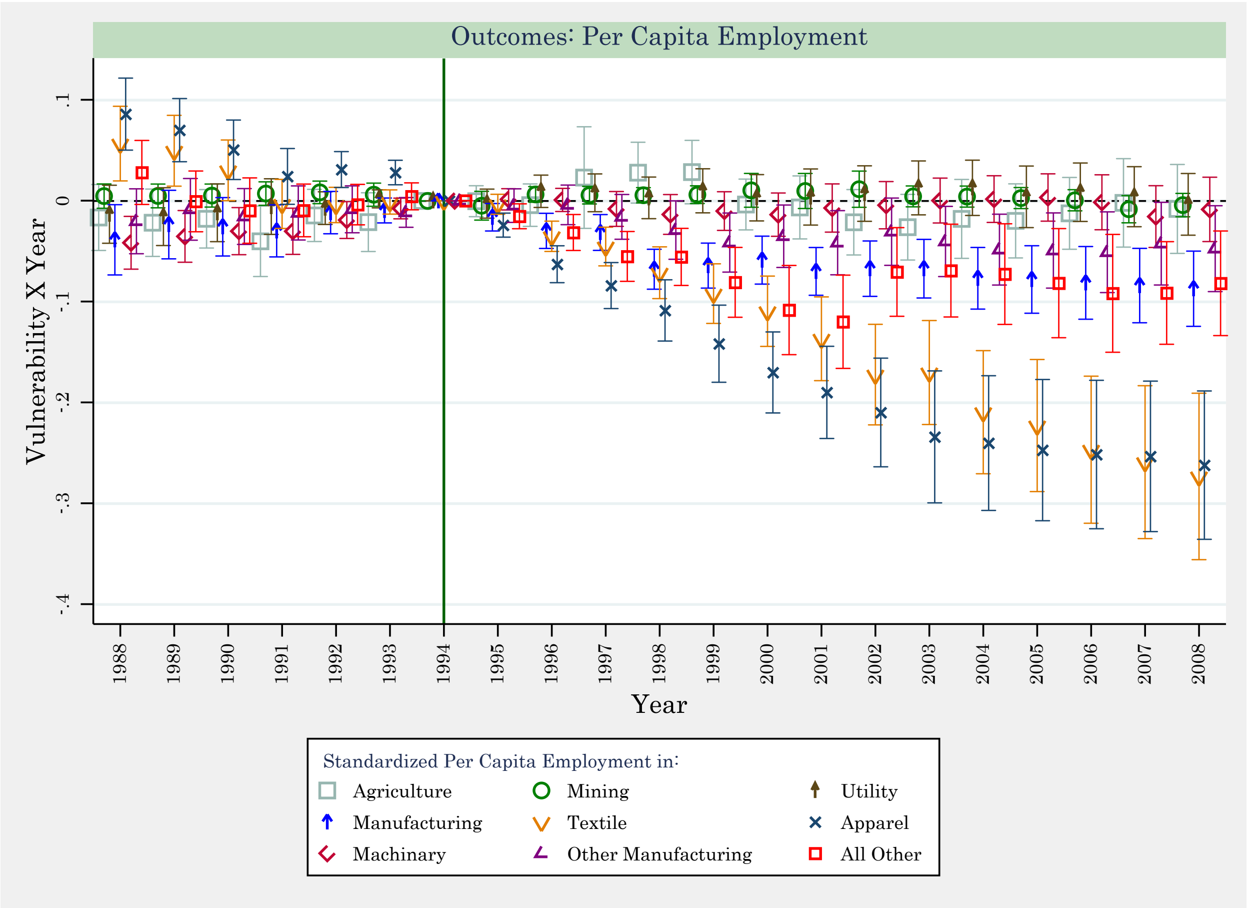


Figure 9 - Event-Study Analysis for the Effect of NAFTA on Employment Outcomes

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties. All outcomes are standardized with respect to the sample mean and standard error.

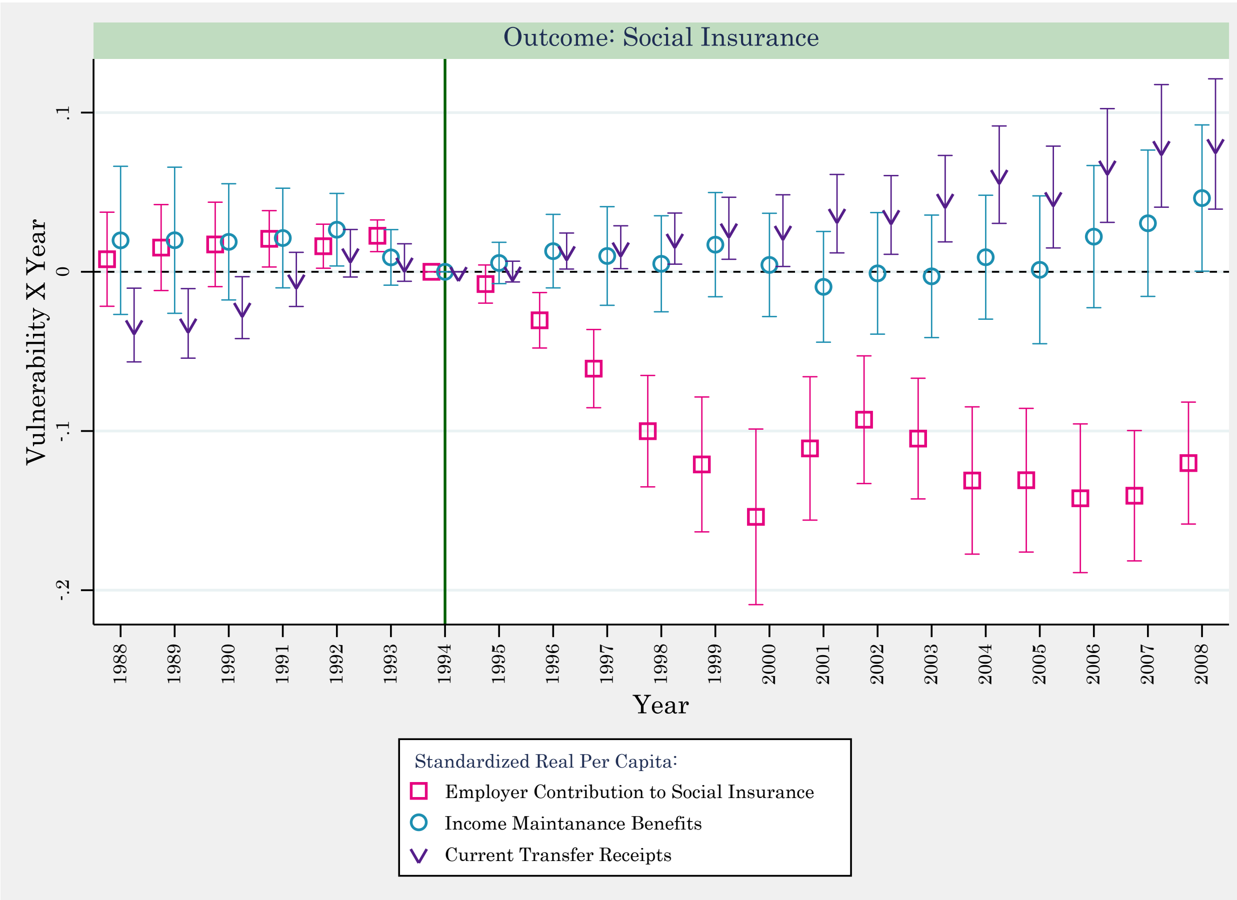


Figure 10 - Event-Study Analysis for the Effect of NAFTA on Social Insurance Receipts

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties. All outcomes are standardized with respect to the sample mean and standard error.

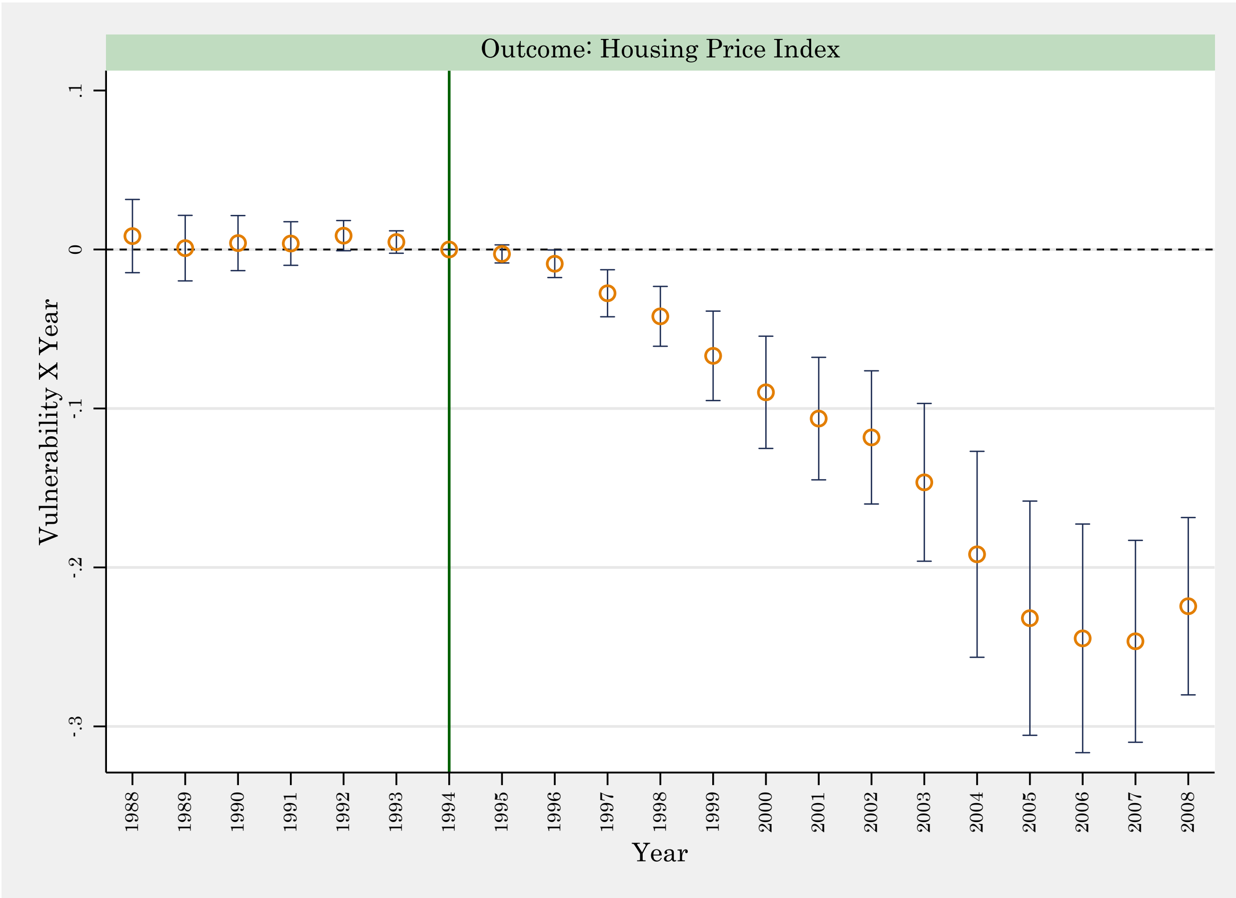


Figure 11 - Event-Study Analysis for the Effect of NAFTA on Housing Wealth

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties.

Appendix A

In section 6.1 and Appendix H, I employ a series of county-level data to explore channels of impact. Appendix Table A-1 provides summary statistics of the data and variables used in analyses of mechanism channels.

Appendix Table A-1 - Summary Statistics of County-Level Measures

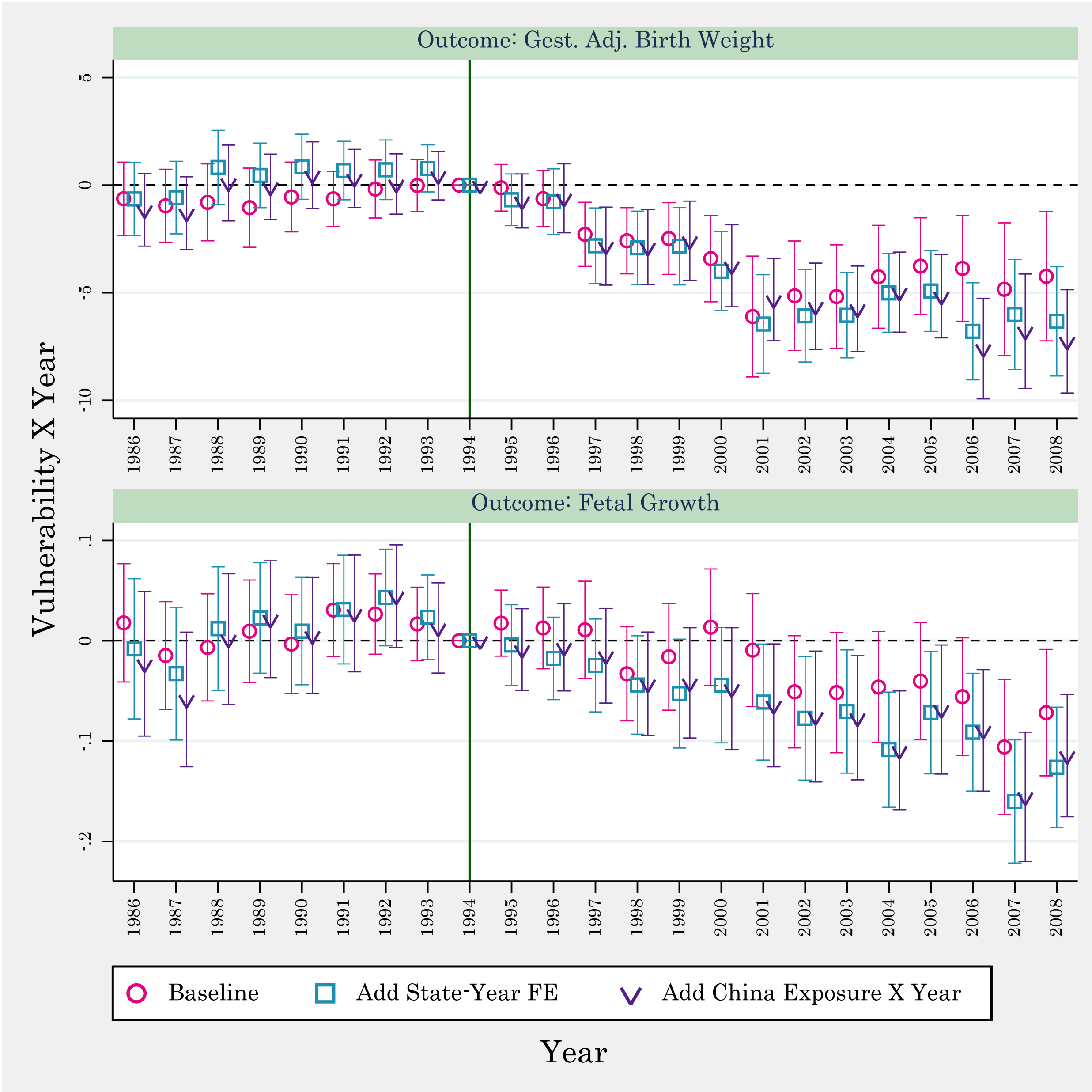
Variable	N	Mean	SD
Trade Exposure Measures:			
I(Year>1994)	82777	0.6784	0.4671
Vulnerability Index	82777	1.3107	0.8227
Exposure to China Index	78361	1.127	0.5941
Multifiber Arrangement (MFA) Exposure Index	78425	0.5814	2.655
Economic Indicators:			
Real Personal Per Capita Income	79042	34602.268	9153.5981
Real Average Weekly Wage	79269	279.1959	68.2887
Real Per Capita Current Transfer Receipt	79042	6280.3755	1958.3053
Real Per Capita Income Maintenance Benefit	79042	609.7524	343.2249
Real Per Capita Employer Contribution to Social Insurance	79042	1113.6262	762.5287
Per Capita Employment in Agriculture	78119	0.0017	0.0042
Per Capita Employment in Mining	78119	0.0042	0.0246
Per Capita Employment in Utility	78119	0.003	0.0068
Per Capita Employment in Construction	78119	0.0154	0.0129
Per Capita Employment in Manufacturing	78119	0.0551	0.0526
Per Capita Employment in Textile Manufacturing	78119	0.0032	0.0117
Per Capita Employment in Apparel Manufacturing	78119	0.0031	0.0104
Per Capita Employment in Machinery Manufacturing	78119	0.0046	0.0109
Per Capita Employment in Other Manufacturing	78119	0.0018	0.0046
Per Capita Employment in Other Industries	78119	0.1317	0.0909
Pollution Measures:			
Co Per County Area (STD)	6048	-0.0201	0.8546
Sulfur Dioxide (SO2) Per County Area (STD)	8905	0.0047	0.8765
Nitrogen Dioxide (NO2) Per County Area (STD)	5714	0.0183	1.0208
Ozone Per County Area (STD)	16062	0.0242	0.8825
Particulate Matters less than 10 μm (PM10) Per County Area (STD)	12668	0.0178	1.3338
Hazardous Air Pollutants (HAPS) Per County Area (STD)	4028	0.0479	1.1589
Lead Per County Area (STD)	3629	-0.0754	0.6694
NONOXYNOL-9Y Per County Area (STD)	4684	0.0523	1.0786
PM10 Speciation Per County Area (STD)	4853	0.0441	1.0203
Co Per County Area (Micrograms per cubic meter per square miles)	6048	2238.1695	6821.3977
Sulfur Dioxide (SO2) Per County Area (parts per billion per square miles)	8905	16.9111	58.8518
Nitrogen Dioxide (NO2) Per County Area (parts per billion per square miles)	5714	56.0187	200.0682
Ozone Per County Area (Micrograms per cubic meter per square miles)	16062	64.0063	132.7987
Particulate Matters less than 10 μm (PM10) Per County Area (Micrograms per cubic meter per square miles)	12668	72.6016	425.3341
Hazardous Air Pollutants (HAPS) Per County Area (Micrograms per cubic meter per square miles)	4028	0.0031	0.0108
Lead Per County Area (Micrograms per cubic meter per square miles)	3629	0.2318	1.0603
NONOXYNOL-9Y Per County Area (parts per billion per square miles)	4684	64.0257	228.9758
PM10 Speciation Per County Area (Micrograms per cubic meter per square miles)	4853	22.9599	51.3875
Crime Rates:			
Total Male Arrest Rate per 100,000	77982	5991.9441	5179.6019
Total Female Arrest Rate per 100,000	77982	1515.6564	1414.9775
Total Arrest Rate per 100,000	77982	3717.7485	3186.5458
Housing Price Measure:			
Housing Price Index	54985	211.4617	130.182
Natural Resource measures:			
Total Gas Production (thousand cubic feet)	78192	6.0837	58.3024
Total Oil Production (thousand barrels)	78192	0.5755	9.5438
Alternative Economic Indicators:			
Real Per Capita Net Earnings	79042	21579.415	7202.1195
Real Per Capita Retirement Income	79042	5501.7645	1722.6128
Real Per Capita Dividend-Rent-Interest	79042	6742.4763	3288.3823
Real Per Job Earning	79042	41940.5	10803.021
Real Per Capita non-Farm Income	79042	27055.027	14437.197

Real Per Capita Proprietary Income	79042	3563.2235	3058.9425
State/County Earning and Expenditure:			
Real Per Capita Total Tax	51487	0.4819	1.14
Real Per Capita Alcohol Tax	51487	0.0008	0.0034
Real Per Capita Educational Expenditure	51487	0.2198	0.6317
Real Per Capita Health Expenditure	51487	0.0742	0.1186
Real Per Capita Police Expenditure	51487	0.0751	0.0948
Real Per Capita Correctional Institution Expenditure	51487	0.0488	0.0998

Notes. Income variables are in 2020 dollars.

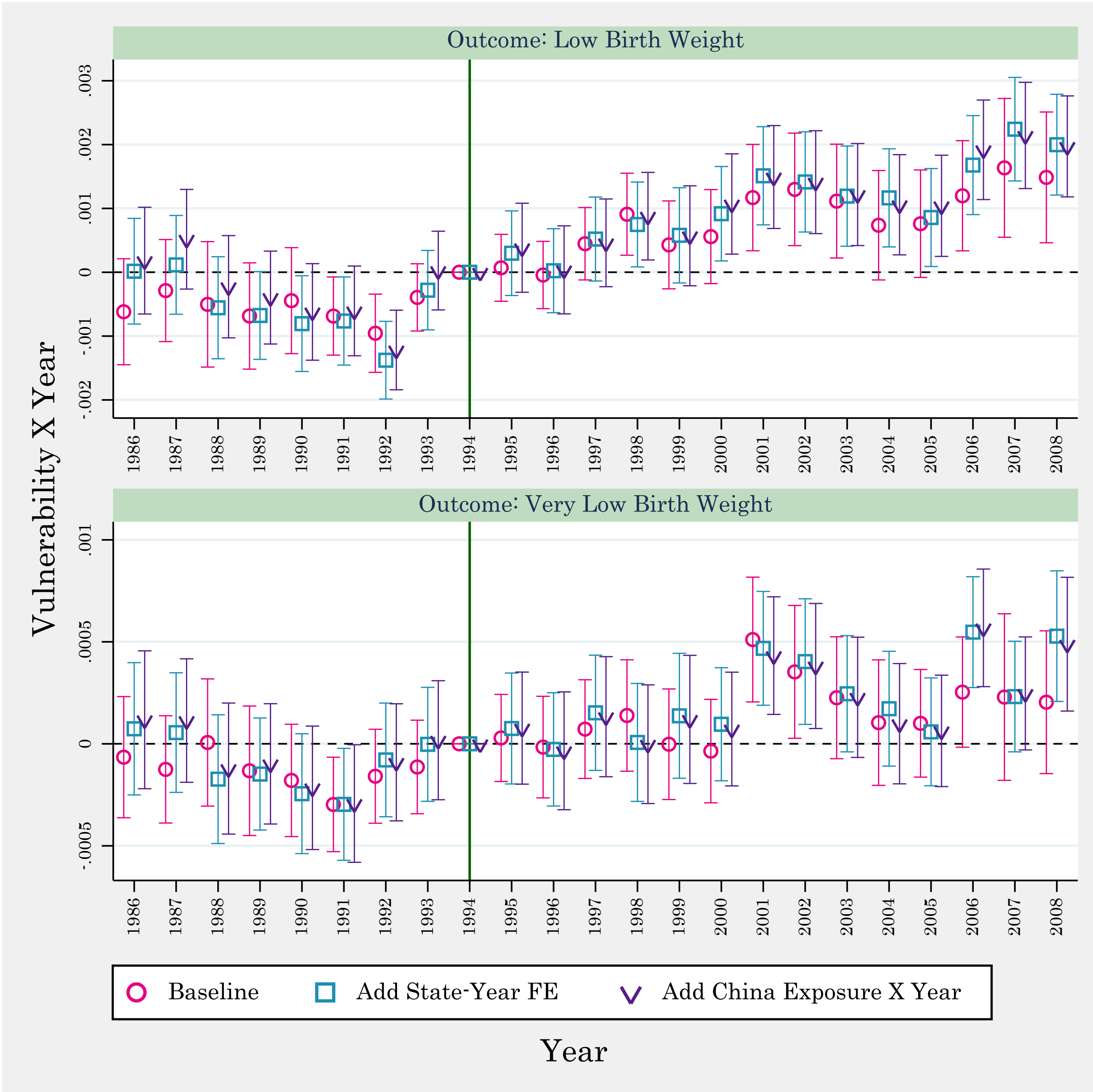
Appendix B

This appendix continues to show the event study results of section 5.2 for alternative measures of health at birth. These results are illustrated in Appendix Figure B-1 through Appendix Figure B-4. In virtually all cases and outcomes, I do not observe a significant pre-trend. However, following NAFTA and in some cases with several years of delay, the negative effects on birth outcomes start to appear, and the coefficients become significant.



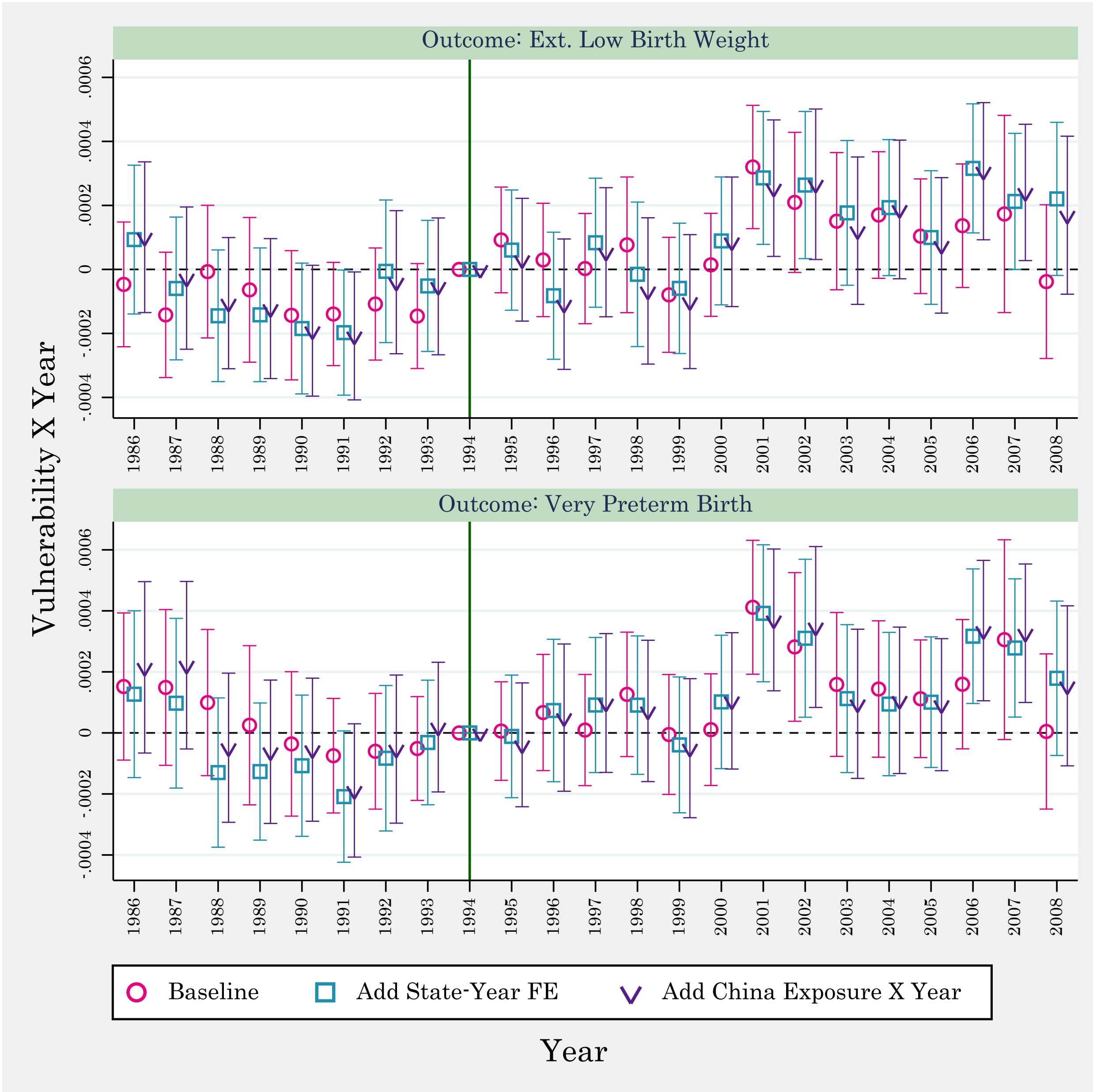
Appendix Figure B-1 - Event-Study Analysis for the Effect of NAFTA on Birth Outcomes: Fetal Growth and Term Birth Weight

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.



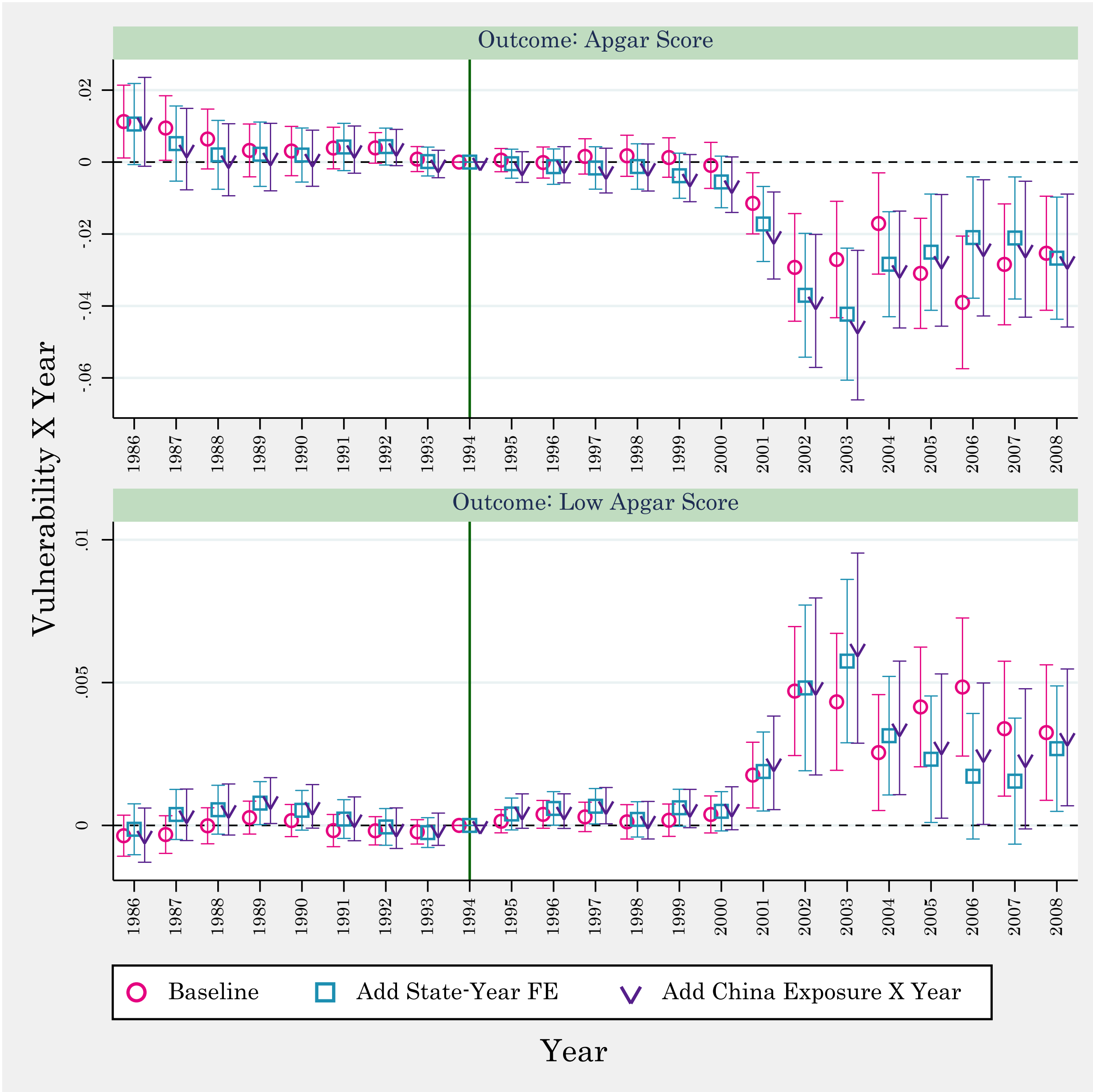
Appendix Figure B-2 - Event-Study Analysis for the Effect of NAFTA on Birth Outcomes: Low Birth Weight and Very Low Birth Weight

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.



Appendix Figure B-3 - Event-Study Analysis for the Effect of NAFTA on Birth Outcomes: Extremely Low Birth Weight and Very Preterm Birth

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.



Appendix Figure B-4 - Event-Study Analysis for the Effect of NAFTA on Birth Outcomes: Apgar Score and Low Apgar Score

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. The regressions control for parental characteristics including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

Appendix C

As discussed in section 5.5, the US trade environment experienced the phase-out of Multifiber Arrangement (MFA) in the years following NAFTA. In this appendix, I explore the robustness of the results to measures of MFA exposure. In so doing, I add a measure of county-level MFA exposure interacted with year-fixed-effects to the regressions. These results are reported in Appendix Table C-1. The point estimates and their statistical significance are quite similar to those reported in Table 3, suggesting that the phase-out of MFA in 1995 did not confound the estimates.

Appendix Table C-1 - Robustness Checks: Controlling for MFA Exposure

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.30613***	-0.03154***	-0.0748***	-3.69624***	-3.64185***	0.00123***
Vulnerability Index	(1.31865)	(0.00778)	(0.02394)	(0.84386)	(1.20024)	(0.00029)
Observations	88881027	88881027	88881027	88881027	79857942	88881027
R-squared	0.06309	0.02592	0.06052	0.02376	0.06976	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.159	-0.081	-0.087	-0.111	-0.107	2.012
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00026***	0.00018***	0.00227***	0.00012*	-0.02072***	0.00182***
Vulnerability Index	(0.00008)	(0.00005)	(0.00059)	(0.00007)	(0.00668)	(0.00062)
Observations	88881027	88881027	88881027	88881027	72259975	72259975
R-squared	0.00493	0.00329	0.01535	0.00404	0.03765	0.00866
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.339	3.590	2.222	1.918	-0.232	6.290

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix D

This appendix complements the heterogeneity analysis of section 5.4. Specifically, I replicate the results of full-specification regressions of Table 3 for various subsamples. Appendix Table D-1 shows the results for the subsample of teenage mothers. The marginal effects, standard errors, and implied percent changes from the mean are quite comparable to the main results. In Appendix Table D-2 and Appendix Table D-3, I replicate the results for the subsample of whites and blacks, respectively. The results do not provide a discernible heterogeneity by race. I focus on the subsample of top-quartile of exposure index counties and replicate the results in Appendix Table D-4. As expected, I observe effect sizes that are considerably larger than those of Table 3.

Appendix Table D-1 - Additional Heterogeneity Analysis across Subsamples: Effects on Mothers Aged less than 25

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.7169***	-0.02716***	-0.06702***	-3.63613***	-2.90476**	0.00134***
Vulnerability Index	(1.28329)	(0.00809)	(0.0257)	(0.87135)	(1.2506)	(0.00033)
Observations	33413069	33413069	33413069	33413069	29581980	33413069
R-squared	0.05314	0.02423	0.0449	0.02073	0.05981	0.01242
Mean DV	3266.531	38.956	83.689	3307.289	3349.354	0.071
%Change	-0.144	-0.070	-0.080	-0.110	-0.087	1.883
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00016**	0.0025***	0.00013	-0.02027***	0.00184***
Vulnerability Index	(0.0001)	(0.00007)	(0.00066)	(0.00009)	(0.00596)	(0.00067)
Observations	33413069	33413069	33413069	33413069	26983965	26983965
R-squared	0.00376	0.0026	0.01362	00.00342	0.03629	0.00815
Mean DV	0.012	0.006	0.115	0.007	8.896	0.033
%Change	2.254	2.719	2.174	1.894	-0.228	5.568

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table D-2 - Additional Heterogeneity Analysis across Subsamples: Effects on White Mothers

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.92669***	-0.02748***	-0.07427***	-3.10877***	-3.39129***	0.00119***
Vulnerability Index	(1.25912)	(0.00654)	(0.02492)	(0.63657)	(1.15944)	(0.00025)
Observations	70348633	70348633	70348633	70348633	64053113	70348633
R-squared	0.04274	0.02043	0.04637	0.01448	0.05301	0.00726
Mean DV	3385.995	39.052	86.579	3331.773	3453.544	0.050
%Change	-0.146	-0.070	-0.086	-0.093	-0.098	2.385
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00025***	0.00014***	0.00183***	0.00007	-0.01462**	0.00196***
Vulnerability Index	(0.00006)	(0.00004)	(0.00042)	(0.00004)	(0.00666)	(0.00068)
Observations	70348633	70348633	70348633	70348633	56656281	56656281
R-squared	0.00152	0.00097	0.00752	0.00123	0.03877	0.00803
Mean DV	0.008	0.004	0.089	0.004	8.932	0.026
%Change	3.156	3.456	2.051	1.775	-0.164	7.528

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table D-3 - Additional Heterogeneity Analysis across Subsamples: Effects on Black Mothers

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.77147***	-0.02325**	-0.08971**	-2.10676*	-2.89636*	0.0013*
Vulnerability Index	(1.77206)	(0.00977)	(0.03549)	(1.2706)	(1.56493)	(0.0007)
Observations	13496017	13496017	13496017	13496017	11266358	13496017
R-squared	0.02645	0.0104	0.0263	0.01022	0.04181	0.00892
Mean DV	3125.537	38.372	81.101	3234.399	3255.400	0.115
%Change	-0.153	-0.061	-0.111	-0.065	-0.089	1.130
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.0001	0.00023	0.00127	-0.00007	-0.03126***	0.001
Vulnerability Index	(0.00028)	(0.0002)	(0.00113)	(0.00022)	(0.00829)	(0.00084)
Observations	13496017	13496017	13496017	13496017	11973462	11973462
R-squared	0.00238	0.00194	0.01026	0.00221	0.02905	0.00642
Mean DV	0.025	0.014	0.165	0.016	8.829	0.043
%Change	0.407	1.642	0.771	-0.416	-0.354	2.328

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table D-4 - Additional Heterogeneity Analysis across Subsamples: Effects on Counties at the Top-Quartile of Vulnerability

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.42263***	-0.01464	-0.11988***	-1.21356	-4.56474***	0.00094**
Vulnerability Index	(1.78398)	(0.01006)	(0.03461)	(1.14257)	(1.67243)	(0.00038)
Observations	21798076	21798076	21798076	21798076	19479288	21798076
R-squared	0.06483	0.02831	0.06134	0.02468	0.07189	0.01629
Mean DV	3329.355	38.906	85.396	3309.923	3412.021	0.064
%Change	-0.163	-0.038	-0.140	-0.037	-0.134	1.474
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00019	0.00015	0.00089	-0.00011	-0.02014**	0.00307**
Vulnerability Index	(0.00014)	(0.0001)	(0.00087)	(0.0001)	(0.00984)	(0.00126)
Observations	21798076	21798076	21798076	21798076	19737158	19737158
R-squared	0.00482	0.00321	0.01569	0.00403	0.04164	0.00871
Mean DV	0.011	0.006	0.106	0.006	8.922	0.030
%Change	1.761	2.517	0.840	-1.860	-0.226	10.243

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix E

In the main analyses of the text, I avoid including any county controls. Instead, I argue that changes in county characteristics could be the operative channel of the relationship between trade exposure and health at birth. I extensively discuss the theory and empirical evidence behind each potential channel in section 2.2. However, to search for the potentially relevant factor, I add county-by-year covariates in different regressions and compare the magnitude of effects with those reported in the main results of Table 3. If the point estimates reduce in size, one would expect that part of the effects operate through added endogenous regressors. However, I am aware that this only provides suggestive evidence and is considered an informal but useful analysis.

In Appendix Table E-1, I add measures of per capita income, including personal income, average weekly wage, net earnings, and rent-interest-dividend earnings. In Appendix Table E-2, I add measures of social spending, including total per capita current transfer receipts, per capita income maintenance benefit, and per capita unemployment insurance payments. In Appendix Table E-3, I add per capita employment measures, including employment in agriculture, mining, utility, construction, food manufacturing, textile manufacturing, wood manufacturing, chemical manufacturing, apparel manufacturing, leather manufacturing, metal manufacturing, machinery manufacturing, computer manufacturing, nonmetal manufacturing, petroleum manufacturing, electrical equipment manufacturing, transportation equipment manufacturing, furniture manufacturing, other manufacturing, and employment in all other sectors. In Appendix Table E-4, I include the share of different demographic groups, including white females, white males, black females, black males, and people in different age groups.

The results suggest that income and employment changes could be responsible for the observed reductions in birth outcomes. Moreover, county-level demographic conditions do not change the point estimates relative to those reported in Table 3.

Appendix Table E-1 - Robustness Checks: Controlling for Various Income and Earning Measures

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-3.99264***	-0.02564***	-0.05144**	-3.07416***	-2.48952**	0.00095***
Vulnerability Index	(1.20721)	(0.00685)	(0.02309)	(0.76468)	(1.13612)	(0.00025)
Observations	88881027	88881027	88881027	88881027	79857942	88881027
R-squared	0.06311	0.02594	0.06053	0.02378	0.06978	0.01611
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.120	-0.066	-0.060	-0.093	-0.073	1.549
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00019***	0.00012**	0.00198***	0.0001	-0.01831***	0.00156**
Vulnerability Index	(0.00007)	(0.00006)	(0.00057)	(0.00007)	(0.00657)	(0.00063)
Observations	88881027	88881027	88881027	88881027	72259975	72259975
R-squared	0.00493	0.00329	0.01535	0.00404	0.03767	0.00866
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	1.754	2.381	1.937	1.652	-0.205	5.367

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table E-2 - Robustness Checks: Controlling for Per Capita Social Spending

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.05666***	-0.0299***	-0.04422*	-3.37806***	-2.49769**	0.00096***
Vulnerability Index	(1.23496)	(0.00724)	(0.02325)	(0.80243)	(1.14497)	(0.00025)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06311	0.02592	0.06053	0.02377	0.06977	0.01611
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.121	-0.077	-0.052	-0.102	-0.073	1.573
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00021***	0.00014**	0.00211***	0.0001	-0.01714***	0.00166***
Vulnerability Index	(0.00007)	(0.00005)	(0.00058)	(0.00007)	(0.0066)	(0.00063)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01535	0.00404	0.03767	0.00866
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	1.948	2.719	2.069	1.674	-0.192	5.713

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table E-3 - Robustness Checks: Controlling for Industry-Specific Per Capita Employment Composition

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.70442***	-0.03057***	-0.06036**	-3.43513***	-3.12531***	0.00111***
Vulnerability Index	(1.26563)	(0.00683)	(0.02447)	(0.75446)	(1.18804)	(0.00026)
Observations	88728226	88728226	88728226	88728226	79731523	88728226
R-squared	0.06296	0.02588	0.06043	0.02364	0.06969	0.01596
Mean DV	3339.920	38.938	85.603	3316.087	3418.743	0.061
%Change	-0.141	-0.079	-0.071	-0.104	-0.091	1.818
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00021***	0.00013**	0.00224***	0.00009	-0.01236*	0.00163**
Vulnerability Index	(0.00007)	(0.00005)	(0.00057)	(0.00007)	(0.00647)	(0.00063)
Observations	88728226	88728226	88728226	88728226	72108275	72108275
R-squared	0.00487	0.00325	0.01523	0.00399	0.03776	0.00865
Mean DV	0.011	0.005	0.101	0.006	8.914	0.029
%Change	1.916	2.576	2.222	1.570	-0.139	5.619

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table E-4 - Robustness Checks: Controlling for Population Demographic Composition

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.76338***	-0.02826***	-0.0691***	-3.22969***	-3.14592***	0.00115***
Vulnerability Index	(1.25504)	(0.00718)	(0.02422)	(0.79537)	(1.15541)	(0.00027)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.0631	0.02592	0.06052	0.02377	0.06976	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.143	-0.073	-0.081	-0.097	-0.092	1.882
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00025***	0.00017***	0.00202***	0.00009	-0.01823***	0.00168***
Vulnerability Index	(0.00007)	(0.00005)	(0.00058)	(0.00007)	(0.00672)	(0.00063)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01535	0.00404	0.03765	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.289	3.359	1.977	1.577	-0.205	5.777

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix F

In the numerator of equation 2, the protection of a specific industry is based on the tariff rate, τ. Therefore, changes in tariff make an industry, hence counties with higher reliance on that industry, vulnerable to trade. Another way of absorbing such degrees of vulnerability is to look at changes in imports. However, while tariff changes are policy-driven (and arguably are more exogenous), the resulting changes in imports depend on supply conditions (e.g., technology adaptations, new investments) and demand conditions (e.g., economic cycles in the importing country). Therefore, I used tariffs as the primary variable in the vulnerability index. As a robustness check, I replace it with actual imports in equation 2 and replicate the results. The estimates are reported in Appendix Table F-1. The effects are statistically and economically significant in most cases and suggest comparable estimates relative to those reported in Table 3.

As an additional check, I drop the agricultural sector in calculations of the vulnerability index to focus on non-agricultural shocks and replicate the results in Appendix Table F-2. The effect sizes are, again, similar to the main results.

Appendix Table F-1 - Robustness Checks for Alternative Measure of Vulnerability: Using Actual Changes in Imports

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.84384***	-0.02451**	-0.10695***	-3.33999***	-4.36549**	0.00143***
Vulnerability Index	(1.82567)	(0.01116)	(0.03359)	(1.25847)	(1.75654)	(0.00038)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02375	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.175	-0.063	-0.125	-0.101	-0.128	2.340
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00024**	0.00018**	0.00219***	0.00004	-0.01838**	0.00161**
Vulnerability Index	(0.0001)	(0.00007)	(0.00083)	(0.0001)	(0.00898)	(0.00073)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03763	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.158	3.688	2.149	0.613	-0.206	5.553

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table F-2 - Robustness Checks for Alternative Measure of Vulnerability: Excluding Agricultural Sector

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-6.19802**	-0.0149181	-0.13170***	-2.64336*	-5.85911**	0.00102*
Vulnerability Index	(2.71440)	(0.0127939)	(0.05069)	(1.53304)	(2.59575)	(0.0005)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02375	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.185	-0.038	-0.153	-0.079	-0.171	1.676
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00018	0.00006	0.00129	-0.00018	-0.00003	-0.00053
Vulnerability Index	(0.00012)	(0.00008)	(0.0009)	(0.00011)	(0.00927)	(0.0007)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03761	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	1.687	1.365	1.271	-3.058	-0.000	-1.837

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix G

The period of the study sample covers the years 1985-2008. In Appendix Table G-1, I restrict the sample to the years 1989-2004. In Appendix Table G-2, I expand the sample to cover the years 1980-2017. The effects seem to be larger for more expanded samples. However, the estimated coefficients in both tables are statistically and economically significant.

Appendix Table G-1 - Replicating the Main Results for the Years 1989-2004

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.0284***	-0.02489***	-0.05585***	-2.7745***	-2.2903**	0.0012***
Vulnerability Index	(1.14371)	(0.00765)	(0.02075)	(0.85306)	(1.01633)	(0.00027)
Observations	58434475	58434475	58434475	58434475	52579807	58434475
R-squared	0.06348	0.02226	0.06229	0.02305	0.06907	0.01688
Mean DV	3351.815	38.991	85.784	3321.006	3430.947	0.060
%Change	-0.120	-0.064	-0.065	-0.084	-0.067	2.004
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00015***	0.00197***	0.00013*	-0.00919*	0.00095***
Vulnerability Index	(0.00008)	(0.00006)	(0.00063)	(0.00007)	(0.00505)	(0.00037)
Observations	58434475	58434475	58434475	58434475	44651582	44651582
R-squared	0.00517	0.00348	0.01597	0.00424	0.03377	0.0061
Mean DV	0.011	0.005	0.100	0.006	8.938	0.028
%Change	2.462	3.015	1.965	2.194	-0.103	3.409

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table G-2 - Replicating the Main Results for the Years 1980-2017

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-4.95277***	-0.03478***	-0.05843**	-3.80852***	-3.53853***	0.00093***
Vulnerability Index	(1.32186)	(0.00751)	(0.02642)	(0.83781)	(1.22585)	(0.0003)
Observations	117465581	117465581	117465581	117465581	104062664	117465581
R-squared	0.0541	0.02743	0.05168	0.02282	0.06568	0.01349
Mean DV	3306.462	38.830	84.908	3301.377	3404.278	0.076
%Change	-0.150	-0.090	-0.069	-0.115	-0.104	1.227
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.0002***	0.00207***	0.00018**	-0.02491***	0.0019***
Vulnerability Index	(0.00008)	(0.00006)	(0.0006)	(0.00007)	(0.0076)	(0.00064)
Observations	117465581	117465581	117465581	117465581	98670781	98670781
R-squared	0.00456	0.0032	0.01377	0.00386	0.03666	0.0095
Mean DV	0.014	0.007	0.114	0.007	8.885	0.033
%Change	1.944	2.790	1.817	2.597	-0.280	5.766

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix H

In this appendix, I complement section 6.1 and show the difference-in-difference results of NAFTA and county characteristics. I implement regressions of the form introduced in equation 5. The results are reported in Appendix Table H-1 and Appendix Table H-2. Consistent with the event studies in Figure 8 through Figure 11, the effects are negative, large, and significant for earnings and employment outcomes. For instance, for a 1-unit change in vulnerability post-versus-pre-NAFTA, per capita income reduces by about $1,800 (column 1 Appendix Table H-1), per capita net earnings drop by about $1,250 (column 1 Appendix Table H-2), and per capita non-farm income drops by about $3,200 (column 5 Appendix Table H-2). In addition, I observe sizeable reductions in per capita employment in exposed industries (columns 9-12 Appendix Table H-1) but no effect on less affected industries (columns 6-8 Appendix Table H-1).

For a similar shock, the total arrest rate raises by 236 incidences per 100,000 population, an increase of 5 percent from the mean (column 12 Appendix Table H-2). In line with this result, I find increases in per capita expenditure on correctional institutions (column 11 Appendix Table H-2).

The housing price index falls by about 32 units, a reduction of 9 percent from the mean of the index over the sample period. I do not observe significant changes in health and education spending (columns 8-9 Appendix Table H-2) or changes in county-level tax collection (column 7 Appendix Table H-2). Finally, there is no statistical evidence that oil-gas extraction and the post-2000 increases in oil-gas production change in high exposed counties (column 14 Appendix Table H-2).

I also show the event study results for a selected set of these outcomes. These analyses are reported in Appendix Figure H-1 for wage and income outcomes, Appendix Figure H-2 for crime rates, and Appendix Figure H-3 for local government expenditure and revenue outcomes. In the event studies of Appendix Figure H-2 and Appendix Figure H-3, I standardize the outcomes to ease the comparisons across given outcomes within a panel.

Appendix Table H-1 - Exposure to NAFTA and County Economic Indicators

	Outcomes:					
	Personal Per Capita Income	Average Weekly Wage	Per Capita Current Transfer Receipt	Per Capita Income Maintenance Benefit	Per Capita Employer Contribution to Social Insurance	Per Capita Employment in Agriculture
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-1790.1124***	-13.9943***	125.75859***	2.54957	-89.72914***	0.00005
Vulnerability Index	(344.47481)	(2.45228)	(35.18451)	(9.80157)	(16.6263)	(0.00006)
Observations	76721	78112	76721	76721	76721	77832
R-squared	0.95825	0.94979	0.97415	0.97125	0.97655	0.80146
Mean DV	42633.774	358.480	5870.062	646.471	1730.662	0.001
%Change	-4.163	-3.904	2.142	0.394	-5.185	4.862
	Per Capita Employment in Mining	Per Capita Employment in Utility	Per Capita Employment in Manufacturing	Per Capita Employment in Textile	Per Capita Employment in Apparel	Per Capita Employment in All Industries
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	-0.00008	0.00009	-0.00174*	-0.00218***	-0.00242***	-0.00675***
Vulnerability Index	(0.00007)	(0.00009)	(0.00104)	(0.00035)	(0.00028)	(0.00255)
Observations	77832	77832	77832	77832	77832	77832
R-squared	0.83085	0.83994	0.92942	0.88401	0.72033	0.97434
Mean DV	0.002	0.002	0.057	0.002	0.002	0.378
%Change	-3.876	4.533	-3.046	-108.847	-121.071	-1.786

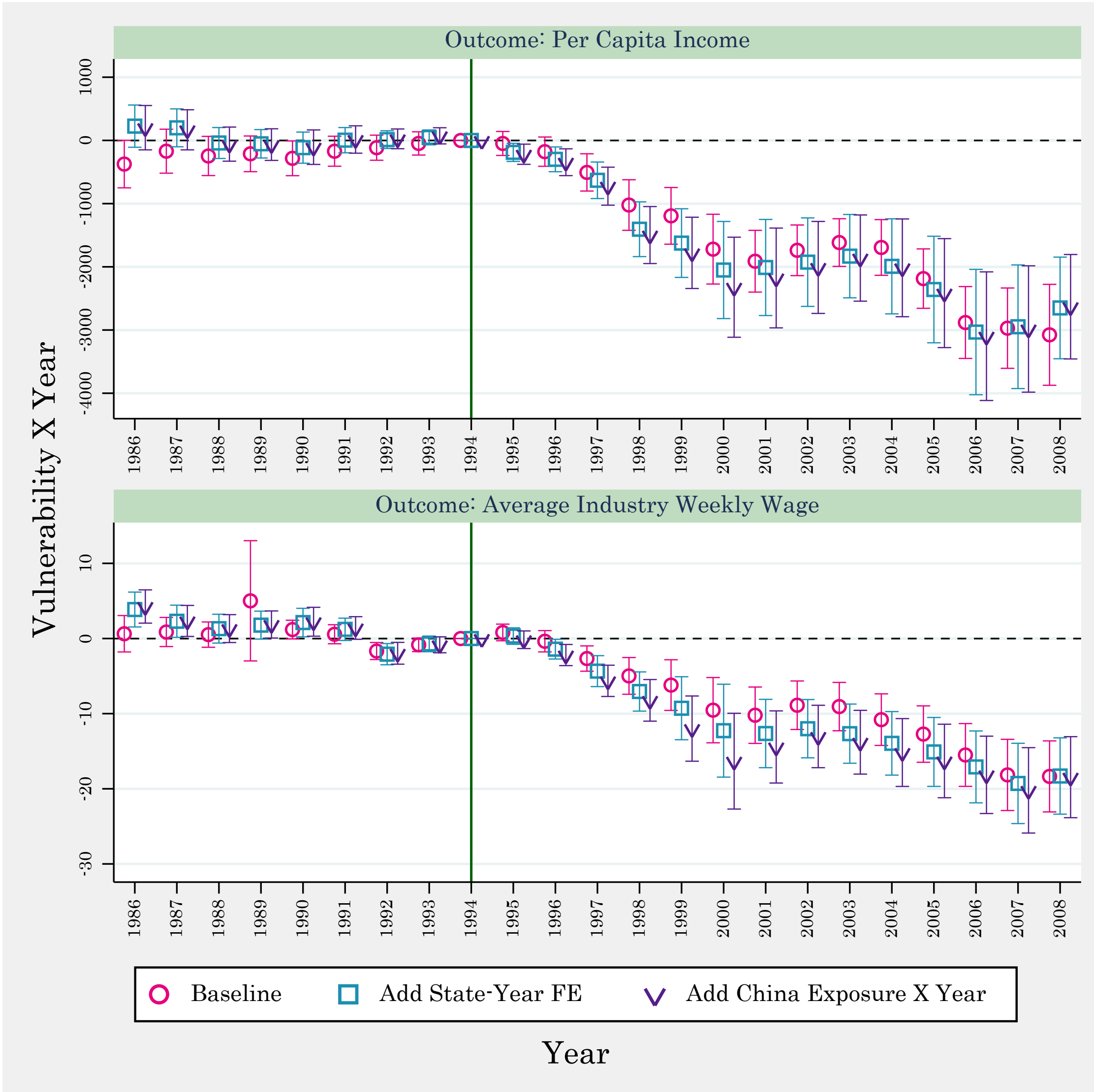
Notes. Standard errors, clustered at the county level, are reported in parentheses. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table H-2 - Exposure to NAFTA and County Indicators

	Outcomes:						
	Per Capita Net Earnings	Per Capita Retirement Income	Per Capita Dividend-Rent-Interest	Per Capita Job Earnings	Per Capita Non-Farm Income	Per Capita Proprietary Income	Per Capita Total Tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(year>1994) ×	-1249.2254***	138.88805***	-666.65666***	-2015.207***	-3210.5985***	-628.15698***	0.00103
Vulnerability Index	(274.38239)	(30.78361)	(108.47443)	(369.11054)	(818.93329)	(118.03482)	(0.00759)
Observations	76721	76721	76721	76721	76721	76721	51378
R-squared	0.95644	0.96898	0.92421	0.94935	0.7383	0.77147	0.93894
Mean DV	2.9e+04	5030.934	8176.678	5.5e+04	3.6e+04	3545.418	0.481
%Change	-4.308	2.761	-8.153	-3.664	-8.918	-17.717	0.215
	Per Capita Education Expenditure	Per Capita Health Expenditure	Per Capita Police Expenditure	Per Capita Correctional Institutions Expenditure	Total Arrest Rates per 100,0000	Housing Price Index	Total Oil-Equivalent Production
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
I(year>1994) ×	0.00362	0.00123	-0.00286*	0.00401**	236.74282*	-32.30445***	0.21517
Vulnerability Index	(0.00512)	(0.00365)	(0.0015)	(0.00171)	(125.29581)	(6.14374)	(1.48116)
Observations	51378	51378	51378	51378	77728	54794	77672
R-squared	0.98999	0.83196	0.92041	0.72797	0.80094	0.96008	0.85624
Mean DV	0.189	0.102	0.077	0.077	4765.850	359.307	12.812
%Change	1.917	1.210	-3.720	5.202	4.967	-8.991	1.679

Notes. Standard errors, clustered at the county level, are reported in parentheses. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties.

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

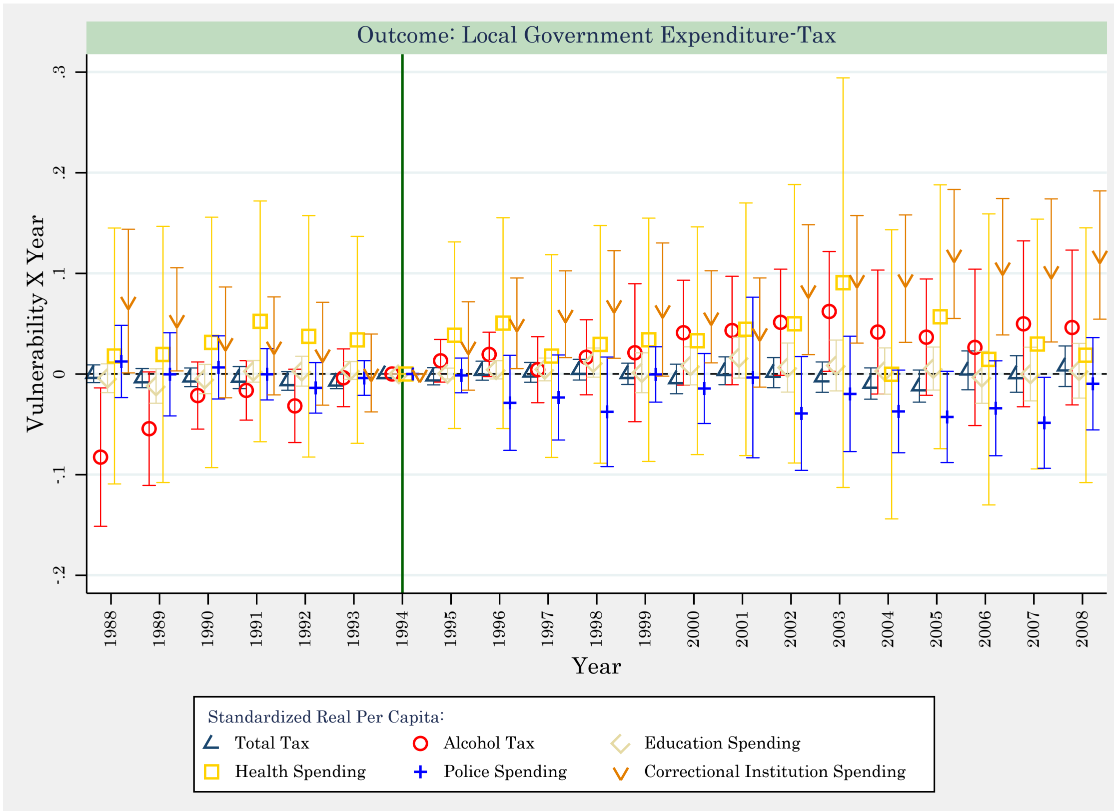


Appendix Figure H-1 – Event-Study Analysis for the Effect of NAFTA on Measures of Income

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties.

Appendix Figure H-2 – Event-Study Analysis for the Effect of NAFTA on Crime Rates

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties. All outcomes are standardized with respect to the sample mean and standard error.



Appendix Figure H-3 – Event-Study Analysis for the Effect of NAFTA on Government Expenditure and Taxes

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties. All outcomes are standardized with respect to the sample mean and standard error.

Appendix I

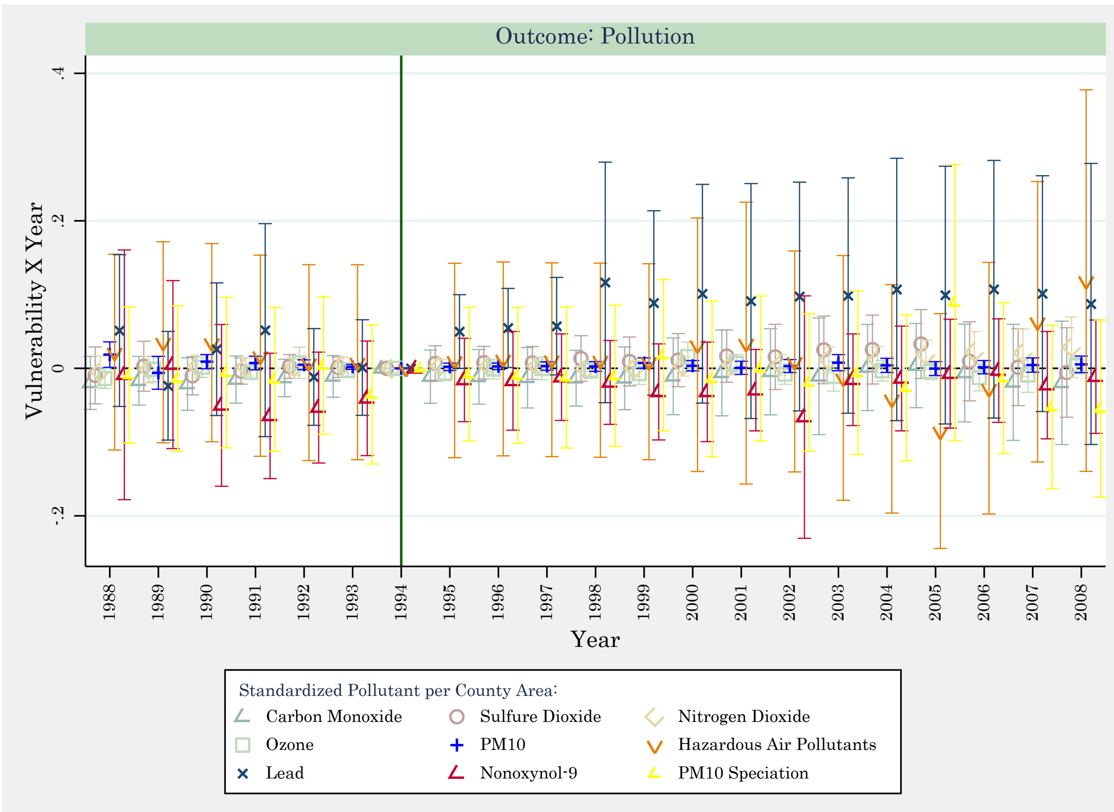
Another set of outcomes of interest that could potentially change by trade-induced labor market shocks is pollution. If more pollutant industries are affected by the trade agreement, the plant closures could benefit environmental air quality (Cherniwchan, 2017; Navaei & Farnoud, 2021). I implement difference-in-difference regressions and event study analysis as introduced in equation 5 to search for the divergence of pollutants per county area following NAFTA. The results are reported in Appendix Table I-1 and Appendix Figure I-1. As it is obvious from the event study of Appendix Figure I-1, there is no discernible and systematic pre-trend or post-trend in pollution outcomes. Moreover, the difference-in-difference results of Appendix Table I-1 also do not provide a significant change in pollutant concentrations.

Appendix Table I-1 - Exploring Changes in Pollution as a Mediatory Channel

	Outcomes: Standardized Pollution Per Area								
	Carbon Monoxide	Sulfur Dioxide	Nitrogen Dioxide	Ozone	PM10	HAPS	Lead	Nonoxynol-9	Pm10 Speciation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
I(year>1994) ×	0.00567	0.01087	0.01277	-0.00109	-0.00259	0.02688	0.05021*	0.0074	0.03873
Vulnerability Index	(0.02549)	(0.02556)	(0.00947)	(0.00542)	(0.0045)	(0.04596)	(0.02978)	(0.02586)	(0.0327)
Observations	5724	8620	5404	15810	12482	3731	3315	4418	4480
R-squared	0.90718	0.93637	0.98334	0.98539	0.94177	0.94134	0.76343	0.90086	0.9584

Notes. Standard errors, clustered at the county level, are reported in parentheses. Regressions are weighted using average birth counts in each county. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties.

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



Appendix Figure I-1 – Event-Study Analysis for the Effect of NAFTA on Pollution

Notes. Point estimates and 95 percent confidence intervals are illustrated. Standard errors are clustered at the county level. Regressions are weighted using average birth counts in each cell. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The data spans the years 1988-2008 and covers 3,074 counties. All outcomes are standardized with respect to the sample mean and standard error.

Appendix J

In the main text, the industry composition and hence the vulnerability index is based on county-level data. One concern in using the county-level data is that the vulnerability of neighboring counties could influence their own county outcomes through spillovers of connected labor markets (Gittings & Roach, 2020). To address this concern, I aggregate the tariff data at the Consistent Public-Use Microdata Area (conspuma) level as defined by IPUMS Ruggles et al. (2020) and used by Hakobyan & McLaren (2016). Therefore, the vulnerability measure of equation 2 is now at the conspuma level. I implement regressions of the form in equation 4 but replace county fixed effects with conspuma fixed effects (541 conspumas). The results are reported in Appendix Table J-1. The comparison of the point estimates with those of Table 3 suggests very small changes in the effects. The main reason is that the vulnerability measure of adjacent counties is highly correlated. This fact is more obvious when I group the vulnerability index into quartiles and look at the geographic distributions (see top panel of Figure 4). Therefore, focusing on the county-level produces quite similar effects compared with more aggregated geographies.

Appendix Table J-1 - Robustness Checks for Assigning the Vulnerability at the Consistent Public-Use Microdata Area (conspuma) Level

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.76089***	-0.04241***	-0.06331**	-4.84193***	-3.64368**	0.00158***
Vulnerability Index	(1.58161)	(0.00817)	(0.03168)	(0.88835)	(1.47082)	(0.00032)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06232	0.02531	0.05991	0.02331	0.06877	0.01594
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.173	-0.109	-0.074	-0.146	-0.107	2.588
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00034***	0.00021***	0.00322***	0.00017**	-0.02256***	0.00205***
Vulnerability Index	(0.00008)	(0.00006)	(0.00062)	(0.00007)	(0.00668)	(0.0007)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00488	0.00325	0.0151	0.004	0.03131	0.00796
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	3.096	4.185	3.154	2.751	-0.253	7.055

Notes. Standard errors, clustered at the conspuma level, are reported in parentheses. All regressions include conspuma fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix K

In this appendix, I explore the robustness of the results to alternative methods of correcting for standard errors. I start by showing that the main results of Table 3 are robust if I use heteroscedastic-robust standard errors (Appendix Table K-1). I then show the robustness of the results to clustering at different levels, including state-level (Appendix Table K-2) and county-year level (Appendix Table K-3). Next, I employ a two-way clustering technique at the county and state-year level to account for both serial and spatial correlations in error terms (Appendix Table K-4).

The recent developments in the literature on *Bartik* instruments and shift-share research design provide insight into how standard errors are deflated and underestimate the true values even after clustering standard errors at the conventional geographic variables (Adão et al., 2019; Borusyak et al., 2022). The idea is that the error terms could be correlated across counties with similar baseline industry composition. Therefore, statistical tests may reject the null hypothesis while there is no effect. In the second part of this appendix, I examine the robustness of the results using the methodology developed by Adão et al. (2019). To follow their pathway, I need to restructure the data and modify the empirical method. I implement a first difference method that exploits the long-difference of counties' outcomes as a function of their vulnerability, as follows:

$$\Delta y_{cs}^{2000-1990} = \varphi Vul_{cs}^{1990} + \beta \Delta X_{cs}^{2000-1990} + \lambda_s + \varepsilon_{cs} \qquad \text{(K-1)}$$

Where Δy is the change in outcome in county c and state s between the years 2000 and 1990. Similarly, ΔX measures long-difference changes in parental characteristics. I show the results with and without state fixed effects (represented by λ). I cluster standard errors at the state level.

The results of equation K-1 are reported in Appendix Table K-5 using ordinary least square regressions. The pint estimates are, to some extent, similar to those of the main results and, in most cases, statistically significant. In Appendix Table K-6, I add state fixed-effects to the models. For some outcomes, such as very low birth weight and extremely low birth weight, the point estimates drop in magnitude and become insignificant. For Apgar score and low Apgar score, on the contrary, the effects rise in magnitude.

I implement the Adão et al. (2019) method and use robust path-cluster standard errors to account for cross-industry correlations in error terms. The results are reported in Appendix Table K-7 and Appendix Table K-8 for regressions without and with state fixed-effects, respectively. Although I observe larger standard errors, the effects are, in most cases, statistically significant. This is true for the primary outcomes such as birth weight, gestational age, low birth weight, and preterm birth.

Appendix Table K-1 – Robustness of the Main Results to Using Heteroscedastic-Robust Standard Errors

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.41496***	-0.03521***	-0.06952***	-3.94414***	-3.60594***	0.00131***
Vulnerability Index	(0.27533)	(0.00127)	(0.00656)	(0.15893)	(0.24318)	(0.00012)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02376	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.162	-0.090	-0.081	-0.119	-0.105	2.147
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00017***	0.00245***	0.00012***	-0.01967***	0.00182***
Vulnerability Index	(0.00005)	(0.00004)	(0.00015)	(0.00004)	(0.00046)	(0.0001)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03764	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.443	3.451	2.399	2.037	-0.221	6.281

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table K-2 – Robustness of the Main Results to Clustering Standard Errors at the State Level

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.41496**	-0.03521***	-0.06952*	-3.94414***	-3.60594*	0.00131***
Vulnerability Index	(2.11659)	(0.01223)	(0.03594)	(1.29238)	(1.86027)	(0.00046)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02376	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.162	-0.090	-0.081	-0.119	-0.105	2.147
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00017**	0.00245***	0.00012	-0.01967***	0.00182**
Vulnerability Index	(0.00007)	(0.00007)	(0.00077)	(0.0001)	(0.00689)	(0.00077)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03764	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.443	3.451	2.399	2.037	-0.221	6.281

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table K-3 – Robustness of the Main Results to Clustering Standard Errors at the County-Year Level

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.41496***	-0.03521***	-0.06952**	-3.94414***	-3.60594**	0.00131***
Vulnerability Index	(1.50406)	(0.00904)	(0.02614)	(0.99327)	(1.33127)	(0.00033)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02376	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.162	-0.090	-0.081	-0.119	-0.105	2.147
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00017***	0.00245***	0.00012	-0.01967**	0.00182**
Vulnerability Index	(0.00008)	(0.00006)	(0.00064)	(0.00007)	(0.00757)	(0.00074)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03764	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.443	3.451	2.399	2.037	-0.221	6.281

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table K-4 – Robustness of the Main Results to Two-Way Clustering Standard Errors at the County and State-Year Level

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.41496***	-0.03521***	-0.06952***	-3.94414***	-3.60594***	0.00131***
Vulnerability Index	(1.37238)	(0.00801)	(0.02566)	(0.8784)	(1.24843)	(0.0003)
Observations	88881197	88881197	88881197	88881197	79858102	88881197
R-squared	0.06309	0.0259	0.06052	0.02376	0.06975	0.0161
Mean DV	3339.603	38.937	85.597	3315.940	3418.557	0.061
%Change	-0.162	-0.090	-0.081	-0.119	-0.105	2.147
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027***	0.00017***	0.00245***	0.00012*	-0.01967***	0.00182***
Vulnerability Index	(0.00008)	(0.00006)	(0.00061)	(0.00007)	(0.00674)	(0.00066)
Observations	88881197	88881197	88881197	88881197	72260140	72260140
R-squared	0.00493	0.00329	0.01534	0.00404	0.03764	0.00865
Mean DV	0.011	0.005	0.102	0.006	8.913	0.029
%Change	2.443	3.451	2.399	2.037	-0.221	6.281

Notes. Standard errors, clustered at the county level, are reported in parentheses. All regressions include county fixed effects, state-by-year fixed effects, and China exposure index by year fixed effects. The regressions control for parental characteristics, including maternal education dummies, maternal race dummies, maternal age dummies, maternal marital status dummies, paternal age dummies, and missing indicators for parental characteristics. The regressions also include a child gender dummy and dummies for birth parity. The data spans the years 1986-2010 and covers 3,074 counties.

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

Appendix Table K-5 – First Difference Estimates without State Fixed Effects and Clustering at the State-Level

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.90817***	-0.03609***	-0.0894***	-2.77414***	-3.19159***	0.0024***
Vulnerability Index	(0.93293)	(0.00461)	(0.02023)	(0.54838)	(0.81835)	(0.00033)
Observations	3131	3131	3131	3131	3131	3131
R-squared	0.13571	0.12516	0.11828	0.12401	0.10775	0.09004
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00057***	0.00036***	0.00156***	0.00037***	-0.00759	0.00333**
Vulnerability Index	(0.00014)	(0.0001)	(0.00048)	(0.0001)	(0.00766)	(0.00138)
Observations	3131	3131	3131	3131	2965	2965
R-squared	0.02855	0.03261	0.09563	0.03629	0.03234	0.04529

Notes. Regressions are weighted using average birth counts in each cell over the years 1990-2000. The regressions control for changes in county-level parental characteristics over the years 1990-2000. The data is at the county-level, and the outcomes show the long-difference in the respective county-level values between the years 1990 and 2000.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table K-6 – First Difference Estimates with State Fixed Effects and Clustering at the State-Level

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.57343***	-0.03924***	-0.06698**	-4.46848***	-2.19803**	0.00246***
Vulnerability Index	(1.22616)	(0.00608)	(0.02689)	(0.72142)	(1.07133)	(0.00045)
Observations	3131	3131	3131	3131	3131	3131
R-squared	0.24214	0.228	0.20921	0.23043	0.22378	0.1462
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027	0.00013	0.00377***	0.00041***	-0.04172***	0.01007***
Vulnerability Index	(0.00019)	(0.00013)	(0.00064)	(0.00014)	(0.01057)	(0.00191)
Observations	3131	3131	3131	3131	2965	2965
R-squared	0.07135	0.06801	0.18423	0.07181	0.06557	0.06765

Notes. Regressions are weighted using average birth counts in each cell over the years 1990-2000. The regressions control for changes in county-level parental characteristics over the years 1990-2000. The data is at the county-level, and the outcomes show the long-difference in the respective county-level values between the years 1990 and 2000.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table K-7 – First Difference Estimates without State Fixed Effects and Correcting Standard Errors based on Adão et al. (2019) Method

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.90817**	-0.03609**	-0.0894*	-2.77414**	-3.19159*	0.0024***
Vulnerability Index	(2.55314)	(0.01391)	(0.04606)	(1.34159)	(1.74476)	(0.00083)
Observations	3131	3131	3131	3131	3131	3131
R-squared	0.13571	0.12516	0.11828	0.12401	0.10775	0.09004
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00057**	0.00036**	0.00156	0.00037***	-0.00759	0.00333
Vulnerability Index	(0.00023)	(0.00014)	(0.00104)	(0.00013)	(0.02199)	(0.00524)
Observations	3131	3131	3131	3131	2965	2965
R-squared	.02855	0.03261	0.09563	0.03629	0.03234	0.04529

Notes. Regressions are weighted using average birth counts in each cell over the years 1990-2000. The regressions control for changes in county-level parental characteristics over the years 1990-2000. The data is at the county-level, and the outcomes show the long-difference in the respective county-level values between the years 1990 and 2000.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table K-8 – First Difference Estimates with State Fixed Effects and Correcting Standard Errors based on Adão et al. (2019) Method

	Outcomes:					
	Birth Weight	Gestational Age	Fetal Growth	Gestational Age-Adjusted Birth Weight	Full-Term Birth Weight	Low Birth Weight
	(1)	(2)	(3)	(4)	(5)	(6)
I(year>1994) ×	-5.57343**	-0.03924**	-0.06698	-4.46848**	-2.19803	0.00246***
Vulnerability Index	(2.32569)	(0.01567)	(0.04288)	(1.68898)	(1.67479)	(0.00086)
Observations	3131	3131	3131	3131	3131	3131
R-squared	0.24214	0.228	0.20921	0.23043	0.22378	0.1462
	Very Low Birth Weight	Extremely Low Birth Weight	Preterm Birth	Very Preterm Birth	Apgar Score	Low Apgar Score
	(7)	(8)	(9)	(10)	(11)	(12)
I(year>1994) ×	0.00027	0.00013	0.00377**	0.00041**	-0.04172	0.01007
Vulnerability Index	(0.00023)	(0.00018)	(0.00148)	(0.00017)	(0.03346)	(0.00873)
Observations	3131	3131	3131	3131	2965	2965
R-squared	0.07135	0.06801	0.18423	0.07181	0.06557	0.06765

Notes. Regressions are weighted using average birth counts in each cell over the years 1990-2000. The regressions control for changes in county-level parental characteristics over the years 1990-2000. The data is at the county-level, and the outcomes show the long-difference in the respective county-level values between the years 1990 and 2000.
*** p<0.01, ** p<0.05, * p<0.1