Trade and Health: Evidence on Trade Reform, Public Health Policy, and Infant Mortality in Brazil*

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Abstract

Using a uniquely rich set of data sources spanning more than 3,000 Brazilian municipalities over a horizon of 25 years, we investigate whether and how infant mortality responds to a permanent shock generated by a trade liberalization reform. We exploit variation in import tariff reductions, together with differences in the baseline industry composition across locations, for identification. We estimate a robust decline in infant mortality in areas with greater exposure to the tariff cuts. In our exploration of mechanisms, we find the most support for the hypothesis that worse labor market opportunities make it less costly undertake health-improving behaviors that are time intensive. Consistent with this hypothesis, we observe a significant decline in female employment rates and an increase in the use of basic health services among women of childbearing ages and infants. We also document that the rollout of a community-based intervention that brings basic health services to the home in a flexible fashion lowers the impacts of the trade shock on infant mortality, providing further evidence in favor of the parental time mechanism. The findings of this paper illustrate that trade policy can have important implications for the household production of child health status and returns of public health policies in a relatively high-mortality context.

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1 Introduction

One of the most striking trends of the last 40 years in health statistics has been the dramatic decline in infant mortality in most parts of the developing world. Between 1990 and 2000 alone, the infant mortality rate dropped by about 21 percent in low-income regions, and for some countries such as Chad and Niger this represents the first sustained reductions in infant mortality ever known.[1] This spectacular decline has coincided with the rise of a phenomenon that has radically transformed the structure of labor markets and potentially altered the household production function and returns of public health policies: globalization. The most notable manifestation of this phenomenon is the series of reforms conducted in the 1980s and 1990s that have lowered import tariff barriers in many developing countries. Does international trade spur or retard infant mortality declines? Existing research on the consequences of trade policy has often focused on workers, firms, and aggregate economic growth. This paper contributes to our knowledge of the distributional implications of trade policy by providing an in-depth analysis of its impacts on infant mortality.

We argue that understanding the link between trade shocks and infant mortality is not only interesting in itself but also important for a number of policy reasons. Infants are particularly sensitive to changes in circumstances surrounding the birth, and thus the impacts on life expectancy and welfare may be substantial. Not surprisingly, improved infant mortality is itself a goal of development and policymakers are highly motivated to promote early childhood well-being. Many of the countries engaging in large-scale trade policy reforms are also intensifying their efforts to reduce infant mortality. Therefore, understanding whether past trade programs have contributed to the historical improvements in infant mortality, or whether such improvements would have occurred at a faster pace in the absence of trade policy changes may offer insights for reducing the high mortality rate that still exists today in the world's poorest regions and for designing trade policies without increasing infant mortality.

A well-established body of studies, conducted in a number of countries such as Colombia, Brazil, India, South Africa, and the United States, have documented that increased exposure to international trade leads to contractions in employment, lower incomes, higher exit of plants, and higher earnings inequality.[2] Based on this observation, one could tentatively hypothesize that exposure to import competition should increase infant mortality. However, a strand of literature in health economics suggests that parental time is a critical input in the child production function (Dehejia and Lleras-Muney, 2004; Miller and Urdinola, 2010; Del Boca et al., 2014). Worse labor

[1] See estimates developed by the UNICEF, WHO, World Bank and UN DESA (`https://data.worldbank.org/indicator/SP.DYN.IMRT.IN`, last accessed on October 20, 2021).

[2] See Kovak (2013), Dix-Carneiro and Kovak (2017),Dix-Carneiro et al. (2018), and Dix-Carneiro and Kovak (2019) for Brazil, Topalova (2010), and Hasan et al. (2012) for India, Erten et al. (2019) for South Africa, and David et al. (2013), Pierce and Schott (2016), Acemoglu et al. (2016), and Caliendo et al. (2019) for the United States.

market opportunities make it less costly undertake health-improving behaviors that are time-intensive, and if this opportunity cost effect is large enough to dominate the countervailing effect of lower household income, then the overall impact on infant mortality could be negative. In addition, the exit of highly polluting plants as consequence of the trade shock may lead to improvements in pollution levels (Cherniwchan, 2017), and this may reduce infant mortality (Chay and Greenstone, 2003; Currie and Neidell, 2005; Arceo et al., 2016). Whether trade shocks increase or reduce infant mortality depends on the relative importance of these mechanisms and thus remains an empirical question.

We study this question systematically in the context of Brazil, a large and heterogeneous developing country that has a uniquely rich set of data sources on trade reforms, health policy, households, and infant mortality spanning more than 3000 Brazilian municipalities over a horizon of 25 years. In the early 1990s, the Brazilian government implemented a major unilateral process of trade liberalization that lowered import tariffs from an average of 30.5 percent in 1990 to 12.8 percent in 1995. This tariff shock was unexpected, sudden, of considerable magnitude, and permanent. Our research design exploits variation in the intensity of the tariff cuts across industries, along with geographic differences in industrial structure, following the neat approach developed by Kovak (2013) and Dix-Carneiro and Kovak (2017). In particular, we construct a Bartik-style measure of exposure to the reform by interacting industry-specific tariff cuts with the baseline industry employment shares in a municipality and refer to this measure as the "regional tariff reduction." We then track the mortality outcomes of infants born to mothers in municipalities with varying exposure intensities before, during, and after the reform. Brazil provides a compelling setting to study this question not only for the unanticipated and plausibly exogenous nature of the trade shock, but also for the availability of vital registries over a long period of time that are deemed of high quality compared to many developing countries.[3] This provides a rare opportunity to examine the short-, medium- and long-run consequences of trade shocks for the production of child health status in a context where infant mortality is a major social issue.[4]

Our findings indicate that the reform had a sizeable impact on infant mortality. We find a meaningful, visually clear, and statistically significant decline in the infant mortality rate in areas

[3] Comparison of official vital registries deaths with indirect estimates from the Brazilian Demographic Health Survey suggests that vital registries underestimate the infant mortality rate by less than 3 percent. For Colombia, the analogous comparison suggests an underreporting rate ranging from 30 to 45 percent (Miller and Urdinola, 2010). For Mexico, Hernández et al. (2012) estimate that the underreporting rate could be about 20 percent. And Greenstone and Hanna (2014) suggest that official vital statistics may understate infant mortality rates by more than 50 percent in India.

[4] According to estimates of the World Bank, the infant mortality rate in Brazil was 52 per 1000 live births in 1990, before the trade liberalization reform, ranking 126th out of 195 countries and making worse than lower income countries such as Zimbabwe, Namibia, Botswana, El Salvador and Honduras. This figure is also comparable to that faced by the United States in the early part of the 20th century, when infant mortality was a significant public health issue (Eriksson et al., 2018).

housing the industries with greater tariff cuts. Before the policy change, more-versus-less-exposed areas exhibit statistically identical trends in the infant mortality rate and begin to diverge only after the introduction of the reform, with a pattern of steadily growing effects that nearly mirrors the dynamic of employment documented in Dix-Carneiro and Kovak (2017). The magnitude of our estimates implies that a one standard deviation in the regional tariff reduction is associated with a decline of about 8-12 percent in the infant mortality rate. We show that these results are not capturing differences in trends related to a wide diversity of baseline characteristics, or other major social reforms affecting mortality such as the introduction of the Family Health Program or the *Bolsa Familia* conditional cash transfer program. When we examine changes in birth rates, we observe an increase in the fraction of births from less advantaged households, suggesting that our estimates are likely to represent lower bounds of true effects.[5] Finally, we do not find any evidence suggesting that our findings may be driven by changes in migration patterns.[6]

We next investigate the relative importance of the opportunity cost effect in driving the observed decline in infant mortality. We begin by revisiting the impacts of the reform on male and female employment. Our estimates imply that men appear to have adjusted to the trade-induced employment shock by transitioning into the self-employment sector, with no meaningful changes in their overall employment rates. By contrast, this adjustment response is not present among women. Women experienced a significant decline in the likelihood of working in exposed industries that is not compensated by an equivalent increase in the self-employment sector, suggesting that women have exited the labor market permanently and arguably allocated more time to home production activities. When we examine changes in household income, we observe a statistically meaningful decline in harder hit locations. This suggests that male adjustment response has not been large enough to compensate for the income losses generated by the trade shock, including those derived from reduced female employment.

These results are broadly consistent with the importance of parental time in the production of child quality. This interpretation is particularly plausible in the context of Brazil, where most preventive and primary health services are provided free of charge, but parents must allocate a considerable amount of time to obtain them (e.g., travelling to distant health facilities and long waiting times in primary care centers). Consistent with this hypothesis, we find that the trade exposure shock is associated with an increase in the probability of preventive visits among women of childbearing ages, which include prenatal care visits, and an increase in the fraction of infants with growth-monitoring and well-care visits. Remarkably, when we examine different

[5] As we discuss in Section 5.3.A, this increase in birth rates reflects to a large extent a decline in fetal deaths.

[6] The lack of a migration response is consistent with the growing evidence suggesting that workers have rather adjusted to the reform by transitioning into other sectors within the same local labor market or by exiting the labor force entirely (Dix-Carneiro and Kovak, 2017, 2019; Ponczek and Ulyssea, 2021). We discuss and present evidence in line with this adjustment mechanism in Section 5.

causes of death, we do not find effects on infant mortality due to causes that are less likely to be related to the use of preventive and primary care services, such as transport accidents, aggression, and other external causes. We also find some evidence consistent in support of a trade-induced improvement in air quality, but the pattern in the data suggests that this channel plays only a minor role. There is limited evidence consistent with other potential mechanisms such as changes in the supply of health services or in the consumption of harmful normal goods (e.g., alcohol and smoking).

Having documented that trade shocks influence the dynamic of infant mortality, we then examine whether and how the returns of public health policies are affected by the trade reform. We focus on the Family Health Program (FHP) and exploit its gradual rollout across municipalities during the 1990s and 2000s for identification. This program provides a number of preventive health services free of charge to the population through a community-based approach. Notably, the provision of these services relies primarily on flexible home visits by health teams, so that in many cases mothers do not need take their infants to primary care centers. This suggests that infants born to parents who work outside of the home and face significant time constraints to visit public health facilities are more likely to benefit from the program. As a result, the trade shock may have affected the returns of the program by reducing the pool of infants for whom the program matters the most (i.e., those born to employed mothers).

Consistent with the hypothesis that parental time matters, we find that the trade shock lowers the impacts of the FHP on infant mortality, or equivalently, the impacts of the trade shock become smaller when the FHP arrives in the municipality. These results provide suggestive evidence that the program serves at least partially as a substitute for parental time in the production of child health status. Thus, policy makers could potentially mitigate some of the unintended consequences of parental work with the adoption of programs like FHP that facilitate the access to primary health care services by bringing them to the home in a flexible fashion. These results also help rule out other potential mechanisms underlying the trade shocks whose relative importance is less likely to be reduced with the arrival of the FHP, such as improvements in air quality. We discuss alternative interpretations of these interactive effects in the final part of the paper.

At this point, there is an important caveat to the interpretation of our findings that we wish to emphasize. Trade liberalization implies access to cheaper goods for all consumers in the country and makes it easier for health care providers to import new technologies, which most likely improved infant welfare. This aggregate mechanism is not captured by the cross-area comparison made here (and in other papers in the literature) and as such, our estimates could underestimate the overall impacts of trade liberalization. One could also think of other mechanisms that might increase overall infant mortality and in this case any extrapolation of our estimates to the aggregate decline in infant mortality should be viewed with greater caution.[7]Nevertheless, at a broad level, our findings suggest that trade policies can have important implications for the production of child health status and the design of public health programs in a context where critical health inputs are inexpensive yet time intensive.

The core contribution of this paper is to provide a comprehensive examination of the impacts of a trade policy change on a dimension that has received relatively little attention in the literature and that is likely to have important long-run implications for well-being. Existing research on the effects of trade has generally focused on adult mortality, motivated in part by the literature on "deaths of despair" (Dorn et al., 2019; Adda and Fawaz, 2020; Pierce and Schott, 2020), but these estimates are not necessarily generalizable to the production of child health status.[8]The few studies linking trade policy and infant mortality have relied on cross-country analyses, with mixed evidence (Levine and Rothman, 2006; Barlow, 2018; Panda, 2020). However, the results from these studies are difficult to interpret because trade reforms typically come together with other national-level shocks and policy changes that could directly determine infant mortality trajectories.[9]Our focus on a unique trade liberalization episode that nearly resembles a once-and-for-all event and the nature of our sub-national level analysis allow us to overcome the challenges that plague most cross-country analysis and make stronger inferences about mechanisms.

The findings of this paper contribute more broadly to a voluminous literature on economic shocks and infant health (see Ferreira and Schady (2009) for a review). While many studies have exploited quasi-random variation in commodity prices (Miller and Urdinola, 2010) and weather conditions (Burgess et al., 2014) for identification, they focus on shocks that are transitory and affect almost exclusively rural markets. It is unclear the extent to which these estimates are generalizable to other shocks of high policy relevance —such as trade reforms —that are permanent and felt by urban households who be better able to adjust to such events. In addition, the generalizability of studies exploiting variation in weather fluctuations to other economic

[7]For example, although there appears to have limited effects on local health spending, the trade shock may have reduced state revenues and thus its capacity to provide public health services or goods at the aggregate level. If such effect is strong enough, the decline in infant mortality implied by our estimates may overestimate the aggregate impacts of the trade reform. However, we know that Brazil experienced an unprecedented expansion in aggregate health spending during the 1990s and 2000s. This suggests that the aggregate revenue-effect induced by the trade shock is unlikely to have been salient enough during this period. But this is admittedly speculative, and we should be cautious when interpreting the aggregate impacts implied by our estimates.

[8]In addition to adult mortality, Dorn et al. (2019) explore the impacts of trade-induced shocks on a number of socioeconomic outcomes, including children living conditions, in the United States. The authors show that children are more likely to live in poverty households as result of the trade shock, but they do not examine directly child health status or other forms of child human capital.

[9]A further difficulty in these cross-country analyses is that the nature of the reforms is often heterogenous across countries, with some implementing bilateral trade agreements while others unilaterally reducing trade barriers, and with trade policies constantly evolving or reversing over time in many countries. Thus, it is difficult to understand whether the estimates reflect the impact of import or export shocks and identify the long run dynamic of the impacts.

shocks is also unclear because abnormal weather conditions could affect not only rural labor markets but also sanitary, water supply and other environmental conditions that have been shown to affect infant health directly (Barreca, 2010; Hanna and Oliva, 2016).[10] We contribute to the literature by documenting the long-run dynamic of a permanent shock hitting mostly urban markets and exploiting a cleaner source of variation. We also provide novel evidence on the interplay between economic shocks and public health programs, an issue of major policy interest that has not been systematically explored in the literature.

Finally, our findings showing interactive effects between the trade reform and health program add to the literature on the impacts of community-based health interventions and the determinants of infant mortality (Rocha and Soares, 2010; Bhalotra et al., 2019; Cesur et al., 2017). We provide suggestive evidence that these programs may play a particularly important role in settings where mothers engage in the labor market and thus face significant time constraints to visit distant, overcrowded health facilities. This is important from a policy perspective in view of the women's rising labor force participation and growing popularity of these programs in the developing world.

The rest of the paper proceeds as follows. Section 2 provides background information on the liberalization reform. Section 3 describes the data and identification strategy. Section 4 presents the main findings. Section 5 explores potential mechanisms and alternative explanations. Section 6 examines the interplay between the reform and FHP. Finally, Section 7 concludes.

2 Background on the Trade Reform in Brazil

In the late 1980s, Brazil entered a liberal phase in its commercial relations, with the beginning of a unilateral trade liberalization policy which put on a finish a pattern of protection of about 100 years. During the pre-liberalization period, the Brazilian economy was characterized by a broad range of protectionist measures that included: (i) high tariff levels; (ii) non-tariff barriers such as lists of banned items —up to 1,300 products —annual maximum limits of external purchases, and import authorizations; and (iii) a set of special regimes guaranteeing tariff reductions or exemptions for a number of transactions (Kume et al., 2003). By 1989, the government partially reduced tariff redundancy and removed a few special regimes and trade-related taxes. But because of the existence of other non-tariff administrative barriers, nominal tariffs did not reflect the actual level of protection against foreign competition.[11]

Trade liberalization effectively started in 1990 with a series of reforms introduced by the newly elected president Collor. Collor suddenly eliminated a number of non-tariff barriers,

[10] Hanna and Oliva (2016) provide a rich discussion of the channels by which weather shocks could affect infant mortality in developing countries, including changes in the prevalence of mosquito vectors that transmit a number of diseases.

[11] Hay (2001) shows that in 1988 the special regimes accounted for approximately 70 percent of all imports.

leaving import tariffs as the main instrument of trade policy. As a result, the import tariff levels started to reflect the actual level of protection since 1990. Following this major policy change, the government implemented a gradual process of unilateral reduction in import tariffs that was complete in 1995. On average, the import tariff fell by about 30 percentage points. This decline was heterogeneous across industries, with the bulk of these declines occurring in sectors with high initial import tariffs. As seen in Figure 1, the slope of the regression of the import tariff change (1990 to 95) on the 1990 import tariffs is -0.73 and the R^2 is 0.93, so the initial import tariff change explains almost all of the variation in the tariff declines during this period.

Remarkably, the tariff cuts were permanent. Figure 2 shows the slope of the regression of the change in import tariffs between 1990 and each year on the 1990 import tariff level. As one can see, the slope is statistically insignificant and small in magnitude during the pre-liberalization period. It becomes increasingly negative since 1991 up to 1995, and then remains very stable during the rest of the post-liberalization period. Our empirical strategy exploits the heterogeneous nature of this once-and-for-all shock, together with variation in the baseline composition of employment across municipalities, to identify the regional impacts of the reform on infant mortality.

3 Data and Research Design

This section describes the data sources, defines the key variables used in the analysis and presents our empirical strategy.

3.1 Data Sources, and Definitions

Our main empirical analysis uses data on infant mortality covering the 1985-2010 period.[12]The choice of the outcome period is determined on the back end by the availability of birth estimates from the population census, whose last round was in 2010. The geographic unit of analysis is a municipality. While most studies use rather a microregion as unit of analysis, we implement all of our analysis at the municipality level primarily for consistency with our interactive exercise in Section 6 where we explore the extent to which the Family Health program affects the impacts of the trade shock. The program was implemented at the municipality level and thus the relevant identifying variation occurs at this level of aggregation. In robustness exercises, we show that our estimates of the effects of the trade shock on infant mortality do not dependent on the

[12]While municipality-level data on infant mortality are available since 1979, they did not cover all municipalities in our sample prior to 1985. Indeed, the Ministry of Health expanded notably the coverage of infant mortality data between 1979 and 1984 in a staggered manner. Therefore, we exclude the 1979-1984 period from the analysis due to the measurement difficulties during these years.

level of aggregation.[13] After accounting for changes in municipality boundaries over time, our sample contains approximately 3567 time-consistent municipalities.[14,15] For simplicity, we use the terms "original municipalities" or simply "municipalities" to refer to these time-consistent municipalities.

Mortality Data. Data on infant mortality come from annual death certificates. We have access to the microdata files from the Brazil's Ministry of Health, which contain comprehensive information on date of death, cause of death, birth date, race, and gender as well as information on mother's place of residence. Local registration offices are responsible for issuing death certificates, which are based on reports from medical practitioners validating the cause of death and the identity of the deceased. By law, no burial can take place without an official death certificate. The coverage of Brazil's death statistics is remarkably high for the standards of developing country vital registries —about 97 percent of all annual deaths.[16] Taken as a whole, we observe about 2 million infant deaths between 1985 and 2010.

Using individual-level data, we generate counts of infant deaths for each municipality-year cell. To obtain infant mortality rates in a given municipality and year, we need information on birth counts. However, official birth statistics at a lower level of geography than a state are only available since the mid-1990s. In the absence of official birth records for our full period of study, we calculate birth counts in each municipality-year using census data as the total of individuals under one year of age. The data indicate that this is an adequate approximation: the coefficient correlation between birth counts from the census and vital registries in 2000 is 99.9 percent (Appendix Figure A.1).[17] We convert infant death counts into infant mortality rates in a municipality-year by dividing the total number of infant deaths by the total number of births

[13] Our municipality-level analysis has the advantage that we are able to exploit greater variation in the local exposure to the reform. A potential disadvantage is that it ignores potential spillover effects between adjacent municipalities due for example to commuting work. In practice, when we implement the same microregion-level analysis, we find effects that are somewhat larger in magnitude (Appendix Table A.1). This suggests that if anything any spillover effects are likely to go in the same direction as the direct effects and thus our results from the municipality-level analysis will tend to be conservative.

[14] Between 1980 and 2010, municipality boundaries changed substantially due to the creation of new municipalities, resulting in an increase of about 40 percent in the number of municipalities during this period. We match child municipalities to parent ones using a cross-walk developed by the Institute of Applied Economic Research. The results are similar if we focus on the sample of municipalities that did not experience any boundary change throughout the entire period. For the interested reader, these results are presented in Appendix Table A.7.

[15] We omit the municipality of Manaus, as it is part of a free trade zone and therefore unaffected by tariff cuts during liberalization. Of course, the exclusion of this single observation does not affect our estimates.

[16] For example, data from the Demography Health Survey (DHS) indicate that the infant mortality rate in Brazil is 26.6 per 1000 births for the 1990-1995. The death certificates from Ministry Health suggest an infant mortality rate of about 25.8 per 1000 births during the same period. This death coverage is strikingly high when compared to Colombia, for example, where underreporting rates range from 30 to 45 percent (Miller and Urdinola, 2010).

[17] We also make this comparison in pre-reform years using data from the Civil Registry, which are available since the 1980s but at the state level only. We find a correlation coefficient of 98 percent.

obtained from the census data. In our dynamic analysis that uses intercensus years, we linearly interpolate birth counts to calculate infant mortality rates during these years. Several previous studies have followed a similar approach to calculate crude infant mortality rates (Clay et al., 2014; Komisarow, 2017; Anderson et al., 2020).

Census Microdata. We use individual-level information from Brazil's population census for the years 1980, 1991, 2000, and 2010. These data provide demographic information, including gender, age, and race, as well as socioeconomic characteristics such as educational attainment, employment, occupation, industry categories, labor income, and nonlabor income. We use these data for several purposes. We employ the census conducted in 1980 to calculate the baseline industry structure of employment across municipalities. This calculation serves as a key input in the construction of the regional measure of exposure to the trade reform. In our investigation of mechanisms and other supplementary exercises, we look at changes in several outcomes between census years. In particular, we explore changes in employment rate and mean household income. We compute the share of adults working outside for money and self-employed occupations, separately for men and women. Finally, in robustness exercises, we control for a number of initial municipality demographic characteristics, all of which are obtained from the 1980 census.

Household Survey Microdata. To examine changes in the use of preventive health services, we use the *Pesquisa Nacional por Amostra de Domicílios Contínua (PNAD)*. The PNADs are nationally representative household surveys that collect a variety of demographic and socioeconomic information. It is conducted every year since 1967 during the month of September by the Brazilian Institute of Geography and Statistics. On average, approximately 400,000 individuals are sampled in each round of the PNAD. The PNADs conducted in 1986, 1998, and 2008 ask individuals about the use of health services. We look at well-child care visits and primary care visits among women of childbearing age. The latter includes prenatal visits and other preventive health services. Since the PNAD is only representative at the state level, we collapse these outcomes into state-year cells.

Family Health Program Data. We use data on the family health program (FHP) to investigate alternative explanations to our main findings and to explore potential interaction effects with the trade liberalization reform. To this end, we obtain data on the date of FHP adoption from Bhalotra et al. (2016), who originally collected this information from the Brazilian Ministry of Health through its Department of Basic Attention. Given the substantial changes in municipality boundaries over time, it is possible that there is more than a date of FHP adoption associated to some of our time-consistent municipalities in our sample. Approximately 17 percent of the original municipalities in our sample have at least two child municipalities with different dates of FHP introduction. In these cases, we define the date of FHP implementation as the earliest date observed in the time-consistent municipality.[18]

Other Data. We collect additional data to strengthen the interpretation of our results and examine alternative hypothesis. Some of these data are available at the State level only, and from 1994 onward. These data include: imports from the rest of the world, obtained from Comex Stat; local health spending from the Brazilian Ministry of Finance; hospitals from the *Pesquisa de Assistência Médico-Sanitária* (1981-1990) and *Cadastro Nacional de Estabelecimentos de Saúde* (2005-2010); municipality-level measures of air pollution from the Emissions Database for Global Atmospheric Research.

3.2 Research Design

Our empirical strategy exploits geographic variation in pre-existing industrial composition and variation in import tariff cuts across industries in a shift-share style design. To illustrate this idea, suppose that the trade liberalization reform led to lower infant mortality rates. Consider two municipalities, one specialized in the agricultural sector and another one in the textile industry. Pre-liberalization tariffs on imports were nearly zero on the agricultural sector and relatively high on the textile industry, so the latter experienced a much more pronounced decline in the levels of protection. This implies that the municipality specialized in the textile sector was disproportionately exposed to the reform and as a result, it should experience larger improvements in infant mortality rates. By comparing the pre-post changes in infant mortality rates in both municipalities, one could identify the regional effects of the policy on this outcome.

Below, we describe our measure of regional exposure to the reform and the empirical model which generalizes the illustration above. We then discuss the key assumption necessary for identification.

3.2.A Measuring Regional Exposure to Trade Policy

Using an approach that is similar in spirit to Dix-Carneiro and Kovak (2017) and Kovak (2013), we construct a measure that indicates the degree of regional tariff reduction (RTR) as follows:

$$RTR_i = \sum_{j \in J} \ell_{ij} \times (Tariff_{1990,j} - Tariff_{1995,j}) \tag{1}$$

where i indexes municipalities, j indexes industries, and ℓ is the baseline share of each industry in the municipality. In computing the baseline employment shares, we employ the 1980 census to focus on historical, persistent differences in industrial composition that were prevalent before

[18] This is the same approach followed in Bhalotra et al. (2016). The results are almost unchanged when we focus on municipalities whose boundaries have remained unchanged during the study period.

the liberalization reform. This avoids endogeneity issues due to mean reversion associated with industry employment in the 1990s. The set of industries J excludes the non-tradable sector, so the exposure measure only exploits variation within the tradable sector. This measure is consistent with the theoretical model of trade developed by Kovak (2013). Intuitively, since non-tradable sector prices move together with prices of the tradable goods in a setting with perfect labor mobility across sectors, the impacts of the tariff cuts depend only on the extent to which the tradable sector is exposed to the shock (see Kovak (2013) for details).[19]In supplementary analyses, we show that the conclusions are essentially the same when we employ alternative constructions of the RTR exposure measure, which include explicitly the non-tradable sector and the exact measure developed by Kovak (2013).

By the way we define the tariff shock, higher values in RTR mean higher declines in the import tariffs. Figure 3 illustrates that there is substantial variation in RTR exposure across municipalities, with a standard deviation of 4.4 percentage points (relative to a mean of 2.3 percentage points).

3.2.B Specification

To estimate the effects of the trade reform on infant mortality, we employ the following specification:

$$y_{it} - y_{i1991} = \alpha_t + \beta_t \, RTR_i + \gamma_t \, (y_{i1990} - y_{i1985}) + \delta_{rt} + \xi_{it} \tag{2}$$

where y is the log infant mortality rate in municipality i, year t, RTR is the regional tariff reduction measure, δ_{rt} denotes microregion-year fixed effects, and $(y_{i1990} - y_{i1985})$ is the pre-reform trend in the outcome of interest.[20] We follow Alsan and Goldin (2019) and use infant mortality rates in logs, as it allows the mortality rates to decrease over time in differential amounts across municipalities. The specification given by equation (2) is essentially the same as that in Dix-Carneiro and Kovak (2017), except that our municipality-level analysis allows us to include a rich set of microregion fixed effects.[21] Microregions are within-state divisions that group several neighboring municipalities with similar economic and social characteristics, so we are able to examine a much finer variation.[22] Even within microregions, there is substantial

[19] An alternative approach would be to include the non-tradable sector by implicitly assuming a zero tariff change. Kovak (2013) shows that this approach is likely to introduce a upward bias. In studying the effects of the liberalization reform on labor market outcomes in Brazil, he shows that including the non-tradable sector leads to estimates that are about four times larger in magnitude.

[20] Since there are a few municipality-year observations in which infant deaths are equal to zero, we calculate the log infant mortality rate as $log[(x+1)/y]$, where x is the number of infant deaths and y is the number of births. In practice, this transformation makes virtually no difference for our results because the frequency of zeroes is low —less than 5 percent. Not surprisingly, when we exclude municipalities with zero infant deaths and estimate the model using the untransformed data, we find extremely similar results.

[21] The unit of analysis in Dix-Carneiro and Kovak (2017) is a microregion, so they are unable to include microregion fixed effects in their specification.

[22] There are 411 microregions in our sample, so they contain 9 municipalities on average. The Brazilian Bureau

variation in the tariff exposure shock. Microregion fixed effects account approximately for 56 percent of the overall variation in the RTR exposure, leaving a fair amount of within-microregion variation for identification.[23] The model (2) is our basic specification, but we will present results without microregion fixed effects and pretrends, as well as results that include a broader set of additional controls. All regressions are weighted by the number of births in 1991 to account for differences in precision with which the coefficients are estimated.

Identification. A causal interpretation of our estimates rests crucially on the assumption that absent the liberalization reform, more- and less-exposed areas would have experienced similar changes (or trends) in the outcome of interest. Applying recent advances in shift-share designs to our setting (Borusyak et al., 2021), the identifying condition can be formally expressed as follows:

$$Cov\left[RTR_i, \xi_i\right] \equiv \sum_j^J \ell_j \Delta Tariff_j \mathbb{E}\left[\frac{\ell_{ji}}{\ell_j} \cdot \xi_i\right] \xrightarrow{p} 0$$

In words, identification requires that the weighted covariance between the tariff shocks and the unobservable term $\mathbb{E}\left[\frac{\ell_{ji}}{\ell_j} \cdot \xi_i\right]$ converge to zero. Intuitively, the latter term represents a weighted averages of the residuals ξ_i.

Note that it is not necessary that more- and less-exposed areas are similar in observable or unobservable dimensions for identification. Identification requires only that any such differences remain approximately constant over time. Since equation (2) is a first-difference model, unobservable factors that are time-invariant will be differenced out. By conditioning on the detailed set of microregion fixed effects, we are identifying the parameter of interest from comparisons between more- and less-exposed municipalities within the same microregion within the same state only. This eliminates potential time-variant factors such as common regional policies and shocks that affect municipalities in the same microregion similarly. Moreover, the inclusion of the pre-liberalization outcome trends means that even if there are omitted factors varying in proportion to the RTR exposure within microregions, such confounders would have to mimic the timing of the reform very closely. Any unobservable shocks that reflect pre-existing, differential long-rung trajectories will be largely captured by the pre-trend outcome control. In sum, our specially demanding specification makes it difficult to think of obvious stories of endogeneity.

Most importantly, we will present a full set of estimates of β_t for the period before, during, and after the trade liberalization reform. If the identifying assumption is valid, then more- versus less-exposed areas should have experienced similar trajectories before the reform and diverge only

of Statistics created these subdivisions for statistical purposes.

[23] We also observe substantial variation in infant mortality rate changes within microregions. For example, microregion fixed effects account for 47 percent of the variation in the log-change in infant mortality rates, leaving a large portion of the overall variation in this outcome unexplained.

after the reform took place. We view this exercise as the most compelling test of the plausibility of the research design. As we shall see, the results are broadly consistent with the identification condition.

Inference. In all of our analysis, we use standard errors clustered at the state level to account for possible spatial correlation in residuals within states. Since we are using a shift-share identification strategy, such spatial correlation may occur even between municipalities in different states due to areas with similar sectoral shares (Adao et al., 2019). In Appendix Table A.2, we show that the results are very similar if we use the inference procedures proposed by Adao et al. (2019) and Borusyak et al. (2021) that address cross-region correlation in residuals in shift-share designs. This suggests that in practice much of the relevant cross-area correlation in residuals occurs within states. We also evaluate the validity of our results to alternative inference checks, including a wild-bootstrap procedure that corrects for small-cluster biases (Appendix Table A.2, column 2), and a randomization inference procedure to construct exact p-values (Appendix Figure A.4).[24]

4 Effects of Trade Liberalization on Infant Mortality

4.1 Main Results

Dynamic Estimates. We begin by presenting the full path of responses of infant mortality to the reform. Figure 4 plots the coefficients β_t and respective 95 percent confidence intervals from estimating equation (2) for each year between 1985 and 2010. As discussed in the background section, the liberalization reform was implemented gradually between 1990 and 1995, and Dix-Carneiro and Kovak (2017) document that the labor market impacts become particularly meaningful only after the reform was complete. We therefore separate the full period into three treatment statuses, denoted by the vertical bars in the figure: "pre-liberalization"=year$\leq$ 1990, "partial liberalization"= year $\in \{1991, 1995\}$, and "post-liberalization"=year$\geq$ 1996. We take the reference year to be 1991 and not 1990 to match the census, but this does not affect the pattern around the year the reform was implemented.

Figure 4 shows that during the pre-liberalization period, the import tariff shock is unrelated to changes in infant mortality rates. The estimated coefficients are small in magnitude and statistically insignificant. Since the reference year is 1991, the fact that the pre-1991 coefficients

[24] As a further check, we follow Ferman (2021) and run a placebo regression where the dependent variable is a random variable drawn from a standard normal distribution with a mean of 0 and a standard deviation of 1 . We repeat this procedure 500 times and compute the fraction of times the coefficient of interest is statistically significant at the 5 percent level. When we implement this approach, we find a fraction of significant effects of approximately 5.8 percent, confirming the reliability of the baseline approach to inference.

are positive suggests that there is a negligible and statistically insignificant decline in the infant mortality rate in 1991 relative to the pre-reform years. What is more important from a causal analysis perspective is that the pre-reform coefficients do not display any clear tendency toward improving or deteriorating infant mortality rates in areas that experienced a larger or smaller exposure to the reform. Indeed, the pre-reform coefficients exhibit a trend that is clearly flat and fluctuate randomly around a mean of 0.72. This provides strong support for the validity of the identifying assumption that more-versus-less-exposed areas would have seen similar changes in the absence of the trade shock.

After the reform was introduced, there appears to have a slight decline in infant mortality rates that becomes highly significant in the post-liberalization period, with a pattern of steadily growing effects. The timing of these effects and their steadily growing magnitude nearly mirror the labor market responses documented in Dix-Carneiro and Kovak (2017), though the response of infant mortality is somewhat slower. This correspondence in the dynamic impacts of the reform on infant mortality and employment outcomes is consistent with changes in local economic conditions and parental work being a key mechanism underlying our results. The slower response of infant mortality suggests that trade-induced employment shocks do not instantaneously affect infant mortality outcomes. We develop this point in more detail in Section 5.

Baseline Estimates. The dynamic results provide visually clear evidence that the reform led to improvements in infant mortality. To summarize magnitudes in tables and evaluate the robustness of our results in a parsimoniously way, we now focus on the 1991-2000 and 1991-2010 periods. The focus on census years is preferred because it avoids the need to interpolate the denominator when computing infant mortality rates and thus reduces measurement error.

These results are reported in Table 1. Panel A presents results for the log-change in infant mortality rates between 1991 and 2000, whereas Panel B focuses on the 1991-2010 period. Column (1) reports estimates based on a specification that does not include any control variable, thus displaying the unconditional correlation between the RTR exposure and changes in log infant mortality rates. This bivariate relationship is negative and significant at less than 1 percent. The coefficient of interest is estimated at -4.31 in Panel A, with a standard error of 0.8. The longer-run coefficient in Panel B is somewhat more negative and similarly very precisely estimated.

In column (2), we condition on state fixed effects, which filter out the effect of state-level policies or contemporaneous economic conditions that affect all municipalities in a state similarly. These controls are important in the context of Brazil given that the state governments have autonomy to implement specific policies and programs in different dimensions, including health. The estimated coefficients decline somewhat in magnitude but are now more precise and remain highly statistically significant, reflecting the substantial reduction in sampling variation. Column

(3) goes one step further and includes the granular set of microregion fixed effects. Now the comparison is between municipalities of the same microregion that differ in the extent to which were exposed to the tariff cuts. This specification is more demanding because it eliminates a part of the identifying variation in the trade shock exposure, as municipalities in the same microregion tend to have similar industry composition and thus face a similar exposure to the tariff shock. While the inclusion of this detailed set of fixed effects reduces the magnitude of the coefficients notably, these controls greatly improve the fit of the regressions. As a result, the results become more precise and continue to be highly significant. This is consistent with the substantial variation in the exposure to the tariff shocks across municipalities.

Column (4) presents the results from our preferred specification, which adds the outcome pretrend variable as an additional control. Now, the magnitude of the estimated coefficients is slightly larger. These estimates imply that a one standard deviation increase in RTR exposure is associated with a decline in the infant mortality rate of 0.09-0.13 log points. This effect represents a decline of approximately 10 percent of a standard deviation in decadal changes in the log infant mortality rate.

Causes of Deaths. Table 2 explores in more detail the nature of the relationship between trade shocks and infant mortality by looking at specific causes of death. We split our outcome variable into nine classifications: infectious and respiratory (column 1); Perinatal (column 2); congenital anomalies (column 3); other internal causes (column 4); transport accidents (column 5); other accidents (column 6); aggression (column 7); and other external causes (column 8). The magnitude and significance of our coefficient of interest is the highest for infant deaths from infectious and respiratory causes. A major factor associated to this category of death is water quality (Bhalotra et al., 2017; Ferrie and Troesken, 2008), and trade-induced unemployment could have improved parental time to collect water of higher quality from distant sources. This is especially likely to be true in our setting as a significant portion of households have still limited access to improved water quality from public systems, and thus many have to spend considerable amounts of time to obtain pure water from alternative sources (Silva, 2009).[25]

In the last four columns, we examine changes in external causes of deaths. The coefficient of interest is much smaller in magnitude and in no case are there statistically significant effects. This is consistent with external causes of deaths being most likely driven by other, more idiosyncratic factors. We interpret this null result as evidence suggesting that the decline in infant mortality from internal causes is unlikely to be the result of omitted factors that affect both external and internal causes of infant deaths.

[25] According to the census conducted in 1991, approximately 20 percent of households did not have access to piped water and only 30 percent was connected to a sewage system.

Sensitivity Analysis. We have carried out a number of sensitivity analyses to evaluate the robustness of our results, most of which are summarized in Figure 5 and presented in detail in the Online Appendix. Appendix Table A.3 shows that our results are robust to controlling for a wide variety of pre-reform socioeconomic characteristics (as measured in 1980) that may also explain the decline in infant mortality, including median household income, employment rate, share of skilled, share of blacks, share of whites, population density, share of households with piped water, share of households with access to a sewage system, hospital presence, a measure of income inequality, and a Metropolitan status dummy. The inclusion of this diverse set of controls yields estimates of β and standard errors that are almost identical to those obtained from our parsimonious baseline specification. Appendix Table A.4 documents the robustness of our findings to alternative constructions of the RTR measure: i) using the 1991 (instead of the 1980) census to compute employment shares; ii) including the non-tradable sector by imputing a zero tariff change in these industries; and iii) using the exact RTR measure developed by Kovak (2013) that adjusts for the cost share of nonlabor factors. These alternative ways of computing the RTR yield results that are qualitatively and quantitatively similar to the baseline. This is unsurprising given the almost perfect correlation between these alternative measures with ours, ranging from 93 to 99 percent.

Appendix Table A.5 shows that excluding each industry one by one, which includes those with the highest "Rotemberg" weights (Goldsmith-Pinkham et al., 2020), does not materially affect the magnitude of the coefficient β. Appendix Figure A.5 excludes sequentially the 50 largest municipalities in the sample, which account for about 30 percent of all the births in 1991 and thus may have been driving our weighted regressions. The results remain unchanged. Appendix Figure A.6 carries out a similar exercise by excluding entire states from the sample one by one, with little impact on the estimated coefficient of interest. Finally, Appendix Table A.6 shows the robustness of the baseline estimates to excluding all municipalities belonging to a Metropolitan area, which as a whole account for approximately 44 percent of the entire population. The omission of these areas makes the results stronger, providing further evidence that our estimates are very unlikely to be driven by a few particular locations or industries.

Overall, we find that greater exposure to the tariff shock is robustly associated with larger declines in infant mortality.

5 Mechanisms and Interpretation

In this section, we investigate the potential role of parental time and air pollution in driving the gains in infant mortality we find. We then examine alternative interpretations and threats to internal validity that could also generate negative estimates of β.

5.1 Adult Time

A potential mechanism underlaying our results is parental time. As documented in previous work, the trade liberalization reform negatively affected formal employment in areas that are home to exposed industries (Dix-Carneiro and Kovak, 2017). This in turn may have improved infant mortality rates by increasing time-intensive health investments. Most preventive and primary health services are free-of-charge in Brazil, but parents must allocate a considerable amount of time to obtain them. For example, routine prenatal and childcare visits often imply to wait in long queues or travel to distant health facilities, which in some cases are not available in the same municipality as that of residence.

To investigate this hypothesis, we first document how the trade shock affected both male and female employment outcomes. In doing so, we collapse employment outcomes from the censuses at the municipality-year level and estimate equations analogous to (2). Since the population census is conducted decennially, the pretrend outcome controls are calculated between 1980 and 1991. We limit the sample to individuals between ages 18 and 45 to focus on those who are most likely to have had young children in the past 10 years, although the conclusions are essentially the same if we include older individuals.

Table 3 reports the estimation results. Columns (1) and (4) look at changes in the share of adults working outside of the home for money, which largely includes formal employment in tradable industries. We observe statistically significant and negative effects on this outcome, with larger effects in magnitude for males.[26] These results do not tell the entirety of the story. Working-age adults may have adjusted to the adverse labor market shock by transitioning into self-employment occupations, which tend to be mostly informal jobs with no health or social security benefits. The extent to which this adjustment is complete, the trade liberalization reform should lead to a one-for-one rise in self-employment.[27] Column (2) documents a statistically significant increase in self-employment among males, with estimates of similar magnitudes to the decline in employment observed in column (1). This pattern is not observed for women. The estimated effects for women are very small in magnitude and, if anything, negative over the long term (column 5). Columns (3) and (6) show that while overall employment rates remain virtually unchanged for men, women experience a meaningful and permanent drop in their overall employment rates.

Given that men have adjusted to the trade-induced shock by transitioning into the self-employment sector, a natural question to pose is how the tariff shocks have affected household

[26] This heterogeneity in the impacts of the reform across genders is consistent with Gaddis and Pieters (2017) and could reflect in part that males are more likely to work in industries that experienced larger import tariff cuts. Moreover, since men have in general higher employment rates than women, it is possible that these differences are also capturing that the scope for employment declines is higher for men than women.

[27] Dix-Carneiro and Kovak (2017, 2019), and Ponczek and Ulyssea (2021) provide evidence consistent with this adjustment response, but they do not distinguish between male and female employment.

income. If the increase in self-employment income is large enough to compensate for the income losses generated by the import tariff cuts, including those derived from reduced female employment, then the net impact on household income may be limited or even positive. This seems an unlikely scenario, as the income earnings in the self-employment sector tend be much lower than in other sectors. Consistent with this fact, column (7) of Table 3 shows that greater exposure to the tariff cuts is associated with lower household income.

In sum, men appear to have completely adjusted to the trade-induced shock by transitioning into the self-employment sector, while women have apparently exited the labor market permanently. If the demand for monetary health inputs is normal, and thus likely decreased with lower household income, then this set of results suggests that maternal time has had important implications for household production of child health. We next turn to examining these implications.

Use of Preventive Health Services. Women who have exited the labor market have arguably a greater ability to invest in health improving behaviors that are inexpensive yet time intensive, including the use of preventive health services. To examine changes in the use of preventive health services, we use data from the PNAD conducted in 1986, 1998 and 2008. To the best of our knowledge, these household surveys are the only available source of data with information on the use of preventive health services both before and after the reform. That said, it bears noting that the PNAD is not representative at the municipality level, so we are forced to perform this analysis at the state level. We do not view this as a particularly important limitation, as there is a great deal of variation in the tariff exposure shock across states, and as we show in Appendix Table A.1, our infant mortality analysis at the state level is consistent with the municipality-level results.

Table 4 reports the estimation results. Columns (1) and (2) shows that the reform led to an increase in the probability of a preventive health visit in the past 12 months among women of childbearing age. This effect is very precisely estimated and thus significant at less than 1 percent. Columns (3) and (4) look at the share of childcare visits. These visits are one of the most important ways of preventing and detecting childhood health problems before they become serious. Moreover, during these visits, parents receive health information on safety behaviors and on how to manage emergencies and illnesses. We find that greater exposure to the tariff shocks is associated with a significant increase in the share of infants with childcare visits.

Taken as a whole, we observe significant gains in the use of basic health services. While we do not observe all of the possible forms of time-intensive health investments, they most likely move in a similar fashion. For example, it is plausible that infants born to more affected mothers are breastfed for longer, which itself could have important positive impacts on infant survival chances. At a broad level, we interpret the evidence in this section as being consistent with the hypothesis that the tariff shock unintendedly improved infant health by improving parents' time inputs.

5.2 Improvements in Air Quality

An important work by Cherniwchan (2017) shows that the exit of "dirty" plants in response to import competition leads to significant improvements in pollution levels in the United States, which could affect infant mortality. To explore this potential mechanism in our setting, we use data from the Emissions Database for Global Atmospheric Research (EDGAR). This database provides worldwide estimates for air pollutants at the 0.1×0.1 degree latitude/longitude grid since 1970.[28] We collapse these data at the municipality-year level and focus on particulate matter (PM_{10}) and monoxide carbon (CO). Arceo et al. (2016) employ a credible quasi-experimental design in the context of a developing county and document that these pollutants have particularly large impacts on infant mortality.[29] We estimate our preferred first-difference model (2) using the 1991-2000 and 1991-2010 log changes in these pollutants as dependent variables.

These results are presented in Table 5. We observe significant reductions in monoxide carbon, but this effect is only apparent over the long run. Our estimates imply that a one standard deviation increase in RTR is associated with a decline of 3.4 percent in CO between 1991 and 2010. The point estimate for the 1991-2000 is only marginally significant and much smaller in magnitude. It implies a statistically significant decline of 0.5 percent. On the other hand, we do not observe statistically significant improvements in particulate matter. The estimated coefficient is small in magnitude and statistically insignificant. A possible explanation for the relatively small gains in pollution levels is that the tariff cuts in the highest polluting industries, such as petroleum, gas, coal and mining, were relatively small and thus less affected by increased import competition.

We can use estimates from Arceo et al. (2016) to gauge the importance of the gains in monoxide carbon for the decline in infant mortality we find. These authors find that a 1 percent increase in monoxide carbon leads to an increase of 0.32 percent in the infant mortality rate. Using this elasticity and our estimates in Table 5, we estimate that a one standard-deviation increase in RTR would generate a decline in the infant mortality rate of about 0.16 percent in the medium run ($0.16 \approx 0.5 \times 0.32$) and 1.1 percent in the long run ($1.1 \approx 3.4 \times 0.32$). These implied effects are small, representing respectively 2 and 13 percent of the medium- and long-run effects of the trade shock on infant mortality reported in column 4 of Table 1.

[28] A 0.1 degree corresponds approximately to 11 kilometers. Using spatial interpolation methods, EDGAR computes values for each grid node from different emission sources, such as fuel combustion, industrial process, savanna burning, waste burning, forest fires, and fossil fuel fires.

[29] Arceo et al. (2016) exploit quasi-experimental variation in pollution levels induced by a meteorological phenomenon known as thermal inversions to estimate the effects of air pollution in Mexico.

Overall, improvements in air quality appear to play some role in driving the effects of the trade shock on infant mortality, particularly over the long run. However, the evidence suggests that these effects are relatively modest and thus are not the primary force behind the overall impacts.

5.3 Alternative Hypotheses: Selection, Coincident Shocks, and Other Potential Mechanisms

In this section, we explore several alternative explanations that may also generate negative estimates of β in our estimation of equation 2. Possible alternative stories can be divided into three categories: *i*) selection into the sample (due to changes in fetal deaths, fertility or migration); *ii*) coincident changes in other determinants of infant mortality (such as changes in the supply of health services and local health infrastructure); and *iii*) alternative mechanisms of impacts.

5.3.A Selection

Fertility and Fetal Deaths. The trade-induced employment shock may reduce not only the opportunity cost of time-intensive health investments, but also the opportunity cost of having children. This could lead to increases in fertility, potentially changing the composition of births in our sample. But note that since the reform affected almost exclusively unskilled workers (Ponczek and Ulyssea, 2021), this increase in fertility most likely would skew the distribution of births toward those from lower-socioeconomic status parents who have infants with higher mortality rates. This would bias our estimates toward finding an increase in infant mortality, the exact opposite to what we observe. Of course, one may also think of a story where the trade shock causally reduces fertility rates and potentially introduces an upward bias in our estimates.

It is also important to note that even in the absence of a change in fertility decisions, one may observe an increase in birth rates if the trade shock has a causal negative impact on fetal mortality. But once again, this scenario most likely would bias our estimates toward finding an increase in infant mortality, as significant reductions in miscarriages and still-births mean an increase in the number of weaker births that were on the margin of survival and have higher postnatal mortality rates. Therefore, under this selection hypothesis, our estimates of the effect of trade shocks on infant mortality should be taken to be lower bounds of true effects.

To examine these issues empirically, we estimate the effects of the tariff shock on birth and fetal death rates in Table 6. We calculate the birth rate in a municipality as the number of births divided by the number of women of childbearing age using census data, while the fetal death rate is computed as the number of fetal deaths divided by the number of births. Column (1) shows that there is an increase in birth rates that is precisely estimated, both in the medium-and long-run differences models. Quantitatively, the magnitude of these results is relatively small. The point estimates indicate that a one-standard deviation increase in the RTR exposure is associated with an increase in the birth rate of about 1.7 and 3.7 percent in the medium- and long-run respectively. The evidence also suggests that this effect on birth rate reflect to a large extent a decline in the prevalence of fetal deaths. Indeed, column (2) documents that a one-standard deviation increase in the RTR implies a decline in the fetal death rate of about 8 and 17 percent in the medium- and long-run respectively.

We also use census data to examine whether and how the trade shock affected the composition of births in the remaining columns of Table 6. Consistent with the hypothesis that the increase in the birth rates is largely driven by parents with lower socioeconomic status, we find that the trade shock is associated with an increase in the share of births from households where the head has lower educational attainment and is more likely to be black, and from households where at least one of the parents is more likely to be absent. We conclude that selection due to fertility or fetal mortality is very unlikely to be the responsible for the decline in infant mortality we observe. If anything, the evidence suggests that our estimates represent lower bounds of true effects

Migration. Since the trade reform generated changes in local labor market conditions, a possible concern is selective migration: different families could move away from areas disproportionally affected by the reform. Out-migration could introduce a bias in our estimates if those who do not move to other areas in response to the economic shock are different to those who do. In practice, however, the available evidence suggests that biases due to migration are likely to be small. First, our state-level analysis yields results that are consistent with our baseline (Appendix Table A.1). Since most migration occurs within (rather than between) states, endogenous migration responses to the trade shock are unlikely to significantly bias our estimates at the state level.[30]Hence, the fact that the estimates are robust to aggregating to the state level is reassuring.

Second, in their comprehensive set of robustness and complementary analyses, Dix-Carneiro and Kovak (2017) convincingly show that individuals have not adjusted to the trade shock by migrating away from negatively affected areas. In fact, the consensus in the literature studying the Brazilian liberalization reform appears to be that the self-employment sector has largely accommodated the declining employment rates induced by the import tariff cuts, potentially explaining the lack of a migration response (Dix-Carneiro and Kovak, 2019; Ponczek and Ulyssea, 2021). In Appendix Table A.8, we confirm Dix-Carneiro and Kovak (2017)'s findings at the municipality level, showing that more-versus-less exposed municipalities experienced statistically

[30] According to the census conducted in 1991, within-state migration accounts for approximately 70 percent of the overall internal migration rate in Brazil.

similar changes in the working-age population. Overall, there is no indication that selection due to migration is a significant problem in our setting.

5.3.B Expansion of Health Services

During the 1990s, the single most important social reform that could confound our estimates is the massive expansion of basic health services with the implementation of the Family Health Program (FHP). This program provides primary health services through home visits by family doctors and other health professionals, and the literature has extensively documented that this program led to a decline in infant mortality rates of about 20 percent (Rocha and Soares, 2010; Bhalotra et al., 2019). The magnitude of the FHP impacts is large enough that it alone could plausibly explain all of our results.

To address this issue, we first take advantage of the fact that the timing of FHP implementation varies substantially across municipalities, with some beginning in the mid-1990s and others in the mid-2000s. Specifically, we estimate the impacts of the reform on infant mortality in subsamples that excludes cumulatively municipalities that had already implemented the FHP. We estimate the first-difference specification for the post-reform years between 1996 and 2001, as more than 75 percent of all municipalities had already implemented the FHP by 2002 and thus the non-FHP sample of municipalities becomes too small in 2002 onwards. Appendix Table A.9 shows that our results hold in the subsample of municipalities and periods where the FHP was not present. Although the point estimates tend to be unsurprisingly less precise due to reduced sample sizes, the point estimates remain extremely similar to the baseline and statistically significant in most cases. One could think of a story where the effect of trade shocks in the FHP sample is caused by the program, while the effect in the non-FHP sample is caused by an unobservable factor that generates a coefficient of the same magnitude. But this seems implausible.

In Appendix Table A.10, we take a different approach and use the full sample of municipalities to estimate a specification that includes a detailed set of year-of-FHP implementation fixed effects. This means that the parameter of interest is now identified by the comparison of municipalities within the same microregion that adopted the FHP in the same year but differ in the exposure to the trade shock. Columns (1) and (2) present the medium-run differences results (1991-2000), while columns (3) and (4) show the long-run differences estimates (1991-2010). Even using this more demanding specification, the estimated coefficients remain very similar to the baseline, reinforcing that our results are very unlikely to be confounded by the introduction of the FHP.

The federal government role in the provision of local health services such as hospitals expanded rapidly during the 1990s, and one might be worried that these interventions confound the interpretation of our results. We investigate this issue by examining the relationship between the trade shock and health spending, hospital presence, and number of hospitals per 1000 inhabitants. Despite the spectacular growth in the availability of health resources during this period, there is no evidence that the trade shock is associated with meaningful changes in these measures (see Appendix Table A.11).

5.3.C The *Bolsa Familia* Program

Another possible confounding factor is the introduction of the *Bolsa Familia* program (BFP), a large-scale conditional cash transfer intervention that was launched in 2004 and rapidly covered nearly all municipalities. Households enrolled in the program are required to bring their children for preventive health care services, among other conditionalities, which could lead to improvements in survival chances. Since this program was implemented well after the trade liberalization reform was complete, it cannot be a confounding factor in our medium-run differences estimates. And the fact that the medium- and long-differences estimates are similar suggests that the introduction of the BFP is unlikely to be significantly biasing the long-differences estimates. Therefore, we are less concerned about bias from the BFP.[31]

5.3.D Consumption of Harmful Normal Goods

One could argue that the trade-induced income shock may have directly reduced the demand for some harmful goods that are potentially normal, such as alcohol, drugs, and tobacco, and that have been shown to be strongly associated with infant mortality due to congenital anomalies at birth, sudden infant death syndrome (SIDS), and fetal alcohol syndrome (FAS) (Scragg et al., 1993; O'Leary et al., 2013; Werler et al., 1991).[32] Nevertheless, this possibility is made less plausible in view of the recent literature on "deaths of despair", which suggests that the consumption of such goods is in fact likely to increase during economic downturns (Case and Deaton, 2015, 2017; Pierce and Schott, 2020). In addition, the fact that our results in Table 2 indicate that the smallest effect of trade shocks on infant mortality is from congenital anomalies, one of the causes that has had the greatest association with maternal smoking and alcohol, also speaks against the empirical importance of this potential mechanism.

Given the granularity of our data, we can take a closer look at these results and explore changes in the prevalence of infant deaths due to SIDS, FAS, specific congenital anomaly condi-

[31] Of course, it is possible that BFP coverage is affected by the trade shock, as harder-hit areas experienced a decline in household income and consequently one may observe a higher fraction of eligible households in these areas. Responses of this sort do not introduce a bias *per se* in our estimates but affect the interpretation of the results. In this case, the BFP can be viewed as an additional mechanism of impact, and it could explain in part why we observe larger impacts in the mid- and late-2000s.

[32] For example, O'Leary et al. (2013) show that the effect of maternal alcohol on SIDS is about 5 times as large as that on other causes of infant mortality.

tions, and other causes that have been shown to be directly related to alcohol, smoking and other harmful goods. We term this variable simply as MASRD (maternal alcohol- and smoking-related deaths). We split our outcome variable into MASRD deaths and all other causes. We do not observe statistically significant effects on these causes of death (Appendix Table A.12). In fact, we find a medium-run coefficient that is positive, suggesting that if anything there appears to have a statistically insignificant increase in these deaths during the first post-reform years.

5.4 Discussion

Summing up, the evidence presented thus far is consistent with an important role for parental time, and speaks against alternative explanations such as selection and other coinciding changes. Note that this is not to say that there is no space for other mechanisms. It is possible that the trade shock has impacted infant mortality through other channels that are difficult to discern from the current methodology and available datasets. For example, while the conditional-cash transfer program cannot explain the trade-induced decline in infant mortality predating its introduction in 2004, it could play a role in explaining part of the effects of the trade shock over the long run. Indeed, the trade-induced income shock is associated with an increase in the number of beneficiary families in harder-hit areas (as shown in Appendix Table A.13), and this could have improved the utilization of those health services tied to the receipt of the program transfer. Moreover, these effects may be amplified in the presence of peer effects in behaviors from affected to unaffected parents and from older to younger parents. Finally, there could be other mechanisms that are more geographically diffuse and thus cannot be identified with our empirical approach. Notably, increased imports as consequence of the tariff cuts may have improved infant health through a consumption channel. Although we do not observe that areas with greater exposure to the trade shocks experienced a disproportionate increased in imports (Appendix Table A.14), these results do not speak directly to the importance of this mechanism, as the "level-shift" effect of increased imports is absorbed in our cross-area first-difference model. Separately identifying each mechanism of impact is beyond the scope of this paper and a possible direction for future work.

6 Interaction with Public Health Policy

In this section, we investigate how the introduction of public health policies shapes the impacts of the trade shock on infant mortality. We focus on the FHP and exploit variation in the timing of its adoption across municipalities for identification. Since the program provides primary health care mainly through home visits by health professionals in a flexible manner, so that in many cases mothers do not need go to primary care centers, it represents a shock particularly important for infants born to mothers participating in the labor market who face significant time restrictions to visit public health facilities. Hence, studying the interaction effects between the trade shock and FHP could further light about mechanisms underlying the link between trade shocks and infant mortality.

6.1 Background on the Family Health Program

As mentioned in Section 5.3.B, the FHP represents one of the most important public health policies implemented during the last three decades in Brazil. Under this program, each citizen is assigned to a team of health professionals (one family doctor, one assistant nurse, and six health community agents) located in walk-in clinics who provide a number of primary health services: immunization, monitoring the health of pregnant women and infants, specialized counseling and some diagnostic and curative services. The provision of these services relies heavily on home visits initiated by community agents, each of whom is responsible for approximately 1000 families within the same neighborhood. The FHP health teams proactively identify individuals with healthcare needs and deal directly with simpler conditions.

For those mothers who work outside of the home, it is possible to flexibly schedule the FHP visits for non-working times, including the weekend. In addition, the health teams also visit day-care centers regularly to reach infants who are not always available at home. Given this flexibility in the provision of health services, employed mothers are likely to benefit the most from the program, this program is likely to make the most difference for employed mothers facing significant time constraints to visit public health facilities. This has often been noted for several commentators and policymakers. For example, when highlighting the role of the FHP during the immunization campaigns, a municipality health official succinctly stressed that:[33]

> *"[the FHP] is a major opportunity for mothers who work during the week and have no time to take their children to be vaccinated."*(Quoted in Itabira Government, May, 2021)

The FHP was launched in 1994 as a pilot program in a few municipalities, and then extended gradually to other municipalities, covering approximately 97 percent of municipalities by 2010. Panel A of Figure 6 shows this substantial variation in the timing of FHP adoption across municipalities. The differences in the timing of FHP adoption are largely the result of differences in the length of time required for the Ministry of Health to coordinate actions with local government and offices, factors that are not directly related to the health status of the local population.

[33]See "Secretária acompanha sábado de vacinação contra a Influenza em FHP do João XXII/Machado", *Itabira Government*, May 1, 2021, at https://turismo.itabira.mg.gov.br/detalhe-da-materia/info/secretaria-acompanha-sabado-de-vacinacao-contra-a-influenza-em-psf-do-joao-xxii-machado/174652 (last on October 14, 2021).

Consistent with this notion, Rocha and Soares (2010) document that the expansion of the FHP is not systematically correlated with pre-FHP trends in a number of health outcomes.

6.2 Specification and Results

In this section, we provide evidence that the FHP has a smaller impact on infant mortality in areas with greater exposure to the trade shocks. We interpret this finding as suggestive evidence that the FHP serves as partial substitute to parental time, benefiting disproportionately mothers who are engaged in the labor market and are less able to make time-intensive health investments in their children.

Effects of FHP. The starting point of our analysis is to estimate the effect of the FHP on infant mortality. While these effects have been documented extensively in previous studies (Macinko et al., 2006; Rocha and Soares, 2010; Bhalotra et al., 2016), we also examine this relationship here to motivate the empirical specification below. We estimate the effects of the FHP employing the following event-study specification:

$$y_{it} = \alpha + \sum_{k=-5}^{10} \beta_k \, \mathbb{1}\big\{\tau = k\big\} + \mu_i + \delta_{rt} + \xi_{it} \tag{3}$$

where $\mathbb{1}\{\cdot\}$'s are indicator variables for the years since the FHP was adopted in the municipality. The coefficient β_k is normalized so that it is equal to zero for $k = -1$, the year before the FHP started. The model (3) includes municipality fixed effects, denoted by μ_i. The rest of variables and parameters are the same as in equation (2). Standard errors are clustered at the municipality level to account for possible serial correlation (Bertrand et al., 2004).

Panel A of Figure 7 plots the coefficients and respective 95 percent confidence intervals from estimating equation (3). The point estimates are small and statistically insignificant in the years before the FHP started, strongly supporting the identifying assumption that treated and control municipalities would have experienced similar trajectories in the absence of FHP. The coefficients fall sharply and immediately following the year that the FHP was adopted. Notably, the magnitude of this effect persistently increases in an approximately linear fashion during the entire post-FHP period. Our estimates indicate that the infant mortality rate fell by almost 20 percent by the tenth year since the FHP was adopted.

Recent analyses of dynamic treatment effects in difference-in-differences approaches suggest that standard estimators may be biased when there is variation in the timing of policy adoption and when the treatment effects are heterogeneous (Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). In these cases, the estimated treatment effects equal a weighted average of the individual treatment effects with weights that may be negative (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). To explore the importance of this issue in our setting, we implement the Callaway and Sant'Anna (2020)'s estimator. Under this alternative approach, we first estimate separate event studies for each municipality and then compute the overall effect as the weighted sum of these estimates, with the weight being given by the one-year population in each treated municipality-year pair. This procedure ensures that all the weights are non-negative and thus the resulting estimate is unaffected by the issues raised in Goodman-Bacon (2021). Appendix Figure A.7 shows that these results are statistically identical to the baseline. As consequence, we focus on our computationally simpler two-way fixed effect approach for the rest of the analysis.

Interactive Effects. Panel B of Figure 7 presents results from estimating the event-study model (3) for low- and high-exposed areas, defined as municipalities below and above the median of the RTR distribution respectively. Since the FHP was launched in the mid-1990s, these results capture the effects of the FHP after the trade liberalization reform was complete. Prior to the introduction of FHP, treated and untreated areas experienced similar trends in either low- or high-exposed areas to the trade shock. This suggests that the common trends assumption is likely to hold for the interaction between the FHP and trade liberalization reform. During the post-FHP period, we observe a sharp decline in the infant mortality rate of areas with low exposure to the trade reform. By contrast, there is little change in high-exposed areas. We are low powered to test whether the differences in individual event years are statistically significant. To test this hypothesis, we rely on the empirical specification below.

To combine the long-first difference specification (2) and the difference-in-difference model (3) into a single and parsimonious model, we employ the following specification:

$$\begin{aligned} y_{i2010} - y_{i1991} = \alpha &+ \beta_{RTR}\, RTR_i + \beta_{FHP}\, t_i^{FHP} \\ &+\ \beta_{int}\, RTR_i \times t_i^{FHP} \\ &+ \gamma_t\,(y_{i1990} - y_{i1985}) + \delta_{rt} + \xi_{it} \end{aligned} \tag{4}$$

In this specification, the term t_i^{FHP} denotes the number of years that the FHP has been operating in municipality i. For example, if the FHP was adopted in 2005 in a given municipality, then t_i^{FHP} would be equal to 2010-2005= 5 years. It captures delayed effects of the FHP on infant mortality, as suggests the evidence in Figure 7. The coefficient of interest is β_{int}, which measures the magnitude of the interaction effects. The model given by equation (4) represents an extended version of the long-difference specification (2), but we could instead estimate an equivalent extended difference-in-difference model to test the hypothesis of interaction effects. In practice, both specifications yield very similar results.[34]

[34] An equivalent difference-in-differences version of model (4) would be:

$$\begin{aligned} y_{it} = \alpha + \beta_{FHP}\,(t - t_i^*) \cdot \mathbb{1}\{t > t_i^*\} + \beta_{RTR} RTR_i \cdot \mathbb{1}\{t > 1995\} \\ + \beta_{int} RTR_i \times (t - t_i^*) \cdot \mathbb{1}\{t > t_i^*\} \end{aligned}$$

Table 7 presents these results. Consistent with the visual evidence, we find that the interaction term coefficient is positive and statistically significant. It implies that exposure to the trade shock significantly reduces the magnitude of the impacts of the FHP. To illustrate the magnitude of these interactive effects, we present the effects of each exposure measure evaluated at different levels of the other (Figure 8). A one standard deviation increase in the trade shock has a long-run impact on infant mortality of about 20 percent when there is no FHP in the municipality. This effect falls to 12 when the FHP has been present in the municipality for 10 years. When looking at the impacts of having the FHP for 10 years in the municipality, we observe a decline in infant mortality of 50 percent for the municipality at the 10th percentile of the RTR distribution. When considering the municipality in the 90th percentile of the RTR distribution, this impact becomes about 25 percent.

6.3 Discussion

Our estimates suggest economically meaningful interactive effects between the FHP and trade shocks. These results are broadly consistent with the importance of parental time. Worse labor market conditions as result of the trade reform make it less costly to undertake time-intensive health investments, such as travelling to distant primary care centers. But this opportunity cost effect becomes less important when the FHP is introduced, as it brings basic health services to the home in a flexible manner. As a result, the effects of the trade reform on infant mortality become smaller in magnitude when the FHP arrives in the municipality. This has important policy implications. Policy makers promoting female labor force participation could potentially mitigate some of the unintended consequences of parental work, in terms of health investments in children, by introducing flexible programs like FHP that facilitates the access to primary health care services even among parents who face significant time constraints.

There are of course alternative hypotheses that could be consistent with these interactive effects. The main candidate alternative explanation is that there could be differences across regions in other health resources that interact with the FHP. In particular, if areas with greater exposure to the tariff shock experienced a decline (or slower expansion) in health resources, then this could explain part of the interactive effects reported in Table 7. However, as we discuss and document in Section 5.3.B, there is no evidence that the tariff shock led to statistically meaningful changes in local health spending or in hospital capacity, casting doubt on the hypothesis that differences in health resources explain the interaction effects between the FHP and trade shock. In Appendix Table A.15, we go one step further and show that the interaction term ($RTR \times t^{FHP}$)

$$+\mu_i + \delta_{rt} + \xi_{it}$$

where the term t^* represents the year of FHP adoption in each municipality. This model captures in a parsimonious way the fact that the effects of the FHP on infant mortality develop gradually over time.

does not predict changes in local health spending or hospital capacity. Thus, differences in health resources seem unlikely to explain much of our results.

Another possible explanation is that the effects of the trade reform on labor market outcomes, and thus on parental time, may differ when the FHP is implemented. Specifically, if the trade-induced decline in female labor force participation is smaller in areas adopting the FHP, this could explain why we observe smaller impacts of the reform on infant mortality when the FHP arrives. However, it is difficult to think of reasons why this would be the case, given that the labor market effects of the reform stem from a demand-side shock and given that the FHP does not directly generate labor market opportunities in affected areas. Consistent with this notion, we do not observe significant interactions between the FHP and RTR when it becomes to labor market outcomes (Appendix Table A.16). The effects of tariff shocks on household income and employment are not statistically affected by the adoption of the FHP.

A third possibility is that the interaction term ($RTR \times t^{FHP}$) may be capturing differences in the implementation of the BFP. This possibility is made somewhat plausible in view of the evidence that the tariff shock led to increases in BFP enrollment. This enrollment effect may become stronger with the family health program if the FHP health teams provide information on the BFP and encourage participation in the program. Note that this would imply that the effects of the trade reform on infant mortality should become *stronger* with the arrival of the FHP, as increased participation in the conditional cash transfer program most likely would lead to improvements in infant mortality. But we find the exact opposite pattern: the impacts of the trade shocks on infant mortality are smaller with the arrival of FHP. Hence, to account for the interactive effects we estimate, exposure to both the trade shock and FHP should lead to a significant *decline* in BFP enrollment. There is no obvious reason why this could occur. In any case, there is no evidence of a statistically significant interaction between the FHP and RTR when the dependent variable is BFP enrollment (Appendix Table A.15, column 4).

Finally, our results may be capturing improvements in the registration of deaths generated by the FHP. This would explain our interactive effects if this registration effect occurred differentially in areas with greater exposure to the trade reform. We are skeptical of this argument since the high coverage of vital registries in Brazil since the 1980s implies that the scope for increased registration of deaths is limited (Bhalotra et al., 2016). More importantly, it is not clear why this registration effect would be more pronounced in areas with greater exposure to the trade reform. A simple way to indirectly test this hypothesis is to look at external causes of death. Our hypothesized mechanism of parental time is less likely to affect infant deaths due to external causes, which is line with the evidence in Table 2. But if the FHP led to more extensive registration of infant deaths, this is likely to have affected death counts due to both internal and external causes. Hence, we would expect to observe significant interaction effects for external causes if improved registration is the responsible of our interactive results. Appendix Table A.17

documents that in no case are there statistically significant interaction effects even when we examine different external causes of death. This provides suggestive evidence that the interaction effects that we observe in Table 7 are not originating from increased death registration.

7 Conclusion

In this paper, we document the effects of a trade liberalization reform that unilaterally lowered import tariffs on infant mortality in Brazil, using a rich dataset that covers more than 3000 municipalities over a horizon of 25 years. We exploit variation in the baseline composition of industries across areas and in the tariff cuts across industries for identification. Our findings indicate that areas housing the industries with greater tariff cuts experienced a sizeable decline in infant mortality. The magnitude of our estimates implies that a one standard-deviation increases in the tariff shock measure is associated with a decline of 8 to 12 percent in the infant mortality rate. The patterns in the data are consistent with the hypothesis that worse labor market opportunities make it less costly undertake health-improving behaviors that are time-intensive. We show that while men have adjusted to the trade shock by transitioning to the self-employment sector, women have permanently exited the labor force and arguably allocated more time to home production activities. We then document significant improvements in the use of primary health services, such as preventive doctor visits among women of childbearing ages, and growth-monitoring and well-care visits among infants. We find limited evidence in support of other potential mechanisms, including changes in the supply of health services, air pollution levels or consumption of harmful normal goods such.

In the final part of the paper, we examine whether and how the returns to public health policy are affected by the trade reform. We focus on the Family Health Program (FHP), a program that provides basic health services free of charge primarily through home visits. We find that the trade shock lowers the impacts of the FHP on infant mortality, or equivalently, the impacts of the trade shock become smaller when the FHP arrives in the municipality. We interpret these findings as evidence consistent with the popular wisdom that employed mothers are likely to benefit the most from this type of programs, as they face significant time constraints to visit distant health facilities for free basic health services. By bringing these services to the home in a flexible manner, the FHP relaxes these restrictions. Women exiting the labor market as result of the trade shock are more able to visit public health facilities and access the same services offered by the FHP, so the FHP has smaller effects in this case. To some extent, this evidence is consistent with the importance of parental time for the household production of child health status. From a policy perspective, these findings suggest that policy makers promoting female labor force participation could mitigate some of the unintended consequences of parental work by introducing flexible programs like FHP that facilitates the access to basic health services even among parents who face significant time constraints.

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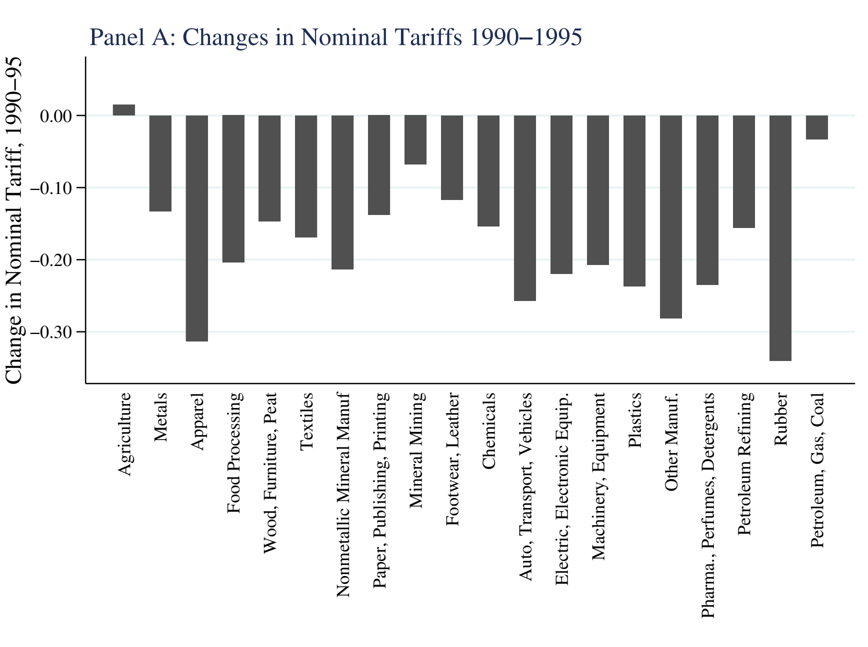
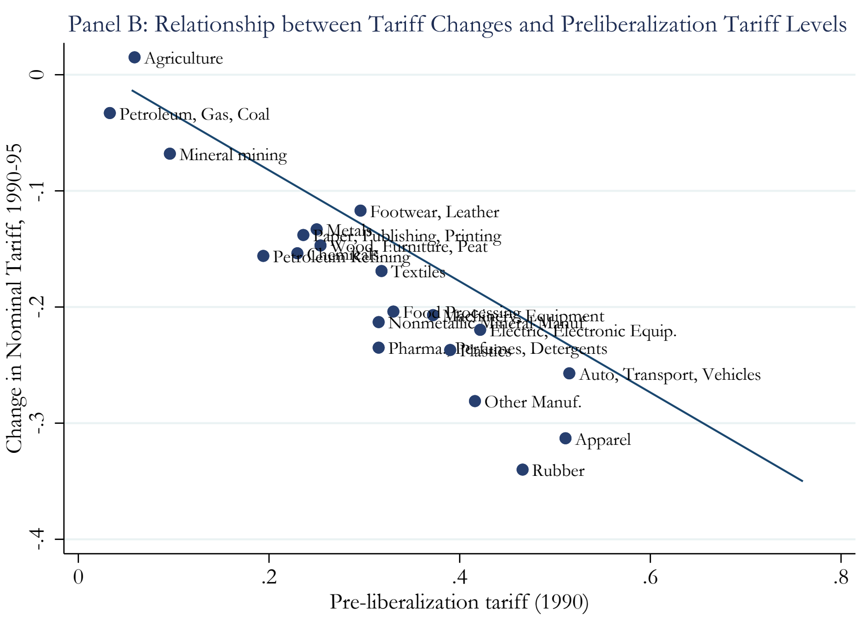
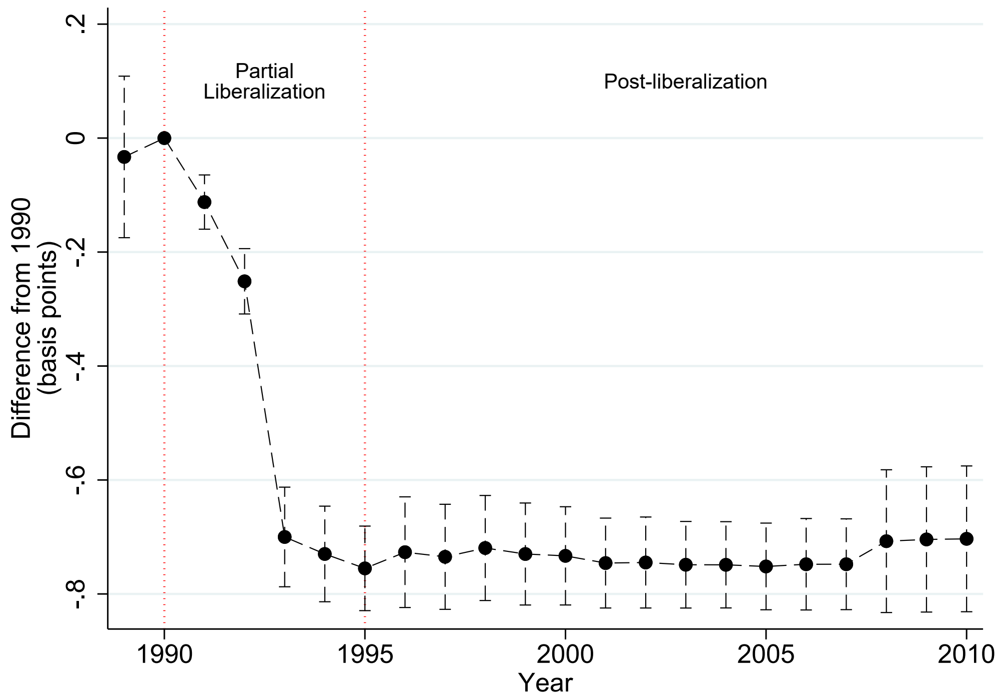
Figure 1: Import Tariff Changes across Industries





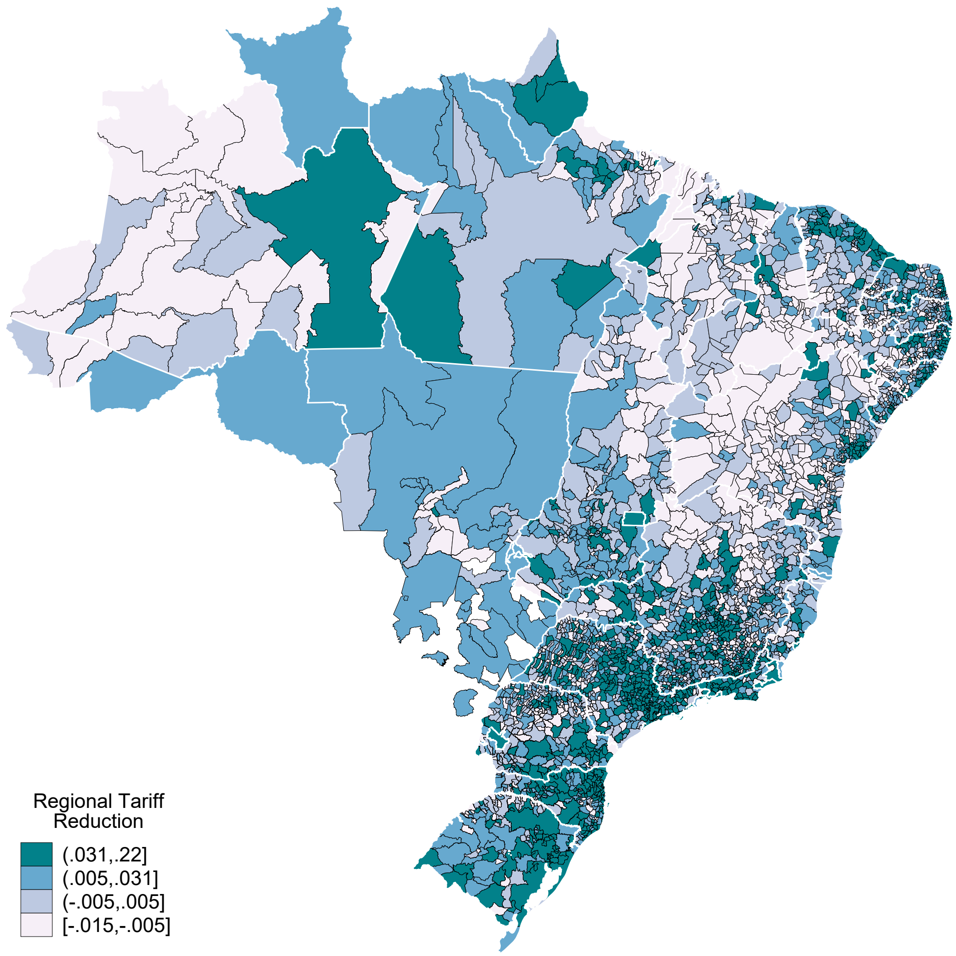
Notes: These figures show the incidence of import tariff cuts between 1990 and 1995 across industries.

Figure 2: Changes in Import Tariffs versus Initial Import Tariffs



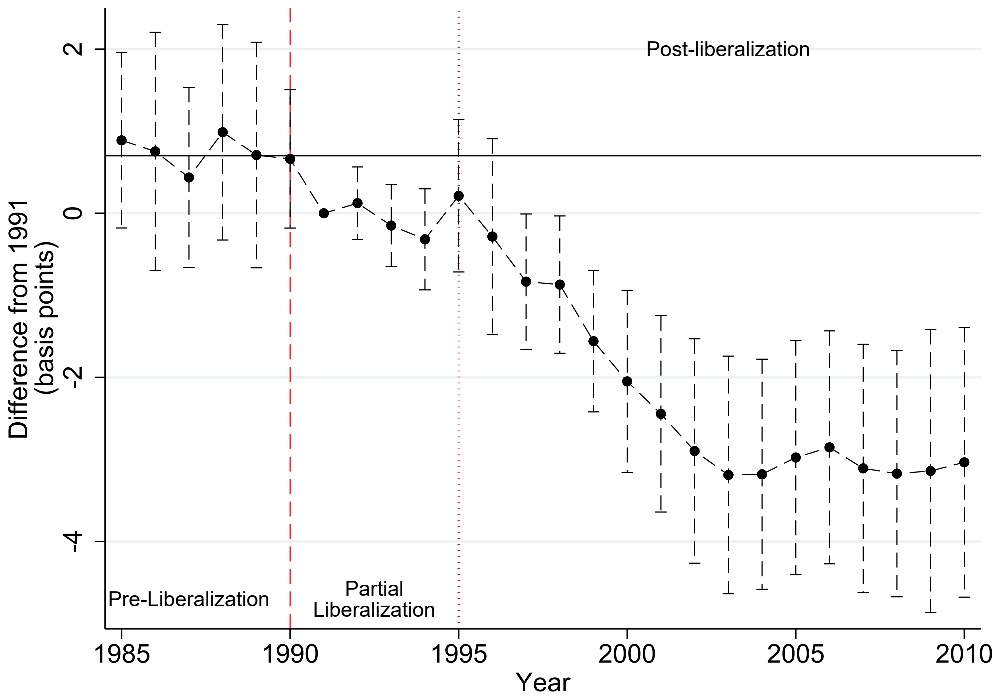
Notes: This figure presents results from estimating univariate regressions of the change tariff between year t and 1990 on the 1990 level. Each dot represents a slope with respective 95 percent confidence interval.

Figure 3: Regional Tariff Reductions



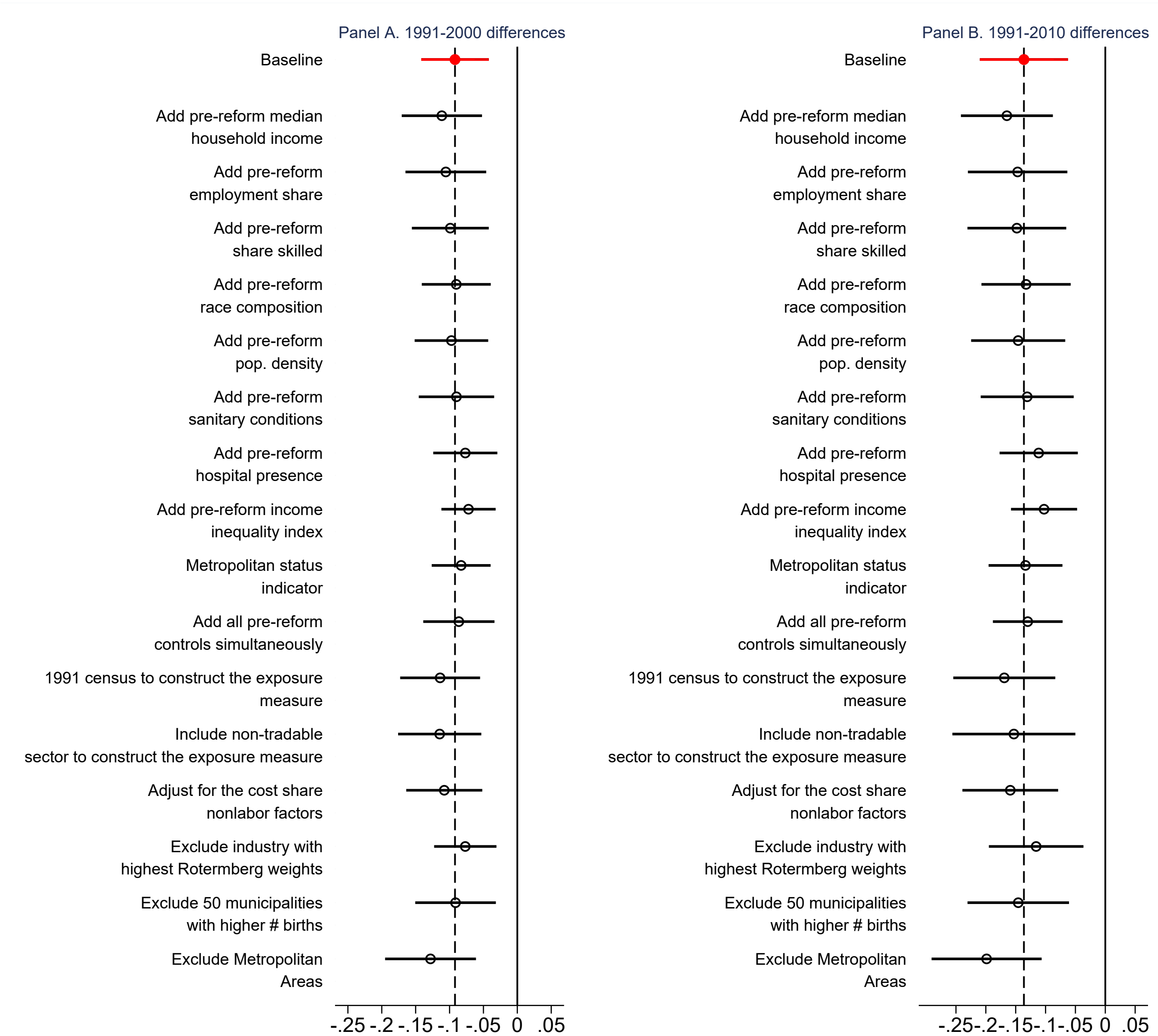
Notes: Municipalities are colored based on the regional tariff reduction measure, RTR, as defined in equation (1). Municipalities with greater tariff reductions are shown as darker. White lines represent state borders.

Figure 4: Regional Tariff Reduction and Log-Changes in Infant Mortality Rates



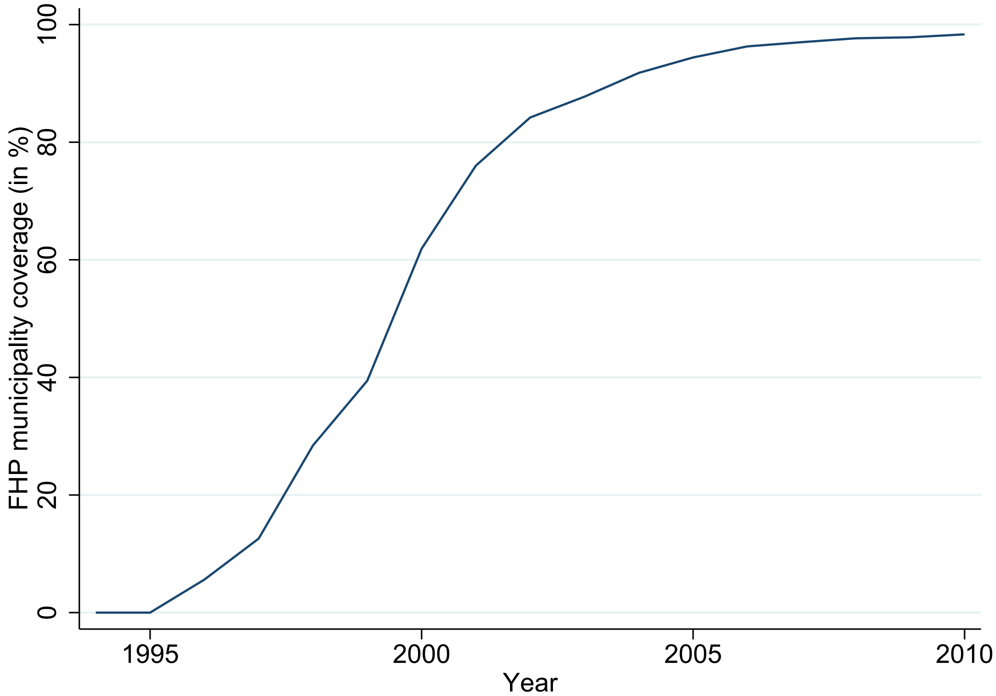
Notes: This figure plots β_t (and 95 percent confidence intervals) from estimating equation 2. All observations are weighted by the number of births in 1991. The variable RTR always reflects tariff reductions from 1990 to 1995. All regressions include microregion fixed effects, and partial- and post-liberalization regressions include the 1985-1990 outcome pretrend. Negative estimates indicate larger declines in the infant mortality rate in areas with greater tariff reductions. Standard errors are clustered at the state-level.

Figure 5: Regional Tariff Reduction and Log-Changes in Infant Mortality Rate (Effect of 1-SD Increase in RTR - Robustness Checks)



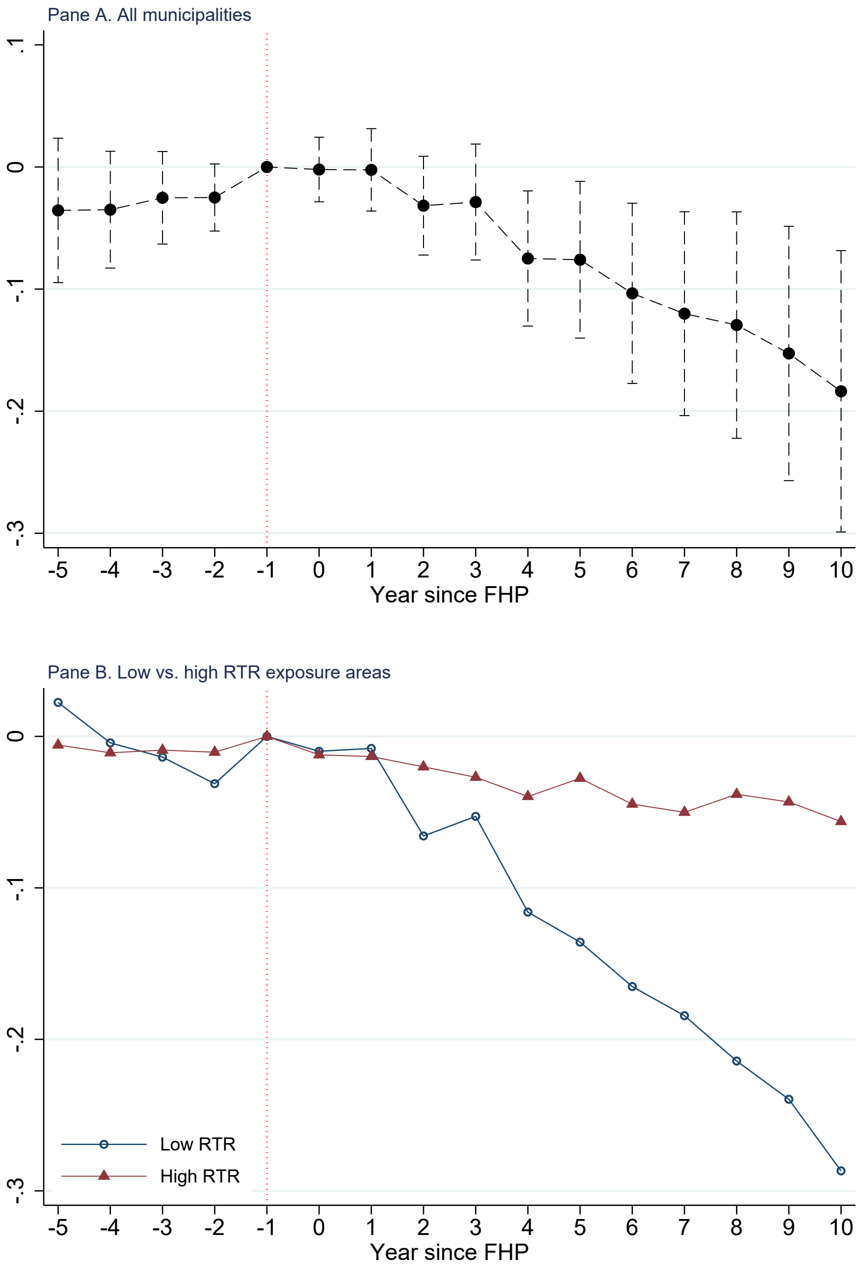
Notes: The figure plots estimates of β and 95 percent confidence intervals (based on standard errors clustered by state). The coefficients are standarized, so they reflect the effect of a one-standard deviation increase in the RTR. Row 1 presents the baseline estimates based on equation (2). Rows 2 to 11 add pre-reform controls as measured in the 1980 Census. Rows 11 to 13 uses alternative constructions of the RTR measure. Row 14 uses a RTR measure that excludes the industry with the highest Rotemberg weight (Apparel). Rows 15 and 16 excludes several municipalities from the estimation sample.

Figure 6: Expansion of the Family Health Program



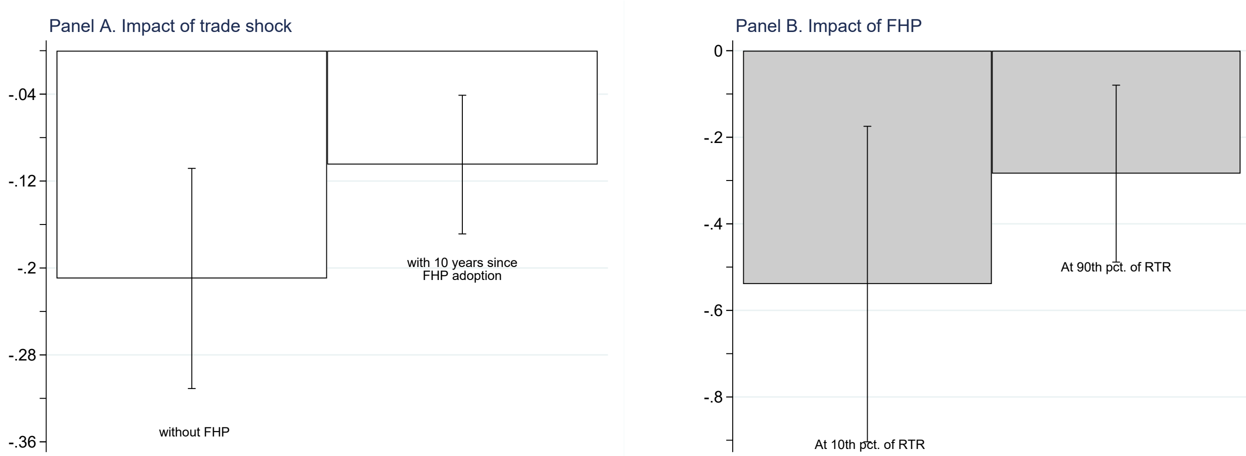
Notes: This figure shows the adoption rates of the Family Health Program across municipalities over time.

Figure 7: Impacts of the Family Health Program on Infant mortality



Notes: These figures show the results from estimating equation (3). The unit of analysis is a municipality. The dependent variable is the log infant mortality rate. Observations are weighted by the number of births in 1991. Panel A presents results for all municipalities. Panel B presents results separately from low- and high-exposed areas to the trade shock. High (low) exposure is defined as a municipality above (below) the median of the regional tariff reduction (RTR) variable. 95 percent confidence intervals in Panel A are derived from robust standard errors clustered at the municipality level.

Figure 8: Interactive Effects of Trade Shocks and Family Health Program



Notes: These figures show the results from estimating equation (4) presented in column (3) of Table 7. The unit of analysis is a municipality. The dependent variable is the log-change in infant mortality rate. Observations are weighted by the number of births in 1991. Panel A shows the effect of the regional tariff reduction on infant mortality when the FHP has zero and 10 years since its adoption in the municipality. Panel B shows the effect of the FHP for municipalities at the 10 and 90th percentiles of the regional tariff reduction distribution, respectively. 95 percent confidence intervals are derived from robust standard errors clustered at the state-level.

Table 1: Regional Tariff Reduction and Log-Changes in Infant Mortality Rates

	No controls (1)	Add state fixed effects (2)	Add microregion fixed effects (3)	Add pretrend in outcome (4)
		Panel A. 1991-2000		
RTR	-4.3143	-3.0384	-2.0031	-2.0488
	[0.8070]***	[0.6132]***	[0.5617]***	[0.5694]***
R^2	0.175	0.363	0.569	0.582
Observations	3657	3657	3657	3657
		Panel B. 1991-2010		
RTR	-5.1459	-3.7379	-2.9881	-3.035
	[1.2414]***	[0.8511]***	[0.8357]***	[0.8428]***
R^2	0.178	0.431	0.648	0.657
Observations	3657	3657	3657	3657

Notes: This table presents the results from estimating different versions of specification (2). The unit of analysis is a municipality. All observations are weighted by the number of births in 1991. Column (1) estimates a model without any control variable. Column (2) adds state fixed effects. Column (3) adds microregion (in stead of state) fixed effects. Column (4) includes the log-change in the infant mortality rate between 1985 and 1990. Standard errors in brackets are clustered at the state-level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table 2: Regional Tariff Reduction and Log-Changes in Infant Mortality Rates (By Causes of Death)

	Infectious and respiratory (1)	Perinatal (2)	Congenital anomalies (3)	Other internal causes (4)	Transport accidents (5)	Other accidents (6)	Aggression (7)	Other external causes (8)
				Panel A. 1991-2000				
RTR	-4.8644	-1.4099	-0.1161	-2.4923	-0.3361	-0.8061	-0.9002	0.9068
	[1.2545]***	[0.5953]**	[0.7609]	[0.8647]***	[0.7509]	[1.2255]	[0.7495]	[0.7948]
R^2	0.508	0.519	0.531	0.52	0.416	0.517	0.585	0.516
Observations	3657	3657	3657	3657	3657	3657	3657	3657
				Panel B. 1991-2010				
RTR	-7.7531	-3.5691	-1.6774	-4.9139	-0.6205	-1.9631	-0.5307	-0.5282
	[1.5770]***	[0.8738]***	[0.8825]*	[1.0832]***	[0.9336]	[1.2883]	[0.7454]	[0.9910]
R^2	0.584	0.608	0.531	0.565	0.594	0.579	0.519	0.441
Observations	3657	3657	3657	3657	3657	3657	3657	3657

Notes: This table presents the results from estimating model (2) for different causes of deaths. The unit of analysis is a municipality. All observations are weighted by the number of biths in 1991. Standard errors in brackets are clustered at the state-level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table 3: Regional Tariff Reduction and Changes in Employment Rates and Log Household Income

	Men			Women			
	Working outside of the home for money	Working in self-employed occupation	Employment	Working outside of the home for money	Working in self-employed occupation	Employment	Household Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A. 1991-2000			
RTR	-0.55785	0.50653	-0.03752	-0.40533	-0.01044	-0.40123	-0.4073
	[0.12872]***	[0.07428]***	[0.15351]	[0.04908]***	[0.02236]	[0.06909]***	[0.1048]***
R^2	0.681	0.734	0.635	0.555	0.379	0.546	0.898
Observations	3657	3657	3657	3657	3657	3657	3657
				Panel A. 1991-2010			
RTR	-0.53471	0.57728	0.05221	-0.35315	-0.22492	-0.55168	-1.2204
	[0.14755]***	[0.09509]***	[0.16617]	[0.06900]***	[0.10818]**	[0.14008]***	[0.1507]***
R^2	0.665	0.791	0.682	0.65	0.492	0.679	0.911
Observations	3657	3657	3657	3657	3657	3657	3657

Notes: This table presents the results from estimating equeation (2) for labor market outcomes. The unit of analysis is a municipality. All observations are weighted by the 1991 population. Data on employment and household income come from the 1980, 1991, 2000, and 2010 censuses. The emploment variables in columns (1)-(6) are in rates. All regressions include microregion fixed effects and a pre-reform trend in the outcome ($y_{i1991} - y_{i1980}$). Standard errors in brackets are clustered at the state-level.

* p < 0.1;** p < 0.05;*** p < 0.01.

Table 4: Regional Tariff Reduction and Changes in the Use of Preventive Health Services (State-level Data)

	Change in share			
	primary care visit (child-bearing age women)		well-child care visit	
	1986-1998	1986-2008	1986-1998	1986-2008
	(1)	(2)	(3)	(4)
RTR	0.1994	0.2203	0.0965	0.0463
	[0.0525]***	[0.0482]***	[0.0386]**	[0.0201]**
R^2	0.254	0.322	0.217	0.082
Observations	26	26	26	26

Notes: The unit of analysis is a state. Outcome variables come from the *Pesquisa Nacional por Amostra de Domicílios Contínua (PNAD)*. The fraction of well-child care visits is measured for children under five. Observations in columns (1) and (2) are weighted by the woman population of childbearing age in 1991. Observations in columns (3) and (4) are weighted by the number of children under five in 1991. Standard errors in brackets are robust to arbitrary heteroscedasticity.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table 5: Regional Tariff Reduction and Log-Changes in Pollution Measures

	Monoxide carbon			Particulate matter		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. 1991-2000			
RTR	-0.0668	-0.1039	-0.1326	0.287	0.1271	0.0175
	[0.1352]	[0.1528]	[0.0748]*	[0.1365]**	[0.0971]	[0.0550]
R^2	0.245	0.511	0.748	0.174	0.428	0.538
Observations	3511	3511	3511	3511	3511	3511
			Panel B. 1991-2010			
RTR	-0.8487	-0.8223	-0.8595	0.537	0.19	0.0143
	[0.3527]**	[0.2901]***	[0.2035]***	[0.3605]	[0.2389]	[0.1760]
R^2	0.297	0.64	0.719	0.244	0.558	0.623
Observations	3511	3511	3511	3511	3511	3511

Notes: This table presents the results from estimating equation (2) for different pollution measures. The unit of analysis is a municipality. Data come from the Emissions Database for Global Atmospheric Research. All regressions include microregion fixed effects and a pre-reform trend in the outcome ($y_{i1990} - y_{i1985}$). All observations are weighted by the 1991 population. Standard errors in brackets are clustered at the state-level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table 6: Regional Tariff Reduction and Changes in Births, Fetal Deaths, and Birth Charactiristics

	Change in dependent variable:				
			Newborn's household characteristics		
	Log birth rate	log fetal death rate	Household head's Years of schooling	Household head is black	Single parent household
	(1)	(2)	(3)	(4)	(5)
			Panel A. 1991-2000		
RTR	0.4284	-1.9771	-3.2947	0.0836	0.273
	[0.1253]***	[0.6174]***	[0.5001]***	[0.0289]***	[0.0249]***
R^2	0.566	0.512	0.473	0.392	0.584
Observations	3657	3657	3657	3657	3657
			Panel B. 1991-2010		
RTR	0.933	-4.3276	-2.6861	0.1613	0.191
	[0.1186]***	[0.8669]***	[0.6312]***	[0.0250]***	[0.0541]***
R^2	0.666	0.649	0.496	0.46	0.458
Observations	3657	3657	3657	3657	3657

Notes: This table presents the results from estimating specification (2) for birth rates, fetal death rates, and birth characteristics. The number of births and household characteristics are calculated from the population census. The birth rate is computed as the number of births divided by the number of women of childbearing ages. Fetal death rates are computed as the number of fetal deaths divided by the number of births. All regressions include microregion fixed effects and a pretrend in outcome variable. Observations are weighted by the number of births in 1991. Standard errors are clustered at the state-level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table 7: Interactive Effects of Trade Shock and Family Health Program

	Change in log infant mortality 1991-2010		
	(1)	(2)	(3)
RTR	-3.035		-5.2411
	[0.8428]***		[1.2991]***
Years Exposed to FHP		-0.0376	-0.0513
		[0.0156]**	[0.0178]***
$RTR \times$			0.2621
Years Exposed to FHP			[0.1050]**
R^2	0.657	0.653	0.663
Observations	3657	3613	3613

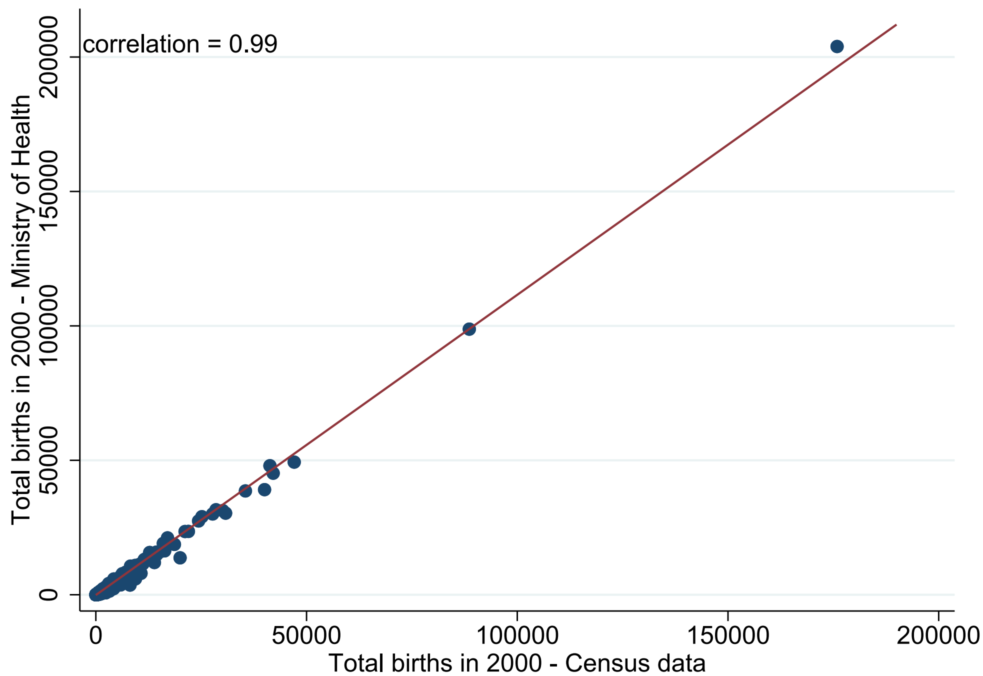
Notes: This table presents the results from estimating equation (4). The unit of analysis is a municipality. All observations are weighted by the number of births in 1991. Years Exposed to FHP is computed as $2010 - t^{FHP}$, where t^{FHP} is the year that the FHP was adopted in a given municipality. If a municipality did not adopt the FHP by 2010, then the total of years exposed to FHP is equal to zero. Standard errors in brackets are clustered at the state-level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Online Appendix

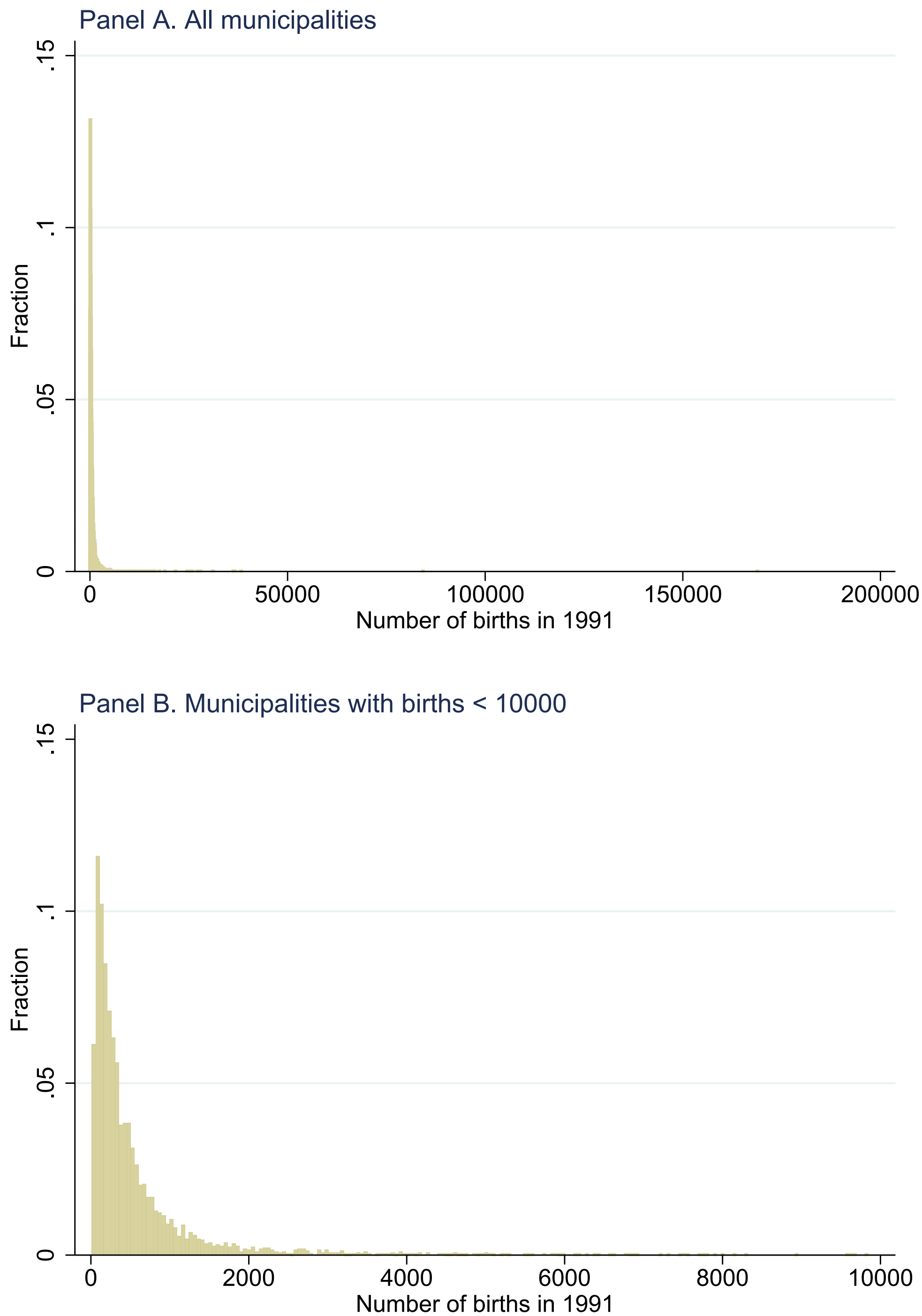
A Additional Figures and Tables

Figure A.1: Comparison of Birth Counts from Census versus Ministry of Health



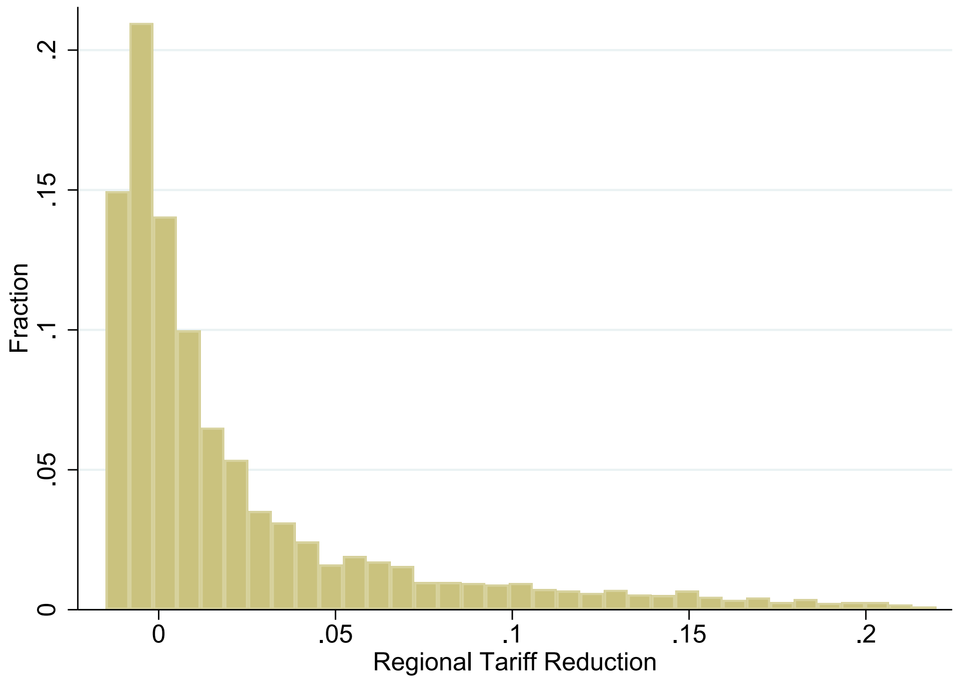
Notes: This figure compares birth counts from the census versus Ministry of Health. The unit of analysis is a municipality.

Figure A.2: Distribution of Births in 1991



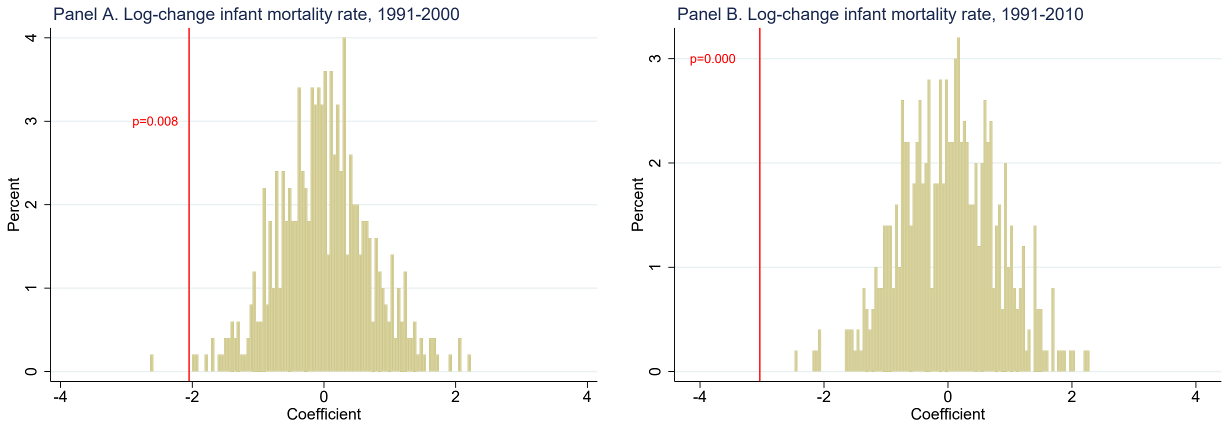
Notes: This figures shows the distribution of births in 1991. Birth counts are calculated from the population census as the total of individuals within one year of age in a municipality.

Figure A.3: Distribution of the Regional Tariff Reduction



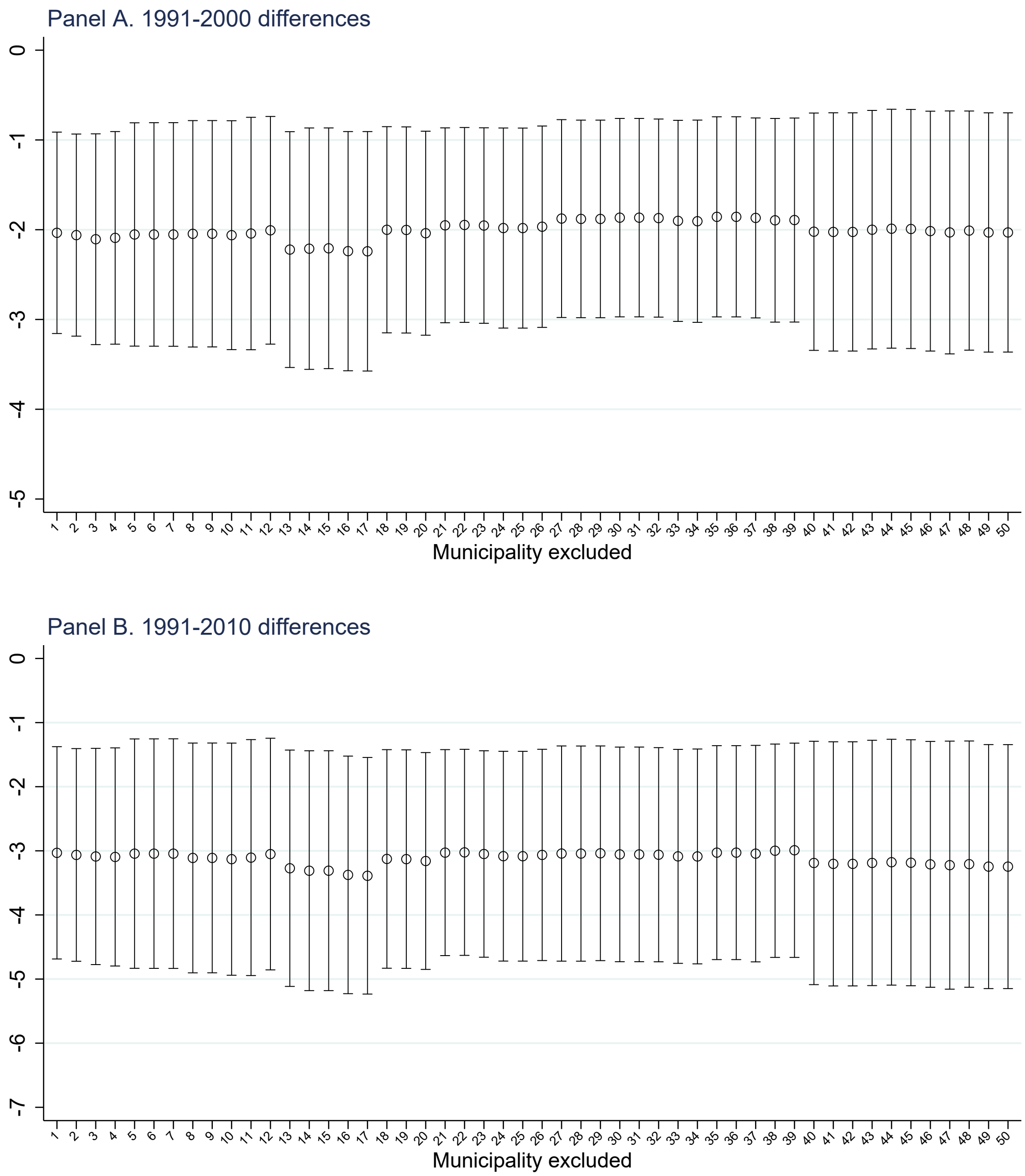
Notes: This figurs shows the distribution of the regional tariff reduction across municipalities.

Figure A.4: Regional Tariff Reduction and Log-Changes in Infant Mortality Rate (Permutation Test)



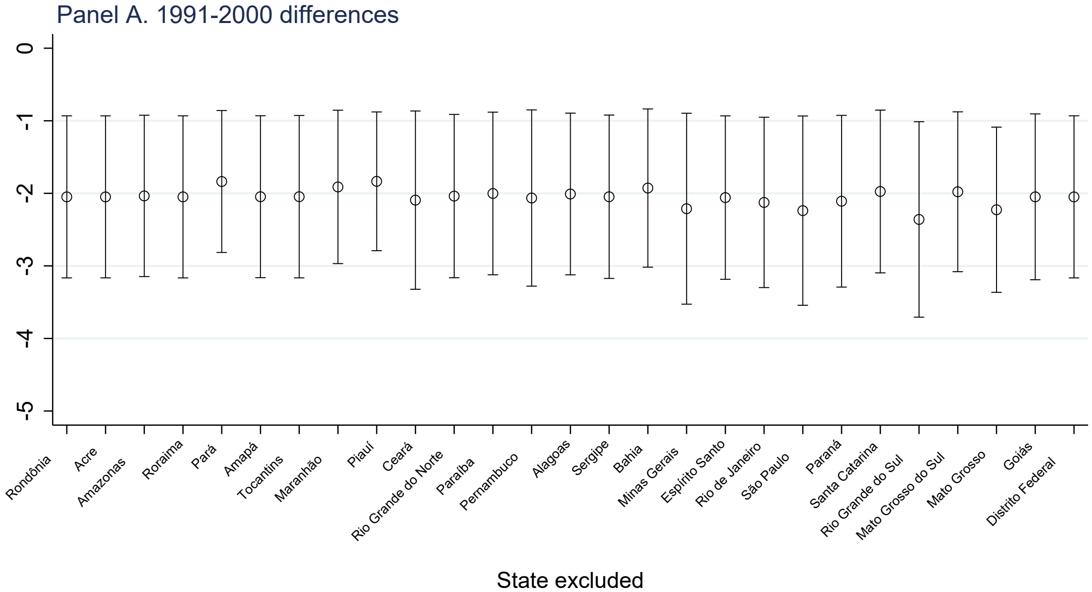
Notes: This figure plots the empirical distribution of placebo effects of the regional tariff reductions on infant mortality rates. To conduct this test, we randomly assign placebo RTR exposures and rerun our baseline specification (2) using this placebo measure. This procedure is repeated 500 times. The share of the absolute placebo coefficients that are larger in magnitude than the "true" coefficient can be taken as a measure of how likely are the results to arise by chance. The randomization process is made separately for each state.

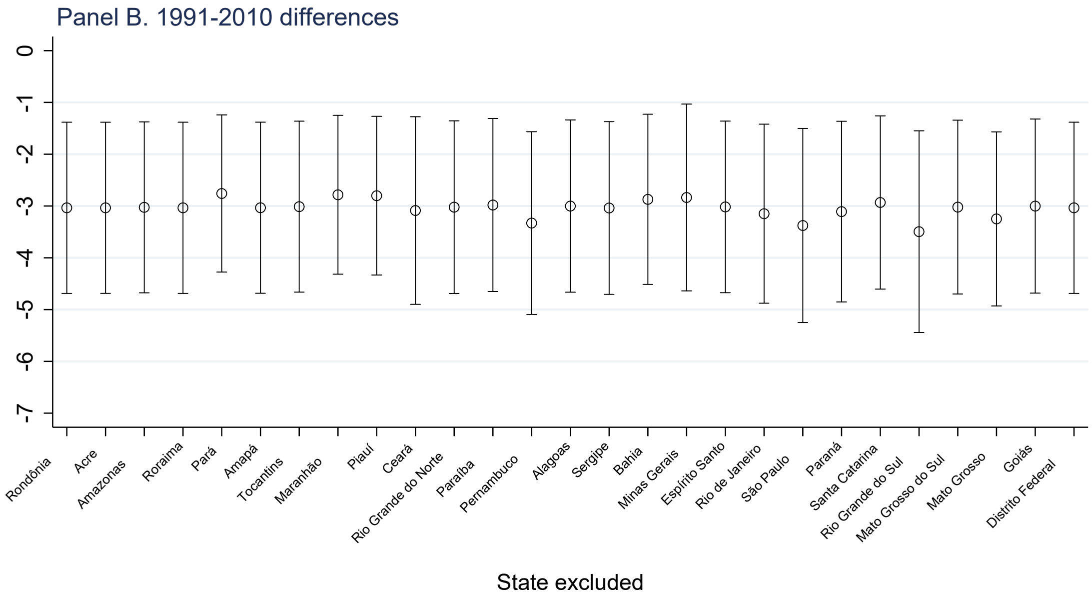
Figure A.5: Regional Tariff Reduction and Log-Changes in Infant Mortality Rate (Exclusion of Municipalities One to Many - Top 50 Based on Births in 1991)



Notes: The figure shows the robustness of the results to excluding the top 50 municipalities with the highest number of births in 1991. These municipalities account for 30 percent of the births in 1991. The estimated coefficients and confidence intervals at 95 percent are reported. Each estimated coefficient and confidence interval emanate from a single estimation. The number on the x-axis indicates the rank of the municipality according to the number of births in 1991.

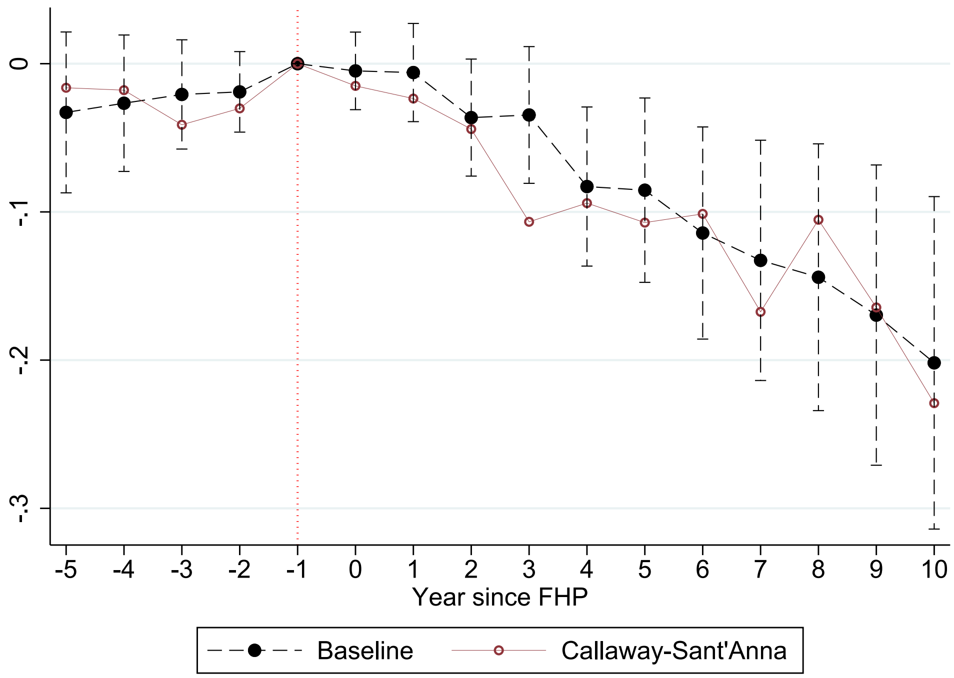
Figure A.6: Regional Tariff Reduction and Log-Changes in Infant Mortality Rate (Exclusion of each State one by one)





Notes: The figure shows the robustness of the results to excluding entire states one by one. The estimated coefficients and confidence intervals at 95 percent are reported. Each estimated coefficient and confidence interval emanate from a single estimation. The number on the x-axis indicates the excluded state.

Figure A.7: Effects of Family Health Program on Log Infant Mortality Rates (Callaway-Sant'Anna Estimator)



Notes: This figure shows the results from estimating equation (3). The unit of analysis is a municipality. The dependent variable is the log infant mortality rate. Observations are weighted by the number of births in 1991. The dashed line represents our baseline estimates. The solid line represents the results from using the Callaway and Sant'Anna (2020)'s estimator. 95 percent confidence intervals are derived from robust standard errors clustered at the municipality level.

Table A.1: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Alternative Levels of Aggregation)

	Unit of analysis					
	Municipality		Microregion		State	
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
	(1)	(2)	(3)	(4)	(5)	(6)
RTR	-2.0488	-3.035	-3.046	-3.1653	-4.5044	-4.9191
	[0.5694]***	[0.8428]***	[0.8151]***	[1.0680]***	[1.1345]***	[1.7707]**
State FE			✓	✓		
Microregion FE	✓	✓				
Pre-trend outcome (1985-90)	✓	✓	✓	✓	✓	✓
R^2	0.582	0.657	0.571	0.601	0.412	0.291
Observations	3657	3657	412	412	27	27

Notes: This table shows the robustness of our baseline results to alternative levels of aggregation. Observations are weighted by the number of births in 1991. Columns (1) and (2) represent the baseline estimates. Columns (3) and (4) use data collapsed at the microregion level. Columns (5) and (6) use data collapsed at the state level. Columns (1)-(4) use standard errors clustered at the state level. Columns (5) and (6) display standard errors robust to heteroscedasticity.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.2: Regional Tariff Reductions and Log-Changes in Infant Mortality Rates (Robustness to Alternative Inference Procedures)

	Clustering by state (Baseline) (1)	Clustering by state (wild bootstrap p-value) (2)	Clustering by microregion (3)	Borusyak et al. (2021) (Robust standard errors) (4)	Adao et al. (2019) (Robust standard errors) (5)
			Panel A. 1991-2000		
RTR	-2.0488	-2.0488	-2.0488	-2.0382	-2.0488
	[0.5694]***	(<0.01)***	[0.5552]***	[0.2983]***	[0.3969]***
R^2	0.582	0.582	0.582	0.851	0.582
Observations	3657	3657	3657	20	3657
			Panel A. 1991-2010		
RTR	-3.035	-3.035	-3.035	-3.0503	-3.035
	[0.8428]***	(<0.01)***	[0.7534]***	[0.4211]***	[0.4613]***
R^2	0.657	0.657	0.657	0.899	0.657
Observations	3657	3657	3657	20	3657

Notes: This table presents alternative approaches to inference on the baseline results from column 4 of Table 1. Standard errors are reported in brackets and wild bootstrap p-value are reported in parenthesis.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.3: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate
(Robustness to Additional Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Panel A. 1991-2000					
RTR	-2.0488	-2.4808	-2.3502	-2.2029	-2.0071	-2.166	-2.0008	-1.7102	-1.6046	-1.8468	-1.9216
	[0.5694]***	[0.6769]***	[0.6807]***	[0.6483]***	[0.5813]***	[0.6198]***	[0.6356]***	[0.5404]***	[0.4572]***	[0.4964]***	[0.6006]***
R^2	0.582	0.582	0.583	0.582	0.582	0.582	0.588	0.584	0.584	0.582	0.597
Observations	3657	3657	3657	3657	3657	3657	3353	3657	3657	3657	3353
						Panel B. 1991-2010					
RTR	-3.035	-3.6704	-3.2698	-3.2982	-2.9545	-3.2495	-2.9116	-2.4837	-2.2836	-2.9758	-2.8925
	[0.8428]***	[0.8796]***	[0.9510]***	[0.9449]***	[0.8534]***	[0.9000]***	[0.8895]***	[0.7484]***	[0.6320]***	[0.7061]***	[0.6666]***
R^2	0.657	0.658	0.658	0.658	0.658	0.658	0.661	0.662	0.663	0.657	0.674
Observations	3657	3657	3657	3657	3657	3657	3353	3657	3657	3657	3353
Munic. Charc in 1980:											
Median income		✓									✓
Share employment			✓								✓
Share Skilled				✓							✓
Race composition					✓						✓
Population density						✓					✓
Sanitary conditions							✓				✓
Hospital presence								✓			✓
Income inequality index									✓		✓
Metropolitan status dummy										✓	✓
Basic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table shows the robustness of our baseline specification (2) to including a number of baseline municipality characteristics. Shared skilled corresponds to the share of the population with 12 or more years of education. Race composition includes the shares of the population that is white and black. Sanitary conditions include the shares of households with piped water and access to a sewage system. The income inequality index is based on the Theil index. Robust standard errors in brackets are clustered at the state level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.4: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Alternative Constructions of the Regional Tariff Reduction, RTR)

	1991 employment composition		Including the non-tradable sector		Adjusting for the cost share of nonlabor factors	
	1991-2000	1991-2010	1991-2000	1991-2010	1991-2000	1991-2010
	(1)	(2)	(3)	(4)	(5)	(6)
RTR	-2.3832	-3.5374	-5.2569	-7.0245	-2.303	-3.397
	[0.6324]***	[0.9160]***	[1.4452]***	[2.4229]***	[0.6147]***	[0.8779]***
R^2	0.584	0.661	0.582	0.656	0.583	0.66
Observations	3657	3657	3657	3657	3657	3657

Notes: This table shows the robustness of our baseline results to alternative constructions of the RTR. Columns (1) and (2) compute exposure to tariff reductions using 1991 (instead of 1980) employment shares. Columns (3) and (4) compute exposure to tariff reductions including the non-tradable sector by imputing a zero tariff change in these industries. Columns (5) and (6) compute exposure to tariff reductions using the measure developed by Kovak (2013) that adjusts for the cost share of nonlabor factors. All regressions control for microregion fixed effects and a pre-reform trend in outcome ($y_{i1990} - y_{i1985}$). Robust standard errors in brackets are clustered at the state level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.5: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Rotemberg Weights and Exclusion of each industry)

Industry	Rotemberg weights	Estimate of β after excluding each industry 1991-2000	1991-2010
Apparel	0.345	-1.924	-2.786
Food Processing	0.140	-1.918	-2.730
Auto, Transport, Vehicles	0.064	-1.950	-3.006
Metals	0.057	-2.081	-2.985
Other Manuf.	0.056	-2.048	-2.975
Machinery, Equipment	0.046	-2.090	-3.032
Textiles	0.045	-2.040	-2.930
Wood, Furniture, Peat	0.042	-1.989	-2.862
Nonmetallic Mineral Manuf	0.040	-1.959	-2.881
Electric, Electronic Equip.	0.035	-2.113	-3.038
Rubber	0.031	-2.071	-3.020
Paper, Publishing, Printing	0.023	-2.034	-2.943
Pharma., Perfumes, Detergents	0.022	-2.089	-3.009
Plastics	0.021	-2.057	-2.979
Footwear, Leather	0.015	-2.019	-2.952
Chemicals	0.014	-2.044	-2.946
Petroleum Refining	0.007	-2.022	-2.940
Mineral Mining	0.001	-2.016	-2.933
Petroleum, Gas, Coal	0.000	-2.028	-2.945
Agriculture	-0.005	-2.029	-2.947

Notes: This table shows the Rotemberg weights and the robustness of our baseline results to excluding each industry. All regressions control for microregion fixed effects and a pre-reform trend in outcome ($y_{i1990} - y_{i1985}$).

Table A.6: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Excluding Mtropolitan Regions)

	1991-2000		1991-2010	
	Baseline	Exclude Metropolitan Regions	Baseline	Exclude Metropolitan Regions
	(1)	(2)	(3)	(4)
RTR	-2.0488	-2.8534	-3.035	-4.4268
	[0.5694]***	[0.7661]***	[0.8428]***	[1.0525]***
R^2	0.582	0.567	0.657	0.656
Observations	3657	3265	3657	3265

Notes: This table shows the robustness of the baseline results to excluding Metropolitan Regions. All regressions include microregion fixed effects and a pre-reform trend in outcome ($y_{i1990} - y_{i1985}$). All observations are weighted by the number of births in 1991.Robust standard errors in brackets are clustered at the state level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.7: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Only Municipalities with no Boundary Changes)

	1991-2000		1991-2010	
	Baseline	Municipalities with no boundary changes	Baseline	Municipalities with no boundary changes
	(1)	(2)	(3)	(4)
RTR	-2.0488	-1.6847	-3.035	-2.8402
	[0.5694]***	[0.4980]***	[0.8428]***	[0.8079]***
R^2	0.582	0.555	0.657	0.635
Observations	3657	2878	3657	2878

Notes: This table shows the robustness of the baseline results to using municipalities with no boundary changes only. All regressions include microregion fixed effects and a pre-reform trend in outcome ($y_{i1990} - y_{i1985}$). All observations are weighted by the number of births in 1991. Robust standard errors in brackets are clustered at the state level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.8: Regional Tariff Reductions and Log-Changes in Working-Age Population

	1991-2000		1991-2010	
	(1)	(2)	(3)	(4)
RTR	-0.0746	-0.0995	-0.2012	-0.0613
	[0.1014]	[0.1509]	[0.1928]	[0.3082]
Microregion FE		✓		✓
Basic controls	✓	✓	✓	✓
R^2	0.31	0.602	0.335	0.675
Observations	3657	3657	3657	3657

Notes: This table estimates the relationship between regional tariff reductions and the log-changes in working-age population. The unit of analysis is a municipality. All regressions include a pre-reform trend in outcome ($y_{i1991} - y_{i1980}$) and are weighted by the 1991 population. Robust standard errors in brackets are clustered at the state level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.9: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Excluding Municipalities that Adopted the Family Health Program)

	1991-1997		1991-1998		1991-1999		1991-2000		1991-2001	
	All	Non-FHP sample	All	Non-FHP sample	All	Non-FHP sample	All	Non-FHP sample	All	Non-FHP sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RTR	-0.8335	-1.0898	-0.8691	-0.9958	-1.5588	-1.0448	-2.0488	-1.5117	-2.4448	-2.8632
	[0.4231]*	[0.4568]**	[0.4291]*	[0.4785]**	[0.4419]***	[0.4947]**	[0.5694]***	[1.0469]	[0.6135]***	[1.2428]**
R^2	0.387	0.378	0.44	0.417	0.529	0.477	0.582	0.573	0.62	0.675
Observations	3657	3202	3657	2631	3657	2233	3657	1422	3657	911

Notes: This table shows the robustness of our baseline results to excluding municipalities that adopted the family health program in year t. All regressions include microregion fixed effects and a pre-reform trend in outcome $(y_{i1990} - y_{i1985})$. All observations are weighted by the number of births in 1991. Robust standard errors in brackets are clustered at the state level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.10: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Controlling for Family Health Program Rollout)

	1991-2000		1991-2010	
	(1)	(2)	(3)	(4)
RTR	-2.0488	-1.8964	-3.035	-2.7168
	[0.5694]***	[0.5407]***	[0.8428]***	[0.7925]***
Year-of-FHP adoption FE		✓		✓
Basic controls	✓	✓	✓	✓
R^2	0.582	0.589	0.657	0.667
Observations	3657	3613	3657	3613

Notes: This table shows the robustness of our baseline results to controlling for year-of-FHP adoption fixed effects. All regressions include microregion fixed effects and a pre-reform trend in outcome ($y_{i1990} - y_{i1985}$). All observations are weighted by the number of births in 1991. Robust standard errors in brackets are clustered at the state level.
* p < 0.1;** p < 0.05;*** p < 0.01.

Table A.11: Regional Tariff Reductions and Changes in Local Health Spending and Hospital Capacity

	Change in log health spending		Change in hospitals per 100 thousands inhabitants		Change in hospital presence	
	1990-2000	1990-2010	1990-2005	1990-2010	1990-2005	1990-2010
	(1)	(2)	(3)	(4)	(5)	(6)
RTR	-1.2816	-3.396	-0.491	2.3737	-0.2494	-0.1964
	[2.4202]	[2.5945]	[1.5241]	[1.5248]	[0.2310]	[0.2480]
R^2	0.318	0.348	0.32	0.347	0.293	0.285
Observations	3657	3657	3657	3657	3657	3657

Notes: This tables presents estimates of the relationship between regional tariff reductions and changes local health spending and hospital capacity. The unit of analysis is a municipality. All regressions include microregion fixed effects and are weighted by the 1991 population. Robust standard errors in brackets are clustered at the state level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.12: Regional Tariff Reductions and Log-Changes in Infant Mortality Rate (Results for Maternal Alcohol- and Smoking-Related Infant Deaths)

	Maternal alcohol- and smoking-related Infant deaths		All other causes	
	1990-2000	1990-2010	1990-2000	1990-2010
	(1)	(2)	(3)	(4)
RTR	0.4692	-0.9228	-1.6511	-2.6372
	[0.6242]	[1.0264]	[0.7117]**	[0.9564]**
R^2	0.602	0.522	0.527	0.59
Observations	3657	3657	3657	3657

Notes: This table presents the results from estimating equation (2) for different causes of infant deaths. The unit of analysis is a municipality. All observations are weighted by the number of births in 1991. Robust standard errors in brackets are clustered at the state level. * $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.13: Regional Tariff Reductions and Log-Number of *Bolsa Familia* Program Beneficiaries

	2004 (1)	2006 (2)	2008 (3)	2010 (4)
RTR	15.8221	15.4645	15.4215	15.58
	[1.8220]***	[1.9022]***	[1.9847]***	[1.9172]***
R^2	0.758	0.778	0.784	0.794
Observations	3650	3657	3657	3657

Notes: This tables shows the relationship between regional tariff reductions and log-number of *Bolsa Familia* program beneficiaries in each year t. The unit of analysis is a municipality. All regressions include microregion fixed effects and are weighted by the 1991 population. Robust standard errors in brackets are clustered at the state level.

* p < 0.1;** p < 0.05;*** p < 0.01.

Table A.14: Regional Tariff Reductions and Log-Changes in Imports

	1991-2000 (1)	1991-2010 (2)
RTR	-6.9981	-10.628
	[4.6573]	[4.8229]**
R^2	0.03	0.056
Observations	26	26

Notes: This tables shows the relationship between regional tariff reductions and log-changes in imports. The unit of analysis is a state. All regressions are weighted by the 1991 population. Robust standard errors in brackets.
* p < 0.1;** p < 0.05;*** p < 0.01.

Table A.15: Interactive Effects of Trade Shock and Family Health Program for Other Outcomes I

	Log-change health spending, 1990-2010	Change in hospitals per 100 thousands inhabitants, 1990-2010	Change hospital presence, 1990-2010	Log number *Bolsa Familia* beneficiaries in 2010
	(1)	(2)	(3)	(4)
RTR	0.0331	-0.0515	-0.0026	0.1454
	[0.0776]	[0.0289]*	[0.0047]	[0.0512]***
Years Exposed to FHP	0.0765	-0.0106	-0.4348	18.4624
	[7.0394]	[2.7262]	[0.5315]	[6.6244]***
$RTR \times$	-0.3613	0.2875	0.0258	-0.4348
Years Exposed to FHP	[0.5546]	[0.2690]	[0.0348]	[0.6539]
R^2	0.351	0.347	0.287	0.82
Observations	3613	3613	3613	3613

Notes: This table presents the results from estimating equation (4). The unit of analysis is a municipality. All regressions include microregion fixed effects and are weighted by the 1991 population. Years Exposed to FHP is computed as $2010 - t^{FHP}$, where t^{FHP} is the year that the FHP was adopted in a given municipality. If a municipality did not adopt the FHP by 2010, then the total of years exposed to FHP is equal to zero. Standard errors in brackets are clustered at the state-level.

* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.16: Interactive Effects of Trade Shock and Family Health Program for Other Outcomes II

	Changes in male employment rates 1991-2010	Changes in female employment rates 1991-2010	Log-change in household income 1991-2010
	(1)	(2)	(3)
RTR	0.17901	-0.35548	-1.053
	[0.25933]	[0.20262]*	[0.3722]***
Years Exposed to FHP	0.00175	0.00094	0.0064
	[0.00282]	[0.00153]	[0.0033]*
RTR ×	-0.0138	-0.01988	-0.0192
Years Exposed to FHP	[0.01835]	[0.01436]	[0.0298]
R^2	0.683	0.681	0.91
Observations	3613	3613	3613

Notes: This table presents the results from estimating equation (4). The unit of analysis is a municipality. All regressions include microregion fixed effects and a pre-reform trend in outcome ($y_{i1991} - y_{i1980}$). All observations are weighted by the 1991 population. Years Exposed to FHP is computed as $2010 - t^{FHP}$, where t^{FHP} is the year that the FHP was adopted in a given municipality. If a municipality did not adopt the FHP by 2010, then the total of years exposed to FHP is equal to zero. Standard errors in brackets are clustered at the state-level.
* $p < 0.1$;** $p < 0.05$;*** $p < 0.01$.

Table A.17: Interactive Effects of Trade Shock and Family Health Program for Infant Mortality due to External Causes, 1991-2010

	Transport accidents	Other accidents	Aggression	Other external causes
	(1)	(2)	(3)	(4)
RTR	-0.3867	-0.5926	-2.2301	2.3617
	[1.8350]	[7.2595]	[1.4288]	[4.5581]
Years Exposed to FHP	-0.0054	-0.0119	-0.0145	-0.0177
	[0.0125]	[0.0210]	[0.0094]	[0.0138]
RTR × Years Exposed to FHP	-0.0177	-0.1161	0.1781	-0.2575
	[0.1624]	[0.6747]	[0.1760]	[0.4240]
R^2	0.595	0.582	0.523	0.47
Observations	3613	3613	3613	3613

Notes: This table presents the results from estimating equation (4). The unit of analysis is a municipality. All regressions include microregion fixed effects and a pre-reform trend in outcome ($y_{i1990} - y_{i1985}$). All observations are weighted by the number of births in 1991. Years Exposed to FHP is computed as $2010 - t^{FHP}$, where t^{FHP} is the year that the FHP was adopted in a given municipality. If a municipality did not adopt the FHP by 2010, then the total of years exposed to FHP is equal to zero. Standard errors in brackets are clustered at the state-level.
* p < 0.1;** p < 0.05;*** p < 0.01.

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