The Effects of Trade Liberalization on Fertility and Child Health Outcomes in India*

S Anukriti†
Todd J. Kumler‡

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Abstract

This paper examines the impact of India's 1991 tariff reform on fertility and child health outcomes in rural areas. In relative terms, women more exposed to tariff cuts are more likely to give birth, and these births are more likely to be female. These results are primarily driven by low caste, uneducated, and low wealth mothers. Moreover, infant mortality decreases for girls (but not boys) born in low status families in rural areas more exposed to tariff reductions, suggesting that socially disadvantaged households invest more in daughters to take advantage of new economic opportunities resulting from trade liberalization. On the other hand, fertility decreases and mortality for girls *increases* for high status women, who also exhibit a weak increase in sex ratio at birth in response to the trade reform.

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†Department of Economics, Columbia University. Email: as3232@columbia.edu.
‡Department of Economics, Columbia University. Email: tjk2110@columbia.edu.

1 Introduction

Several developing countries, including India, have increasingly become more open to international trade. International trade theory predicts that free trade enhances total welfare, but trade liberalization can also result in short and medium term adjustment costs. In particular, workers in formerly protected industries may face lower wages or reduced employment opportunities as the economy reallocates across regions and sectors in response to trade liberalization. The removal of tariff barriers in India in 1991 has been shown to cause slower reductions in poverty in affected rural districts (Topalova 2010). As a result, these districts also experienced slower improvements in children's schooling and smaller declines in child labor (Edmonds, Pavcnik, and Topalova 2010). Our study examines whether changes in trade policy also affect fertility behavior and investment in children's health.

India's trade liberalization in the early 1990s provides a good context for such an exercise. The policy reform was externally imposed by the International Monetary Fund (IMF) in response to a severe balance of payments crisis, which we argue was an exogenous shock to industry-level tariffs in India. Moreover, the resulting changes in tariff- and non-tariff barriers (NTBs) were quite large in magnitude. In the manufacturing sector, the average tariff declined from 117 percent to 39 percent and the share of imports covered by NTBs fell from 82 percent to 17 percent between 1990-91 and 1999-2000 (Gupta and Kumar 2008). These tariff reductions were much more drastic than in other countries which underwent similar transitions in the recent past, such as Indonesia, Brazil, Colombia, Argentina and Mexico. Following an identification strategy used by Topalova (2010) and others, we exploit heterogeneity in pre-reform industrial composition of Indian districts, combined with differences in tariff cuts by industry, to identify districts that were more or less exposed to trade liberalization. We then estimate the effect of this differential exposure on our outcomes of interest.

There are several channels through which trade liberalization can affect fertility and child health outcomes. Standard of living is the most obvious one: Topalova (2010) finds evidence for a relative decrease in wages in impacted industries and a relative increase in poverty for Indian districts more exposed to trade reform.[1] To the extent that negative income shocks and poverty are linked to investments in children's health[2] and parents' decisions about the number and sex-composition of their children in developing countries (see, for instance, Edlund and Lee (2009) and Chung and Gupta (2007)), we expect the short-run adjustment costs associated with tariff cuts to influence fertility decisions and outcomes. Secondly, more open trade may influence relative commodity prices in an economy, and hence consumption levels (Porto 2007). Changes in the amount and type of food (and nutrients) consumed by the mother and her children due to differences in dietary preferences across districts could affect child health outcomes, in general, and infant mortality, in particular (Cutler, Deaton, and Lleras-Muney 2006). Additionally, structural

[1] Although, using state-level data, Hasan, Mitra, and Ural (2007) conclude that greater exposure to trade openness is not associated with slower reduction in poverty in rural India. For a more detailed discussion, we refer the reader to Topalova (2010).

[2] See, for example, Strauss and Thomas (1998), Strauss and Thomas (2008), Case (2001), Case (2004) for South Africa, Paxson and Schady (2005) for Peru.

adjustments resulting from trade liberalization may change the relative demand for female labor (Katz and Murphy (1992), Kucera (2001), and Kucera and Milberg (2000)), or the gender wage gap (Wood (1991) and Black and Brainerd (2004)) and thus, influence fertility through the female labor force participation channel. Similarly, if parents' decisions to have female versus male children are influenced by their relative economic values, any effect of trade liberalization on the demand for female labor could also influence the observed sex ratio at birth (Qian 2008).

Using retrospective birth histories, we find differential effects for low and high socio-economic status women. Low caste, uneducated and less wealthy women are significantly more likely to give birth in districts that are relatively more exposed to tariff reform. These births are also more likely to be female. Moreover, they are significantly less likely to die within one, six and twelve months of birth. On the other hand, mortality for girls born to high caste, more educated and wealthier mothers *increases* significantly. High status women are also less likely to give birth and more likely to give birth to boys, but these results are not as strong.

Based on the evidence from recent empirical literature about the effects of trade liberalization, we distinguish between three potential mechanisms that could explain our findings - relative increase in poverty, gains in relative female bargaining power, and higher relative returns (to parents) from daughters due to better economic opportunities. Our findings suggest that parents from socially disadvantaged groups are taking advantage of the new economic opportunities, by investing more in daughters than previously. This effect is coming from two sources. Firstly, these parents exhibit a greater relative demand for daughters, which results in more girls being born, likely due to reduced use of sex-selective abortions. Secondly, they take better care of the daughters – conditional on being born, infant mortality for low status girls decreases. Low status boys do not benefit equally either because their potential earning opportunities have not increased as much, or due to limited occupational mobility for low status men (Munshi and Rosenzweig (2009)). It is possible that low status parents strategically choose not to over-invest in their sons so that they do not lose out on the traditional occupational networks of low-caste men (Munshi and Rosenzweig (2006)).

Our paper contributes to a large empirical literature[3] that examines the costs and benefits of freer trade and a smaller one that focuses on the Indian experience.[4] Most papers in the field of international economics have focused on the effects of liberalization on outcomes such as productivity, industrial composition, and wage inequality. However, it is equally important to examine the implications of these macroeconomic changes for individuals' decisions about fertility and investments in health and human capital to develop a broader understanding of the distributional effects of more open trade. Despite methodological shortcomings, existing literature suggests that trade openness has not unambiguously benefited everyone (Goldberg and Pavcnik (2007a)). Any resulting differential effects on fertility and child health out-

[3]Trefler (2004) on U.S. and Canada, Hanson (2007) on Mexico, Goldberg and Pavcnik (2007b) on Colombia, and Kovak (2012) on Brazil, and the following review papers: Tybout (2003), Goldberg and Pavcnik (2007a), and Harrison, McLaren, and McMillan (2011).

[4]Edmonds, Pavcnik, and Topalova (2010), Topalova (2010), Hasan, Mitra, and Ranjan (2009), Hasan, Mitra, and Ural (2007).

comes can potentially play an important role in exacerbating or combating socio-economic inequalities in society. Our paper is also related to the vast literature on the determinants of child well-being and the sex composition of children in developing countries.[5] Lastly, our ability to control for state-specific time trends and mother fixed-effects in the regression analysis makes our identification more robust than previous literature on the effect of tariff reform on household and individual outcomes.

The rest of the paper proceeds as follows. In Section 2, we provide a brief summary of the Indian trade reform. In Section 3, we outline our empirical methodology and describe the data. Section 4 discusses the empirical estimates of the relationship between tariffs and various outcomes of interest. Section 5 presents some robustness checks and Section 6 concludes the paper.

2 India's Trade Liberalization

We analyze the effect of trade liberalization on household fertility decisions and children's health in the context of India's 1991 trade reform. Faced with a balance of payments crisis in August 1991, the Indian government embarked on several major economic reforms as conditions of an International Monetary Fund (IMF) bailout. Included among these requirements was a unilateral

[5] Since beginning work on this paper, we have become aware of another study, Chakraborty (2012), analyzing the impact of the Indian trade liberalization on sex ratios in India. Our paper differs from hers in a number of ways. First, she uses birth histories from the 1999 National Family Health Survey of India (NFHS), a much smaller dataset than ours, the 2002-2004 District-Level Household Survey of India (DLHS). Second, while her measure of trade exposure is similar to ours, she only includes tariffs in the manufacturing sector; we include tariffs in all traded industries, including agriculture, the main sector of employment for rural India. Finally, our empirical strategies differ significantly; we believe our empirical specifications and larger sample size allow us to better isolate the causal effect of trade liberalization on fertility outcomes.

reduction in the overall level and the dispersion of import tariffs as well as the removal of non-tariff barriers (NTBs), such as import licensing.

The period after the IMF bailout, therefore, marks a sharp break in Indian trade policy. The maximum tariff fell immediately from 400 percent to 150 percent, with later revisions bringing the maximum tariff to approximately 45 percent by 1997 (Hasan, Mitra, and Ural 2007). Meanwhile, the average tariff fell from 80 percent in 1990 to 37 percent in 1996 and the standard deviation of tariffs declined by 50 percent (Topalova 2010). NTBs also fell, with the proportion of goods subject to quantitative restrictions receding from 87 percent in 1987 to 45 percent by 1994 (Topalova 2010).

In addition to the sharp decline in trade protection, the 1991 episode possesses several important features that are valuable for our analysis. Since the policy reform was imposed as part of the IMF bailout, the tariff cuts were largely unanticipated by firms and households in India. As other commentators have observed, the removal of trade barriers was implemented swiftly as a form of "shock-therapy" and was not part of any pre-existing development plan (Bhagwati (1993), Goyal (1996)). It is, therefore, unlikely that our results are driven by any adjustment in fertility in anticipation of these reforms.

The quick initiation of the liberalization episode also reduces concerns that industries with greater political influence or higher productivity shaped the structure of the tariff reforms in a way that would undermine our empirical strategy (described in detail in the following section). Topalova (2007) finds that industry-level tariff changes are uncorrelated with several proxies of an industry's political influence prior to the Indian reform, such as the number of employees, proportion of skilled workers, or industrial concentration. Previous studies also find no correlation between an industry's future tariffs and its productivity before 1991 or productivity growth during 1989-1997 (Topalova 2004). Finally, tariff changes through 1997 were spelled out in India's Eighth Five Year plan (1992-1997), suggesting little room for manipulation of tariffs based on political economy concerns during this time period.

It must be noted that like Edmonds, Pavcnik, and Topalova (2010) we focus only on the effects of tariff reductions and ignore changes in NTBs. This is primarily due to data availability issues. As mentioned previously, removal of NTBs was an important part of the Indian trade reform and thus, our results measure the effect of one important dimension of it, namely tariff cuts. The exclusion of NTBs is potentially harmful for our empirical strategy if the trends in NTBs were in the opposite direction as compared to tariffs. But as mentioned by Edmonds, Pavcnik, and Topalova (2010), existing literature suggests that there is a positive correlation in tariffs and NTBs during our sample period. Thus, our results are biased to the extent that some of the effects that we assign to tariff cuts were actually caused by removal of NTBs.[6]

3 Empirical Strategy

3.1 Measuring Exposure to Tariff Reduction

The impact of trade liberalization on a developing economy such as India can be felt through many channels. The availability of cheaper imported final

[6]Edmonds, Pavcnik, and Topalova (2010) also argue that despite incomplete removal of NTBs by 1997, volume of imports did increase due to reductions in tariffs. Thus, tariff declines were a significant and important part of the 1991 Indian trade liberalization.

goods can be welfare-improving for consumers, while the reduction in tariffs on intermediate inputs can increase firm productivity. Although a decrease in consumer prices could certainly influence household fertility behavior, this effect will be common across all households in India. On the other hand, an increase in supply of cheaper, imported products that compete with domestic goods can reduce employment and wages at domestic firms. Our measure of tariff protection emphasizes this latter effect of trade openness on employment, as in many other papers in the literature.

National tariff protection varies across industries and over time in India. Moreover, there is substantial heterogeneity in the industrial composition of Indian districts prior to 1991. Therefore, depending on their industrial composition of employment at the time of reform, some Indian districts experienced relatively larger reductions in trade protection than others. Following Topalova (2010) and others, our identification strategy relies on this comparison to estimate the causal effect of tariff reform.

Specifically, we interact the national nominal ad-valorem tariff faced by industry i in year t , $tariff_{it}$, with the share of employment in industry i and district d in 1991, $empshare_{id}^{1991}$, to construct a measure of tariff for district d in year t:

$$tariff_{dt} = \sum_{i} empshare_{id}^{1991} \times tariff_{it} \tag{1}$$

Since the employment shares are based on a district's industrial composition *before* the initiation of trade liberalization, our tariff measure will be free of any endogenous changes in employment composition that take place due to the removal of tariff barriers.

Even though tariff cuts took place across a wide range of industries, there were certain "non-traded" industries such as cereals and oilseeds production,[7] for which only the government was allowed to import these products. Since there is no corresponding $tariff_{it}$ for non-traded industries, $tariff_{dt}$ assigns a zero tariff to them for the entire time period. But this means that districts with higher levels of employment in the non-traded sector will mechanically have lower $tariff_{dt}$ (Hasan, Mitra, and Ural (2007)). Since a large proportion of non-traded employment is in the cereal and oilseeds sectors, and workers in these industries tend to be poor rural farmers, this introduces a negative correlation between poverty and the tariff measure, $tariff_{dt}$.

Previous studies address this concern by constructing a second measure of district tariffs that only depends on employment in traded industries (Hasan, Mitra, and Ural (2007), Topalova (2007), Topalova (2010)). We follow the literature and create this measure as follows:

$$tradedtariff_{dt} = \frac{\sum_{i \in traded} employment_{id} \times tariff_{it}}{\sum_{i \in traded} employment_{id}} \tag{2}$$

The only difference between the two measures of tariff protection in (1) and (2) is that the latter only uses employment in traded industries as weights for industry-level tariffs, and excludes employment in non-traded industries. The traded tariff measure is, therefore, independent of the proportion of workers in the non-traded sector and is uncorrelated with initial poverty levels within a district.

[7] Other non-traded industries during our sample period were services, trade, transportation, and construction.

3.2 Regression Framework

The question of interest in this paper is how the removal of tariff barriers influences households' fertility decisions and children's health outcomes. In particular, we investigate whether reductions in tariff protection faced by a woman (based on her district of residence) impact the probability that she gives birth in a year, the sex ratio of these births and their mortality rates. Our regression framework is similar to Edmonds, Pavcnik, and Topalova (2010) and Topalova (2010) and compares women/births in districts that were more or less exposed to tariff cuts.

We start by reshaping the retrospective birth data to create a woman-year panel and construct a dummy variable $birth_{mdt}$ that equals one if a woman m in district d gives birth in year t, and is zero otherwise. Then, we estimate the following base specification using ordinary least-squares:

$$birth_{mdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{mdt} + \gamma_d + \tau_t + \delta_s t + \epsilon_{mdt} \tag{3}$$

The main regressor of interest, $tariff_{dt}$, represents the level of tariff protection assigned to women based on their district of residence. Although the variation in $tariff_{dt}$ occurs at the district level, we also control for a vector of individual covariates, X_{mdt}, that may impact the outcome variables, including indicators for a woman's age in year t, the number of previous births, the household's caste[8] and religion.[9] Inclusion of district fixed-effects, γ_d, controls

[8]Caste categories are scheduled caste (SC), scheduled tribe (ST), other backward caste (OBC) and general caste.

[9]Religion categories are Hindu, Muslim, Sikh, Christian and Others.

for time-invariant differences across districts while year fixed-effects, τ_t, control for any India-wide shocks that may influence our outcomes. The inclusion of year fixed-effects also highlights that our empirical strategy *does not* estimate the overall effect of trade liberalization on fertility, sex ratios at birth or infant mortality, since any economy-wide impact on consumer prices or productivity will be captured by the year-effects. Since our data spans all years between 1987 and 1997, we also include linear state-specific time trends in our regressions.

The sex ratio and child mortality regressions are run using the retrospective panel of *births*. The base OLS specification is similar to (3):

$$y_{imdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{imdt} + \gamma_d + \tau_t + \delta_s t + \epsilon_{imdt} \tag{4}$$

where i indexes a child born to mother m, in district d, and year t. For mortality regressions, the outcome y_{imdt} is an indicator variable for whether a child dies before Q months of birth, where we allow Q to equal one, six, or twelve months.[10] For the sex ratio regressions, the outcome y_{imdt} is an indicator variable that equals one if the child is male, and zero if the child is female. The remaining controls are the same as in (3).

Since a large majority (89%) of women in our sample report giving birth to more than one child during 1987-1997, we also run specifications with mother fixed-effects:

$$birth_{mdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{mdt} + \tau_t + \phi_m + \epsilon_{mdt} \tag{5}$$

[10]Infant mortality is defined as death before age 1, while child mortality usually refers to death before age 5.

$$y_{imdt} = \beta_0 + \beta_1 tariff_{dt} + \beta_2 X_{imdt} + \tau_t + \phi_m + \epsilon_{imdt} \tag{6}$$

where γ_m represents the mother fixed-effect and controls for all unobserved, time-invariant heterogeneity across women that could influence her fertility decisions. X_{mdt} and X_{imdt} now include just the indicators for number of previous births and woman's age in year t. By including mother fixed-effects, we are essentially comparing the birth outcomes for the same woman under different levels of tariff protection for her district. Our ability to control for state-specific time trends and mother fixed-effects makes our identification more robust than previous literature. A positive (negative) β_1 implies that tariff decline is associated with a decrease (increase) in the outcome of interest, *relative to the national trend*.

The coefficient β_1 is identified under the assumption that changes in our tariff measure are uncorrelated with district-specific, unobserved, time-varying shocks (or mother-specific unobserved time-varying shocks in (5)-(6)) that influence fertility, sex ratios or child mortality. Since we interact a district's pre-reform industrial composition with national changes in industry tariffs to construct $tariff_{dt}$, any source of bias would have to be correlated with both pre-reform industrial composition and national tariff changes by industry. Like Topalova (2010), Edmonds, Pavcnik, and Topalova (2010) and others, we assume that this is not the case. Nevertheless, we test the validity of this assumption by checking that our results are robust to the inclusion of other observable district-specific, time-varying shocks, such as rainfall shocks (in Section V).

For our sex ratio regressions, t refers to the year of conception, instead of the year of birth. Since ultrasound followed by an induced abortion is believed to be the primary channel through which parents exercise control over the sex of their births in India during our sample period (Bhalotra and Cochrane (2010)), and these technologies are most effective and safe during the first or second trimester of birth (Epner, Jonas, and Seckinger (1998)), district tariff protection during the year of conception is more relevant for explaining the effect of trade reform on sex ratios at birth. We define the year of conception as the year nine months prior to the month of birth, thereby implicitly assuming that no birth is premature.

One concern, previously discussed, is that $tariff_{dt}$ may be correlated with the pre-reform size of a district's non-traded sector and hence correlated with its initial level of poverty. If this is the case, OLS estimates in specifications (3)-(6) will be biased. We deal with this issue by using traded tariff, $tradedtariff_{dt}$ as an instrument for $tariff_{dt}$. As Figure 2 shows, $tradedtariff_{dt}$ is significantly correlated with $tariff_{dt}$ (first-stage regression estimates presented later). Moreover, it is independent of the baseline proportion of workers in the non-traded sector and therefore, uncorrelated with initial poverty levels within a district. This validates the use of traded tariff as an instrument.

3.3 Data

We use data from the second round of the District Level Household Survey (DLHS-2) of India. The DLHS-2 surveyed 507,000 currently-married women (aged 15 - 44 years) from 620,000 households in 593 districts during March

2002 - October 2004. This survey includes a complete retrospective birth history for every women interviewed, containing information on month and year of the child's birth, birth order, age of the mother at birth, and the age at which the child died, if the child is deceased.

Since we focus on district as the geographical unit of interest, ideally we would like to know the district in which a birth takes place. But DLHS-2 only includes district of residence identifiers at the time of survey. As a result, we assume that all births to a woman take place in her district of residence at the time of survey. This implicitly assumes that mothers do not migrate to a different district after initiating child-bearing. This might seem like an unreasonable assumption, but in practice, inter-district migration in India is low and mostly consists of women relocating as a result of marriage. Using National Sample Survey (NSS) data, Topalova (2010) shows that only three to five percent of women moved across districts within the last ten years. We would expect this number to be even lower for women who have already given birth to their first child. In addition, this assumption is problematic only if the measurement error induced by it varies, systematically, with our measures of district-level tariff protection.

We focus our analysis on rural areas within Indian districts. Topalova (2010) finds an insignificant relationship between tariff protection and poverty in urban areas of Indian districts, which she attributes to pre-existing trends in poverty and the presence of other reforms in addition to trade liberalization that impacted urban areas. Due to concerns of simultaneous reforms and pre-existing trends in urban areas, we focus on rural areas only.

We restrict our analysis to the 1987-1997 time period. There are two reasons for this. First, 1987 is the earliest year for which we have tariff data. In addition, the tariff changes during 1992-1997 were spelled out in India's Eighth Five Year Plan, so they are unlikely to have been influenced by political economy decisions. After 1997, however, industry-level tariffs are negatively correlated with that industry's current productivity (Topalova 2004), suggesting that these latter changes may be endogenous to industry performance. For this reason, we only focus on years up to 1997.

We impose three additional sample-selection criteria. First, we only include births for whom the mother's age at birth was between 13 and 40. Second, we exclude birth parities of 11 or higher. We use these restrictions due to the small number of births to women outside of the 13-40 age range and the small number of births with parity above 10. However, our results are not sensitive to the exclusion of these observations. The DLHS questionnaires were also administered to women who are not permanent residents of the household, but happened to be visiting at the time the survey took place. Since there is no information on their actual district of residence, we exclude them from our analysis. Moreover, we only focus on the rural sector. Our final sample comprises 464,916 births, to 269,661 women, in 408 districts.

The district-level tariff data comes directly from Topalova (2010). Industry- and district-wise employment data comes from the 1991 Indian Census of Population while tariff data at the six-digit level was collected by Petia Topalova from the Indian Ministry of Finance publications. The rainfall data used later comes from the annual district-level precipitation time series created by Ram

Fishman using Indian Meteorological Department database.

Figure 1 shows the evolution of nominal national industry-level ad-valorem tariff during 1987-1997. Average tariff fell from about 95% in 1987 to about 30% in 1997. Figure 2 plots the constructed tariff and traded tariff measures used in our analysis for the same time period. There is a strong correlation between both measures and they exhibit a sharp downward trend.[11] Since non-traded industries are automatically assigned a zero tariff for all years, the average tariff measure is substantially lower than the average traded tariff measure by construction. The average traded tariff fell from about 88% to 31%, and the average tariff fell from about 7% to 2% during 1987-1997.

Next, we look at the time trends in fertility, sex ratio at birth and child mortality in India during our study period. The total fertility rate declined from 4.1 in 1987 to 3.3 in 1997 (Figure 3). The male-female sex ratio in the 0-6 age group has been rising rapidly (Figure 4), especially since the 1980s. Increased availability of technology for sex-selection combined with declining fertility and son preference (especially in north-western states) are widely believed to explain this growing gender imbalance in the child population. According to DLHS-2 data, under-5 mortality in rural India fell from 127 deaths per 1000 live births in 1987 to 95 deaths per 1000 live births in 1997. As Figure 5 shows, child mortality has been declining over time. Mortality before age 1 is much higher than mortality during ages 1-4. Mortality for girls is larger during ages 1-4. It is important to keep in mind that our identification strategy does not

[11] The only exception is an increase in the average tariff in 1993. Due to concerns of measurement error in the tariff variable for this year, we also run regressions excluding 1993; our results are robust to excluding this year. These results are available upon request.

estimate the causal impact of tariff reductions in explaining these aggregate trends. Instead, we estimate the effect of tariff reductions on *deviations from the trend*. Table 1 provides a description of the socio-economic characteristics of our sample.

4 Results

4.1 Effects of Tariff Reduction on Fertility

We begin by looking at the effect of changes in district level tariff exposure in a year on the probability that a woman gives birth in that year.[12] Column (1) in Table 2 presents the baseline results controlling for district and year fixed effects. In Column (2) we also control for mother's years of schooling, indicators for mother's age in that year, her number of previous births, household's caste and religion. In Column (3) we add state-specific linear time trends. Finally, Column (4) controls for mother fixed effects. In all specifications, robust standard errors are clustered at the district level and district-level sampling weights are used.[13]

The OLS results in Panel A indicate a positive and significant relationship between our district-level tariff measure and the probability that a woman gives birth. While the coefficient becomes insignificant and small when we include state-specific linear time trends in Column (3), the results remain significant, and increase five-fold, when we include mother fixed effects (Column

[12]We use the term fertility throughout this paper to indicate probability of birth in a given year. It must be noted that a higher probability of birth in a given year does not necessarily imply higher completed fertility. It is possible that our results capture changes in timing of births rather than changes in overall fertility levels.

[13]The unweighted regressions yield very similar results, which are available upon request.

(4)). These positive coefficients suggest that districts more exposed to trade liberalization (i.e. a relative decline in our tariff measure) witnessed a relative *decrease* in fertility.

For reasons previously described, however, changes in the tariff measure utilized in Panel A may be negatively correlated with a district's initial poverty level. If women in initially poorer districts also experience relatively smaller declines in fertility over our time period for reasons unrelated to trade liberalization, OLS will overestimate the causal effect of tariff protection on fertility. We, therefore, instrument for our tariff protection measure using traded tariff protection, which is uncorrelated with the size of the non-traded sector, as argued previously. Panel B of Table 2 shows the first-stage regression of a district's tariff measure on a district's traded tariff protection. In all specifications, traded tariff has a significant and strong first-stage impact on district tariff protection, indicating that traded tariff is a strong instrument for district tariff.[14]

When we use traded tariff as an instrument (Panels C and D of Table 2), district tariff protection in a year has a *negative* effect on the probability that a woman gives birth in that year, although we lose significance when mother fixed effects are included. The fact that our coefficient of interest changes sign when instrumenting for district tariff protection suggests that including non-traded industries in the tariff measure introduces a significant upward bias due to the correlation between initial poverty and changes in the tariff measure. The reduced form coefficient of traded tariff is also negative throughout and

[14]The first stage F-statistic is large in all specifications.

mostly significant (except in the mother fixed-effects specification).

The IV coefficients indicate that the Indian trade reform had a substantial effect on fertility – a woman living in a district that experienced the average decline in tariff protection of 7 percentage points was between 0.6 percentage points (Panel C, Column 1) and 1.2 percentage points (Panel C, Column 3) more likely to give birth in a given year.[15]

4.2 Effects of Tariff Reduction on Sex Ratio at Birth

Having established that a reduction in a district's relative tariff exposure leads to an increased probability of birth in rural India, we turn our attention to the sex composition of these births. Sex ratio at birth (SRB) deviates from the natural SRB if female fetuses are terminated more frequently than male fetuses due to less pre-natal care or sex-selective abortions. In India, pre-natal sex-determination is illegal, but widely prevalent, leading to a large number of female fetuses being aborted. Bhalotra and Cochrane (2010) estimate that approximately 480,000 sex-selective abortions took place in India annually from 1995 to 2005. Trade liberalization can affect the SRB by (a) changing the demand for sex-selective abortions due to changes in the relative demand for sons, (b) changing the demand for sex-selective abortions due to changes in parents' ability to afford pre-natal sex determination and abortion resulting from changes in income, (c) through income shocks which impact the fetal environment, affecting fetal viability differentially based on the sex of the

[15] Table A.1 in the Appendix presents the IV results for urban areas and we find a similar increase in the likelihood of birth in response to tariff cuts. Coefficients are negative and significant across all four specifications.

fetus (Trivers and Willard (1973)),[16] or (d) through greater access to sex-determination technology *via* imports of ultrasound machines, for example.[17]

Sex-determination can be effectively performed through ultrasound around 12 weeks of gestation or much earlier, through *amniocentesis*, around 8-9 weeks of gestation. If a mother has an induced abortion, it is likely to take place during the first or second trimester of pregnancy. This suggests that the relevant tariff variable to examine the effect of trade liberalization on the sex of a birth is not the tariff at the time of birth, but the tariff during the first two trimesters of pregnancy. Therefore, we use tariff in the district of birth in the year of conception as the explanatory variable for all sex ratio regressions.

Using the retrospective panel of births, Table 3 presents the results from OLS and IV regressions of an indicator for male birth on district-level tariff during the year of conception.[18] The OLS coefficients in Panel A show that a child born in a district with a relative decline in tariff protection during the year of conception is *less likely to be a boy*; but the effect is not significant at conventional levels. Panels B, C, D present the instrumental variables regression estimates. The first stage coefficients of traded tariff are positive and highly significant throughout. The IV and reduced form coefficients of district tariff in the year of conception are always positive, but only significant when we include the mother's fixed effects.[19] For a district with the average

[16]The Trivers-Willard hypothesis suggests that negative shocks to the fetal environment make births less likely to be male.

[17]Changes in pre-natal sex-determination technology, however, are likely to impact the entire country, or at least all districts within a state similarly. Since our measure of tariff protection varies at the district level, this channel is unlikely to explain our results.

[18]Each cell indicates a separate regression. As before, all regressions use district level sampling weights and robust standard errors are clustered at the district level.

[19]However, the coefficients for non-mother fixed effects specifications are also significant

decline in tariffs of 7 percentage points, column (3) in Panel C suggests that the likelihood of a male birth decreases by 0.6 percentage points. Thus, the reduction in trade protection seems to have caused some relative improvements in the probability of a female birth in rural Indian districts more exposed to tariff declines, although the overall effects are not highly significant.[20]

The fact that we find significant results when we control for mother fixed effects and not without them highlights the importance of time-invariant unobserved heterogeneity in factors that influence decisions about sex-selection. Apart from the monetary cost of pre-natal sex detection and sex-selective abortion, unobserved subjective son preference is likely to be an important factor in parents' decisions about sex-selection. Although in specifications without mother fixed-effects we control for some observable socio-economic characteristics that are likely to be correlated with son preference, for example religion, it seems that they do not fully capture unobservable heterogeneity across women. In section 4.4, we present evidence for heterogeneity in the effects on the sex ratio at birth across socio-economic groups.

4.3 Effects of Tariff Reduction on Infant Mortality

Next, we examine the effect of tariff decline on infant mortality. Following the same format, Table 4 presents results from OLS (Panels A) and IV regressions (Panels B and C) of an indicator for whether a child dies within one, six and twelve months of birth on district-level tariff protection. Across

when we use alternate levels of clustering, e.g. district-year. Here, we report results with more conservative standard errors, which in our case are obtained from clustering at the district level.

[20]Table A.1 in the Appendix shows that the effects on SRB in urban areas were in the same direction, but insignificant even for the mother fixed effects specification.

all specifications, OLS coefficient estimates in Column (1) are negative and mostly significant, indicating that a larger decline in tariff protection within a district is associated with a relative increase in infant mortality within one, six and twelve months of birth. The coefficients are similar in magnitude and significance when we add controls such as mother's age fixed-effects, indicators for previous births, religion and caste fixed effects as in Column (2). When we add state-specific linear time trends to our regressions in Column (3) we lose significance. Since initial industry composition is highly correlated across districts within states and our tariff measure exhibits a generally linear downward trend, it does not seem possible to separately identify the impact of the district-level tariff measures from the state-specific linear time trends. But it is reassuring that when we add mother fixed-effects, which control for any unobserved time-invariant mother characteristics, the coefficients remain significant.

The magnitude of our coefficient estimates increases as we change our outcomes from mortality within one month of birth to mortality within six months or twelve months of birth. The fact that we find significant results on mortality within the first month of birth for some specifications suggests that trade liberalization also influences households' ability to invest in the health of a child while in-utero.[21] However, the growth in the coefficients as we look at mortality within six months and twelve months implies that trade liberalization

[21] Investments in health while in-utero are also likely to be affected by the tariff in the year of conception. In order to examine this channel, we also run regressions using tariff in the year nine months prior to the year of birth as the explanatory variable. The tariff coefficients are negative but not always significant and smaller in magnitude in comparison to the coefficients in Table 4.

prevents families from making the necessary investments in a child's health to prevent infant death even after birth.[22]

Moreover, the estimated effects are economically significant. For example, our coefficient estimate of -0.118 in Column (4), Panel C3 of Table 4 indicates that, relative to other districts, a district that experienced the average decline in tariff protection of 7 percentage points witnessed an increase in infant mortality within 12 months of birth of 0.8 percentage points – about a 9% increase with respect to the baseline (1987) mortality within a year of birth in all districts (9%).

In Table 5, we interact the tariff measure with a dummy for the child being male to test if the effect on mortality differs by child's sex. Previous research on the Trivers-Willard Hypothesis indicates that male children are less likely to survive relative to females in harsher environments (Almond and Edlund 2007). If a decline in tariff protection increases poverty and decreases health investments for pregnant women or for newborn children, we might expect trade liberalization to have a greater effect on mortality of male children. The main effect of our tariff measure suggests that there is a significant increase in mortality within 12 months of birth for girls.[23] The coefficient of the interaction term is positive, suggesting a smaller effect on boys, although the overall effect is an increase in mortality for boys as well. In Columns (1)-(3), the interaction term is not significant, implying that both boys and girls experienced an equal and significant increase in mortality. In Column (4), both the

[22]We find no significant effect on any mortality outcome for urban areas in Table A.1 in the Appendix.

[23]Similar results are obtained for mortality within 1 and 6 months of birth and are available upon request.

main and interaction effects are significant and of opposite signs, indicating a much smaller increase in mortality for boys relative to girls. According to the coefficients in Column (4), in a district that experienced the average decline in tariff protection of 7 percentage points, girls witnessed an increase in infant mortality within 12 months of birth of 1.6 percentage points as opposed to boys for whom mortality within 12 months increased by a much lower 0.1 percentage points. These effects are consistent with prior evidence on post-natal neglect and discrimination in care against girls in India. To the extent that parents are able to exercise their preference for male children at the pre-natal stage through sex-selection, an increase in sex-selective abortions can lead to a decline in relative female mortality (Lin, Liu, and Qian (2010)). But if poverty makes sex-selection less affordable, then we can expect the pattern we observe: lower sex ratio at birth, but higher relative female mortality. As we show in the next section, there is substantial heterogeneity in the effects on mortality across socio-economic groups.

One potential concern with our identification strategy is the presence of other time-varying district-specific omitted variables. Since our tariff exposure index varies at the district-year level, we cannot include district-year fixed effects to prevent this omitted variable bias. We check the robustness of results presented in the previous sections by controlling for district-level annual rainfall shocks. Annual fluctuations in rainfall are an important determinant of economic outcomes in agriculture-dependent developing countries, such as India.[24] We re-estimate our main specifications by also including as an

[24]Paxson (1992), Rosenzweig and Wolpin (1993), Townsend (1994), Jayachandran (2006).

explanatory variable an indicator that is equal to one if the district experiences a rainfall shock in a given year, and zero otherwise. We define a rainfall shock as a deviation of more than 30% from historic annual mean precipitation in the district. The point estimates on tariff measures in all specifications remain consistent with our previous results (with similar signs, magnitudes and significance). These results are available upon request.

5 Mechanisms

Our results so far suggest that relative declines in tariffs in rural Indian districts lead to a significant increase in the probability of birth and these births are more likely to be female, although the latter effect is significant only for the mother fixed-effects specification. Moreover, the likelihood that a child dies within one, six or twelve months of birth significantly increases. Next, we explore the mechanisms underlying these results.

We attempt to distinguish between three potential channels: 1) poverty, 2) female bargaining power, and 3) relative returns from daughters. Unfortunately, our data does not contain information on household income, consumption expenditure, wages, or mother's labor force participation status, making it difficult to directly test for the aforementioned causal channels. Other data sources that measure consumer expenditure and wages in India, such as the National Sample Surveys, cannot be used for our purposes since they do not collect woman-level birth histories or information on mortality.[25] As a second

[25] In the NSS data, the exact date of birth is not recorded. However, it is possible to deduce the year of birth from the reported age of the household member. Nevertheless, the reported household consumption expenditure is for the year preceding the survey and/or the year of survey. We can potentially use the sub-sample of children aged 1 or less in the

best approach, we examine heterogeneity in effects by the socio-economic characteristics of mothers to provide suggestive evidence about the causal mechanisms underlying our main results. This approach is based on the premise that the three channels mentioned above should affect our three outcome variables differently, thus helping us deduce the underlying mechanisms. Moreover, it is also possible that mechanisms differ across socio-economic groups. Before we proceed to the regression results, we first discuss the expected effect on our outcome variables through each of these three channels.

As previously shown by Topalova (2010), districts more exposed to trade liberalization witnessed a relative increase in poverty. Households that suffer a negative income shock due to tariff cuts may be less able to afford modern birth control methods and sex-selective abortions, causing an increase in births, especially female births.[26] In addition, the supply of free or subsidized contraception may decline if government finances decline as a result of trade liberalization. Moreover, if poverty impacts the probability that a child survives to adulthood or the likelihood that a child is male, households may choose to increase fertility if decisions about the number and the sex composition of children are made jointly. Poverty can also lead to increases in infant mortality if families reduce investments in infant health as a result of a decline in income. Furthermore, if the additional girls born as a result of the

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NSS households and use the household level consumption expenditure as an intermediate explanatory variable for their sex. However, we would still not be able to determine the effect on fertility (since it is not possible to link mothers to their children) or mortality. Moreover, our analysis would have to be restricted to a simple pre-post comparison, without controlling for trends, since only two cross-sectional rounds of NSS are available for our sample period.

[26] Bhalotra and Cochrane (2010) show that wealthier families in India are more likely to practice sex-selection.

increase in poverty and the resulting inability to control fertility are viewed as "unwanted," we would expect the increase in infant mortality to be higher for daughters relative to sons. Thus, if the relative increase in poverty from trade reform is the underlying channel, we should observe an increase in fertility, a decrease in the sex ratio at birth, and an increase in infant mortality, more so for girls. On the other hand, if poverty decreases as a result of trade reform, we would expect the opposite effects i.e. lower fertility, a higher sex ratio at birth, and lower mortality.

Aguayo-Tellez, Airola, and Juhn (2010) show that a NAFTA-related decrease in tariffs increased bargaining power of women within the household in Mexico. They believe this is due to two reasons. Firstly, technology upgrading by firms in response to trade liberalization makes physically demanding skills less important in blue-collar jobs. As a result, the relative wage and employment of women improves in blue-collar occupations, as shown by Juhn, Ujhelyi, and Villegas-Sanchez (2012). Secondly, trade reform leads to growth which is concentrated in initially female-intensive industries, and thus benefits women in these industries relatively more if male and female labor are imperfect substitutes. If intra-household bargaining is the primary channel through which trade reform affects fertility and infant mortality, then we expect to see lower fertility due to higher opportunity cost of childbearing (Chiappori, Fortin, and Lacroix (2002), Rosenzweig and Wolpin (1980)), and lower mortality due to higher relative income of mothers. It is not clear, however, which direction the sex ratio at birth would shift. For a given degree of son preference, a higher opportunity cost of "unwanted" children for working mothers might cause greater sex-selection and thus, result in higher sex ratios at birth. Women in the labor force may also have a lower search cost of accessing prenatal sex determination and abortion.

Lastly, Munshi and Rosenzweig (2006) find that globalization mainly benefits lower-caste girls in India. Despite increases in returns to non-traditional white-collar occupations, low caste parents continue to educate their sons in local language schools (that lead to traditional blue-collar jobs) in order to continue benefitting from caste-based networks. However, historically, low-caste girls have not participated in these caste-based occupational networks due to low labor market participation, and are, hence, not constrained by them. As a result, low caste parents continue to channel boys into traditional occupations despite higher returns in more modern jobs, but their daughters benefit as a result of these improved employment opportunities. In a similar vein, Jensen and Miller (2011) show that parents in rural India strategically try to prevent sons from migrating to urban areas to take advantage of better income opportunities because they want them to work on the farm. They find large gains in education for girls but not much for boys in response to greater employment opportunities in urban areas. They conclude that these results are driven by changes in returns (to parents) from sons and daughters. In our context, an increase in the relative demand for daughters, due to a relative increase in returns to parents from girls, would imply that lower caste parents should now be more likely to give birth to daughters, who might also experience a *decrease* in mortality. In other words, the increase in female births in this scenario is driven by more "wanted" girls, unlike the poverty channel where more "unwanted" girls are born due to reduced affordability of sex-selection or lower opportunity cost of children. If the decrease in female mortality is sufficiently large, we will also observe an overall decrease in mortality across all births. The impact on the likelihood of birth depends on the extent to which parents substitute between sons and daughters in the short and the long run.

The following table summarizes the predictions discussed above.

Channel	Birth	SRB	Mortality
↑ Poverty	+	–	+
↑ Female bargaining power	–	+/–	–
↑ Relative returns to daughters	+/–	–	–

So far, our main findings appear consistent with the increased poverty channel, although the results on SRB are weak. Armed with these predictions, we now turn to analyzing heterogeneity in our effects across three dimensions – household's caste, mother's education level, and household's wealth index – to distinguish between potential mechanisms.

5.1 Heterogeneous Effects

We begin with the ***household's caste***. We divide all women into four categories - scheduled caste (SC), scheduled tribe (ST), other backward caste (OBC) and general caste - and interact our tariff variable with indicators for these categories. Table 6 presents these results for the birth dummy, male birth dummy and mortality within twelve months. General caste is the omitted category. For discussion purposes, we focus on column (3) which includes state-specific linear time trends. Panel A shows that the interaction terms are negative and highly significant for SC and OBC mothers, implying that lower caste women experience a significantly *larger increase* in the probability of birth relative to upper caste mothers. For upper caste women, the effect on fertility is either positive (i.e. fertility *decreases* for them) or not significantly different from zero in column (3). Thus, our overall findings for fertility seem to be driven mainly by higher fertility for lower-caste mothers. There are no significant differences in our sex ratio results across caste groups, however. The fertility effects are consistent with either the poverty or the returns channel working for the low caste households. Scheduled and other backward castes have historically been more economically and socially disadvantaged in India. A relative increase in poverty levels is therefore likely to affect them more strongly than upper caste households. But, as the mortality results show, the effect on mortality due to tariff cuts is significantly *smaller* for births to lower caste mothers, relative to general castes. This is not consistent with the increased poverty channel[27] and suggests that it is the increased relative returns to female children that are causing these effects. Moreover, when we separate the effect on mortality by child's sex in Table 7, we find that the lower mortality results for SCs and OBCs are completely driven by girls, lending further support to the returns story for low caste girls. Upper caste girls, however, experience a rise in mortality.

In Table 8 we repeat the same exercise by ***mother's education level***. We

[27]Another possibility is that low caste households actually *benefit* from tariff cuts and experience a relative decrease in poverty which lowers mortality. But in that case, we would not expect to see their fertility increase, which is what we find in Panel A.

divide women into three categories - uneducated, with 1-5 years of schooling and with more than 5 years of schooling. Column (3) in Panel A shows that there is no effect on the likelihood of birth for "more educated" mothers. But, births *increased* significantly and the probability that these births are male *decreased* significantly for uneducated mothers. For mortality within a year of birth, we observe a similar pattern as our caste results. For uneducated mothers, there is a relative decrease in mortality, whereas more educated mothers experience an increase. When we split the mortality results by child's sex, we again find a pattern similar to Table 7. There are no significant effects on boys, but girls born to uneducated mothers experience a relative decrease in mortality whereas girls born to mothers with more than primary education experience a relative increase in mortality. To the extent that lower caste status and mother's education attainment might be positively correlated, these results together highlight the possibility of gains for girls born to lower status parents from tariff reductions through a relative increase in returns on the labor market.

To further explore the mechanisms, we next examine how our effects vary by the ***wealth index of the household***. DLHS combines information on ownership of durables, type of toilet facility, cooking fuel, housing, source of lighting, and drinking water to calculate a standard of living score for each household. On the basis of these scores, households are divided into three categories: low, medium and high standard of living (SLI). Ideally, we would like to know the household wealth score for each year in our sample period. But unfortunately, since we create our birth and woman panels retrospectively from a single cross-section, we only know which category a household belongs to at the time of survey. Since trade reform has been shown to affect standards of living, the wealth index variable is unlikely to be exogenous. However, if most households move within a wealth category, and not across categories (e.g. a high SLI family becoming low SLI) due to tariff reduction, this comparison is still informative. With these caveats in mind, Panel A in Table 10 shows that the increase in births and decrease in probability of male births is mainly driven by low SLI families. In fact, Medium and High SLI families experienced a significant decrease in likelihood of birth. Unlike the overall weak effects on SRB in Table 3, we now see that while medium SLI households did not experience a significant effect and high SLI households saw a slightly significant increase in SRB, relatively poorer low SLI families experienced a significant decrease in SRB i.e. the likelihood that their births are female went up. The magnitude of this effect is remarkably similar across all specifications and suggests that in districts with an average relative decline in tariff of 7 percentage points, SRB decreased by 1.6 percentage points in low SLI households. Yet again, the mortality results confirm that the effect for low SLI women is coming from the returns channel. Mortality *decreases* for births to low SLI women, but increases for medium and high SLI families.

5.2 Discussion

To sum up, we see substantial differences in how trade liberalization has affected women and children across social strata. Broadly, we find that low socioeconomic status women experience an increase in fertility which is driven by more female births. Since we also observe a relative decrease in mortality for their daughters, we interpret this as a higher demand for daughters by low status families. The same does not hold for high status women. There is some evidence that they have fewer children, driven by fewer girls, and we find strong evidence that girls born to high status mothers fare worse in terms of higher mortality. Boys, however, do not seem to be affected by the trade reform, irrespective of their parents' socio-economic status.

Thus, there appears to be a strong gender component to the effects of trade liberalization. If tariff reforms have improved earning opportunities for women in blue-collar occupations, as recent literature suggests, we would expect the gains to be derived by girls born to low status families. It is not entirely clear what mechanisms are driving our results for high status women and their daughters. There can be at least two different explanations. First, to the extent that upper caste and more educated women in India are less likely to participate in blue-collar occupations, we do not expect girls in high status families to benefit from the new labor market opportunities in relatively blue-collar jobs as much as low status women. Second, if returns from more-skilled jobs have increased mainly for men in India, high status families will prefer to have more sons. Unfortunately, our data does not allow us to delve deeper into these explanations.

We do not find any evidence in favor of increased relative bargaining power for women in terms of its effect on their fertility and child health outcomes. Lastly, the apparent increase in valuation of girls in low status families and decrease in high status families in response to trade reform can also be due to differential returns from children on the marriage market as suggested by Edlund and Lee (2009).

6 Conclusions

This paper analyzes whether India's trade liberalization, beginning in 1991, affected fertility, infant mortality and observed sex ratios at birth. To identify the impact of this trade policy reform, we compare rural districts more exposed to tariff cuts to rural districts less exposed to tariff reductions. Previous research using a similar empirical strategy finds that districts subject to greater reductions in tariffs experience slower declines in poverty as well as slower increases in school enrollment ((Topalova (2010), Edmonds, Pavcnik, and Topalova (2010)). We find that low caste, less educated and less wealthy women in districts with a higher relative trade reform exposure are more likely to give birth and these births are more likely to be female. Moreover, infant mortality (within one, six and twelve months of birth) decreases for these girls. In contrast, girls born to upper caste, more educated and wealthier mothers experience relatively higher mortality. They are also less likely to be born.

It is important to emphasize that these results do not suggest that trade liberalization leads to overall increases or decreases in fertility, sex ratios or infant mortality.[28] Our results do confirm, however, that trade reform has important distributional consequences along these dimensions, especially for girls. Data limitations prevent us from a rigorous analysis of the exact channels through which removal of trade barriers affects individuals' decisions about fertility and

[28] We also note that this paper only examines the effect of tariff reductions and ignores removal of non-tariff barriers.

investment in children, but we highlight the role played by differential returns from children. This remains a fruitful area for future research.

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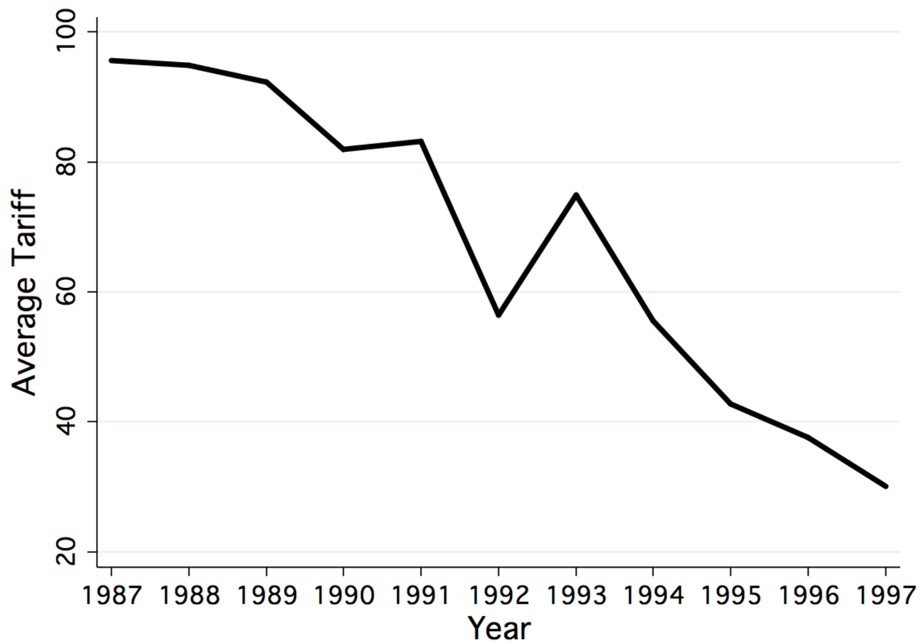
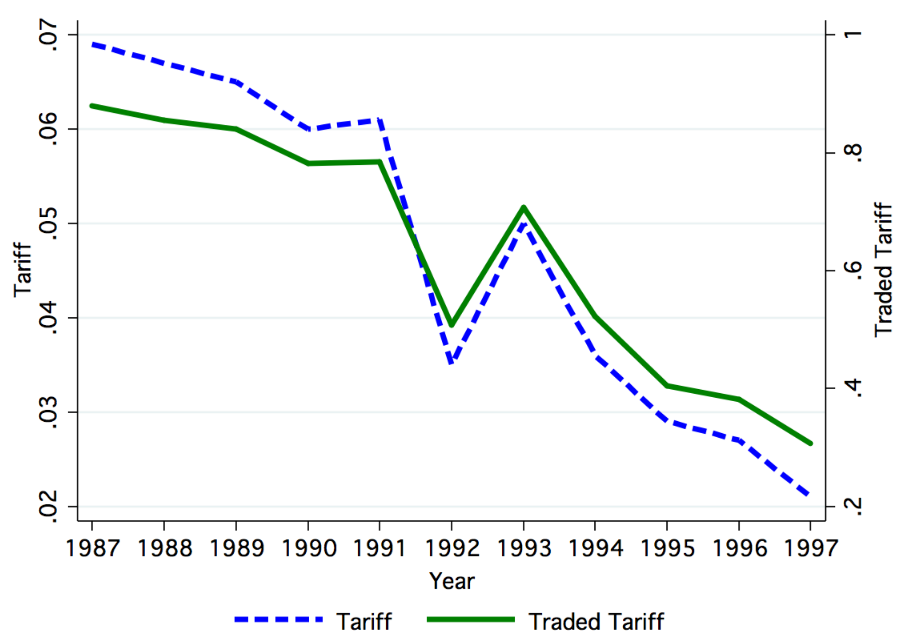
Figures

Figure 1: Average Industry-level Tariff



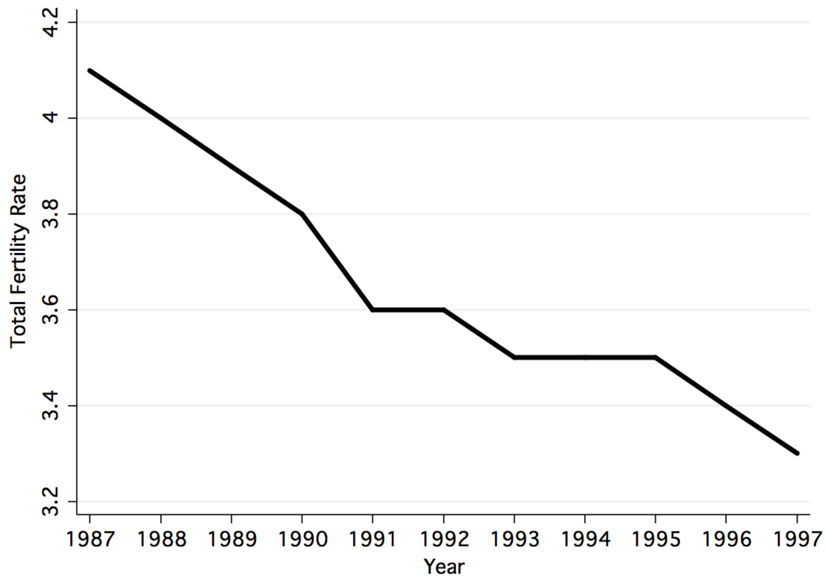
NOTE: This figure plots the yearly averages of nominal, national, industry-level ad-valorem tariffs using data provided by Petia Topalova.

Figure 2: Average District-level Tariff and Traded Tariff, by Year



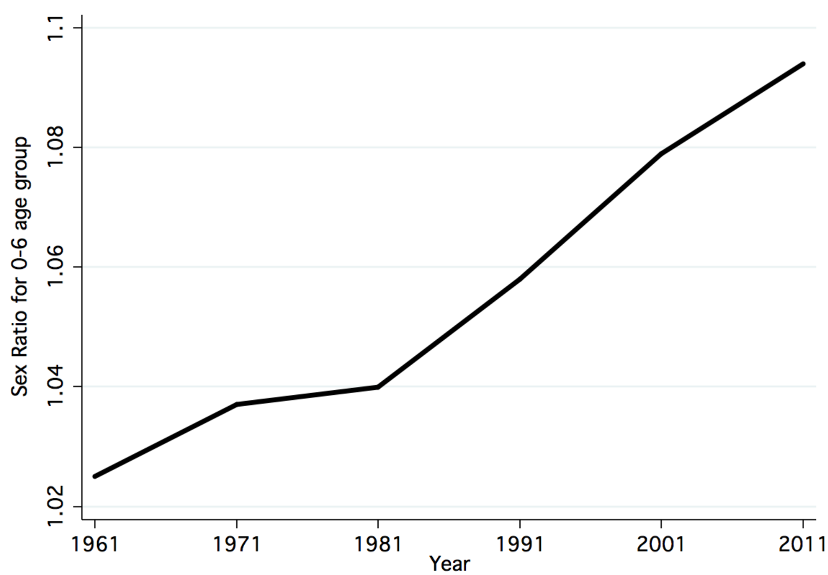
NOTE: This figure plots the yearly averages of the district-level tariff and traded tariff measures used in this paper. District-year data on both measures was provided by Petia Topalova. Tariff is constructed as the district-specific employment weighted sum of industry-specific national tariffs. Traded tariff is constructed in a similar way, but only uses employment in traded sectors within a district. District employment shares in 1991 are used as weights. More details are available in Section 3.1.

Figure 3: Total Fertility Rate in India, by year



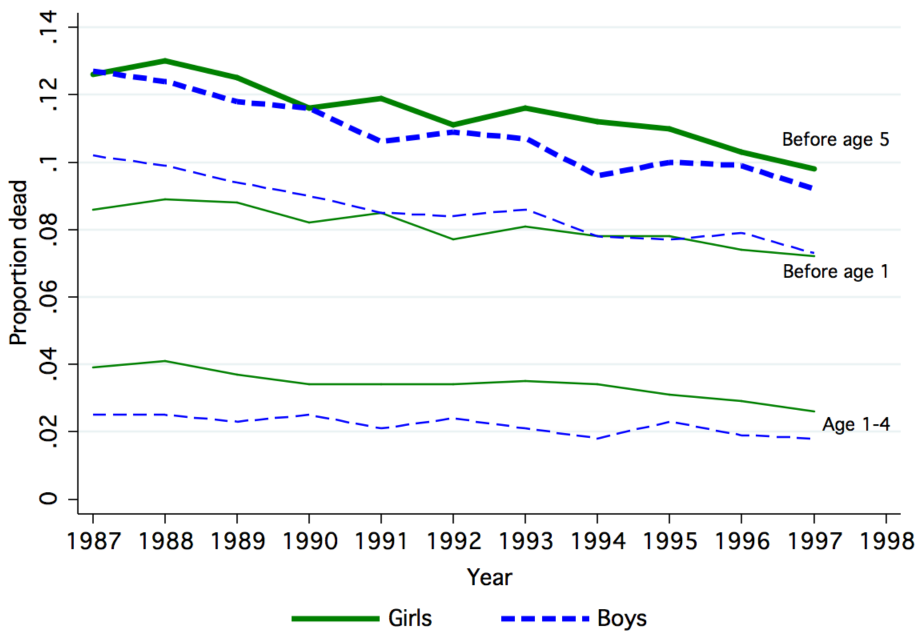
SOURCE: Ministry of Health and Family Welfare, Govt. of India (*accessed from Indiastat*)

Figure 4: Child Sex Ratio (0-6) in India, by year



SOURCE: Census of India

Figure 5: Child Mortality in Rural India, by year of birth and gender



NOTE: This figure plots the yearly average proportion of children who died before age 1, during ages 1-4 and before age 5, by year of birth and gender. All-India sample weights used. Data source is DLHS (2002-04).

Tables

Table 1: Summary Statistics for Rural Sample, for 1987 and 1997

Variable	1987	1997
Panel of Births		
Birth is male	0.517	0.521
Parity of birth	2.34	2.96
Mother's age at birth	21.04	23.28
Mother's years of schooling	1.90	2.51
Hindu	0.78	0.77
Muslim	0.09	0.10
Sikh	0.03	0.02
Christian	0.07	0.08
Scheduled Caste	0.18	0.19
Scheduled Tribe	0.19	0.22
Other Backward Caste	0.38	0.38
Died before 1 month of birth	0.06	0.05
Died before 6 months of birth	0.08	0.06
Died before 12 months of birth	0.09	0.07
Low HH Wealth Index	0.60	0.67
Medium HH Wealth Index	0.30	0.25
High HH Wealth Index	0.10	0.08
N(births)	31,356	48,755
Panel of Women		
Birth	0.22	0.18
N(women)	139,478	269,347
N(districts)	408	408

Notes: This table presents summary statistics for the earliest (1987) and the latest (1997) years included in our rural sample. Our regressions include all eleven years during 1987-1997.

Table 2: The Effect of Tariff Reduction on Probability of Birth

	(1)	(2)	(3)	(4)
A. OLS				
Tariff in YOB	0.129***	0.104***	0.009	0.581***
	[0.031]	[0.030]	[0.030]	[0.109]
B. First Stage				
Traded Tariff in YOB	0.217***	0.217***	0.203***	0.212***
	[0.031]	[0.031]	[0.026]	[0.030]
F-stat	48.16	48.19	61.55	49.86
C. IV				
Tariff in YOB	-0.081	-0.118**	-0.172***	-0.108
	[0.054]	[0.055]	[0.048]	[0.118]
D. Reduced form				
Traded Tariff in YOB	-0.018*	-0.026***	-0.035***	-0.023
	[0.010]	[0.010]	[0.008]	[0.025]
N	1,857,834	1,857,834	1,857,834	1,857,834
District FE	x	x	x	
Year FE	x	x	x	x
Covariates		x	x	x
State-specific linear time trends			x	
Mother FE				x
Clustered by District	x	x	x	x

Notes: YOB stands for year of birth. Each cell constitutes a separate regression. (2) - (4)include indicators for mother's age at birth and number of previous births. (2) and (3) also include mother's years of schooling and household's religion and caste dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 3: The Effect of Tariff Reduction on Probability that a Birth is Male

	(1)	(2)	(3)	(4)
A. OLS				
Tariff in YOC	0.078	0.080	0.074	0.035
	[0.050]	[0.050]	[0.053]	[0.101]
B. First Stage				
Traded Tariff in YOC	0.201***	0.201***	0.187***	0.182***
	[0.031]	[0.031]	[0.025]	[0.028]
F-stat	42.45	42.47	54.94	42.44
C. IV				
Tariff in YOC	0.097	0.097	0.085	0.314***
	[0.066]	[0.066]	[0.075]	[0.113]
D. Reduced form				
Traded Tariff in YOC	0.019	0.020	0.016	0.057**
	[0.014]	[0.014]	[0.015]	[0.025]
N	449,065	449,065	449,065	449,065
District FE	x	x	x	
YOC FE	x	x	x	x
Covariates		x	x	x
State-specific linear time trends			x	
Mother FE				x
Clustered by District	x	x	x	x

Notes: YOC stands for year of conception defined as the year nine months prior to the month of birth. Each cell constitutes a separate regression. (2) - (4) include indicators for mother's age at birth and number of previous births. (2) and (3) also include mother's years of schooling and household's religion and caste dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 4: The Effect of Tariff Reduction on Infant Mortality

Mortality in 1 month	(1)	(2)	(3)	(4)
A1. OLS				
Tariff in YOB	-0.035**	-0.041**	-0.010	-0.023
	[0.016]	[0.016]	[0.017]	[0.039]
B1. Reduced form				
Traded Tariff in YOB	-0.009	-0.011*	-0.002	-0.015
	[0.006]	[0.006]	[0.006]	[0.011]
C1. IV				
Tariff in YOB	-0.045	-0.052*	-0.012	-0.080*
	[0.028]	[0.029]	[0.031]	[0.047]
Mortality in 6 months				
A2. OLS				
Tariff in YOB	-0.046**	-0.054***	-0.010	-0.036
	[0.018]	[0.019]	[0.019]	[0.043]
B2. Reduced form				
Traded Tariff in YOB	-0.011*	-0.013*	-0.002	-0.020
	[0.007]	[0.007]	[0.007]	[0.012]
C2. IV				
Tariff in YOB	-0.055*	-0.064*	-0.011	-0.106**
	[0.033]	[0.033]	[0.035]	[0.054]
Mortality in 12 months				
A3. OLS				
Tariff in YOB	-0.052**	-0.063***	-0.016	-0.041
	[0.020]	[0.021]	[0.021]	[0.046]
B3. Reduced form				
Traded Tariff in YOB	-0.014*	-0.016**	-0.003	-0.022
	[0.007]	[0.008]	[0.008]	[0.013]
C3. IV				
Tariff in YOB	-0.067*	-0.076**	-0.017	-0.118**
	[0.036]	[0.037]	[0.039]	[0.058]
First Stage				
Traded Tariff in YOB	0.207***	0.207***	0.193***	0.185***
	[0.032]	[0.032]	[0.025]	[0.028]
F-stat	42.85	42.88	58.23	43.14
N	473,430	473,430	473,430	473,430
District FE	x	x	x	
Year FE	x	x	x	x
Covariates		x	x	x
State-specific linear time trends			x	
Mother FE				x
Clustered by District	x	x	x	x

Notes: YOB stands for year of birth. Each cell constitutes a separate regression. All regressions include indicators for mother's age at birth and number of previous births. (2) - (4) include indicators for mother's age at birth and number of previous births. (2) and (3) also include mother's years of schooling and household's religion and caste dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 5: IV Estimates for the Effect on Infant Mortality: By Child's Gender

Mortality w/i 12 months	(1)	(2)	(3)	(4)
Tariff in YOB * Boy	0.071	0.063	0.063	0.212***
	[0.046]	[0.046]	[0.046]	[0.066]
Tariff in YOB	-0.104**	-0.109**	-0.049	-0.226***
	[0.044]	[0.044]	[0.047]	[0.068]
N	473,430	473,430	473,430	473,430
District FE	x	x	x	
Year FE	x	x	x	x
Covariates		x	x	x
State-specific linear time trends			x	
Mother FE				x
Clustered by District	x	x	x	x

Notes: YOB stands for year of birth. Each column constitutes a separate regression. (2) - (4)include indicators for mother's age at birth and number of previous births. (2) and (3) also include mother's years of schooling and household's religion and caste dummies. Main effect of Boy is included in all specifications, but not reported. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 6: IV Estimates for the Effect of Tariff Reduction: By Caste

A. Birth=1	(1)	(2)	(3)
Tariff in YOB * SC	-0.449***	-0.362***	-0.293***
	[0.096]	[0.085]	[0.078]
Tariff in YOB * ST	-0.730***	-0.731***	-0.614***
	[0.147]	[0.142]	[0.125]
Tariff in YOB * OBC	-0.250***	-0.211***	-0.039
	[0.077]	[0.069]	[0.061]
Tariff in YOB	0.191**	0.126*	-0.019
	[0.078]	[0.072]	[0.062]
N			
B. Boy=1			
Tariff in YOC * SC	0.031	0.035	0.041
	[0.138]	[0.138]	[0.140]
Tariff in YOC * ST	0.083	0.081	0.162
	[0.154]	[0.154]	[0.162]
Tariff in YOC * OBC	-0.036	-0.034	-0.051
	[0.113]	[0.113]	[0.115]
Tariff in YOC	0.093	0.092	0.072
	[0.097]	[0.097]	[0.103]
N	449,065	449,065	449,065
C. Mortality w/i 12 months			
Tariff in YOB * SC	0.248***	0.247***	0.200**
	[0.081]	[0.080]	[0.080]
Tariff in YOB * ST	-0.127	-0.131	0.044
	[0.085]	[0.086]	[0.075]
Tariff in YOB * OBC	0.138**	0.136**	0.073
	[0.058]	[0.058]	[0.060]
Tariff in YOB	-0.136***	-0.142***	-0.085
	[0.052]	[0.052]	[0.055]
N	473,430	473,430	473,430
District FE	x	x	x
Year FE	x	x	x
Covariates		x	x
State-specific linear time trends			x
Clustered by District	x	x	x

Notes: YOB stands for year of birth. YOC stands for year of conception. General caste households are the excluded group. Main effects of SC, ST, OBC are included in all regressions but not reported. (2)-(3) include indicators for mother's age at birth and number of previous births. (2) also includes mother's years of schooling and household's religion dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 7: IV Estimates for the Effect of Tariff Reduction on Mortality: By Caste and Child's Sex

Mortality w/i 12 months	Girls	Boys
Tariff in YOB * SC	0.346***	0.072
	[0.114]	[0.103]
Tariff in YOB * ST	0.011	0.083
	[0.109]	[0.105]
Tariff in YOB * OBC	0.217***	-0.052
	[0.084]	[0.081]
Tariff in YOB	-0.171**	-0.014
	[0.078]	[0.075]
N	227,881	245,549
District FE	x	x
Year FE	x	x
Covariates	x	x
State-specific linear time trends	x	x
Clustered by District	x	x

Notes: YOB stands for year of birth. General caste households are the excluded group. Main effects of SC, ST, OBC are included in all regressions but not reported. All regressions include indicators for mother's age at birth, number of previous births, mother's years of schooling and household's religion dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 8: IV Estimates for the Effect of Tariff Reduction: By Mother's Education

A. Birth=1	(1)	(2)	(3)
Tariff in YOB * Uneducated	-0.138**	-0.396***	-0.351***
	[0.067]	[0.069]	[0.065]
Tariff in YOB * 1-5 years	0.309***	0.102	0.068
	[0.081]	[0.065]	[0.064]
Tariff in YOB	-0.080	0.060	0.034
	[0.060]	[0.053]	[0.056]
N	1,354,769	1,354,769	1,354,769
B. Boy=1			
Tariff in YOC * Uneducated	0.305**	0.292**	0.287*
	[0.143]	[0.142]	[0.151]
Tariff in YOC * 1-5 years	-0.152	-0.161	-0.138
	[0.188]	[0.187]	[0.187]
Tariff in YOC	-0.091	-0.082	-0.132
	[0.125]	[0.124]	[0.141]
N	277,601	277,601	277,601
C. Mortality w/i 12 months			
Tariff in YOB * Uneducated	0.207***	0.258***	0.181***
	[0.066]	[0.067]	[0.066]
Tariff in YOB * 1-5 years	-0.068	-0.044	-0.056
	[0.071]	[0.071]	[0.072]
Tariff in YOB	-0.173***	-0.216***	-0.147**
	[0.056]	[0.058]	[0.063]
N	290,653	290,653	290,653
District FE	x	x	x
Year FE	x	x	x
Covariates		x	x
State-specific linear time trends			x
Clustered by District	x	x	x

Notes: YOB stands for year of birth. YOC stands for year of conception. Women with more than 5 years of education are the excluded group. Sample is restricted to women above age 20 at the time of survey. Main effects of education groups are included in all regressions but not reported. (2)-(3) include indicators for mother's age at birth and number of previous births. (2) also include household's caste and religion dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 9: IV Estimates for the Effect of Tariff Reduction on Mortality: By Mother's Education and Child's Sex

Mortality w/i 12 months	Girls	Boys
Tariff in YOB * Uneducated	0.256***	0.110
	[0.087]	[0.082]
Tariff in YOB * 1-5 years	-0.070	-0.058
	[0.110]	[0.103]
Tariff in YOB	-0.169**	-0.122
	[0.085]	[0.079]
N	139491	151162
District FE	x	x
Year FE	x	x
Covariates	x	x
State-specific linear time trends	x	x
Clustered by District	x	x

Notes: YOB stands for year of birth. Women with more than 5 years of education are the excluded group. Sample is restricted to women above age 20 at the time of survey. Main effects of education groups are included in all regressions but not reported. All regressions include indicators for mother's age at birth, number of previous births, and household's caste and religion dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

Table 10: IV Estimates for the Effect of Tariff Reduction: By HH Wealth Index

A. Birth=1	(1)	(2)	(3)
Tariff in YOB*Low SLI	-0.835***	-0.768***	-0.614***
	[0.097]	[0.090]	[0.072]
Tariff in YOB*High SLI	0.066	0.014	0.012
	[0.051]	[0.046]	[0.047]
Tariff in YOB	0.366***	0.301***	0.167***
	[0.054]	[0.052]	[0.049]
N	1,857,834	1,857,834	1,857,834
B. Boy=1			
Tariff in YOC*Low SLI	0.239**	0.234**	0.222*
	[0.110]	[0.111]	[0.120]
Tariff in YOC*High SLI	-0.247	-0.245	-0.263*
	[0.150]	[0.150]	[0.151]
Tariff in YOC	-0.022	-0.018	-0.026
	[0.097]	[0.097]	[0.103]
N	449,065	449,065	449,065
C. Mortality w/i 12 months			
Tariff in YOB*Low SLI	0.201***	0.217***	0.195***
	[0.054]	[0.054]	[0.053]
Tariff in YOB*High SLI	0.078	0.046	0.047
	[0.063]	[0.063]	[0.064]
Tariff in YOB	-0.197***	-0.212***	-0.142***
	[0.045]	[0.046]	[0.046]
N	473,430	473,430	473,430
District FE	x	x	x
Year FE	x	x	x
Covariates		x	x
State-specific linear time trends			x
Clustered by District - Year	x	x	x

Notes: YOB stands for year of birth. YOC stands for year of conception. Medium SLI households are the excluded group. Main effects of High SLI and Low SLI are included in all regressions but not reported. (2)-(3) include indicators for mother's age at birth and number of previous births. (2) also includes mother's years of schooling and household's religion and caste dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

A Appendix Tables

Table A.1: IV Estimates for the Effect of Tariff Reduction in Urban India

Birth = 1	(1)	(2)	(3)	(4)
A1. First Stage				
Traded Tariff in YOB	0.317***	0.317***	0.305***	0.315***
	[0.030]	[0.030]	[0.026]	[0.029]
F-stat	108.05	108.3	134.23	118.72
B1. IV				
Tariff in YOB	-0.100**	-0.141***	-0.151***	-0.230*
	[0.049]	[0.047]	[0.051]	[0.127]
N	895,134	895,134	895,134	895,134
Boy = 1				
A2. First Stage				
Traded Tariff in YOC	0.292***	0.292***	0.285***	0.279***
	[0.028]	[0.028]	[0.023]	[0.022]
F-stat	111.27	111.56	147.64	158.02
B2. IV				
Tariff in YOC	-0.040	-0.033	0.060	-0.251
	[0.110]	[0.110]	[0.122]	[0.155]
N	186,953	186,953	186,953	186,953
C1. Mortality w/i 1 month				
Tariff in YOB	-0.012	-0.009	0.003	-0.013
	[0.034]	[0.034]	[0.036]	[0.052]
C2. Mortality w/i 6 months				
Tariff in YOB	-0.030	-0.027	-0.020	-0.025
	[0.035]	[0.035]	[0.036]	[0.055]
C3. Mortality w/i 12 months				
Tariff in YOB	-0.033	-0.030	-0.017	-0.038
	[0.038]	[0.039]	[0.040]	[0.059]
C4. First Stage				
Traded Tariff in YOB	0.300***	0.300***	0.291***	0.283***
	[0.029]	[0.029]	[0.026]	[0.023]
F-stat	105.37	105.63	130.03	149.57
N	198,400	198,400	198,400	198,400
District FE	x	x	x	
Year FE	x	x	x	x
Covariates		x	x	x
State-specific linear time trends			x	
Mother FE				x
Clustered by District	x	x	x	x

Notes: YOB stands for year of birth. YOC stands for year of conception. Each cell constitutes a separate regression. (2) - (4)include indicators for mother's age at birth and number of previous births. (2) and (3) also include mother's years of schooling and household's religion and caste dummies. All regressions use district-level sampling weights. ***Significant at 1%, **Significant at 5%, *Significant at 10%.