Women's Worth: Trade, Female Income, and Fertility in India

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I. Introduction

Macroeconomic policies have microeconomic consequences. This paper focuses on one of the most important structural changes in recent history—the removal of international trade barriers—and examines its effects on fertility, sex selection, and child mortality in rural India to highlight the distributional effects of trade policy across gender and socioeconomic strata. Specifically, we study India's 1991 trade liberalization, which, we argue, was an exogenous shock to industry-level tariffs in India. The resulting changes in tariff and nontariff barriers (NTBs) were quite large in magnitude. In the manufacturing sector, the average tariff declined from 117% to 39%, and the share of imports covered by NTBs fell from 82% to 17% between 1990–91 and 1999–2000 (Gupta and Kumar 2008).

We exploit heterogeneity in the prereform industrial composition of 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. Similar identification strategies have been used by several prior papers (e.g., Hasan, Mitra, and Ural 2006–7; Topalova 2007; Edmonds, Pavcnik, and Topalova 2010; Topalova 2010; Gaddis and Pieters 2014), and like them, our estimates do not capture the level effect of trade liberalization for India as a whole. Rather, we measure the relative effect of tariff cuts on districts that were more or less exposed to trade liberalization relative to the national trend.

We find that births to women of lower socioeconomic status (i.e., lower-caste, uneducated, and less wealthy women) in districts that were relatively

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more exposed to the tariff cuts were more likely to be female relative to the national trend. Moreover, these girls experienced a lower likelihood of death within 1, 6, and 12 months of birth. However, mortality for girls born to upper-caste, more educated, and wealthier mothers increased in relatively more exposed districts compared to the national baseline. These high-status women were also relatively more likely to give birth to boys, although the sex ratio effects are not as strong as the mortality results. In addition, fertility rose for low-status women and declined for high-status women in districts with relatively larger reductions in tariff protection.

We also show that female employment (relative to male employment) increased (decreased) for lower castes (upper castes) in districts with larger tariff declines relative to the national trend. If male and female wages were equally affected by the trade reform, the employment effects would also translate into a relative increase (decrease) in female income compared to male income among lower-caste (upper-caste) households.[1] However, larger tariff cuts led to slower increases in total household consumption expenditure, with lower-caste families suffering a larger slowdown in consumption growth than upper-caste families. Altogether, our findings are consistent with sons being investment goods irrespective of caste and daughters being consumption (investment) goods for upper-caste (lower-caste) families. Given the time costs of pregnancy and childcare, it is somewhat surprising, however, that both fertility and female employment contemporaneously increase for lower-caste women.

Our paper is related to three distinct strands of literature: (1) the effects of trade liberalization on income and employment, especially in the context of India; (2) the determinants of discrimination against female children in several developing societies; and (3) the relationship between relative female income and fertility-related outcomes.

The existing trade literature suggests that trade openness has not benefited everyone (Goldberg and Pavcnik 2007a). However, the identities of winners and losers are not precisely known. Topalova (2010) finds heterogeneous impacts of the Indian tariff cuts on poverty decline, with the poorest being the most negatively affected. Edmonds et al. (2010) show that more exposed Indian districts experienced smaller improvements in children's schooling outcomes, especially for girls. Our results paint a slightly different picture in the gender dimension. While more open trade worsened the relative labor market and demographic and health outcomes for females in high-status families compared to the national trend, the winners were also female, albeit at the other end of the social hierarchy—from lower-caste, less educated, and relatively poor house-

[1] Intercaste marriages, esp. those between upper-caste and lower-caste individuals, are rare in India.

holds. Topalova (2010) also finds that tariff cuts in India affected relative wages but not aggregate employment due to low factor mobility. We show that focusing on aggregate employment hides the differential effects of trade liberalization on employment by gender and caste. Moreover, since the tariff cuts also affected fertility, the estimated impacts on per capita household consumption expenditure in previous studies measure the net effect of changes in total household consumption expenditure and household size.

The paper most closely related to ours is by Chakraborty (2015), who analyzes the impact of trade liberalization on sex ratios in Indian districts. Our paper differs from hers in a number of ways. First, she primarily uses data from the 1999 National Family Health Survey (NFHS) of India, whereas we utilize the 2002–4 District-Level Household Survey (DLHS) of India. The NFHS is a much smaller data set than ours and, more importantly, is not representative at the district level, making it less suitable for district-level analyses. Second, while trade exposure is defined in a similar manner in both papers, Chakraborty (2015) includes only tariffs in the manufacturing sector; we include tariffs in all traded industries, including agriculture, the main sector of employment for rural India. This is a crucial difference since manufacturing workers represent a relatively small fraction of the rural Indian population. Third, unlike Chakraborty (2015), we also examine the impacts of tariff cuts on fertility, which makes our analysis more complete. This is especially important since fertility effects of trade shocks can independently alter sex ratios and gender gaps in health investments and may also be simultaneously determined. Fourth, our analysis of the underlying mechanisms, although imperfect, offers substantial improvements over the discussion in Chakraborty (2015). Finally, our empirical strategies differ significantly; for instance, we show that our results are robust to the inclusion of district-specific trends and mother fixed effects. Overall, we believe our empirical specifications and larger sample size allow us to better isolate the causal effect of trade liberalization on fertility outcomes.

This paper also adds to the literature on the determinants of discrimination against female children in several developing societies. While overall fertility and child mortality have decreased, the sex ratio at birth has sharply increased in many Asian countries, including India. A preference for sons relative to daughters is widely believed to be the underlying cause of prenatal and postnatal discrimination against girls, but rigorous causal explanations for this difference in valuation are limited.[2] Our results emphasize the economic roots of seemingly subjective discrimination.

[2] Prior research has explored a wide range of factors—e.g., religion, poverty, patrilineal kinship systems, absence of institutional old-age support, and dowry payments—that might make sons more

Our analysis of the relationship between demographic outcomes and household expenditure and women's relative income is also related to Rosenzweig and Schultz (1982) and Qian (2008). However, sex-selection technology was not widely available during the time periods examined by these two studies. Thus, one of our contributions is that, in addition to postnatal mortality, we examine the effects of total household expenditure and relative female income on prenatal discrimination against girls. Moreover, unlike in Qian (2008), where fertility is largely exogenous due to China's One Child Policy, in our setting, fertility is endogenous. Hence, our results are relatively more applicable to other developing countries that do not have restrictive fertility policies.

Finally, on the technical side, our ability to control for district-specific time trends and mother fixed effects in the regression analysis makes our identification more robust than previous literature on the effects of tariff reforms on household and individual outcomes.

Most papers in the field of international economics have examined the effects of trade liberalization on industrial outcomes such as productivity, employment, and wages (Tybout 2003; Trefler 2004; Goldberg and Pavcnik 2007a, 2007b; Hanson 2007; Harrison, McLaren, and McMillan 2011; Kovak 2013). We highlight that trade policy can also have substantial implications for less obvious outcomes such as fertility and child mortality.[3] These distributional effects can play an important role in exacerbating or combating historical socioeconomic inequalities, and hence it is crucial to take them into account while estimating the total costs and benefits of trade policies.

II. India's Trade Liberalization

We analyze the effect of trade liberalization on fertility decisions, children's health, employment, and household expenditure 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 reduction in the overall level and dispersion of import tariffs as well as the removal of NTBs such as import licensing. The period after the IMF bailout, therefore, marks a sharp break in Indian trade policy. The maximum tariff immediately fell from 400% to 150%, with later revisions bringing it down to about 45% by 1997 (Hasan et al. 2006–7). Meanwhile, the average

desirable (Boserup 1970; Bardhan 1974; Dyson and Moore 1983; Das Gupta and Shuzhuo 1999; Rahman and Rao 2004; Bhaskar and Gupta 2007; Das Gupta 2010). Carranza (2014), who explores causal explanations for this, is an exception.

[3] In a similar vein, Edmonds et al. (2010) and Kis-Katos and Sparrow (2011) examine the impact of trade policy on children's schooling and child labor in India and Indonesia, respectively.

tariff fell from 80% in 1990 to 37% in 1996, and the standard deviation of tariffs declined by 50%. NTBs also fell, with the proportion of goods subject to quantitative restrictions receding from 87% in 1987 to 45% in 1994 (Topalova 2010).

In addition to the sharp decline in trade protection, the 1991 liberalization event possesses several features that are valuable for our analysis. Since the policy reform was imposed as part of an 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 preexisting development plan (Bhagwati 1993; Goyal 1996). It is, therefore, unlikely that our results are driven by any adjustments in anticipation of these reforms.

The quick initiation of liberalization also reduces concerns about industries with greater political influence or higher productivity shaping the structure of the tariff reform in a way that would undermine our empirical strategy (described in detail in Sec. IV). Topalova (2007) finds that industry-level tariff changes are uncorrelated with several proxies of an industry's political influence (e.g., number of employees, proportion of skilled workers, and industrial concentration) prior to the Indian reform. Previous studies also find no correlation between an industry's future tariffs and its productivity before 1991 or productivity growth during 1989–97 (Topalova 2004). Finally, tariff changes through 1997 were spelled out in India's Eighth Five Year Plan (1992–97), suggesting little room for manipulation of tariffs based on political economy concerns during this time period.

Like Edmonds et al. (2010), we ignore changes in NTBs, primarily due to data availability issues. Thus, our results measure the effect of only one important dimension of the trade reform, that is, the tariff cuts. The exclusion of NTBs is potentially harmful for our empirical strategy if the trends in NTBs were in the opposite direction compared to tariffs. However, tariffs and NTBs during our sample period are positively correlated (Edmonds et al. 2010). Hence, our results are biased only to the extent that some of the effects we assign to tariff cuts may have instead been caused by the removal of NTBs.[4]

III. Data

We use data from several sources. To examine the effects on fertility, sex ratios, and child mortality, we employ the second round of the District-Level Household Survey (DLHS-2) of India. DLHS-2 surveyed 507,000 currently married women (aged 15–44) in 593 districts during 2002–4. This survey in-

[4] Edmonds et al. (2010) also argue that despite incomplete removal of NTBs by 1997, the volume of imports increased in response to tariffs cuts, suggesting that the latter were a significant and important part of the 1991 reform.

cludes a complete retrospective birth history for every woman interviewed, containing information on the month and year of each child's birth, birth order, age of the child's mother at birth, and, if the child is deceased, the age at which the child died. To measure the impacts on labor market outcomes, like Topalova (2010), we utilize the 43rd (1987–88) and the 55th (1999–2000) rounds of the Employment-Unemployment Survey conducted by the National Sample Survey (NSS) Organization. These NSS rounds are repeated cross-sections representative at the district level, with a sample size of about 120,000 households per round. Following prior literature, we focus our analysis on rural areas within districts due to concerns about simultaneous reforms and preexisting trends in urban areas (Topalova 2010).

Since we focus on district as the geographical unit of interest, for our fertility analysis, we would ideally like to know the district in which a birth takes place. However, DLHS-2 includes only 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 the survey. This implicitly assumes that mothers do not migrate to a different district after initiating childbearing. This is a reasonable assumption in our context since interdistrict migration in India is low and mostly consists of women relocating at the time of marriage. In addition, this assumption is problematic only if the measurement error induced by it varies, systematically, with our measures of district-level tariff protection. Later, in Section VII, we show that the tariff cuts have no effect on the gender composition of in-migrants from other districts.

We restrict our DLHS-2 sample to the 1987–97 period. There are two reasons for this. First, 1987 is the earliest year for which we have tariff data. Second, the tariff changes during 1992–97 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 the industry's current productivity (Topalova 2004), suggesting that these latter changes may be endogenous to an industry's performance. Hence, we focus only on years up to 1997. Additionally, we exclude births for whom the mother's age is below 15 years or above 40 years and births with parity above 10 due to the small number of observations in these subgroups. However, our results are not sensitive to the exclusion of these observations. We also exclude women who were visiting the household at the time of the survey and were hence interviewed, since there is no information on their permanent district of residence. For comparability with the DLHS data, we restrict the NSS sample to the 15–40 age group.

The district-level tariff data come directly from Topalova (2010). Figure 1 shows the evolution of nominal national industry–level ad valorem tariff. The average tariff fell from about 95% in 1987 to about 30% in 1997. The rainfall

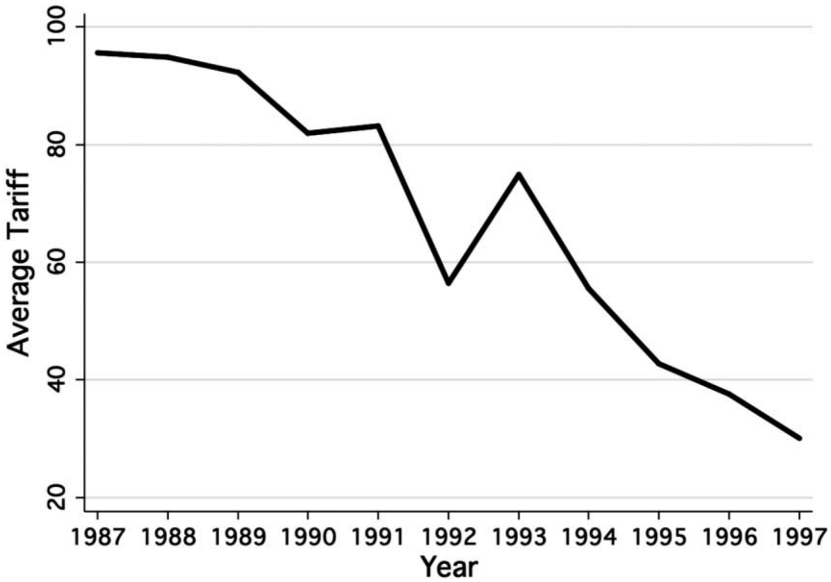

Figure 1. Yearly averages of nominal national industry–level ad valorem tariffs based on data provided by Petia Topalova.

data used later for robustness checks come from the annual district-level precipitation time series created by Ram Fishman using the Indian Meteorological Department database. After matching with the tariff data, our DLHS-2 sample comprises 464,916 births to 269,661 women in 408 districts, and the NSS sample consists of 321,154 adult males and females from 399 districts.[5]

IV. Empirical Strategy

A. Measurement of Exposure to Tariff Reduction

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 at the time of reform, some Indian districts experienced relatively larger reductions in trade protection than others. Following an extensive prior literature, our identification strategy relies on this comparison to estimate the causal effect of tariff reform.

[5] There were several changes in district boundaries during our sample period. We match our DLHS districts to the 43rd round of the NSS and then merge them with the tariff data provided to us by Petia Topalava using the same NSS district codes. This means that if a district was split into multiple districts after 1987–88, we combine them back into the original parent district to assign them the tariff variables. The only 13 districts that we exclude from our sample are the ones that were created after 1987–88 as a combination of multiple districts because we cannot link the DLHS households in these combined districts back to their parent district in 1987–88.

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, $\text{empshare}_{id}^{1991}$, to construct a measure of tariff protection for district d in year t:

$$T_{dt} = \sum_i \text{empshare}_{id}^{1991} \times \text{tariff}_{it}. \tag{1}$$

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

Although a wide range of industries was affected by the tariff cuts, certain industries, such as cereal and oilseed production, remained unaffected because only the government was allowed to be an importer of goods in these nontraded industries.[6] Consequently, T_{dt} assigns a zero tariff to the nontraded industries for the entire time period. This implies that districts with higher levels of employment in the nontraded sector in 1991 will mechanically have lower T_{dt} (Hasan et al. 2006–7). Since a large proportion of nontraded employment is in the cereal and oilseed sectors and workers in these industries tend to be poor rural farmers, this introduces a negative correlation between poverty and T_{dt}.

Previous studies have addressed this concern by constructing a second measure of district tariff protection that depends only on employment in traded industries (Hasan et al. 2006–7; Topalova 2007, 2010). We follow the literature and create this measure as follows, where emp_{id}^{1991} is the employment in a traded industry i in district d in 1991:

$$\hat{T}_{dt} = \frac{\sum_{i \in \text{traded}} \text{emp}_{id}^{1991} \times \text{tariff}_{it}}{\sum_{i \in \text{traded}} \text{emp}_{id}^{1991}}. \tag{2}$$

The only difference between the two measures in (1) and (2) is that the latter excludes employment in nontraded industries while constructing weights for industry-level tariffs. Therefore, $\hat{T}_{dt}$ is independent of the proportion of workers in the nontraded sector and is uncorrelated with initial poverty levels within a district.

As in other papers in the trade literature, our tariff measures emphasize the effect of trade openness through the employment channel and ignore alternate pathways, for example, the availability of cheaper imported goods to consumers and cheaper inputs to firms.

[6] Other nontraded industries during 1987–97 were services, trade, transportation, and construction.

B. Regression Framework

The research question we seek to answer in this paper is how the removal of tariff barriers affected fertility decisions and children's health outcomes in India. In particular, we investigate whether reductions in tariff protection faced by a woman (based on her district of residence) impacted the probability that she gives birth in a year, the sex ratio of these births, and their mortality rates. Our regression framework essentially relies on comparing women (and births) in districts that were more or less exposed to the tariff cuts.

We start by reshaping the retrospective birth histories from DLHS-2 to create a woman-year panel data set and construct a dummy variable, birth_{mdt}, that equals one if a woman m in district d gave birth in year t and is otherwise zero. Then we estimate the following base specification using ordinary least squares (OLS):

$$\text{birth}_{mdt} = \alpha + \beta T_{dt} + \phi X_{mt} + \gamma_d + \tau_t + \delta_d t + \epsilon_{mdt}. \tag{3}$$

The main regressor of interest, T_{dt}, represents the level of tariff protection assigned to a woman based on her district of residence. Although the variation in T_{dt} occurs at the district level, we also control for a vector of individual covariates, X_{mt}, that may impact the outcome variables, including indicators for a woman's age in year t, for the number of previous births, and for the household's caste and religion.[7] The district fixed effects, γ_d, control for time-invariant differences across districts, while year fixed effects, τ_t, control for any India-wide yearly 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 mortality, since any economy-wide impact on consumer prices or productivity will be captured by the year effects. Since our DLHS-2 sample spans all years between 1987 and 1997, we also include district-specific linear time trends ($\delta_d t$) in our regressions. The coefficient of interest, β, thus, captures the effect of tariff changes on the likelihood of birth in a year relative to the national trend.

The sex ratio and mortality specifications are similar to (3), except that we restrict the retrospective panel to years in which a birth takes place. Specifically, we estimate

[7] The caste categories in DLHS-2 are scheduled caste (SC), scheduled tribe (ST), other backward class (OBC), and general caste. SCs, STs, and OBCs have been recognized as socioeconomically disadvantaged by the Indian Constitution and are beneficiaries of caste-based affirmative action. According to the 2005–6 NSS, the SC, ST, and OBC population shares are, respectively, 20%, 9.2%, and 40.2%. The religion categories are Hindu, Muslim, Sikh, Christian, and others.

$$y_{jmdt} = \alpha + \beta T_{dt} + \phi X_{jmt} + \gamma_d + \tau_t + \delta_d t + \epsilon_{jmdt}, \tag{4}$$

where j indexes a child born to mother m in district d and year t. For mortality regressions, the outcome is an indicator variable for whether a child dies before Q months of birth, where Q equals 1, 6, or 12 months. For the sex ratio regressions, the outcome indicates a male birth, and t refers to the year of conception instead of the year of birth. Since an ultrasound test, 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 because these technologies are most effective and safest during the first or second trimesters of pregnancy (Epner, Jonas, and Seckinger 1998), district-level tariff protection during the year of conception is more relevant for examining the effect of trade reform on sex ratios at birth. We define the year of conception as the year 9 months prior to the month of birth, thereby implicitly assuming that no birth is premature. The remaining controls in (4) 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 the sample time period, we also run specifications with mother fixed effects:

$$\text{birth}_{mdt} = \alpha + \beta T_{dt} + \phi X_{mt} + \tau_t + \rho_m + \delta_d t + \epsilon_{mdt}, \tag{5}$$

$$y_{jmdt} = \alpha + \beta T_{dt} + \phi X_{jmt} + \tau_t + \rho_m + \delta_d t + \epsilon_{jmdt}, \tag{6}$$

where ρ_m represents the mother fixed effect and controls for all unobserved time-invariant heterogeneity across women that could influence fertility decisions. Now X_{mt} and X_{jmt} 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 in her district, relative to the national trend.

The coefficient β 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, and infant mortality. Since we interact a district's prereform industrial composition with national changes in industry tariffs to construct T_{dt}, any source of bias would have to be correlated with both prereform industrial composition and national tariff changes by industry. Like Edmonds et al. (2010), Topalova (2010), and others, we assume that this is not the case. Nevertheless, we check that our results are robust to the inclusion of other observable district-specific, time-varying variables, such as rainfall shocks and year effects that vary by a district's prereform conditions, such as the share of workers employed in agriculture, mining, manufacturing, trade, transport, and services; the share of scheduled caste (SC) and scheduled tribe (ST) populations; and the share of literate population in a district.[8]

Note that child age, especially the month of birth, is likely to suffer from measurement error due to imperfect recall by the mother. Moreover, such misreporting of the month of birth is potentially more severe for mothers with lower socioeconomic status and with relatively older children. For instance, in the DLHS-2 data, the "child age in months" variable displays peaks or stacking at 12-month intervals. Although these peaks are not too big, they suggest that our mortality and year of conception variables—that utilize information on the month of birth—are likely to suffer from nonclassical measurement error. Our fertility estimates are, however, free from this concern.

To examine the channels underlying our fertility and mortality results, we also estimate the impact of tariff cuts on adult labor market outcomes using NSS data since DLHS-2 does not contain any information on work histories of the surveyed women. However, NSS provides only cross-sectional information on labor market outcomes. Since our tariff data are limited to the 1987–99 time period, we can match only two rounds of the NSS's Employment-Unemployment Survey to them. This implies that, like those of Edmonds et al. (2010) and Topalova (2010), our specifications for the labor market outcomes are based on only 2 years of data. We estimate the following OLS specification for individual i in district d and year t:

$$y_{idt} = \alpha + \beta T_{dt} + \phi X_i + \gamma_d + \theta \mathrm{Post}_t + \delta I_{d,1987} \times \mathrm{Post}_t + \epsilon_{idt}. \qquad (7)$$

Here Post_t is equal to one for individuals surveyed in the 55th round and zero otherwise, and it controls for any India-wide shocks that may have influenced the outcome variables. The vector of covariates X_i comprises a third-order polynomial in i's age and indicators for i's sex, marital status, literacy, caste, and religion.[9] To allow for time-varying effects of prereform conditions, we interact Post_t with a vector of district characteristics in 1987, $I_{d,1987}$, comprising the share of workers employed in agriculture, mining, manufacturing, trade, transport, and services; the share of SC and ST populations; the share of the literate population; and indicators for prolabor state laws.

[8] Construction is omitted from the industry category. The industry categories used to define our tariff variables are finer than the ones included in the vector of initial district conditions.

[9] The caste categories in the NSS data are SC, ST, and other. Note that the NSS rounds used in our analysis do not distinguish between OBCs and the remaining upper castes.

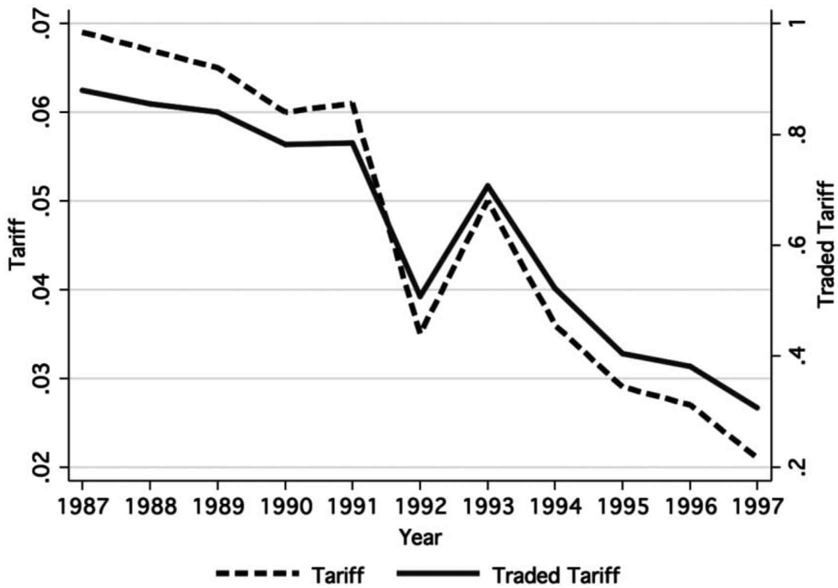

Figure 2. Yearly averages of the district-level tariff and traded tariff measures used in this paper, with district-year data on both measures provided by Petia Topalova. "Tariff" is constructed as the district-specific employment-weighted sum of industry-specific national tariffs. "Traded tariff" is constructed in a similar way but uses only employment in traded sectors within a district. District-level employment shares in 1991 are used as weights. More details are available in Sec. IV.A.

As mentioned earlier, T_{dt} is correlated with the prereform size of a district's nontraded sector. If the initial size of the nontraded sector is correlated with variables that may affect our outcomes, then the OLS estimates will be biased. In addition to controlling for district-specific time trends (in specifications using DLHS-2 data) and year effects that vary by a district's initial conditions, we resolve this issue by using traded tariff $\hat{T}_{dt}$ as an instrument for T_{dt}. Figure 2 plots both these tariff measures for our sample time period. Since nontraded industries are automatically assigned a zero tariff for all years, T_{dt} is, by construction, substantially lower than the average traded tariff measure, $\hat{T}_{dt}$. While $\hat{T}_{dt}$ declined from about 88% to 31%, T_{dt} decreased from about 7% to 2% during 1987–97. There is a significant correlation between the two measures, and they both exhibit a sharp downward trend.[10] Moreover, $\hat{T}_{dt}$ is independent of the initial size of the nontraded sector, further validating its use as an instrument for T_{dt}.

[10] The first-stage regression estimates will be presented later. The only exception to the sharp downward trend is an increase in tariffs from 1992 to 1993. To assuage any measurement error concerns, we test whether our results are robust to the exclusion of data from 1993, and they are. These results are available on request.

The coefficient of interest, β, thus uses within-district variation in tariff exposure to capture how the outcome variables would have changed if the trade reform altered only national tariffs and everything else remained the same. A positive (negative) β implies that tariff decline is associated with a decrease (increase) in the outcome of interest. To examine heterogeneity in these effects, we interact T_{dt} with an individual's caste, educational attainment, household wealth status, and sex.

In all specifications, we cluster standard errors by district and use appropriate sampling weights. Specification (7) is identical to the one used in table 4 of Edmonds et al. (2010), except that they use state-year clustering. To the extent that we can control for district-specific linear time trends and mother fixed effects, specifications (3)–(6) are more robust than those used in prior literature on the effects of Indian trade liberalization.

C. *Descriptive Statistics*

Figures 3 and 4 plot the time trends in fertility, the sex ratio of children aged 0–6, and infant and child mortality in India during the past few decades. Total fertility rate declined from 4.1 in 1987 to 3.3 in 1997. The male-female child-sex ratio has been rapidly increasing, especially since the 1980s. Increased availability of technology for sex selection combined with declining fertility and a strong preference for sons are widely believed to be the underlying causes for this growing sex imbalance in the child population. Under-5 mortality in

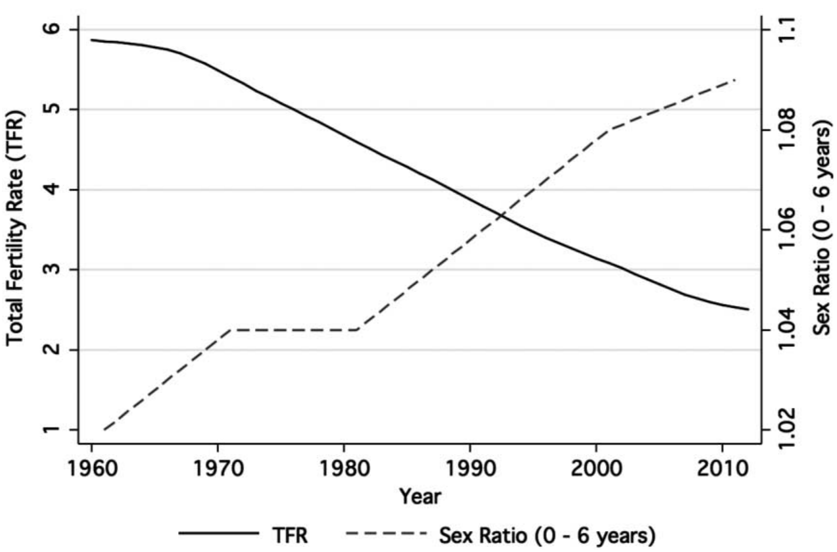

Figure 3. Total fertility rate and child (0–6 years) sex ratio in India, by year. The annual total fertility rate data are from the World Bank Indicators, and the child sex ratio data are from the decennial Census of India.

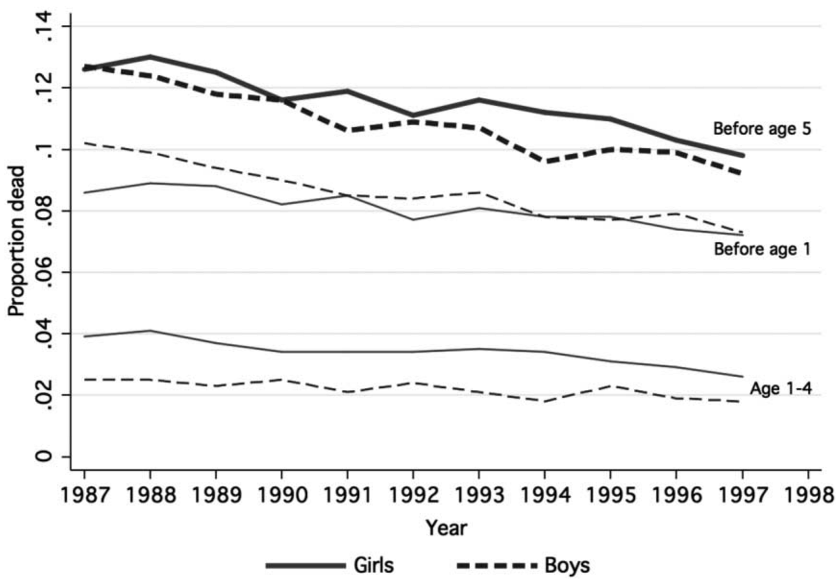

Figure 4. Average proportion of children who died in rural India before age 1, between the ages of 1 and 4, and before age 5, by year of birth and sex. All-India sample weights were used, and data were sourced from the District-Level Household Survey of India (2002–4).

rural India fell from 127 deaths per 1,000 live births in 1987 to 95 deaths per 1,000 live births in 1997. Infant mortality has also been declining over time. Mortality before age 1 is much higher than mortality during ages 1–4, and mortality for girls is greater during ages 1–4. Our identification strategy, however, does not estimate the causal impact of the tariff reductions in explaining these aggregate trends. Instead, we estimate the effect of the tariff cuts on deviations from the trend.

Tables 1 and 2 provide summary statistics for our DLHS-2 and NSS samples.

V. Results

A. Fertility

We begin by examining the effect of changes in district-level tariff exposure in a year on the probability that a woman gives birth in that year.[11] Column 1 in table 3 presents the baseline results controlling for district and year fixed effects. In column 2, we also control for mother's years of schooling, for indicators for mother's age in that year, for her number of previous births, and for the household's caste and religion. In column 3, we add district-specific

[11] Throughout this paper, we use the term "fertility" to indicate probability of birth in a given year. A higher probability of birth does not necessarily imply higher completed fertility. It is possible that our results capture changes in the timing of births rather than changes in overall fertility levels.

TABLE 1

SUMMARY STATISTICS FOR THE SAMPLE FROM THE SECOND ROUND OF THE RURAL DISTRICT-LEVEL HOUSEHOLD SURVEY OF INDIA (DLHS-2), 1987 AND 1997

Variable	1987	1997
Panel of births:		
Birth is male	.52	.52
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	.78	.77
Muslim	.09	.10
Sikh	.03	.02
Christian	.07	.08
Scheduled caste	.18	.19
Scheduled tribe	.19	.22
Other backward caste	.38	.38
Died within 1 month of birth	.06	.05
Died within 6 months of birth	.08	.06
Died within 12 months of birth	.09	.07
Low household wealth index	.60	.67
Medium household wealth index	.30	.25
High household wealth index	.10	.08
N (births)	31,356	48,755
Panel of women:		
Birth	.22	.18
N (women)	139,478	269,347
N (districts)	408	408

Note. This table presents summary statistics for the earliest (1987) and the latest (1997) years included in our rural DLHS-2 sample. All regressions include every year during 1987–97.

TABLE 2

SUMMARY STATISTICS FOR THE RURAL NATIONAL SAMPLE SURVEY SAMPLE

	43rd Round (1987–88)		55th Round (1999–2000)	
Variable	*N*	Mean	*N*	Mean
Male	173,636	.50	147,518	.50
Literate	173,462	.52	147,371	.63
Married	173,636	.69	147,448	.66
Age	173,636	26.17	147,518	26.61
Scheduled caste	173,636	.16	147,518	.18
Scheduled tribe	173,636	.14	147,518	.13
Hindu	173,636	.81	147,518	.81
Muslim	173,636	.09	147,518	.10
Christian	173,636	.06	147,518	.04
Sikh	173,636	.03	147,518	.03
log(MCE)	69,981	6.65	60,555	7.69
Men:				
In labor force	87,378	.84	74,242	.83
Employed	73,305	.95	61,545	.95
Wage worker	68,410	.40	57,698	.43
Manufacturing worker	69,435	.07	58,639	.07
Women:				
In labor force	86,258	.35	73,276	.38
Employed	29,828	.95	28,032	.96
Wage worker	27,502	.39	21,313	.44
Manufacturing worker	28,371	.07	26,922	.08

Note. MCE = nominal monthly household consumption expenditure in rupees.

TABLE 3
EFFECT OF TARIFF CUTS ON THE PROBABILITY OF BIRTH

	(1)	(2)	(3)	(4)	(5)
	A. Ordinary Least Squares				
Tariff in YOB	.129***	.104***	.069**	.581***	.478***
	(.031)	(.030)	(.030)	(.109)	(.104)
	B. First Stage				
Traded tariff in YOB	.217***	.217***	.212***	.212***	.208***
	(.031)	(.031)	(.030)	(.030)	(.029)
F-statistic	48.16	48.19	49.14	49.86	50.92
	C. Instrumental Variables				
Tariff in YOB	−.081	−.118**	−.163***	−.108	−.244*
	(.054)	(.055)	(.061)	(.118)	(.130)
	D. Reduced Form				
Traded tariff in YOB	−.018*	−.026***	−.035***	−.023	−.051**
	(.010)	(.010)	(.010)	(.025)	(.025)
N	1,857,834	1,857,834	1,857,834	1,857,834	1,857,834
District fixed effects	x	x	x		
Year fixed effects	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother fixed effects				x	x

Note. Each cell constitutes a separate regression. Cols. 2–5 include indicators for mother's age at birth and number of previous births. Cols. 2 and 3 also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. YOB = year of birth.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

linear time trends. Finally, columns 4 and 5 also control for mother fixed effects without and with district-specific linear time trends, respectively. In all specifications, robust standard errors are clustered at the district level and district-level sampling weights are used.[12]

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 across all columns. These positive coefficients suggest that women in districts more exposed to trade liberalization (i.e., a relative decline in our tariff measure) witnessed a decrease in fertility (relative to national trends). Although this result holds even when district-specific trends are controlled for, for reasons previously described, 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

[12] The unweighted regressions yield very similar results, which are available on request.

over our time period for reasons unrelated to trade liberalization, and if this difference is inadequately captured by the linear district-specific trends, then OLS will yield a positively biased estimate of the causal effect of tariff protection on fertility. We therefore instrument for our tariff protection measure using traded tariff protection, which is, as previously argued, uncorrelated with the size of the nontraded sector. Panel B of table 3 shows the coefficients from the first-stage regression of a district's tariff measure on a district's traded tariff measure. 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. The first-stage F-statistic is large (>40) in all specifications.

Our instrumental variable (IV) regressions (table 3, panels C and D) imply that reductions in district tariff protection in a year significantly increase the probability that a woman gives birth in that year. The fact that our coefficient of interest changes sign when we use an instrument for district tariff suggests that the inclusion of nontraded industries in the tariff measure introduces a significant upward bias, likely 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 significant (except in column 4).

The IV coefficients indicate that the Indian trade reform had a substantial effect on fertility—all else equal, a woman living in a district that experienced the average decline in tariff protection of 7 percentage points was between 0.8 percentage points (panel C, column 2) more likely to give birth in a given year relative to the national baseline.[13] As fertility declined in India over this time period, this finding implies that districts experiencing tariff cuts experienced a smaller decline in fertility relative to what their fertility decline would have been in the absence of tariff cuts.

B. Sex Ratio at Birth

Having established that a reduction in a district's relative tariff exposure leads to an increase in the relative probability of birth in rural India, we turn our attention to the sex composition of these births. The sex ratio at birth deviates from its natural level if female fetuses are terminated more frequently than male fetuses due to less prenatal care or sex-selective abortions. Prenatal sex determination is illegal in India but widely prevalent, leading to a large number of female fetuses being aborted. Bhalotra and Cochrane (2010) estimate

[13] Table A2 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. The coefficients are negative and significant across all five specifications.

that approximately 480,000 sex-selective abortions took place in India annually during 1995–2005.

Sex-determination technology became available in India in the early 1980s (Sudha and Rajan 1999) and gradually spread from urban to rural areas. Bhalotra and Cochrane (2010) identify the 1985–94 time period as the "early diffusion" period and post-1995 years as the "late diffusion" period in terms of the spread of this technology. Our sample years (1987–97) overlap both these periods, although the bulk of the sample falls in the early diffusion period. Bhalotra and Cochrane (2010) also find that, in general, sex-selective abortions are more likely to take place among richer, more educated, upper-caste, and urban households.

Prenatal sex determination can be effectively performed through an ultrasound test around 13 weeks of gestation or through amniocentesis during 16–18 weeks of gestation. If a mother has an induced abortion after sex determination, it is likely to take place during the 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 in all sex ratio regressions.

Using the retrospective panel of births, table 4 presents the results from OLS and IV regressions of an indicator for male birth on district-level tariff during the year of conception. Each cell indicates a separate regression. All regressions use district-level sampling weights, and robust standard errors are clustered at the district level. 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 relatively less likely to be a boy; but the effect is not significant at conventional levels. Panels B, C, and D present the IV regression estimates. As before, 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 fixed effects in columns 4 and 5.[14] For a district with the average decline in tariffs of 7 percentage points, column 4 in panel C suggests that the likelihood of a male birth decreased by 2 percentage points, relative to the national baseline. Thus, the reduction in trade protection seems

[14] However, the coefficients in cols. 1–3 are significant when we use alternate levels of clustering, e.g., district year. To err on the side of caution, we report results with more conservative standard errors, which in our case are obtained from clustering at the district level.

TABLE 4

EFFECT OF TARIFF CUTS ON PROBABILITY THAT A BIRTH IS MALE

	(1)	(2)	(3)	(4)	(5)
	A. Ordinary Least Squares				
Tariff in YOC	.078	.080	.084	.035	.055
	(.050)	(.050)	(.053)	(.101)	(.106)
	B. First Stage				
Traded tariff in YOC	.201***	.201***	.196***	.182***	.178***
	(.031)	(.031)	(.030)	(.028)	(.027)
F-statistic	42.45	42.47	43.02	42.44	42.34
	C. Instrumental Variables				
Tariff in YOC	.097	.097	.107	.314***	.356***
	(.066)	(.066)	(.070)	(.113)	(.127)
	D. Reduced Form				
Traded tariff in YOC	.019	.020	.021	.057**	.063**
	(.014)	(.014)	(.015)	(.025)	(.028)
N	449,065	449,065	449,065	449,065	449,065
District fixed effects	x	x	x		
Year fixed effects	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother fixed effects				x	x

Note. Each cell constitutes a separate regression. Cols. 2–5 include indicators for mother's age at birth and number of previous births. Cols. 2 and 3 also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. YOC = year of conception, defined as the year 9 months prior to the month of birth.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

to have caused some relative improvements in the probability of a female birth in rural Indian districts more exposed to tariff declines.[15]

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 prenatal 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 socioeconomic characteristics that are likely to be correlated with son preference, for example, religion, it is possible that they do not fully capture the unobserved heterogeneity

[15] Table A2 in the appendix shows that the effects on the sex ratio at birth in urban areas are insignificant even for the mother fixed effects specification. Moreover, the coefficients are of the opposite sign compared to the rural results.

across women. In Section V.D, we present evidence for heterogeneity in the effects on the sex ratio at birth across socioeconomic groups.

C. Infant Mortality

Next we examine the effect of the tariff cuts on infant mortality. Following the same format as before, table 5 presents results from OLS (rows A1–A3) and IV regressions (rows B1–B3 and C1–C3) of indicators for whether a child dies within 1, 6, or 12 months of birth on district-level tariff protection. Across all specifications, the coefficient estimates are negative, indicating that a larger decline in tariff protection within a district, relative to the national trend, is associated with a relative increase in mortality within 1, 6, and 12 months of birth. However, our OLS and IV estimates lose significance at conventional levels when we add district-specific linear time trends to the regressions (except for the OLS specification for mortality within 12 months).

The magnitude of our coefficient estimates increases as we change our outcome variable from mortality within 1 month of birth to mortality within 6–12 months of birth. The fact that we find significant effects on mortality within the first month of birth for some specifications suggests that trade liberalization also influences parents' ability or willingness to invest in the health of a child while in utero.[16] However, the increase in the magnitude of the coefficients as we examine mortality within 6 and 12 months implies that trade liberalization prevents families from making the necessary investments in a child's health to prevent infant death even after birth.[17]

Moreover, the estimated effects are economically significant. For example, the coefficient estimate of -0.118 in column 4, row C3, of table 5 implies that, relative to the national baseline, a district that experienced the average decline in tariff protection of 7 percentage points witnessed a relative increase in mortality within 12 months of birth of 0.8 percentage points—about a 9% increase with respect to the baseline mortality within a year of birth in all districts (9%). However, as we show in the next subsection, there is substantial heterogeneity in the effects on infant mortality across socioeconomic groups.

D. Heterogeneous Effects

We begin our examination of heterogeneity with a household's caste. We divide our sample into four categories—SC, ST, other backward class (OBC),

[16] 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, like the sex ratio specifications, we also run mortality regressions using the tariff in the year 9 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 5.

[17] We find no significant effect on any mortality outcome for urban areas in table A2 in the appendix.

TABLE 5

EFFECT OF TARIFF CUTS ON INFANT MORTALITY

	(1)	(2)	(3)	(4)	(5)
			Mortality in 1 Month		
A1. OLS:					
Tariff in YOB	−.035**	−.041**	−.025	−.023	−.013
	(.016)	(.016)	(.017)	(.039)	(.041)
B1. Reduced form:					
Traded tariff in YOB	−.009	−.011*	−.007	−.015	−.010
	(.006)	(.006)	(.006)	(.011)	(.011)
C1. IV:					
Tariff in YOB	−.045	−.052*	−.036	−.080*	−.058
	(.028)	(.029)	(.030)	(.047)	(.047)
			Mortality in 6 Months		
A2. OLS:					
Tariff in YOB	−.046**	−.054***	−.031	−.036	−.018
	(.018)	(.019)	(.019)	(.043)	(.044)
B2. Reduced form:					
Traded tariff in YOB	−.011*	−.013*	−.008	−.020	−.014
	(.007)	(.007)	(.007)	(.012)	(.013)
C2. IV:					
Tariff in YOB	−.055*	−.064*	−.040	−.106**	−.080
	(.033)	(.033)	(.035)	(.054)	(.055)
			Mortality in 12 Months		
A3. OLS:					
Tariff in YOB	−.052**	−.063***	−.038*	−.041	−.019
	(.020)	(.021)	(.022)	(.046)	(.046)
B3. Reduced form:					
Traded tariff in YOB	−.014*	−.016**	−.010	−.022	−.016
	(.007)	(.008)	(.008)	(.013)	(.013)
C3. IV:					
Tariff in YOB	−.067*	−.076**	−.051	−.118**	−.091
	(.036)	(.037)	(.038)	(.058)	(.058)
First stage:					
Traded tariff in YOB	.207***	.207***	.201***	.185***	.180***
	(.032)	(.032)	(.030)	(.028)	(.027)
F-statistic	42.85	42.88	43.88	43.14	43.44
N	473,430	473,430	473,430	473,430	473,430
District fixed effects	x	x	x		
Year fixed effects	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother fixed effects				x	x

Note. Each cell constitutes a separate regression. All regressions include indicators for mother's age at birth and number of previous births. Cols. 2–5 include indicators for mother's age at birth and number of previous births. Cols. 2 and 3 also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. OLS = ordinary least squares; YOB = year of birth; IV = instrumental variable.

* Confidence: 90%.

** Confidence: 95%.

*** Confidence: 99%.

and general caste—and interact the tariff measure with indicators for these categories. Table 6 presents the IV estimates for the birth dummy, the male birth indicator, and mortality within 12 months. General or upper caste is the omitted category. As the discussion of heterogeneous effects involves even more relative comparisons, in the interest of clarity, in this section we do not always explicitly state that all coefficients indicate changes relative to the national trend, but it is always implicitly implied.

Panel A of table 6 shows that our overall findings for fertility in table 3 are driven by lower-caste mothers; upper-caste mothers experience either zero change or a relative decline in fertility when exposed to larger tariff cuts (relative to the national baseline). The coefficient of the main effect of tariff in the year of birth is positive (insignificant in col. 3), while the interaction terms for

TABLE 6

INSTRUMENTAL VARIABLE ESTIMATES OF FERTILITY AND SEX RATIO AT BIRTH: BY CASTE

	(1)	(2)	(3)
		A. Birth = 1	
Tariff in YOB × SC	−.449***	−.362***	−.326***
	(.096)	(.085)	(.082)
Tariff in YOB × ST	−.730***	−.731***	−.793***
	(.147)	(.142)	(.151)
Tariff in YOB × OBC	−.250***	−.211***	−.145**
	(.077)	(.069)	(.063)
Tariff in YOB	.191**	.126*	.059
	(.078)	(.072)	(.075)
N	1,857,834	1,857,834	1,857,834
		B. Boy = 1	
Tariff in YOC × SC	.031	.035	.010
	(.138)	(.138)	(.138)
Tariff in YOC × ST	.083	.081	.123
	(.154)	(.154)	(.158)
Tariff in YOC × OBC	−.036	−.034	−.061
	(.113)	(.113)	(.112)
Tariff in YOC	.093	.092	.110
	(.097)	(.097)	(.101)
N	449,065	449,065	449,065
District fixed effects	x	x	x
Year fixed effects	x	x	x
Covariates		x	x
District-specific linear trends			x

Note. General caste households are the excluded group. The main effects of scheduled caste (SC), scheduled tribe (ST), and other backward caste (OBC) are included in all regressions but not reported. Cols. 2 and 3 include indicators for mother's age at birth and number of previous births. Col. 2 also includes mother's years of schooling and household's religion dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. YOB = year of birth; YOC = year of conception.

* Confidence: 90%.

** Confidence: 95%.

*** Confidence: 99%.

SC, ST, and OBC mothers are, respectively, negative, highly significant, and larger in magnitude than the main effect. There are no significant differences in our sex ratio results across caste groups, however. We distinguish between the heterogeneous effects on mortality by a child's sex, in addition to caste, in panel A of table 7. Irrespective of caste, there are no significant effects of the tariff cuts on the mortality rates of boys. On the other hand, SC and OBC

TABLE 7
INSTRUMENTAL VARIABLE ESTIMATES FOR MORTALITY: BY CHILD'S SEX

	Girls	Boys
	A. By Caste	
Tariff in YOB × SC	.334***	.097
	(.110)	(.100)
Tariff in YOB × ST	−.059	−.074
	(.111)	(.103)
Tariff in YOB × OBC	.216***	−.028
	(.078)	(.078)
Tariff in YOB	−.167**	−.050
	(.069)	(.072)
N	227,881	245,549
	B. By Mother's Education	
Tariff in YOB × uneducated	.292***	.140*
	(.084)	(.078)
Tariff in YOB × 1–5 years	−.063	−.050
	(.109)	(.102)
Tariff in YOB	−.177**	−.164**
	(.076)	(.075)
N	139,491	151,162
	C. By Household Wealth Index	
Tariff in YOB × low SLI	.178***	.174**
	(.068)	(.071)
Tariff in YOB × high SLI	−.090	.023
	(.084)	(.082)
Tariff in YOB	−.160**	−.173***
	(.063)	(.062)
N	227,881	245,549
District fixed effects	x	x
Year fixed effects	x	x
Covariates	x	x
District-specific linear trends	x	x

Note. Dependant variable is mortality in 12 months. The excluded groups are general caste households in panel A; women with more than 5 years of education in panel B; and households with a medium standard of living (SLI) in panel C. The main effects of the relevant socioeconomic status characteristics 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 religion dummies. Additional controls are caste dummies in panels B and C and mother's years of schooling in panels A and C. The sample is restricted to women above age 20 at the time of survey in panel B. Robust standard errors are in parentheses and have been clustered at the district level. YOB = year of birth; SC = scheduled caste; ST = scheduled tribe; OBC = other backward caste.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

girls experience a relative decline in mortality, both relative to the national baseline and relative to upper-caste girls, who experience a rise in mortality relative to the national baseline.

In table 8, we repeat the same exercise by the mother's education level. We divide women into three categories: uneducated women, those with 1–5 years of schooling, and women with more than 5 years of schooling. There is no effect on the relative likelihood of birth or the sex ratio of birth for mothers with more than primary education. However, fertility increased significantly, and the probability that these births are male decreased significantly for uneducated mothers relative to the national baseline. For mothers with 1–5 years of education, the results are the opposite; that is, fertility decreased significantly, and the probability of a birth being male increased, albeit insignificantly. When we split the mortality results by child's sex, along with mother's education, in panel B of table 7, we find a pattern somewhat similar to the caste results. Although both boys and girls born to mothers with more than a pri-

TABLE 8

INSTRUMENTAL VARIABLE ESTIMATES OF FERTILITY AND SEX RATIO AT BIRTH: BY MOTHER'S EDUCATION

	(1)	(2)	(3)
		A. Birth = 1	
Tariff in YOB × uneducated	−.138**	−.396***	−.338***
	(.067)	(.069)	(.067)
Tariff in YOB × 1–5 years	.309***	.102	.109*
	(.081)	(.065)	(.065)
Tariff in YOB	−.080	.060	.002
	(.060)	(.053)	(.056)
N	1,354,769	1,354,769	1,354,769
		B. Boy = 1	
Tariff in YOC × uneducated	.305**	.292**	.267*
	(.143)	(.142)	(.142)
Tariff in YOC × 1–5 years	−.152	−.161	−.159
	(.188)	(.187)	(.187)
Tariff in YOC	−.091	−.082	−.066
	(.125)	(.124)	(.129)
N	277,601	277,601	277,601
District fixed effects	x	x	x
Year fixed effects	x	x	x
Covariates		x	x
District-specific linear trends			x

Note. Women with more than 5 years of education are the excluded group. The sample is restricted to women above age 20 at the time of survey. The main effects of education groups are included in all regressions but not reported. Cols. 2–3 include indicators for mother's age at birth and number of previous births. Col. 2 also include household's caste and religion dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. YOB = year of birth; YOC = year of conception.

* Confidence: 90%.

** Confidence: 95%.

*** Confidence: 99%.

mary education experience an increase in infant mortality relative to the national baseline, among uneducated mothers, daughters exhibit a decline in mortality relative to the national baseline and sons are unaffected. Compared to a 1.2 percentage point increase in infant mortality for the daughters of educated mothers, infant mortality for girls born to uneducated mothers decreases by 0.8 percentage point for the average tariff cut of 7 percentage points.

Last, we examine how our effects vary by the wealth status of a household. For each household, DLHS-2 reports a household standard of living score.[18] On the basis of these scores, we divide households into three categories: low, medium, and high standard of living (SLI) households at the time of survey.[19] Table 9 shows that our overall fertility and sex ratio results are mainly driven by low-SLI families. In fact, medium- and high-SLI families experience a significant decrease in the likelihood of birth, relative to the national baseline. Unlike the weak effects in table 4, there is a significant decrease in the sex ratio at birth for the relatively poorer low-SLI families and a slightly significant increase for the high-SLI households in response to the tariff decline, relative to the national baseline. 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, the sex ratio at birth decreases by 1.5 percentage points in low-SLI households. When we split the mortality results by child's sex in panel C of table 7, we find that both boys and girls born to medium- and high-SLI mothers experience an increase in infant mortality (like education results) relative to the national baseline, whereas those born to low-SLI mothers experience a significant decrease relative to medium-SLI mothers and a small decrease relative to the national baseline. Unlike the results in panels A and B, the difference between the results for boys and girls is not remarkable, however.

The results presented in this section suggest that tariff cuts in rural Indian districts relative to the national baseline lead to an increase in the probability of birth, the likelihood of these births being female, and infant mortality. However, there is substantial heterogeneity in these effects. Relatively low-status women—that is, those who are relatively less educated or belong to lower castes or less wealthy households—experienced an increase in fertility, a decrease in the sex

[18] This score combines information on asset ownership, type of toilet facility, cooking fuel, and housing, as well as sources of lighting and drinking water.

[19] Ideally, we would like to know the household wealth score for each year in our sample period. But, unfortunately, since we create our panel data set retrospectively from a single cross-section, we know a household's wealth status only at the time of the survey. To the extent that the trade reform affects standards of living, the wealth index variable is not exogenous. However, if most households move within a wealth category and not across categories (e.g., a high-SLI family does not become a low-SLI family) due to the tariff cuts, this comparison is still informative.

TABLE 9
INSTRUMENTAL VARIABLE ESTIMATES OF FERTILITY AND SEX RATIO AT BIRTH: BY HOUSEHOLD WEALTH INDEX

	(1)	(2)	(3)
		A. Birth = 1	
Tariff in YOB × low SLI	−.835***	−.768***	−.710***
	(.097)	(.090)	(.085)
Tariff in YOB × high SLI	.066	.014	.005
	(.051)	(.046)	(.046)
Tariff in YOB	.366***	.301***	.235***
	(.054)	(.052)	(.054)
N	1,857,834	1,857,834	1,857,834
		B. Boy = 1	
Tariff in YOC × low SLI	.239**	.234**	.226**
	(.110)	(.111)	(.112)
Tariff in YOC × high SLI	−.247	−.245	−.249*
	(.150)	(.150)	(.150)
Tariff in YOC	−.022	−.018	−.009
	(.097)	(.097)	(.102)
N	449,065	449,065	449,065
District fixed effects	x	x	x
Year fixed effects	x	x	x
Covariates		x	x
District-specific linear trends			x

Note. Medium standard of living (SLI) households are the excluded group. The main effects of high SLI and low SLI are included in all regressions but not reported. Cols. 2–3 include indicators for mother's age at birth and number of previous births. Col. 2 also includes mother's years of schooling and household's religion and caste dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. YOB = year of birth; YOC = year of conception.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

ratio at birth, and an improvement in relative female child survival in response to the tariff cuts, relative to the national trend. On the other hand, relatively high-status women, that is, those who are relatively more educated or belong to upper castes or wealthier households, experienced a decrease in fertility, an increase in the sex ratio at birth, and a worsening of relative female child survival due to the reform, again relative to the national trend.

VI. Mechanisms

What are the underlying mechanisms for these effects? The tariff cuts may change total household income and relative female income differentially for different socioeconomic groups, thereby affecting fertility, sex ratios (Edlund and Lee 2009; Almond, Li, and Zhang 2013), and infant mortality (Strauss and Thomas 1998, 2008; Case 2001, 2004; Paxson and Schady 2005) through changes in the intrahousehold bargaining power of mothers, the relative returns from sons and daughters (Wood 1991; Katz and Murphy 1992; Kucera and

Milberg 2000; Kucera 2001; Black and Brainerd 2004; Gaddis and Pieters 2014; Bhalotra, Chakravarty, and Gulesci 2016), or a pure income channel.[20] Indeed, prior literature has shown that trade liberalization affects incomes and bargaining power, as well as returns from children. Topalova (2010) finds that rural Indian districts more exposed to trade liberalization witnessed slower declines in poverty rates and smaller increases in average per capita household consumption expenditure, and these effects were most pronounced for households at the bottom of the income distribution.[21] Aguayo-Tellez, Airola, and Juhn (2013) show that a NAFTA-related decrease in tariffs increased the intrahousehold bargaining power of women in Mexico. Munshi and Rosenzweig (2006) find that the new economic opportunities resulting from globalization in Mumbai, India, have mainly benefitted lower-caste girls by increasing their likelihood of attending English-medium schools relative to lower-caste boys. In a similar vein, Jensen and Miller (2011) show that parents in rural India strategically prevent their sons from migrating to urban areas to take advantage of better income opportunities because they want them to work on the farm. Consequently, greater employment opportunities in urban areas result in large gains in education for girls but not much for boys. The authors conclude that these results are driven by changes in relative returns (to parents) from sons and daughters.

An analysis of the mechanisms is further complicated by the fact that changes in fertility levels resulting from tariff cuts can also directly affect the sex composition of children (Jayachandran 2017) and the levels of and gender gaps in child mortality through the quantity-quality trade-off, driven by the budget constraint (Anukriti, Bhalotra, and Tam 2016). Moreover, the compositional changes in the socioeconomic characteristics of births resulting from differential effects of tariff cuts on fertility and sex selection across socioeconomic strata can also affect postnatal gender gaps (Anukriti et al. 2016).

To test for the bargaining and income channels, we need data on household income or expenditure and on male and female labor market outcomes. Unfortunately, these variables are not available in DLHS-2. As a second-best approach, we utilize data from the NSS Employment-Unemployment Survey to provide suggestive evidence about the causal mechanisms underlying our re-

[20] More open trade may also 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).

[21] Although, using state-level data, Hasan et al. (2006–7) 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).

sults. In table 10, we assess the effects of the tariff cuts on male and female adult employment by caste using specification (7). The outcome variables are indicators for being employed, for being a wage worker, and for being a manufacturing worker. For each outcome variable, we run separate specifications for upper- and lower-caste individuals.[22] SC and ST households comprise the lower-caste group, while the remaining sample comprises the upper-caste group.[23] Since the 43rd round of the NSS does not distinguish OBCs from general castes, our lower-caste results underestimate the actual effects.[24] The coefficients of "tariff" estimate the overall effect of tariff cuts for women, and the interaction coefficients in the first row capture the differential effect on men.

Column 1 of table 10 shows that employment increases significantly for lower-caste women in districts experiencing larger tariff cuts, relative to the national baseline. The effect on lower-caste men is significantly smaller, and the overall effect is close to zero. The coefficients for upper-caste men and women in column 2 are of opposite signs and much smaller in magnitude compared to the effects in column 1 and insignificant. These results imply a significant increase in relative female employment for lower castes and a decrease in relative female employment (albeit insignificant) for upper castes. The pattern of results for the likelihood of being a wage worker (in cols. 3, 4) or a manufacturing worker (in cols. 5, 6) are similar to the employment results.

However, increases in relative employment do not always translate into higher relative income. To understand the effects on income, we also need to measure the effect of the tariff cuts on wages. Topalova (2010) finds that the tariff cuts lead to decreases in agricultural wages and wages in the registered manufacturing sector (using Annual Survey of Industries data) but does not find a significant effect on wage premia when NSS data are used.[25] Moreover, the magnitude of the decrease in agricultural wages is larger than the de-

[22] An alternate way to conduct this analysis would be to divide the sample by gender and interact by caste. However, we choose to instead divide the sample by caste and interact by gender because we also seek to understand the impact of tariff cuts on the intrahousehold relative bargaining power of women. Since most marriages in India occur between individuals from the same caste, focusing attention on the caste group gets us closer to the intrahousehold interaction than if we focus on the male or female subsample. Nevertheless, we have also estimated specifications where we divide the sample by gender and interact by caste; the results do not change.

[23] Separate regressions for SC and ST individuals yield similar results.

[24] OBCs fall in between SC–STs and general castes in terms of their socioeconomic status.

[25] According to Topalova (2010), the tariff cuts lowered producer prices, which passed through as lower wages due to limited factor mobility. The wage data from the rural sample of the 43rd round of NSS are unreliable due to an unusually low fraction of individuals reporting a nonzero wage (Topalova 2010).

TABLE 10

INSTRUMENTAL VARIABLE ESTIMATES FOR ADULT LABOR MARKET OUTCOMES, NATIONAL SAMPLE SURVEY (NSS) DATA

	Employed		Wage Worker		Manufacturing Worker	
	Lower Caste (1)	Upper Caste (2)	Lower Caste (3)	Upper Caste (4)	Lower Caste (5)	Upper Caste (6)
Tariff × male	.292***	−.025	1.024***	.143	.226*	−.11
	(.087)	(.070)	(.249)	(.208)	(.127)	(.100)
Tariff	−.201*	.034	−.877**	−.035	−.185	−.031
	(.119)	(.131)	(.405)	(.286)	(.181)	(.140)
N	64,812	127,998	58,876	115,881	61,458	121,737

Note. These are estimates for specification (7) using NSS data. Each column represents a separate regression. The sample is restricted to men and women in the 15–40 age group. "Lower caste" refers to scheduled caste and scheduled tribe households; "upper caste" refers to the remaining sample. Robust standard errors are in parentheses and have been clustered at the district level.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

crease in registered manufacturing wages (although these effects are not based on the same data set).

If male and female wages are equally affected by the tariff cuts, improvements in relative female employment still translate into higher relative female income, although the effect on total household income now depends on the relative changes in employment and wages. Also, since lower-caste women (men) are now relatively more (less) likely to be in manufacturing (a sector with smaller wage cuts), this sectoral change in male-female employment may also contribute to higher relative female income despite wage declines. Finally, to the extent that a large share of female workers in India do unpaid work in the informal sector, the increased likelihood of being a wage worker and being a manufacturing worker in table 10 are also likely to result in additional relative female income among lower castes.

If wage declines due to the trade reform are sufficiently large, then despite increases in female employment and relative female income, total household income decreases. In the absence of reliable income data, we examine the effect of the tariff cuts on household consumption expenditure using NSS data. As table 11 shows, both lower- and upper-caste households experience a decrease in their log monthly consumption expenditure, relative to the national baseline, in response to the tariff decline, and the effect is substantially larger for the lower castes. In fact, Topalova (2010) finds that the tariff cuts led to a slower decline in poverty and slower improvements in per capita household consumption expenditure in rural districts and that this effect has been more pronounced for households at the bottom of the income distribution, which are also more likely to be lower caste in our context. To the extent that fertility

TABLE 11

INSTRUMENTAL VARIABLE ESTIMATES FOR LOG MONTHLY HOUSEHOLD CONSUMPTION EXPENDITURE: BY CASTE

	Lower Caste	Upper Caste
Tariff	1.470***	.610*
	(.555)	(.333)
N	32,969	80,311

Note. These are estimates at the household level for specification (7) using National Sample Survey data. The dependent variable is log of monthly household consumption expenditure in rupees in 1986–87 prices. "Lower caste" refers to scheduled caste and scheduled tribe households; "upper caste" refers to the remaining sample. Robust standard errors are in parentheses and have been clustered at the district level.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

is also affected by the tariff cuts, the effects on per capita household consumption reported by Topalova (2010) capture not just the changes in household expenditure but also the adjustments in household size that take place in response to the reform.

Although our NSS results are based on comparisons of one prereform year with one postreform year, and hence suffer from the resulting caveats, the estimates in table 10 suggest that the tariff cuts caused an increase (decrease) in the labor market returns for lower-caste (upper-caste) females, relative to males, and to the extent that most marriages in India take place within the same caste, this also translated into higher (lower) relative female income in lower-caste (upper-caste) households. These changes in relative female income are also accompanied by declines in total household consumption expenditure for both lower- and upper-caste families. Thus, tariff cuts affected household poverty and female bargaining power, as well as relative expected returns to daughters, differentially across socioeconomic groups.

How do our heterogeneous demographic results square with our findings on the economic effects of tariff cuts? For the rest of this discussion, we assume that children are not inferior goods and decisions about fertility, prenatal sex selection, and postnatal investments are jointly made. The relationship between relative or total income and our demographic outcomes depends on whether parents perceive children as consumption goods (i.e., they provide direct utility) or investment goods (i.e., they yield indirect utility by providing, e.g., old-age support). At low levels of income, children are more likely to be investment goods. But as incomes rise, the consumption motive is presumably more relevant for fertility decisions. Moreover, given that son preference is widely prevalent in India, it is possible that sons and daughters enter parents' utility function differently; although it is unlikely that only daughters, and not sons, are considered investment goods. Parents might also have a taste-based or subjective preference for sons due to noneconomic reasons.

If both boys and girls are investment goods, parents choose family size for an optimum combination of current and future income. A decrease in current household expenditure, therefore, increases fertility. Moreover, an increase in perceived returns from girls should lower the sex ratio at birth and increase relative health investments in daughters.[26] These predictions are consistent with our findings for lower-status households. The discussion so far assumes that parents have perfect control on fertility. A negative relationship between total income and fertility may also result if poorer individuals are less able to afford or access modern birth control methods and sex-selective abortions. In the latter case, a fertility increase is accompanied by a lower sex ratio at birth.[27] Note, however, that our results also reveal improvements in female bargaining power and increases in labor market opportunities for lower-status women, which should lower fertility by raising the opportunity cost of their time (Rosenzweig and Wolpin 1980; Chiappori, Fortin, and Lacroix 2002). Additionally, the time costs of pregnancy and childcare directly interfere with the ability of a woman to participate in the labor force at least for some time. But the fact that we find contemporaneous increases in fertility and employment for lower-status women suggests that the effect of lower total household expenditure dominates the effect of bargaining power.

If sons are investment goods and daughters are consumption goods, a decrease in total current income increases the demand for sons but decreases the demand for daughters due to the income effect (Becker 1960). Thus the sex ratio at birth should increase, but fertility may increase, remain unchanged, or decline depending on the relative changes in the demand for sons and daughters. Lower perceived returns from boys (due to higher relative female income) should have an additional dampening effect on the demand for sons and translate into lower fertility. Since daughters are consumption goods, their demand does not depend on future returns from girls in this case. These predictions are consistent with our findings for higher-status households.

[26] However, in case of a subjective preference for sons for noneconomic reasons, lower fertility will increase the sex ratio so that parents can have the minimum desired number of boys. The empirical estimates for the sex ratio will capture the net effect.

[27] Income shocks may also impact fetal viability differentially based on the sex of the fetus (Trivers and Willard 1973). The Trivers-Willard hypothesis suggests that negative shocks to the fetal environment make births less likely to be male.

VII. Robustness

Although prior literature on Indian trade liberalization (Edmonds et al. 2010; Topalova 2010) shows that there is no significant factor mobility in response to tariff cuts, we explicitly check for endogenous sorting by gender and caste. We cannot use DLHS-2 data for this exercise since they report only a woman's district of residence at the time of survey. Consequently, we use NSS data to estimate the effect of tariff cuts on the caste and gender composition of a district's in-migrants using specification (7). In-migrants are individuals whose district of residence at the time of survey is different from their last place of usual residence (i.e., a place where the person has stayed continuously for a period of 6 months or more). According to the IV estimates in table 12, there is no significant change in the share of female in-migrants and lower-caste female in-migrants in a district due to the tariff reform (rows 1 and 2). The same is also true for the share of short-run migrants (i.e., those who have moved within the past 10 years) in rows 3 and 4 of table 12.

Although we include district-specific linear time trends in all regressions, a 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. Instead, we reestimate our specifications by controlling for year effects that vary by initial district conditions, namely, the share of workers employed in agriculture, mining, manufacturing, trade, transport, and services; the share of the SC and ST population; and the share of the literate pop-

TABLE 12
INSTRUMENTAL VARIABLE ESTIMATES: MIGRATION FROM OTHER DISTRICTS BY CASTE AND GENDER

Dependent Variable	Coefficient of Tariff
District population share of:	
1. Female in-migrants	.091
	(.082)
2. Lower caste female in-migrants	.043
	(.030)
3. Female in-migrants who have moved within the past 10 years	.041
	(.035)
4. Lower caste female in-migrants who have moved within past 10 years	.017
	(.015)
N	722

Note. These estimates are based on the 43rd and the 55th rounds of the National Sample Survey (NSS). Each coefficient is from a different regression. Robust standard errors are in parentheses and have been clustered at the district level. Lower caste refers to scheduled caste and scheduled tribe individuals. In-migrants are individuals whose place of enumeration is in a district different from their last place of usual residence. Regressions are weighted by the number of households in a district and control for district and year fixed effects and initial district conditions that are interacted with the postreform indicator.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

ulation. As table A1 shows, the IV estimates for the tariff measures in all specifications remain consistent with our previous results (with similar signs, magnitudes, and significance).

We also check whether our results are robust to the inclusion of district-level annual rainfall shocks. Annual fluctuations in rainfall are an important determinant of economic outcomes in agriculture-dependent developing countries, such as India (Paxson 1992; Rosenzweig and Wolpin 1993; Townsend 1994; Jayachandran 2006). We define rainfall shock as an indicator variable that is equal to one if the annual rainfall in a district deviates by more than 30% from its historic annual mean precipitation and zero otherwise. These results are also similar to our main findings and are available on request.

VIII. Conclusions

This paper analyzes whether India's trade liberalization, beginning in 1991, affected fertility, infant mortality, and sex ratios at birth. To identify the causal effects, we compare rural districts more or less exposed to the tariff cuts. We find that women of lower socioeconomic status experienced an increase in fertility relative to the national trend, which was driven by relatively more female births. Moreover, infant mortality decreased for these girls relatively. In contrast, there is some evidence that women of higher socioeconomic status had relatively fewer children, driven by relatively fewer girls, and we find strong evidence that girls born to high-status mothers fared worse in terms of higher mortality, relative to the national trend. Mortality rates for boys, however, do not seem to have been significantly affected by the trade reform, irrespective of their parents' socioeconomic status.

We also show that the tariff cuts increased (decreased) relative female earnings in lower-caste (upper-caste) households but, regardless of caste, decreased total household expenditure, again relative to the national baseline. Together, the heterogeneous impacts on various demographic and economic outcomes suggest that, irrespective of socioeconomic status, sons are considered investment goods in India, whereas daughters are considered investment goods in households of lower socioeconomic status but consumption goods in upper-status families.

Thus, the labor market outcomes for men and women from different social strata changed differentially due to trade liberalization in rural India, and households have responded to this altered economic landscape via fertility and investments in children's human capital. It is important to note that our results do not suggest that more open trade led to overall increases or decreases in fertility, sex ratios, or infant mortality. However, they confirm that removal of trade barriers in India had significant distributional consequences along these dimensions, especially for women and girls.

Appendix

TABLE A1
INSTRUMENTAL VARIABLE ESTIMATES: CONTROLLING FOR INITIAL DISTRICT CONDITIONS

	Birth = 1		Male = 1		Mortality in 12 months	
	(1)	(2)	(3)	(4)	(5)	(6)
Tariff	−.191***	−.363***	.141**	.530***	−.062	−.116*
	(.062)	(.134)	(.069)	(.139)	(.041)	(.063)
N	1,817,333	1,805,250	437,926	385,229	461,618	410,685

Note. Shown are results from specifications (3)–(6) with the addition of initial district condition covariates. Cols. 2, 4, and 6 include mother fixed effects, while the rest do not. Standard errors are in parentheses.
* Confidence: 90%.
** Confidence: 95%.
*** Confidence: 99%.

TABLE A2
INSTRUMENTAL VARIABLE (IV) ESTIMATES: URBAN INDIA

	(1)	(2)	(3)	(4)	(5)
			Birth = 1		
A1. First stage:					
Traded tariff in YOB	.317***	.317***	.325***	.315***	.323***
	(.030)	(.030)	(.029)	(.029)	(.028)
F-statistic	108.05	108.3	126.43	118.72	135.13
B1. IV:					
Tariff in YOB	−.100**	−.141***	−.143***	−.230*	−.307***
	(.049)	(.047)	(.045)	(.127)	(.115)
N	895,134	895,134	895,134	895,134	895,134
			Boy = 1		
A2. First stage:					
Traded tariff in YOC	.292***	.292***	.302***	.279***	.288***
	(.028)	(.028)	(.027)	(.022)	(.024)
F-statistic	111.27	111.56	122.24	158.02	149.55
B2. IV:					
Tariff in YOC	−.040	−.033	.050	−.251	−.238
	(.110)	(.110)	(.109)	(.155)	(.151)
N	186,953	186,953	186,953	186,953	186,953
			Mortality = 1		
C1. Mortality in 1 month:					
Tariff in YOB	−.012	−.009	.012	−.013	.017
	(.034)	(.034)	(.033)	(.052)	(.051)
C2. Mortality in 6 months:					
Tariff in YOB	−.030	−.027	−.009	−.025	−.001
	(.035)	(.035)	(.035)	(.055)	(.055)
C3. Mortality in 12 months:					
Tariff in YOB	−.033	−.030	−.002	−.038	−.001
	(.038)	(.039)	(.038)	(.059)	(.057)
C4. First stage:					
Traded tariff in YOB	.300***	.300***	.308***	.283***	.290***
	(.029)	(.029)	(.028)	(.023)	(.023)
F-statistic	105.37	105.63	121.76	149.57	152.44
N	198,400	198,400	198,400	198,400	198,400

TABLE A2 (*Continued*)

	(1)	(2)	(3)	(4)	(5)
District fixed effects	x	x	x		
Year fixed effects	x	x	x	x	x
Covariates		x	x	x	x
District-specific linear trends			x		x
Mother fixed effects				x	x

Note. Each cell constitutes a separate regression. Cols. 2–5 include indicators for mother's age at birth and number of previous births. Cols. 2 and 3 also include mother's years of schooling and household's religion and caste dummies. Robust standard errors are in parentheses and have been clustered at the district level. All regressions use district-level sampling weights. YOB = year of birth; YOC = year of conception.

* Confidence: 90%.

** Confidence: 95%.

*** Confidence: 99%.

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