# Daughters Left Behind: How Trade Liberalization Harms Girls in China when Government Restricts Migration*

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**Abstract**

China's accession to the WTO created new economic opportunities in certain cities. A shift-share identification strategy shows that residents of adjacent rural areas migrated in and advanced economically. Longitudinal panel data on children reveals that their sons benefit, but counter-intuitively, daughters suffer worse mental and physical health, complete fewer years of schooling, and remain poor later in life. We explore why, and learn that *hukou* policy that restricts migrant children's access to urban schools is a factor. Triple difference research designs reveal that migrant parents become discontinuously more likely to leave middle-school-aged daughters (but not sons) behind in rural areas – often without *either parent present* – exactly when and where *hukou* policy makes schooling more expensive. 69 million Chinese children are left behind in rural areas, and girls are harmed even when trade liberalization increases family income.

**Keywords:** Trade Liberalization, Migration Restrictions, Gender, *Hukou*

**JEL Codes:** F16, J16, R23, O12

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# 1 Introduction

Rural-to-urban migration is integral to the process of economic development. 11% of the Chinese population in the 2005 census – 145 million people – were rural-urban migrants. Migrants traveled in response to new economic opportunities afforded by trade liberalization. However, internal mobility restrictions in China make it difficult for parents to migrate *with* their children. As a result, 69 million Chinese children were left behind by their migrant parents in 2015 (UNICEF, 2018). This paper tracks the determinants and consequences of this massive societal disruption. Whether trade liberalization benefits or harms children *on net* is unclear, because parents' earnings capacity and their presence are both valuable for child development (Yang, 2008).

China joining the WTO in 2001 produced large labor demand shocks in certain cities (Erten and Leight, 2021; Facchini et al., 2019) and created massive new economic opportunities in those locations. Consistent with the literature, we first show that workers from adjacent rural areas migrate into those cities in response, find work in more skill-intensive occupations and sectors, earn higher wages, and enjoy higher family income and consumption. We then use a longitudinal survey that tracks rural children from Gansu province over 15 years to analyze the long-run consequences for the children of those migrants. We find that male children in rural Gansu adjacent to cities that experienced labor demand shocks fared better than male children in other parts of rural Gansu where those opportunities were not as easily accessible. In contrast, daughters in those same migrant families fare much *worse* later in life, both relative to boys, and relative to girls in other families without the same migration opportunities. Girls who had a one standard deviation greater exposure to the 2001 trade policy shock are 2.7 percentage points *less* likely to complete junior middle school, have 15% *lower* income in their twenties, have worse physical and mental health as adults, and experienced more psychological and behavioral problems as children. These problems are not unique to Gansu: we see the exact same gendered patterns of effects of trade liberalization in larger-scale, nationally representative surveys.

The rest of the paper explores why the enhanced economic opportunities for parents were evidently a curse for their daughters. Detailed data from multiple rounds of the longitudinal surveys, plus other nationally representative surveys including the census provide some clues. First, parents are more likely to emigrate and separate from daughters than sons. Second, migrants who leave daughters behind remit 30% less per child than migrants who leave sons behind. Girls therefore receive less parental time *as well as* less money compared to boys. Third, daughters generally do more housework than sons, but the gender gap in time allocation to household chores gets magnified when parents emigrate.

To understand the source of these gendered effects, we explore the connection to China's *hukou* policies which erects mobility barriers and can force parents to separate from their children. Migrants with a rural *hukou* are required to pay a large fee called *zanzhufei* to enroll their children in urban schools (Chan and Buckingham, 2008). *Zanzhufei* for junior middle school enrollment is about 10% of the average migrant's earnings – a big financial deterrent. Such constraints on migrant parents are becoming even more acute over time as cheaper schools specifically designated for migrant children are shut down in Beijing and other popular migration destinations (Yang, 2016).

We first use variation in the stringency of *hukou* restrictions across Chinese cities interacted with discontinuous jumps in schooling costs to analyze how rural parents decide whether and when to migrate and leave their sons or daughters behind. Children must transition from primary to junior middle school at a certain age, and *Zanzhufei* for junior middle school is 53% larger than for primary school. Using a triple difference setup, we find that parents from rural areas near *hukou*-restrictive cities become 3.5 percentage points more likely to separate from daughters exactly when they reach the legal enrollment age for junior middle school. There is no such effect for sons, or near cities with relatively lax *hukou* policies. This increase in schooling costs does not affect rural parents' decisions on *whether to* stay at home or migrate (not surprising, given the large wage premium in cities); it only affects the margin of whether to migrate *with or without their daughters*.

We again see that same effect on daughters when in 2014 the Chinese government urged "mega cities" - defined as those with a population of over five million in the city central district area - to rigidly control the population. This new "migrant population control policy" led to a tightening of *hukou* restrictions and shutting down of migrant schools in mega-cities (Figure A5). Using this alternative triple-difference identification strategy, we find that parents who had previously migrated to cities above the 5-million-population cutoff become 7 percentage points more likely to leave their middle-school-aged daughter behind *after* 2014, relative to parents who had previously migrated to cities below the mega-city population cutoff. That same effect does not exist for boys across cities on either side of the population cutoff. The effect is robust when we restrict attention to migrants who had made their destination choices before the 2014 policy was announced, which addresses concerns about endogenous destination choice in response to restrictions placed on children.

Thus, using multiple research designs with different types of data variation, we find that although China's policy of mobility restrictions is not gender-specific in its intent or design, it produces a gendered effect in which daughters become more likely to be separated from their parents when they reach a certain age. Most of those girls are left behind without *either* parent present. Connecting the two parts of the paper, we see that trade liberalization's unintended adverse consequences on girls were more acute in areas with more restrictive *hukou* policy. Finally, we show that son-biased preference is the most likely explanation for parents' gendered reactions to the migration restrictions imposed on them.

Taken together, our results suggest that girls suffer disproportionately when strict mobility restrictions are imposed on migrant workers in a rapidly developing and urbanizing society. When it is expensive for migrants to keep their children with them, they are more likely to separate from daughters than sons, and daughters receive less time, attention, and money from their parents. Separation undermines their human capital accumulation and hurts girls throughout their lives, despite the rest of the family thriving with the new economic opportunities that trade liberalization brings.

Given China's one-child policy, son preference, and the resulting imbalanced sex ratios (Qian, 2018), there are legitimate concerns that families with daughters may have systematically different characteristics than families with sons. For example, daughters may have more siblings than sons given fertility stopping-rules. We include family size fixed effects in our models to account for this. Some families may have better access to sex-selective abortion technology. We show that our results hold even for the first child, where there is documented gender parity (Almond et al., 2019).

Beyond China, restrictions on migrants' access to services including schooling are formalized in Vietnam through their similar *Hokhau* system (Cameron, 2012). Millions of internal migrants in India also find it difficult to access urban public services (Imbert and Papp, 2020). And the most typical international labor migrants – South, South-East Asians, and Africans working in East Asia and the Gulf – are discouraged or prevented from bringing families with them to work locations (Mobarak et al., 2023). As a result, millions of Filipino, Indian, Pakistani, Egyptian children are also growing up without a parent present.

**Related Literature:** A closely related literature explores the converse question: how did China's accession to WTO affect wages and employment of U.S. workers (Autor et al., 2013; Pierce and Schott, 2016)? Other papers on this trade liberalization episode study effects on local economic development in China (Erten and Leight, 2021; Facchini et al., 2019; Li, 2018), and on gender inequality among adult workers (Keller and Utar, 2022; Tang and Zhang, 2021). We complement these by documenting a new surprising fact: daughters are worse off later in life even when parents' earnings capacity improves.

The literature on the effects of trade shocks on children (e.g. Leight and Pan, 2020) highlight various pathways through which children are affected: their psychological health (Colantone et al., 2019), remittances received (Yang, 2008), the burden of chores (Edmonds et al., 2010), and the risk of child labor (Bai and Wang, 2020). We uncover a new channel: trade liberalization interacts with mobility restrictions to harm daughters in environments with pre-existing son preference. We thereby add to the literature on the sources of gender disparities[1] by identifying a new mechanism by which disparities might emerge even if the underlying policy (of mobility restrictions) has no explicit gender dimension.

Also related are papers on the effects of migration on children's educational outcomes (Chen, 2013; Zhang et al., 2014), but we are among the first to document the long-run consequences in adulthood, and the first – to the best of our knowledge – to study the effects of interactions between trade policy and mobility restrictions. We thereby contribute to the literature on the adverse welfare effects of spatial immobility and add a gender dimension to the distributional consequences of migration.[2] Most closely related, Kinnan et al. (2018) and De Brauw and Giles (2018) find that relaxing *hukou* restrictions improves consumption outcomes for rural Chinese households.

Finally, we fit into the big literature on the phenomenon of children left behind by migrant parents, in Mexico (Antman, 2011, 2012), the Philippines (Wong, 2023; Yang, 2008) and China (Chen, 2013). Dang et al. (2016) points out the role that urban school fees play in parents' decisions to leave children behind, which explains the discontinuous jumps we observe at middle-school age.

[1] Beaman et al. (2012); Blau and Kahn (2017); Card et al. (2016); Goldin (2014); Goldin et al. (2021); Barth et al. (2017); Hannum et al. (2022); Qian (2008); Bhalotra et al. (2019); Dahl and Moretti (2008); Chetty et al. (2016).

[2] Bryan et al. (2014); Clemens et al. (2014); Facchini et al. (2019); Gollin et al. (2014); Imbert et al. (2022); Khanna et al. (forthcoming); McKenzie and Rapoport (2007).

The remainder of this paper proceeds as follows. Section 2 discusses the trade liberalization episode (China's WTO entry in 2001) and the *hukou* system. Section 3 describes the data. Second 4 provides estimates of the effects of trade liberalization on adult workers, and on their sons and daughters. Section 5 explores why daughters are harmed while sons benefit when trade liberalization creates new economic opportunities, and identifies the phenomenon and consequences of daughters left behind. Section 6 connects this phenomenon to *hukou*-related migration restrictions. Section 7 explores whether son-biased preferences can explain these empirical patterns. Section 8 concludes.

## 2 Institutional Background

### 2.1 Trade Liberalization Episode: China's Accession to WTO

China joined the WTO in December 2001, initiating two decades of export-driven rapid economic growth (Figure A1a). This significantly reduced tariff uncertainty faced by Chinese firms exporting to the US (Facchini et al., 2019; Pierce and Schott, 2016). Figure A1b shows that Chinese cities that benefited more from the reduction in tariff uncertainty given their pre-WTO sectoral composition subsequently experienced higher levels of export growth. This creates greater economic opportunities for rural workers living near those cities. Figure A2 shows that those workers indeed responded by emigrating to nearby cities. Figure A3 shows that city-level exposure to trade liberalization varies greatly across China, and even within Gansu province – which is the setting for the detailed longitudinal panel data we will use.

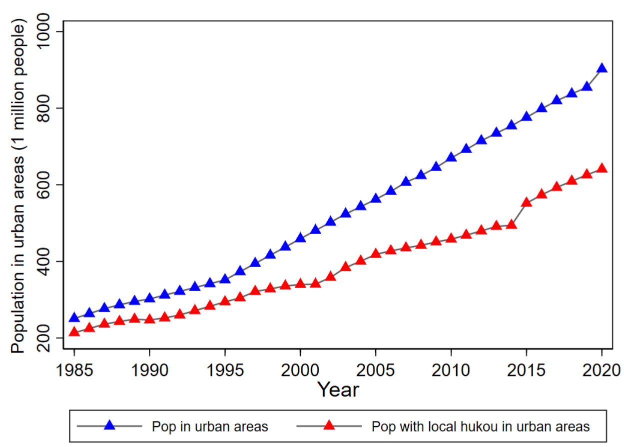
### 2.2 Hukou Restrictions, Migration, and Children Left Behind

China instituted a *hukou* system in 1958 to control internal migration. The system required that each person be classified as rural or urban, and be assigned a locality of *hukou* registration typically corresponding to the person's birth location. This determined a person's eligibility to receive state-provided goods and services in a specific place. While Chinese citizens can migrate to cities, they cannot access many government-provided benefits at the destination without an urban *hukou*. Most importantly, it is expensive and difficult for the children of migrants to enter urban schools.

The recent economic growth in China has triggered a dramatic increase in rural-urban migration. The population of Chinese cities surged from 200 million in 1985 to 900 million in 2020 (Figure 1). Only a subset of those migrants were granted urban *hukou*, so the number of urban residents without urban *hukou* privileges also increased dramatically during this period. Obtaining an urban *hukou* requires levels of professional skills or educational attainment that are very difficult for the vast majority of rural migrants to attain (Khanna et al., forthcoming). Children inherit their parents' *hukou* status. The restrictions on school access have therefore led to large increases in the number of children left behind in rural areas without parents present. In 2018, approximately 69 million children in rural China were growing up without their parents (UNICEF, 2018). There are many clear indications that left-behind rural children experience worse educational quality: teachers in rural schools are less qualified (Table A1), are less professionally accomplished (Table A2), and students have access to worse facilities (Table A3) than their counterparts in urban schools.

In summary, China's accession to the WTO created new economic opportunities that triggered large-scale migration into certain cities. But due to *hukou* policy restrictions, many of those migrants were forced to leave children behind in rural areas. These children are often raised by grandparents.

Figure 1: More and More People Don't Have Local Urban *Hukou* as China Urbanizes




*Note*: The blue line denotes the population in urban areas, and the red line shows the population holding local urban *hukou* in urban areas. Data come from the *China Statistical Yearbook*.

# 3 Data

## 3.1 Longitudinal Data on Children

The Gansu Survey of Children and Families (GSCF) is a longitudinal survey of rural children conducted by the University of Pennsylvania and the Gansu Bureau of Statistics in five waves in 2000, 2004, 2007, 2009 and 2015. The first wave surveyed a representative random sample of 2,000 children aged 8-14 across 100 villages in Gansu Province. Subsequent waves track these children for 15 more years, which allows us to link their long-term socioeconomic outcomes during adulthood, including educational achievement, earnings, and migration status, with their childhood experience of exposure to a trade shock, which led their parents to migrate, earn more, but possibly leave the children behind in rural areas. We construct individual-level longitudinal panel data by combining GSCF 2000, 2004, 2009 and 2015. We restrict our analysis to the 1447 individuals who appear in the 2015 wave. These individuals were interviewed by phone if they were not physically present in Gansu. The survey attrition rate from 2000 to 2015 is not significantly different between those had above (25.9%) versus below (27.7%) median exposure to the trade shock related to WTO accession (as defined below in Section 4.1), and that trade shock will be our source of identifying variation.

## 3.2 Other Data on Migrants and Their Children

We also use the China Family Panel Studies (CFPS) 2010 survey, the 2010 Population Census of China (a 0.095 percent sample), and the China Migrants Dynamic Survey (CMDS).

CFPS is a nationally representative survey of Chinese families and individuals. We focus on individuals who were born with a rural *hukou* and had not yet completed compulsory schooling when China joined WTO. We use the 2010 wave of CFPS, because it included detailed information on individual birthplaces. We construct a measure of exposure to trade liberalization based on birth location. Our final sample includes 2616 rural children from 24 provinces. While GSCF provides detailed long-run data on Gansu children, CFPS allows us to replicate results for all of China.

China conducts its national population census every ten years, and the 2010 Population Census is the most recent decennial census with individual-level data available to researchers. The census records demographic characteristics of parents and their children, including age, gender, education, *hukou* type (rural or urban), *hukou* location, and current residential location. The 0.095% random sample includes 72,902 children in 54,596 rural households.

The census data allows us to characterize the three types of location choices rural parents can make: stay in the village with children, move to a city with children, or move to a city while children remain in the rural area. Following Facchini et al. (2019) and Khanna et al. (forthcoming), we define migrants as those who move out from their *hukou* prefecture. Rural and urban areas within the same prefecture can be within commuting distance, so those moves would not necessarily correspond to children being separated from parents. 16.5% of the rural population in the 2010 census – 111 million people – had migrated from their *hukou* prefecture, up from 76 million (10.2% of the rural population) in 2005.

China Migrants Dynamic Survey (CMDS) is the largest nationally representative survey of China's migrant population. The CMDS sampling frame consists of migrants who have lived in cities for more than one month but have no local *hukou*. The survey records detailed socioeconomic information of migrant parents and their children, including age, gender, education, and residential location. It also includes information on remittances sent by parents, which we analyze in Section 5.

We combine six waves of the CMDS survey from 2011 to 2016 to construct a pooled cross-sectional dataset. Unlike the census data, the timing of these surveys allows us to leverage the 2014 Migrant Population Control Policy for identification, and to control for city-and year- specific unobservables using fixed effects. Our CMDS analysis sample (children of migrants in cities where we can measure *hukou* restrictions) includes 171,859 children across 30 provinces, of whom 47,121 are junior middle school aged and 124,738 children are primary school aged.

### 3.3 Data to Construct Exposure to Trade Shocks

We leverage shocks to labor demand in nearby cities due to trade policy changes to identify the long-term consequences on children. China's accession in the WTO in 2002 resulted in a differential reduction in the trade policy uncertainty faced by Chinese exporters across different sectors. As in Pierce and Schott (2016), we use Normal Trade Relations (NTR) gap to measure to exposure to trade liberalization in this trade episode. Prior to joining the WTO, the US Congress needed to continually renew the preferential NTR tariffs bestowed upon China. Joining the WTO reduced the renewal uncertainty defined to be the difference between the non-NTR tariff and the NTR tariff.

To construct a shift-share measure to identify effects on internal migration from nearby rural areas and on long-term consequences for the children of those migrants, we aggregate the industry-level NTR gap measure at the city-level using as weights the export shares of different industries in the export basket of each Chinese city prior to China's WTO entry (1997-1999). We gather data on city-and product-specific export shares from Facchini et al. (2019) and data on product-level NTR gap from Pierce and Schott (2016). And we then map HS product to 4-digit ISIC Industrial Classification.

### 3.4 *Hukou* Restrictions Data

In the second part of the paper, we use the *hukou* index constructed by Zhang et al. (2019) to measure the stringency of *hukou* regulations across Chinese cities. The main mechanisms by which migrants qualify for an urban *hukou* include tax payment and investment, home purchase, and employment. Their measure ignores family reunification rules because it is quantitatively unimportant. The requirements associated with each mechanism differ by city, and the composite *hukou* index measures the overall difficulty for adult migrants to obtain a local *hukou*. Because China experienced significant changes in the *hukou* policy in 2014, Zhang et al. (2019) construct city-level *hukou* index specific for the periods of 2000–2013 and 2014–2016. Appendix Tables A4-A5 report summary statistics of the key variables used in the analysis.

## 4 The Effects of Trade Liberalization on Children

### 4.1 Shift-Share Identification Strategy

We construct a shift-share variable which measures each rural region's *exposure* to the reduction in trade tariff uncertainty via their proximity to nearby industrial cities. The trade shocks experienced by each city is defined as the NTR gap for industry $k$ ($NTRGap_k$) weighted by the importance of that industry to city $d$ (as in Facchini et al., 2019), as measured by that city's pre-period (1997-1999) export share of that industry ($\frac{EX_{k,d}}{\sum_j EX_{j,d}}$), prior to China's accession to WTO. Every city experiences these trade shocks, so each rural region's exposure is determined by their proximity to every "potential" migration destination. We therefore weight the city-specific exposure to trade liberalization by the inverse of the distance from rural people's birth location $c$ (where they have local rural *hukou*) to every urban destination $d$, to create the shift-share variable for rural region $c$:

$$NTR_c = \sum_d \left(\frac{1}{dist_{dc}}\right) \left(\sum_k NTRGap_k \times \frac{EX_{k,d}}{\sum_j EX_{j,d}}\right) \tag{1}$$

We standardize $NTR_c$ for ease of interpretation. We assign non-zero weights only to potential destination cities that are located within a 400 km radius of birthplace $c$, but our results are not sensitive to this choice.

We employ the shift-share identification strategy not only on census data and nationally representative CMDS data, but also on the longitudinal data on children born in rural Gansu province. As Figure A3 shows, Gansu is a large, geographically-spread province, and GSCF survey districts abut a variety of cities that experienced different intensities of trade demand shocks: Xi'an, Chengdu, Xining. This creates sufficient variation: the range of our measure of NTR Gap in the GSCF dataset exceeds three standard deviations. The *hukou* stringency index we use in later analysis also displays large variation across Gansu.

Recent literature demonstrates that identification based on shift-share variables either relies on the orthogonality of shifters or of exposure shares (Borusyak et al., 2022; Goldsmith-Pinkham et al., 2020). In our context, the validity of identification depends on the conditional exogeneity of shifters, i.e. industry-level NTR gap at ISIC4 level. Our key identification assumption is that, conditional on shock-level controls, the industry-level NTR gaps are orthogonal to regional confounding unobservables in China.

Several facts makes this assumption reasonable for our context. First, for a long time prior to China's economic reform, the US had started to use two different tariff rates – NTR tariffs versus non-NTR tariffs – for market economies and non-market economies, respectively. US imports from non-market economies were subject to relatively high tariff rates originally set under the Smoot-Hawley Tariff Act of 1930. Second, neither NTR or non-NTR tariff rates are specific for China. NTR tariff rates are the same for all market economies in the world. Current Chinese political-economy drivers could not have affected Smoot-Hawley tariff rates, which were set by the U.S. Congress in the 1930s. Third, U.S. NTR tariff rates are the result of U.S. multilateral negotiations with all WTO member nations and are unlikely to have been impacted by *local* conditions in any particular Chinese city, especially because China was not part of the WTO at the time the NTR rates were set by the U.S. (the end of the Uruguay Round 1986-1994).

### 4.2 Effects of Trade Liberalization on Parents

We first study the effects of trade demand shocks in nearby cities (as measured by the standardized NTR Gap defined in equation 1) on the choices of adult workers with a rural *hukou*. For brevity, Table 1 only reports the coefficient on the NTR gap. Panel A uses the nationally representative Census 2005 data. The results show that a one standard deviation increase in NTR Gap in nearby cities results in a 26% increase (0.024/0.094) in the out-migration rate and a 16% increase in wages, which corresponds to 68 RMB per month. Those workers move into occupations and industries that are more skill-intensive.[3]

Panel B uses 2000 and 2004 survey rounds of GSCF to examine economic outcomes for the parents of the tracked Gansu children soon after their exposure to the trade demand shock induced by China joining the WTO. As in the rest of China, rural parents in Gansu become 5.7 percentage points more likely to out-migrate in response. Those new migration opportunities allows rural parents to diversify away from agriculture. A one SD increase in exposure to trade liberalization increases the number of off-farm days by 72% for rural parents in Gansu, which is an increase of 4 days per month. At the extensive margin, that one SD increase raises the probability of having a non-agricultural job by 19.6%. Trade liberalization is also significantly positively associated with total household consumption and food consumption increases for rural households in Gansu.

In summary, rural adult workers migrate to cities in response to the new economic opportunities stemming from China's trade liberalization, they shift away from agriculture and into skilled work, and this improves the family's economic status. The next sub-section explores the subsequent effects on their children.

[3] We follow Ahsan and Chatterjee (2017) to define industry-specific skill intensity as $EI_{ind} = \sum_{f=1}^{L_{ind}} (\frac{\omega_f}{\sum_f^{L_{ind}} \omega_f}) \times e_f$; where $e_f$ is individual $f's$ education category, $\omega_f$ is an individual's sampling weight, and $L_{ind}$ is the total number of workers within an industry. We categorize a respondent's educational level into various rankings: not literate (=0), below primary school (=1), primary school(=2), middle school (=3), high school (=4), technical secondary school (=5), pre-college (=6), college (=7), master (=8) and PhD (=9). We define occupation-specific skill intensity in the same way.

Table 1: The Effects of Trade Liberalization on Parents

| Dep. Var. | Effect on Parents | Mean of Dep. Var. |
|---|---|---|
| **Panel A: Population Census of China 2005** | | |
| Migrate(=1) | 0.0242*** | 0.0940 |
| | (0.00461) | |
| IHS (Income) | 0.160*** | 6.459 |
| | (0.0338) | |
| Income (Chinese Yuan) | 68.16*** | 460.5 |
| | (15.10) | |
| Occupation-specific skill intensity | 0.157*** | 7.540 |
| | (0.0330) | |
| Industry-specific skill intensity | 0.193*** | 7.530 |
| | (0.0339) | |
| **Panel B: GSCF 2000 and 2004** | | |
| Migrate(=1) | 0.0573** | 0.124 |
| | (0.0196) | |
| IHS (Off-farm Days) | 0.720** | 0.995 |
| | (0.297) | |
| Number of Off-farm Days | 3.978** | 5.472 |
| | (1.753) | |
| Non-agricultural Job (=1) | 0.196** | 0.280 |
| | (0.0798) | |
| Household Food Expenditure (Chinese Yuan) | 1,075** | 1,955 |
| | (436.0) | |
| Household Total Expenditure (Chinese Yuan) | 2,407* | 6,147 |
| | (1,178) | |

*Notes*: Each row represents a separate regression. In Panel A, we use China Population Census 2005 and perform individual-level regressions. Column 1 shows the dependent variable for each regression, and the independent variable of interest is $NTR_c$ defined in equation 1. We control for fixed effects for the number of children, import tariffs, contract intensity, input tariffs, and export licences. Migrate is dummy variable for whether an individual had been away from *hukou* location. Robust standard errors are reported in parentheses. In Panel B, we combine GSCF 2000 and 2004. Column 1 shows the dependent variable for each regression, and the independent variable of interest is the interaction between $NTR_c$ and the post-2002 dummy. Migrate is dummy variable for whether an individual had been away from *hukou* location for more than 3 months. Regarding migration choices, the number of off-farm days, and the dummy for non-agricultural job, we conduct individual-level regressions and control for individual FE, year by prefecture tier FE and the number of children FE. We also control for interactions between trade controls (import tariffs, contract intensity, input tariffs, and export licences) and the post 2002 dummy. Regarding household food expenditure and total expenditure, we conduct household-level regressions and control for household FE, year by prefecture tier FE, and interactions between trade controls and the post 2002 dummy. Robust standard errors clustered at the *hukou* location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

### 4.3 Effects of Trade Liberalization on Children's Later-life Outcomes

Positive import demand shocks in nearby cities can affect rural children through a variety of mechanisms. Children should benefit from enhanced economic opportunities for parents. If the shock persists, it could create future economic opportunities for the children as they enter adulthood, and it could also thereby raise their perceived returns to education. Conversely, they may drop out to take advantage of factory job opportunities. If children are left behind by migrant parents, that parental separation could adversely affect children's health and socioeconomic outcomes.

We empirically evaluate how exposure to trade liberalization when children are school-aged affects their later-life outcomes by estimating the following equation using the 2015 round of the GSCF longitudinal panel survey. GSCF randomly sampled 8-14 year olds in 2000, which means that these children were 10-16 years old in 2002 and still in junior middle school, after China joined the WTO and the massive urban labor demand shock ensued. When surveyed in 2015, these "children" were 23-29 years old, and their entire schooling history therefore completed.

$$\begin{aligned} Y_{icn,t} =& \beta_0 + \beta_1 (\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times Female_i + \beta_2 NTR_c \times \\ & (\overline{Age}_{sch} - Age_{2002})_i + \beta_3 NTR_c \times Female_i + \beta_4 (\overline{Age}_{sch} - Age_{2002})_i \\ & \times Female_i + \beta_5 Female_i + \beta_6 (\overline{Age}_{sch} - Age_{2002})_i + \xi_c \\ & + \eta_n + \varphi_{num} + \varepsilon_{it} \end{aligned} \tag{2}$$

$Y_{icn,t}$ represents the outcomes of child $i$ who was born in the rural area of prefecture $c$ in year $n$, including their education, psychological health and labor market outcomes. All the children in our sample had rural *hukou* in their birth location during their school age. $\overline{Age}_{sch}$ represent the age cutoff for child $i$ to complete compulsory junior middle school, which is 15 for those born before September 1 and 16 for those born after September 1. $Age_{2002}$ was child $i's$ age in 2002. As China joined WTO in December 2001, $\beta_2$ captures the effect of *a year of exposure* to the one SD increase in trade liberalization before completing compulsory schooling for boys. $\beta_1 + \beta_2$ represents this same effect for girls. This empirical strategy can only identify the *net* effect of trade shocks on children, not any specific mechanism highlighted above. $\xi_c$ and $\eta_n$ are birth location fixed effects and age cohort fixed effects, respectively. Due to China's One Child Policy (OCP), in some provinces, the local government allows rural parents to have a second child only if their first-born is a girl. As a result, the gender of the child may be systematically correlated with family size. We therefore control for fixed effects for the number of children ($\varphi_{num}$). Borusyak et al. (2022) suggest controlling for observation-level exposure-weighted average of shock-level controls. Translating this to the context of our analysis, we control for the interaction between exposure-weighted average of import tariffs (for each child's birth location), $Female_i$ and $(\overline{Age}_{sch} - Age_{2002})_i$. As China's trade policies may interact with *hukou* restrictions to affect productivity and labor demand. We control for the quadruple interaction between exposure-weighted average of import tariffs, $Female_i$, $(\overline{Age}_{sch} - Age_{2002})_i$ and an indicator for *hukou* policy restrictiveness in nearby cities.[4] [5]

Panel A of Table 2 reports results on children's educational attainment. The first column shows the effect of a year of exposure to trade liberalization on boys (coefficient $\beta_2$), the second column shows the effect on girls (coefficient $\beta_1 + \beta_2$), and the third column reports the p-value of a t-test of the gender difference in effects. The table therefore compares the effects of trade liberalization on daughters to two different counterfactuals: (a) how do they fare relative to girls from *other* rural areas *less exposed* to trade liberalization, and (b) how do they fare relative to boys in the same location?

A one SD increase in exposure to trade liberalization for every year before the child completes compulsory schooling *raises* probability of graduating from full-time pre-college by 4.6 percentage points for boys, but it *reduces* girls' probability of completing full-time pre-college by 3.2 percentage points. The gender differential is statistically significant. These effects are non-trivial, given that only 14% of children in our data had a full-time pre-college degree in 2015. Trade liberalization also benefits boys by increasing their probability of completing junior middle school, but reduced this propensity for girls. Girls with greater exposure to trade liberalization while in school age are significantly less likely to speak the official national language, Mandarin fluently (rural folks often use local dialects for oral communication). In contrast, exposure to trade liberalization generally benefits

[4] Specifically, we define an indicator for whether the inverse-distance weighted average of *hukou* index in cities (within 400km of an individual's birth location) is above the average level.

[5] We use robust standard errors clustered at the level of birth location in our baseline results of Table 2. Our results are robust to different error term structures that we assume for inference. In Appendix A.3 Table A6, we perform wild cluster bootstrapping procedures (Kline and Santos, 2012) given the small number of clusters, and this yields very similar results. Appendix B.3 Table B5 presents shock-level equivalent estimates using exposure-robust standard errors (as in Borusyak et al., 2022)) and also shows similar results.

boys, and the gender differential in effects is statistically significant for most outcomes.

Panel B tracks later-life socioeconomic and labor market outcomes that can be constructed from the GSCF survey. A one SD increase in trade liberalization exposure per year reduced girls' later-life hourly income by 15%, and reduced their likelihood of escaping poverty (>US$1.90 per day) by 6.5 percentage points. Again, the effects of the 2001 trade liberalization on male children in rural Gansu were generally positive, with increased likelihood of non-agricultural work with formal contracts. Gender differentials of the effects of export demand shocks are generally statistically significant.

Panel C tracks later-life health and socio-economic status. We create an index of psychological problems (based on survey questions on depression, anxiety, loneliness, etc.), and greater exposure to trade liberalization increases such problems for girls by 0.06 SD, while it improves boys' mental health by 0.09 SD. Boys have significantly better health markers in terms of height and BMI, and the effects of girls is always significantly worse than the effects on boys.

Another notable gender difference is the effect of trade liberalization on children's later-life migration propensities. Boys exposed to the trade shock during their school years are more likely to work in cities later in life as adults and even obtain an urban *hukou*, but these effects are not evident for girls (p-value of gender gap <0.03). The form of later-life migration also varies by the child's gender: boys exposed to trade liberalization are less likely to leave their own children behind in adulthood, while girls become more likely to separate from their own children. There are indications of inter-generational persistence of parental separation, but only for the daughters of the rural workers who initially benefited from China's accession to the WTO.

Taken together, the pattern clearly evident is that rural parents and their sons benefit from trade liberalization, but daughters are worse off in the long run. Parents migrate and family economic conditions improve significantly. Daughters fare worse *despite* these economic opportunities.

Table 2: The Effects of Trade Liberalization on Children's Outcomes in 2015

| Dep. Var. | Effect on Boys | Effect on Girls | P-value of Diff. | Mean of Dep. Var. |
|---|---|---|---|---|
| **Panel A: Education Outcomes and Skills Later in Life** | | | | |
| Graduated from Full-time Precollege (=1) | 0.0464** | -0.0323** | 0.000 | 0.140 |
| | (-0.0189) | (0.0103) | | |
| Enrolled in Full-time Precollege (=1) | 0.0194 | -0.0406** | 0.000 | 0.151 |
| | (0.0240) | (0.0135) | | |
| Enrolled in First-Tier College (=1) | 0.0130 | -0.0360*** | 0.000 | 0.039 |
| | (0.00804) | (0.00830) | | |
| Completed Junior Middle School (=1) | 0.0641*** | -0.0269* | 0.005 | 0.834 |
| | (0.0165) | (0.0133) | | |
| Drop off High School (=1) | -0.0185 | 0.00755* | 0.249 | 0.041 |
| | (0.0187) | (0.00407) | | |
| Good Mandarin (=1) | 0.0357 | -0.0501* | 0.004 | 0.298 |
| | (0.0324) | (0.0262) | | |
| **Panel B: Labor Market Outcomes** | | | | |
| IHS (Hourly Income) | 0.185 | -0.150** | 0.069 | 1.835 |
| | (0.204) | (0.0552) | | |
| Above Poverty Line (=1) | 0.0519 | -0.0650** | 0.010 | 0.645 |
| | (0.0508) | (0.0206) | | |
| Work in Non-agricultural Sector (=1) | 0.0459** | -0.0117 | 0.059 | 0.765 |
| | (0.0146) | (0.0270) | | |
| Have Formal Contract (=1) | 0.0472** | -0.0130 | 0.000 | 0.414 |
| | (0.0160) | (0.0252) | | |
| **Panel C: Health and Welfare Status** | | | | |
| Psychological Problem Index | -0.0881*** | 0.0592* | 0.000 | 0.001 |
| | (0.0193) | (0.0274) | | |
| Log Height | 0.00760*** | -0.000416 | 0.003 | 5.124 |
| | (0.00131) | (0.00200) | | |
| Height $<$Gender-specific Median | -0.0901** | 0.0685** | 0.011 | 0.451 |
| | (0.0336) | (0.0224) | | |
| Underweight (BMI $<$18.5) | -0.0439*** | 0.0257 | 0.000 | 0.107 |
| | (0.00843) | (0.0174) | | |
| Work in Urban Areas (=1) | 0.0573** | -0.00904 | 0.000 | 0.444 |
| | (0.0222) | (0.0266) | | |
| Move to Cities and Get Urban Hukou (=1) | 0.0185*** | -0.00532 | 0.022 | 0.100 |
| | (0.00377) | (0.00658) | | |
| Single Parents (=1) | -0.0464* | 0.0141 | 0.007 | 0.147 |
| | (0.0232) | (0.0141) | | |
| Leaving Children Behind (=1) | -0.0237*** | 0.0429*** | 0.000 | 0.091 |
| | (0.00545) | (0.0130) | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use GSCF 2015 to perform individual-level regressions. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and an interaction between import tariffs, female dummy, $(\overline{Age}_{sch} - Age_{2002})_i$, and an indicator for *hukou* policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences), female dummy and $(\overline{Age}_{sch} - Age_{2002})_i$. The effect on boys is measured by $\beta_2$ in equation 2, and the effect on girls is measured by $\beta_1 + \beta_2$ in equation 2. Above poverty line is a dummy variable for whether a particular individual earns more than 1.9 US dollars per day, which is the poverty line specified by the World Bank. The definition of underweight is based on WHO standard: https://www.who.int/europe/news-room/fact-sheets/item/a-healthy-lifestyle—who-recommendations. We construct an inverse-covariance weighted summary index of various psychological outcomes including depression, anxiety, loneliness, and self-dissatisfaction, and we standardize the psychological index. Robust standard errors clustered at the level of birth location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

### 4.4 Robustness and Specification Tests for the Shift-Share Strategy

In Appendix B, we conduct a battery of tests to examine the validity of our identification strategy, following the guidance from a recent applied econometrics literature on shift-share strategies. Section B.1 Table B1 summarizes the distribution of industry-specific shifters as well as the industry-level exposure weights (i.e. average exposure shares across locations for each industry $S_k$). The distribution of shocks has a mean of 0.34 (which implies that the difference between NTR and non-NTR tariff rates is 32.7 pp on average), a standard deviation of 0.15, and an inter-quartile range of 0.18. The inverse of its Herfindahl index (HHI) $1/\sum_k S_k^2$ is 21.1 across industries,[6] which indicates sufficient and sizeable variation in exposure weights $S_k$.

We implement falsification tests in Section B.2. Table B2 conducts industry balance tests. We examine the potential association between industry-level NTR gaps and a set of potential confounders that may affect international trade between China and other countries. Eight of nine industry-level factors do not have any significant relationship with our shifters. In particular, baseline contract intensity, export licences, input tariffs, and measures of performance (ratio of labor to value-added, ratio of capital of value-added, average return on assets and return on equity) – all defined in Appendix B – do not predict changes in industry-level tariff uncertainty (i.e. NTR gaps) driven by China's accession to WTO. The only exception is industry-level average import tariffs, which appears to be significantly associated with NTR gaps.[7] We follow the guidance in Borusyak et al. (2022) and control for observation-level exposure-weighted mean of import tariffs throughout our analysis. Specifically, we construct shift-share controls that follow equation 1 but replace $NTRGAP_k$ with industry-level baseline import tariffs. So our identification assumption is that our industry-level shocks (NTR gaps) are exogenous, *conditional on* baseline import tariffs.

Table B3 conducts regional balance tests. We assess balance with respect to baseline city-level demographic and education indicators (Panel A) and baseline economic and employment indicators (Panel B). After conditioning on city-level weighted average of base-

[6] We then follow Borusyak et al. (2022) to use the inverse of its Herfindahl index (HHI) $1/\sum_k S_k^2$ to examine whether there is a high concentration of industry exposure. If $1/\sum_k S_k^2$ is low, exposure weights would be so concentrated that only shocks in a few industries drive the variation of shift-share variables.

[7] This could be because China imposed higher import tariffs to protect firms in industries facing a higher export tariff uncertainty, to reduce the competition from foreign firms in domestic market.

line import tariffs, our shift-share variable does not have any significant association with these city-level baseline factors and their changes prior to China's entry into WTO.

Borusyak et al. (2022) shows that the orthogonality between a shift-share variable and an unobserved residual can be represented as the orthogonality between the underlying shocks and a shock-level unobservable, conditional on observed confounding factors. Section B.3 follows the procedure proposed by Borusyak et al. (2022) to show shock-level equivalence results for the main outcomes we will report in this section (children's outcomes in 2015), and recast the conditional orthogonality of the shift-share variable at the shock level. This also allows us to perform estimates using exposure-robust standard errors proposed by Borusyak et al. (2022). Table B5 shows that, if anything, our results are more precisely estimated using the exposure-robust standard errors.

In Section B.4, we follow Goldsmith-Pinkham et al. (2020) to calculate Rotemberg weights to measure the "importance" of each industry in driving the variation of shift-share variables. Table B6 lists top 30 ISIC4 industries regarding Rotemberg weights. Goldsmith-Pinkham et al. (2020) suggest examining the exposure shares of top 5 industries in terms of Rotemberg weights. We re-estimate the effect of trade liberation and control for interactions between the gender of children and location-and industry-specific exposure shares for those top 2 and top 5 industries, respectively. Accounting for potential confounders associated with exposure shares of these "important" industries leaves our main empirical results unaffected (Tables B7 and B8).

### 4.5 Beyond Gansu: Effects of the Trade Shock on Children Nationwide

The longitudinal data from Gansu is useful for tracking longer-run effects of trade liberalization and for delving into mechanisms, but it is important to interrogate whether these results are representative for the rest of China. Results may differ because the nationally representative China Migrant Dynamics Survey data reveals that 60% of children left behind by migrant parents across China grow up without *either parent present*, but female out-migration rates appear a lot lower in Gansu province, where 85% of left-behind children have their mother present at home.

In Table 3, we use a national representative survey–China Family Panel Studies 2010 (CFPS) to revisit the association between trade liberalization and gender inequalities using a sample of 2616 rural children aged $< 16$ in 2002 from across 24 provinces. We find very similar results as in Gansu. Girls with greater exposure to trade demand shocks re-

Table 3: Trade Liberalization and Children's Outcomes: CFPS 2010

| Dep. Var. | Effect on Boys | Effect on girls | P-value of Difference | Mean of Dep. Var. |
|---|---|---|---|---|
| Self-reported Unhappiness (=1) | -0.00357 | 0.00679* | 0.064 | 0.034 |
| | (0.00366) | (0.00365) | | |
| IHS (Years of Education) | -0.0193 | -0.0491*** | 0.213 | 2.664 |
| | (0.0152) | (0.0174) | | |
| Score in Word Test | -0.00809 | -0.0117** | 0.572 | 0.720 |
| | (0.00503) | (0.00571) | | |
| Score in Math Test | -0.00423 | -0.00859 | 0.518 | 0.609 |
| | (0.00550) | (0.00564) | | |
| Bottom 10% in Word Test (=1) | 0.00857 | 0.0177** | 0.335 | 0.101 |
| | (0.00599) | (0.00789) | | |
| Bottom 10% in Math Test (=1) | 0.00680 | 0.0207*** | 0.165 | 0.107 |
| | (0.00719) | (0.00706) | | |
| Self-reported bad health (=1) | -0.00393 | 0.00718** | 0.026 | 0.026 |
| | (0.00436) | (0.00356) | | |
| Blood Disease (=1) | -0.000437 | 0.00558** | 0.019 | 0.002 |
| | (0.000364) | (0.00263) | | |
| Respiratory Disease (=1) | -0.00522* | 0.00616 | 0.011 | 0.027 |
| | (0.00301) | (0.00393) | | |
| Bottom 10% income (=1) | -0.00266 | 0.00786 | 0.040 | 0.050 |
| | (0.00391) | (0.00531) | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use CFPS 2010 to perform individual-level regressions. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and the interaction between import tariffs, female dummy, $(\overline{Age_{sch}} - Age_{2002})_i$, and the indicator for *hukou* policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences), female dummy and $(\overline{Age_{sch}} - Age_{2002})_i$. The effect on boys is measured by $\beta_2$ in equation 2, and the effect on girls is measured by $\beta_1 + \beta_2$ in equation 2. In CFPS 2010, there is a survey question: how happy are you?. (1=very unhappy; 2=unhappy, ···, 5=very happy). Based on this question, we define a dummy for unhappiness: D=1, if the answer is 1-2; =0, if the answer is 3-5. There is also a survey question: how would you rate your health status?. (1=healthy, ···,3=relatively unhealthy, 4=unhealthy, 5=very unhealthy). Based on this question, we define a dummy for bad health: D=1, if the answer is 3-5; =0, if the answer is 1-2.We normalize the scores in word test and math test to one. Robust standard errors clustered at the level of birth location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

port being unhappier and in worse physical health, complete fewer years of education, and perform worse in word and math tests administered by surveyors. In contrast, the same trade demand shocks have little meaningful associations with boys' outcomes. These gender differences in the effects of trade liberalization are sometimes statistically significant, as shown in column 3.

# 5 Why Trade Liberalization Disproportionately Hurt Girls

## 5.1 Girls Are More Likely to Be Left Behind

It is quite evident in the descriptive data that there are gender differences in the propensity to leave children behind whenever migration opportunities arise for parents. In rural areas with above-average exposure to trade liberalization, 29.5% of girls tracked in GSCF 2004 were left behind by parents, but only 22.9% of rural boys were (p-value of gender gap $<0.05$). No such gender difference exists in the other half of the sample with lower exposure to trade shocks, where only 11.3% of rural girls and 12% of rural boys were left behind by parents.

Table 4 further performs regression analysis to examine whether the children tracked in GSCF 2004 were left behind in rural areas by migrant parents when new economic opportunities arose in cities, and whether that propensity varies by child gender. A one SD increase in exposure to trade demand shocks increases the likelihood that a boy is left behind by his migrant parent by 4.4 percentage points. In contrast, greater exposure to trade increases parental separation by 7.5 percentage points for girls. This gender differential is highly statistically significant. These represent *large* effects of trade liberalization, because only 20.1% of children in our sample were left behind by parents.

If son preference leads Chinese parents to employ different fertility-stopping rules depending on their child's gender, then gender may be systematically related to family size. We therefore add fixed effects for the number of children in all specifications in Table 4.[8]

## 5.2 Insights from Early Life Outcomes for Children

If girls are more likely to be left behind by parents when they move to cities to take factory jobs, that could lead to larger adverse effects on their mental health. Moreover, young girls' mental health is more vulnerable to parental absence than young boys', according to literature in psychology and sociology (Culpin et al., 2013; Wu et al., 2019; Zhao and

[8]Column 1 controls for prefecture-tier fixed effects, since rural areas adjacent to the city of Lanzhou may differ from other prefectures. We add age cohort fixed effects to account for age-specific unobservables. Column 2 controls for fixed effects for age cohort by gender. In columns 3-5, we control for fixed effects for their *hukou* prefecture to account for location-specific unobservables associated with exposure to trade liberalization. These fixed effects absorb our measure of exposure to trade liberalization ($NTR_c$), so only the differential gender effect is identified in these columns.

Table 4: The Effects of Trade Liberalization on Parental Absence

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Dependent Variable: | Being Left behind by Parents(=1) | | | | |
| Standardized NTR × Female | 0.0311*** | 0.0333*** | 0.0371*** | 0.0369*** | 0.0404*** |
| | (0.00884) | (0.00863) | (0.0101) | (0.0104) | (0.00720) |
| Standardized NTR | 0.0443** | 0.0403* | | | |
| | (0.0148) | (0.0214) | | | |
| Female | 0.0418** | | 0.0460*** | | |
| | (0.0138) | | (0.0140) | | |
| Prefecture-tier FE | Yes | Yes | No | No | No |
| Prefecture FE | No | No | Yes | Yes | Yes |
| Cohort FE | Yes | No | Yes | No | No |
| Cohort by Gender FE | No | Yes | No | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes | Yes |
| Trade Control | No | Yes | No | No | Yes |
| Observations | 1,296 | 1,296 | 1,296 | 1,296 | 1,296 |
| Adjusted R-squared | 0.027 | 0.042 | 0.077 | 0.082 | 0.084 |
| Mean of Dep. Var | 0.201 | 0.201 | 0.201 | 0.201 | 0.201 |

*Notes*: We use GSCF 2004 to perform individual-level regressions. The dependent variable is an indicator for whether a particular child had been separated from parents for no less than three months in 2004. We control for indicators for whether grandparents are alive (two indicators for mother side and father side, respectively), fathers' years of schooling. In column 5, we control for the interaction between import tariffs, female dummy, and the indicator for *hukou* policy restrictiveness in nearby cities. Robust standard errors clustered at the level of birth location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Yu, 2016).[9] And mental disorders in adolescence are more likely to carry over into young adulthood for girls than for boys (Patton et al., 2014). We return to earlier (2004 and 2009) rounds of the GSCF survey to examine whether exposure to trade liberalization had gender-differentiated effects on mental health and education outcomes earlier in life. We use the same empirical strategy outlined in equation 2.

Table 5 Panel A presents children's outcomes when they were 12-18 years old in 2004, two years after China's accession to WTO. While trade liberalization does not have any meaningful effect on boys' mental health, a one SD greater exposure to trade shocks per year leads to a 0.04 SD increase in the psychological problem index for girls. Trade liberalization also hurts girls' human capital acquisition in various ways. Girls with greater

[9] Left-behind girls are significantly more likely to suffer from learning anxiety, social anxiety, self-accusation, and phobia compared to left-behind boys (Wu et al., 2019; Zhao and Yu, 2016). Culpin et al. (2013) documents that father absence in early childhood increases the risk for adolescent depressive symptoms, and the effect is stronger for girls than for boys.

exposure to trade shocks are less willing to pursue high school education, perform worse in math, and are more likely to do house work, to cut class, to be distracted in class due to hunger and to drop out school. The gender difference in the effects of trade liberalization is often statistically significant.

Table 5 Panel B reports the effect in 2009, seven years after China's accession to WTO. At this stage, the tracked Gansu children were 20 years old on average. We document a similar gendered pattern. Owing to trade demand shocks in nearby cities, girls have worse mental health, they are less likely to be enrolled in a key high school, and they perform worse in school. In contrast, boys with greater exposure to export demand shocks are more likely to complete high school, receive greater education expenditure, and are less likely to smoke.

Our longitudinal data provide strong indications that new economic opportunities for parents lead to more girls getting left behind, and this harms their early-life mental health and education outcomes. Section 4 showed that this ultimately translates into disadvantaged socioeconomic status in adulthood.

### 5.3 Gender Differences in Housework for Left-Behind Children

The gender gaps in outcomes from new migration opportunities could also emerge if young girls are treated more poorly at home compared to young boys when children are left behind by parents. This could be because grandparents – who make decisions when parents leave – represent an older generation that is even more gender-biased.[10] We use the 2004 round of the GSCF survey to examine gender differences in housework responsibilities, and whether that changes when migrant parents leave children behind. Results in Table 6 show that (a) left-behind children spend almost twice as much time on housework compared to children whose parents live with them in rural Gansu, (b) girls spend significantly more time on housework than boys when parents are around, and (c) the gender gap in housework magnifies when parents leave. In particular, girls do 1.7 hours more housework per week than boys when their parents migrate away, and 1.3 hours more housework when parents stay with them in the village.

[10] China's Women Social Status Survey 2000 shows that older people are more likely to express gender-biased views.

## Table 5: The Effects of Trade Liberalization on Early-Life Outcomes in 2004 and 2009

| Dep. Var. | Effect on Boys | Effect on girls | P-value of Diff. | Mean of Dep. Var. |
|---|---|---|---|---|
| **Panel A: Trade Liberalization and Children's Outcomes in 2004** | | | | |
| Psychological Problem Index | -0.00559 | 0.0410*** | 0.053 | 0.001 |
| | (0.0236) | (0.00806) | | |
| Willing to Study in High School(=1) | 0.00823 | -0.0273*** | 0.009 | 0.911 |
| | (0.0111) | (0.00468) | | |
| Bad Math (=1) | -0.0192 | 0.0188*** | 0.004 | 0.063 |
| | (0.0108) | (0.00346) | | |
| Time on housework per day | -0.0758 | 0.0882 | 0.000 | 0.525 |
| | (0.0537) | (0.0618) | | |
| Time on Earning Money per day | -0.0392 | -0.00647 | 0.282 | 0.132 |
| | (0.0471) | (0.0621) | | |
| Do Housework (=1) | -0.0470** | 0.0453* | 0.001 | 0.302 |
| | (0.0172) | (0.0237) | | |
| Earn Money (=1) | -0.0140 | -0.00320 | 0.154 | 0.027 |
| | (0.00791) | (0.00769) | | |
| Often Cut class (=1) | -0.00918* | 0.0126*** | 0.000 | 0.011 |
| | (0.00430) | (0.00337) | | |
| Often Be Distracted in Class due to Hunger (=1) | 0.000449 | 0.0188*** | 0.000 | 0.015 |
| | (0.00387) | (0.00264) | | |
| Drop out of School(=1) | 0.00396 | 0.0300** | 0.092 | 0.099 |
| | (0.0183) | (0.0127) | | |
| **Panel B: Trade Liberalization and Children's Outcomes in 2009** | | | | |
| Psychological Problem Index | 0.00151 | 0.103*** | 0.019 | 0.000 |
| | (0.0484) | (0.0252) | | |
| Complete High School (=1) | 0.0370*** | 0.0127 | 0.079 | 0.185 |
| | (0.0101) | (0.0181) | | |
| Enrolled in Professional High School (=1) | 0.0334** | 0.00900 | 0.002 | 0.029 |
| | (0.0111) | (0.00779) | | |
| Enrolled in key high school (=1) | 0.0247* | -0.0185* | 0.004 | 0.113 |
| | (0.0131) | (0.00869) | | |
| Pass High School Entrance Exam (=1) | 0.0664*** | -0.0146 | 0.000 | 0.379 |
| | (0.0145) | (0.0107) | | |
| Good Academic Performance (=1) | -0.00610 | -0.0535*** | 0.113 | 0.117 |
| | (0.0154) | (0.0143) | | |
| IHS (Education Expenditure) | 0.0605** | -0.0332 | 0.074 | 5.629 |
| | (0.0244) | (0.0258) | | |
| Willing to Receive College/Precollege Education (=1) | 0.0198 | -0.0131 | 0.008 | 0.163 |
| | (0.0213) | (0.0164) | | |
| Drop out of School due to Weariness (=1) | -0.0282** | 0.00349 | 0.064 | 0.194 |
| | (0.0124) | (0.0108) | | |
| Smoke (=1) | -0.0343** | 0.000870 | 0.061 | 0.080 |
| | (0.0152) | (0.00241) | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. Panel A uses GSCF 2004 to perform individual-level regressions. Panel B uses GSCF 2009 to perform individual-level regressions. In both Panels A and B, we control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and the interaction between import tariffs, female dummy, $(\overline{Age}_{sch} - Age_{2002})_i$, and the indicator for *hukou* policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences), female dummy and $(\overline{Age}_{sch} - Age_{2002})_i$. The effect on boys is measured by $\beta_2$ in equation 2, and the effect on girls is measured by $\beta_1 + \beta_2$ in equation 2. In Panel A, we construct an inverse-covariance weighted summary index of various psychological outcomes including depression, bad temper, combativeness, tiredness, and self-dissatisfaction, and we standardize the psychological index. In GSCF 2004, there is a survey question: Compared to your peers, how do your rate your Math ability?. (1=very poor; 2, ···, 5=very good). Based on this question, we define a dummy for bad math: D=1, if the answer is 1; =0, if the answer is 2-5. In Panel B, we construct an inverse-covariance weighted summary index of various psychological outcomes including unhappiness, bad temper, combativeness, poor independence, and self-dissatisfaction, and we standardize the psychological index. In GSCF 2009, there is a survey question: Compared to your classmates, how do your rate your academic performance?. (1=very good; 2=good, ···, 4=poor). Based on this question, we define a dummy for good academic performance: D=1, if the answer is 1-2; =0, if the answer is 3-4.Robust standard errors clustered at the level of birth location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Weekly Hours of Housework by gender

| Dependent Variable: | Weekly Hours of Housework | | | |
|---|---|---|---|---|
| | Left-behind | Stay in rural | Left-behind | Stay in rural |
| Female | 1.666*** | 1.287*** | 1.651*** | 1.267*** |
| | (0.296) | (0.184) | (0.372) | (0.197) |
| Coeff diff p-value | 0.000 | | 0.000 | |
| Observations | 368 | 1,464 | 368 | 1,464 |
| Adjusted R-squared | 0.0284 | 0.0400 | 0.0669 | 0.0897 |
| City-tier FE | Yes | Yes | No | No |
| City FE | No | No | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Mean of Dep. Var. | 2.927 | 1.676 | 2.927 | 1.676 |

*Notes*: We use GSCF2004 to perform individual-level regressions. "Coeff diff p-value" reports the p-value of a test of equality of the coefficient on the female dummy between children staying with their parents in the village and those left behind by their parents, using the Fisher's permutation test (following Cleary (1999), Brown et al. (2010) and Keys et al. (2010)). This bootstraps to calculate empirical p-values that estimate the likelihood of obtaining the observed differences in coefficient estimates if the true coefficients are, in fact, equal. Columns 1 and 3 use the sample of children who are left behind by parents. Columns 2 and 4 use the sample of children whose parents stay with them in the village. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

## 5.4 Gender Differences in Remittances for Left-Behind Children

Both parental time and money are useful for a child's human capital development, so gender differentials could also stem from any difference in the money remitted back to left-behind sons versus daughters. We study remittance patterns using the China Migrants Dynamic Survey (CMDS) 2011-2012, and limit the sample to families with children left behind in the village. Table 7 displays the patterns of remittances sent back by migrant parents as a function of the number of girls and boys left behind. Parents remit over 1000 RMB for every additional son left behind, but only about 70% as much for every additional daughter left behind. This gender difference is highly statistically significant (p<0.002).[11] Panel B shows the gender difference in remittance receipts gets even larger when children reach junior-middle-school age. In this sample, remittances are 50% lower for girls.

In summary, girls are more likely to be left behind, left-behind daughters are forced to do more housework, and receive less remittances. Receiving less time *and* less money from parents can explain the striking gender-differentiated effects in early and later life mental,

[11] The various columns control for city-by-year fixed effects (to absorb unobservables at migrant parents' destination city), or a triple interaction between city-, year- and *hukou* province- fixed effects (to absorb any differences in attitudes towards boys' versus girls' between migrants from different areas), or age cohort fixed effects.

physical health, and education outcomes in which sons benefit and daughters suffer when parents migrate in response to new economic opportunities.

**Age of Taking Factory Jobs:** Yet another possibility, for which we find no support in the data, is that trade shocks induce more girls to drop out of school early to take factory jobs in cities compared to boys.[12] We estimate equation 2 to assess how exposure to trade liberalization affects the age of first job, which was recorded in the GSCF 2009 survey wave. Table A7 shows that there is no effect on either boys' or girls' propensity to start working before the age of 14, 15, or 16, so this does not appear to be a relevant mechanism.

Table 7: Remittance Sent to Rural Children by Gender

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent Variable: | The Amount of Remittance | | | |
| **Panel A: Full Sample** | | | | |
| Number of boys | 1,025*** | 1,011*** | 1,089*** | 1,075*** |
| | (124.4) | (124.5) | (132.6) | (131.7) |
| Number of girls | 691.3*** | 676.4*** | 745.4*** | 735.4*** |
| | (124.5) | (125.5) | (129.9) | (130.7) |
| Coeff diff p-value | 0.002 | 0.002 | 0.001 | 0.002 |
| Observations | 39,556 | 39,556 | 39,556 | 39,556 |
| Mean of Dep. Var. | 7,937 | 7,937 | 7,937 | 7,937 |
| **Panel B: Junior Middle School Age** | | | | |
| Number of boys | 1,758*** | 1,757*** | 1,823*** | 1,822*** |
| | (237.0) | (237.4) | (229.5) | (229.6) |
| Number of girls | 993.9*** | 990.7*** | 917.7*** | 918.3*** |
| | (187.9) | (189.4) | (182.1) | (182.2) |
| Coeff diff p-value | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 8,018 | 8,018 | 8,018 | 8,018 |
| Mean of Dep. Var. | 8,094 | 8,094 | 8,094 | 8,094 |
| Household Control | Yes | Yes | Yes | Yes |
| City FE×Year FE | Yes | Yes | No | No |
| City FE×Year FE×*Hukou* Province FE | No | No | Yes | Yes |
| Cohort FE | No | Yes | No | Yes |

*Notes*: Panel A shows results for all children aged below 16, and panels B shows results for children at junior middle school age. "Coeff diff p-value" reports the p-value of a test of equality of the coefficient on the number of boys and the coefficient on the number of girls. Data come from China Migrants Dynamic Survey (CMDS). We use the CMDS 2011 and 2012 to perform estimation as only the two waves of CMDS contain information about remittance. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[12] Heath and Mobarak (2015) actually finds the exact opposite effect in which manufacturing growth led to *more* schooling investments for girls in Bangladesh.

# 6 Migration Restrictions: Why Daughters are Left Behind

## 6.1 Institutional Background for Empirical Identification

To understand why daughters suffer when trade liberalization creates new economic opportunities for rural workers, we need to understand why parents choose (or are forced) to leave their children behind in rural areas when they migrate to cities to take advantage of those opportunities. Further, we need to explore why rural parents are disproportionately more likely to separate from their daughters than their sons.

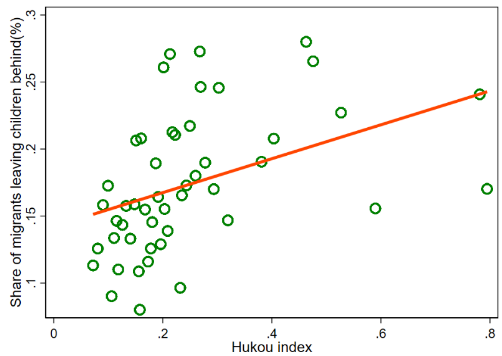
**Accessing Urban Schools with a Rural *Hukou* is Costly:** To keep their school-aged children with them in the city, migrant parents who have not secured an urban *hukou* either have to pay an extra fee called *zanzhufei*, or send their children to "migrant schools" set up in cities specifically for poor migrant children without a local *hukou*. Migrant schools are of poorer quality than urban public schools.[13] They also charge fees that are expensive for migrants, but these are lower than *zanzhufei* charged by public schools for migrant students. Many cities closed migrant schools in recent years (Table A8) forcing parents to pay steeper *zanzhufei* if they want to keep their children with them.

School access and *zanzhufei* are important components of *hukou* policy restrictions that induce migrants to leave their children behind in rural areas, but they are not the only relevant constraint. We use a more general measure of *hukou* policy stringency for our empirical work, as a proxy for the general difficulty migrant parents face in keeping their children with them. *Zanzhufei* is indeed higher in cities that have more stringent *hukou* restrictions (Figure A4). There is a general positive association between this *Hukou* policy stringency measure and migrants' propensity to leave their children behind (Figure 2).

**Junior Middle Schools More Restricted than Primary Schools:** Education is compulsory in China. By law, parents must enroll their children in primary school if they turn six by September 1 in a given year and must enroll them in junior middle school if they turn 12 by that day. Primary school covers grades 1–6 and junior middle school (roughly equivalent to "middle school" in the US) provides the last 3 years of the nine-year compulsory education required for all Chinese citizens. *Hukou* restrictions are a much bigger constraint on migrant families with junior middle school-aged children. Junior middle schools charge a

[13]Teachers in migrant schools often do not have adequate credentials or experience to obtain jobs in city public schools. Migrant schools are often overcrowded and have worse infrastructure.

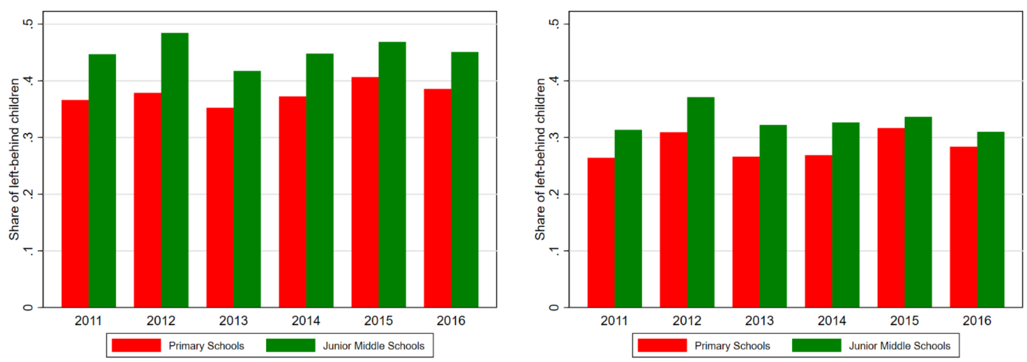
Figure 2: *Hukou* Index and the Share of Migrants Leaving Children Behind




*Notes*: This figure shows the relationship between the share of migrants leaving children behind [Data from the *China Migrants Dynamic Survey*] and the stringency of *hukou* regulations in migrants' destination cities [Data from Zhang et al. (2019)]. Cities are grouped into fifty groups according to the quantile of the *hukou* index. The vertical axis denotes the mean value of the share of migrants leaving children behind and the horizontal axis denotes the mean value of the *hukou* index in each quantile.

substantially higher amount of *zanzhufei* than primary schools (Table A9), and the number of available school seats is also more limited.

Figure 3: Share of Left-behind Children by School Age




(a) Highly restrictive cities

(b) Less restrictive cities

*Notes*: We divide cities into two groups based on the stringency of *hukou* restrictions. Highly restrictive cities are those in which the *hukou* index is above the national mean, and less restrictive cities are those in which the *hukou* index is below the national mean. *Hukou* index measures the stringency of *hukou* regulation and the difficulty for migrants to obtain local *hukou*. Data on left-behind children come from the *China Migrants Dynamic Survey (CMDS)*, and data on the *hukou* index come from Zhang et al. (2019).

Figure 3 shows that migrant workers are always more likely to leave middle-school-aged children behind compared to primary-school-aged children. They are also more likely to leave children of all ages behind when they migrate to cities with more stringent *hukou* restrictions.[14] We will use both dimensions of variation to construct a triple difference identification strategy to explore how parents' decisions to leave children behind differ across sons and daughters.

**The 2014 Migrant Population Control Policy Increased Restrictions:** In 2014, the State Council of China issued "National New-Type Urbanization Planning (2014-2020)" and "Opinions on Promoting the *Hukou* System Reform", which urged mega-cities - categorized as those with a population of over five million in the city central district area - to "exercise strict control over the population". Those mega-cities were required to set a population target by 2020, and local government performance would be evaluated against that target. As a result, in 2014 local governments in mega cities start strongly restricting the inflow of unskilled migrants by imposing even more stringent restrictions on school enrollment for migrant children. Conversely, the same policies led to a relaxation of *hukou* restrictions in small and medium-sized cities. We will examine how the leave-behind decisions of migrant parents attached to mega-cities changed after 2014, relative to migrants attached to other cities close in size but below the population cutoff for mega-cities.

### 6.2 Effects of *Hukou* Policy on Parents' Migration Choices

Each parent $i$ with a rural *hukou* for prefecture $c$ has three $Choices_{ic}$ which we index by $n$ ($n$=1,2,3): they can decide to (1) remain in the village with their children, or (2) migrate to a city leaving their children behind in the rural area, or (3) migrate with their children. We construct a multinominal logit model to analyze how the restrictiveness of *hukou* regulations in potential destination cities affect parents' propensity to choose $n$=1, 2 or 3. The multinomial logit is the appropriate modeling framework to capture parents' simultaneous decisions on whether to migrate, and whether to take children with them. We embed a difference-in-differences type setup into the multinomial logit model, to compare lax-*hukou* versus *hukou*-restrictive cities, and primary versus middle school-aged children

[14]Guangzhou – a popular destination for migrant workers – offers an interesting case study on what happens to migrant children as they transition from primary to middle school age (Table A10). In 2012, about 53% of the children in migrant households studied in primary schools in Guangzhou, but only around 32% of junior middle school aged migrant children stayed in the city. Only 20% took the high school entrance exam.

for whom urban schooling costs differ. We examine whether parents' propensity to leave children behind shifts at the age cut-off for middle school entrance, by limiting our sample to children whose ages are just below versus above this cut-off. The narrower age range better accounts for unobservables potentially correlated with age that might affect parental separation choices. Our model therefore has a regression discontinuity flavor around the age threshold. Studying parents' choices separately for male and female children creates the triple difference. We run a linear version of this model in the next section to formally implement the triple difference. Indirect utility as defined below leads to a trichotomous choice regression we estimate:

$$\begin{aligned} logV_{ic}(Choice = n) = &\psi_{0n} + \psi_{1n} SchoolAged_i \times Des\_Hukou_c \times Female_i + \\ &\psi_{2n} SchoolAged_i \times Des\_Hukou_c + \psi_{3n} SchoolAged_i \times \\ &Female_i + \psi_{4n} Des\_Hukou_c \times Female_i + \\ &\psi_{5n} SchoolAged_i + \psi_{6n} Des\_Hukou_c + \psi_{7n} Female_i + \\ &\psi_{8n} T_i \times SchoolAged_i + \psi_{9n} T_i + \varphi_{num} + v_{icn} \end{aligned} \tag{3}$$

$SchoolAged_i$ is an indicator for whether child $i$ is above the enrollment age for junior middle school, based on their exact date of birth relative to the September 1 school entry date. $Des\_Hukou_c$ denotes the stringency level of restrictions that rural migrants would face in cities near their origin location $c$, which is defined the inverse distance-weighted sum of the *hukou* index across potential destination cities, $\sum_d \left(\frac{1}{dist_{dc}} Hukou\ Index_d\right)$. We assign non-zero weights only to potential destination cities that are located within a 400 km radius of *hukou* location $c$, but our empirical results are not sensitive to this choice. As in Section 4.3, we control for fixed effects for the number of children ($\varphi_{num}$).

Our primary variable of interest is the triple interaction between $SchoolAged_{it}$, $Female_{it}$ and $Des\_Hukou_c$, which examines whether there is any differential shift in the probability of leaving children behind exactly at the junior middle school enrollment age ($T_i = 0$)[15], in rural areas near cities with more restrictive *hukou* policies. The triple interaction with gender identifies whether this decision to separate from children varies by the child's gender.

[15]The running variable $T_i$ is the gap between the child's age and the middle school enrollment age cutoff. Following Imbens and Lemieux (2008) and Gelman and Imbens (2019), we use a local linear control function for the running variable $T_i$, and select two years as the bandwidth. Results are robust to alternative bandwidths and control functions for $T_i$.

Table 8: Multinomial Logit Results

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Panel A | Coefficient estimates from multinomial logit model | | | | | |
| | Sample of Daughters | | Sample of Sons | | Full Sample | |
| Dependent variable | All in Rural | Separation | All in Rural | Separation | All in Rural | Separation |
| School-aged | -0.133 | 0.276 | -0.105 | -0.238 | -0.146 | -0.115 |
| | (0.165) | (0.295) | (0.146) | (0.249) | (0.121) | (0.198) |
| School-aged × Standardized weighted *hukou* index | 0.105 | 0.402*** | 0.0548 | 0.0260 | 0.0542 | 0.0258 |
| | (0.0928) | (0.140) | (0.101) | (0.179) | (0.101) | (0.179) |
| School-aged × Standardized weighted *hukou* index × Female | | | | | 0.0511 | 0.374** |
| | | | | | (0.131) | (0.190) |
| Observations | 9,020 | 9,020 | 10,640 | 10,640 | 19,660 | 19,660 |
| Panel B | Marginal Effect on different choices of parents | | | | | |
| | Sample of Daughters | | | Sample of Sons | | |
| Dependent variable | All in Rural | Separation | All in City | All in Rural | Separation | All in City |
| School-aged | -0.0271 | 0.0178 | 0.00928 | -0.00333 | -0.00711 | 0.0104 |
| | (0.0179) | (0.0127) | (0.0140) | (0.0175) | (0.0111) | (0.0134) |
| School-aged × Standardized weighted *hukou* index | -0.00355 | 0.0139** | -0.0104 | 0.00599 | -0.00109 | -0.00490 |
| | (0.00987) | (0.00607) | (0.00796) | (0.0142) | (0.00917) | (0.00901) |
| Observations | 9,020 | 9,020 | 9,020 | 10,640 | 10,640 | 10,640 |
| Number of children FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear | Linear | Linear |

*Notes*: We estimate a multinomial logit model. All in rural represents the choice of staying in the village with children. Separation represents the choice of migrating to cities and leaving children behind. All in city represents the the choice of migrating to cities with children. We use the choice of migrating to cities with children as the base category. The bandwidth is two years. We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. Data come from 2010 Population Census. Robust standard errors clustered at the *hukou* prefecture level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

*Hukou* policies in potential destination cities may be correlated with other city characteristics (e.g. population density, local industrial policy), so the main effect of $Des\_Hukou_c$ cannot be interpreted causally. Parents' decisions to separate from children may be related to child age and gender for a variety of reasons (e.g. safety considerations), so the coefficients on the running variable $T_i$ and the main effect of $Female_i$ are not easily interpretable either. Controlling for those main effects, the triple interaction term at the school-age cutoff identifies whether parents' decisions to migrate and to separate from children changes exactly when the cost of keeping children with them increases discontinuously near *hukou*-restrictive cities, *and* whether that varies by the gender of the child.

Table 8 Panel A reports the coefficient estimates of the multinomial logit model, and Panel B reports the corresponding marginal effects. In Panel A, option 3 ("migrate to a city with children") is the omitted category against which RHS variables' effects on options 1 (don't migrate) and 2 (migrate without children) are compared. We first split the sample between boys and girls. The child's age, or its interaction with *hukou* policy restrictiveness has no effect on parents' decision to migrate versus stay in the rural area in either sample (columns 1 and 3). But the interaction has a significant effect on parents' propensity to separate from *daughters* as opposed to migrating *with* the child, as shown in column 2. Marginal effects computed in Panel B show that daughters who cross the age threshold for middle school entry are 1.4 percentage points more likely to separate from parents for every one standard deviation increase in the stringency of *hukou* restrictions in nearby cities. This discontinuous jump in separation from daughters exactly when schooling becomes more expensive is sizable: it represents a 24% jump in the probability of separation, because overall, 5.9% of primary-school-aged daughters with rural *hukou* are left behind in China.

There is no such effect on boys in Table 8 Panel A column 4. In contrast to daughters, elevated barriers to enter junior middle school do not induce rural parents to leave *sons* behind in their rural hometown, irrespective of the stringency of *hukou* restrictions in nearby cities. Panel A Columns 5 and 6 combine the boys' and girls' samples, and the triple interaction shows that the discontinuous jump in the probability of separation from daughters is indeed statistically larger than the corresponding effect on sons. When faced with restrictions on children's educational opportunities in cities, rural parents appear more willing to separate from daughters than from sons. Although China's *hukou* regulations are not gender-specific in intent, they create a gendered inequity.

Panel A columns 1, 3, 5 in Table 8 show that changes in children's urban schooling access does not affect parents' decision on *whether* to migrate for work. This is not surprising, given the vast wage differences between rural and urban areas (Appendix Figure A6). Instead, the decision on whether to bring their children with them or not is the margin that adjusts as schooling costs change.[16]

### 6.3 Are Children Left Behind without *Either* Parent Present?

In Table 9 we study four choices for the household: Stay in the rural origin, migrate with children, one parent migrates leaving the child behind, or *both parents* migrate and leave the child behind. We find that about 60% of the discontinuous jump in the propensity to leave daughters behind when they turn middle-school age near *hukou*-restrictive cities are cases where the daughter is left behind *without either parent present* in the rural area. Such cases account for 0.8 of the 1.4 percentage point effect reported in Table 8. This is relevant

[16]Table A11 shows that these results are robust to RD design variations in which we extend the bandwidth or use a quadratic control function for the running variable.

because the emotional toll and developmental burden on children are likely larger when both parents are absent (Zhang et al., 2014). Other descriptive data from China show that in such cases, grandparents are most often asked to take care of children left behind without either parent in rural areas.

Table 9: Marginal Effects on the Probability that One versus Both Parents are Away

| Dependent variable | All in Rural | One Parent is Away | Both Parents are Away | All in City |
|---|---|---|---|---|
| **Panel A: Female** | | | | |
| School-aged × Standardized weighted *hukou* index | -0.00346 | 0.00603 | 0.00783 | -0.0104 |
| | (0.00993) | (0.00425) | (0.00485) | (0.00794) |
| Observations | 9,020 | 9,020 | 9,020 | 9,020 |
| **Panel B: Male** | | | | |
| School-aged × Standardized weighted *hukou* index | 0.00654 | 0.00196 | -0.00364 | -0.00486 |
| | (0.0137) | (0.00616) | (0.00445) | (0.00905) |
| Observations | 10,640 | 10,640 | 10,640 | 10,640 |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear |

*Notes*: We estimate the average marginal effects on the probability of parents' choices. All in rural represents the choice of staying in the village with children. One parent is away represents the choice of leaving children behind with one parent present. Two parents are away represents the choice of leaving children behind without either parent present. All in city represents the the choice of migrating to cities with children. The bandwidth is two years. We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. Data come from *2010 Population Census*. Robust standard errors clustered at the *hukou* prefecture level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

### 6.4 A Linear Model of Parents' Decisions on When to Leave Children Behind

Since the decision on *whether* to migrate at all appears unaffected by the variables of interest (Table 8), we can take advantage of more detailed data from a much larger sample of migrants in the China Migrant Dynamic Survey (CMDS) to study migrants' child-separation decisions. This allows us to adopt a linear framework instead of multinomial, which has the added advantage that the triple difference identification with the age cutoff has a more standard interpretation. We can also add a variety of fixed effects in a linear model to address identification concerns,[17] and we can also conduct the standard identification tests for

[17]We add city-by-year fixed effects to control for industrial structure and economic development plans of local government, etc. that may be correlated with the city's *hukou* policies. We control for birth cohort fixed effects to account for any changes in other policies (e.g. the One Child Policy) that affect child outcomes. We control for fixed effects for the number of children in case child gender is correlated with family size. In other columns we control for a triple interaction between city-, year- and *hukou* province- fixed effects to absorb any differences in attitudes towards boys' versus girls' education between migrants from different areas.

the age cutoff, as suggested by the econometrics literature on regression discontinuities.[18]

Table 10 shows that across all specifications for daughters, the interaction of the above-enrollment-age indicator and the high-*hukou*-restriction indicator is statistically significant, and the coefficient implies that a girl becomes 3.2-3.5 percentage points more likely to be left behind exactly when she reaches the legal enrollment age for junior middle school and her parents work in a city with restrictive *hukou* policy.[19] 34% of girls in migrant households in China are left behind in rural areas, so the discontinuous jump at that age-cutoff represents a 10% increase at the mean. The coefficient on the above-enrollment-age dummy is close to zero, which suggests that the discontinuity does not exist for parents who migrated to cities with relatively relaxed *hukou* policies.

Across all specifications for sons, both the above-enrollment-age indicator and its interaction with the high-*hukou*-restriction indicator are statistically indistinguishable from zero. Table A15 formally demonstrates that the school-age discontinuity in restrictive *hukou* cities is statistically larger for girls than it is for boys. As with the 2010 census data in Section 6.2, the CMDS data also shows that migrant parents appear to leave their daughters rather than their sons in their rural hometown in response to strict *hukou* restrictions, whereas there is no obvious gender bias for parents in cities with relaxed *hukou* policies.

Results remain similar when we re-run these regressions limiting the sample to first-born children only (Table A16), given concerns about sex selection at higher birth orders.[20] Penalties for violating One Child Policy (OCP) guidelines vary by province and ethnicity, and our results are robust to controlling for triple interactions between parents' ethnicity fixed effects, cohort fixed effects, and parents' *hukou*-province fixed effects (which governs the OCP guidelines they are subjected to) (Table A17).

[18]Potential concerns include (a) parents change destinations or try harder to obtain a local *hukou* when their children reach middle school enrollment age. Table A12 directly tests for this and finds that parents' migration decisions or their probability of getting a local *hukou* do not meaningfully change at that school-age cutoff; (b) perhaps families disappear from our dataset entirely due to changes in their migration choices. To explore, we follow Cattaneo et al. (2020) and perform a data-driven manipulation test, in which we compare the density of observations around the RD cutoff. As reported in Table A13, we find no discontinuity in the sample distribution at the school-age cutoff for either male or female children. This mitigates concerns about "sorting" (e.g. changing *hukou* location) based on their child's school entry date.

[19]Table A14 shows that the results remain similar under RD design variations in which we extend the bandwidth or use a quadratic control function for the running variable.

[20]Gender ratio of *first-born* children in China matches biological expectations (Almond et al., 2019).

Table 10: School Enrollment Age and Left-behind Children

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | | | | | |
| | Female | Male | Female | Male | Female | Male | Female | Male |
| School-aged × Highly restricted cities (=1) ($\rho 1$) | 0.0320** | 0.00356 | 0.0326** | 0.00469 | 0.0350** | 0.00904 | 0.0355** | 0.0102 |
| | (0.0145) | (0.0149) | (0.0144) | (0.0148) | (0.0145) | (0.0171) | (0.0144) | (0.0168) |
| School-aged ($\rho 2$) | -0.00365 | 0.000557 | -0.00461 | 0.00105 | -0.00377 | 0.000727 | -0.00639 | 0.000849 |
| | (0.0158) | (0.0135) | (0.0159) | (0.0133) | (0.0176) | (0.0153) | (0.0178) | (0.0152) |
| P-value of $\rho 1+\rho 2$ | 0.0288 | 0.682 | 0.0343 | 0.567 | 0.0143 | 0.375 | 0.0264 | 0.329 |
| Coeff diff p-value | 0.000 | | 0.000 | | 0.000 | | 0.000 | |
| Observations | 31,071 | 40,854 | 31,071 | 40,854 | 31,071 | 40,854 | 31,071 | 40,854 |
| Adjusted R-squared | 0.174 | 0.147 | 0.175 | 0.147 | 0.209 | 0.185 | 0.210 | 0.186 |
| Mean of Dep. Var. | 0.365 | 0.357 | 0.365 | 0.357 | 0.365 | 0.357 | 0.365 | 0.357 |
| Household Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| City FE×Year FE | Yes | Yes | Yes | Yes | No | No | No | No |
| City FE×Year FE×Hukou Province FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Cohort FE | No | No | Yes | Yes | No | No | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear | Linear | Linear | Linear | Linear |

*Notes*: The bandwidth is two years. We use the sample to children who are two years older or younger than the enrollment age of junior middle school. "Coeff diff p-value" reports the p-value of a test of equality of the coefficient on "School-aged × Highly restricted cities (=1)" between the female and male, using the Fisher's permutation test (following Cleary (1999), Brown et al. (2010) and Keys et al. (2010)). This bootstraps to calculate empirical p-values that estimate the likelihood of obtaining the observed differences in coefficient estimates if the true coefficients are, in fact, equal. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. We use a local linear control function for the running variable. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

## 6.5 Alternative Identification using Mega-city Migrant Population Control Policy

Our use of cross-city variation in the stringency of *hukou* restrictions introduces a concern that unobserved educational preferences drives the choice of city that parents migrate to. We use the 2014 "migrant population control policy" which imposed new restrictions on people who had already migrated to certain "mega cities" to construct another triple difference research design to again test for gender biases in migrant parents' decisions on whether to leave their children behind. The CMDS data spans 2011 to 2016, covering periods before and after this 2014 change in policy. This new policy forced local governments in mega-cities to impose new restrictions on migrants' access to local public services. Since "mega-cities" have a precise definition (population exceeding five million in the city central district), we construct the following specification based on that population threshold:

$$\begin{aligned} Left\ behind_{ijt} =& \alpha_0 + \alpha_1 School\ Age_{it} \times I(Pop > 5\ million)_j \times I(t > 2014) + \\ & \alpha_2 School\ Age_{it} \times I(Pop > 5\ million)_j + \alpha_3 School\ Age_{it} \\ & \times I(t > 2014) + \alpha_4 School\ Age_{it} + \xi_{jt} + \eta_n + \varphi_{num} + v_{ijt} \end{aligned} \quad (4)$$

where $School\ Age_{it}$ is an indicator for children who have reached middle-school enrollment age by year $t$, $I\left(Pop > 5\ million\right)_j$ is an indicator for the mega-cities subjected to the new policy because their central district population at baseline exceeded 5 million, and $I\left(t > 2014\right)$ is an indicator for the post-treatment period. The running variable in this design is the city-specific difference between baseline city population and 5 million, which is absorbed by city-by-year fixed effects ($\xi_{jt}$). We restrict the sample to cities with populations between 2 and 8 million, to limit the impact of unobservable differences between very large and very small cities. We also restrict the sample to parents who made their migration destination choices before 2014, to mitigate any reverse causality concerns about parents choosing destinations based on children's access to urban schools. Given the pre-post 2014 policy variation, we do not need to use the variation in *Hukou* stringency at all.

Columns 1 and 3 of Table 11 show that for female children, the variable of interest – the triple interaction between having reached the junior middle school enrollment age; the indicator for cities with above-5-million population; and the indicator for post-2014 – is positive and significantly different from zero. In response to the new policy, parents who had migrated to mega-cities prior to 2014 become 7 percentage points more likely to leave daughters behind. The second row shows that parents were not exhibiting that behavior before the policy went into effect. Columns 3-4 show that there is no such effect for boys in migrant households. All these coefficients jointly imply that new migration restrictions that increase the cost of raising children in the city pushes parents into discriminating against their daughters.

A potential concern with this design is that parents anticipate the 2014 population control policy, and those with middle-school-aged kids choose to relocate from mega-cities. Table A18 tests this directly, and the triple interaction condition does not predict relocation. Table A19 restricts the sample of migrant parents further to those who made destination choices before 2013 or 2012, and our results remain robust in these sub-samples.

## 7 Does this Reflect Parents' Gender Bias?

New economic opportunities for workers in cities induced by trade liberalization benefit sons but harm daughters, because migrant parents choose to leave children behind in rural areas – especially their daughters. Section 6 establishes that restrictions on migration instituted by the Chinese government can explain why children are left behind. In this section

Table 11: Effects of Mega-City Population Control Policy on Leaving Children Behind

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | |
| | Female | Male | Female | Male |
| School-aged ×I(Population>5 million) ×I(Year>2014) | 0.0720*** | -0.0413 | 0.0776** | -0.0300 |
| | (0.0210) | (0.0358) | (0.0303) | (0.0262) |
| School-aged ×I(Population>5 million) | -0.00384 | 0.0179 | -0.00900 | 0.00844 |
| | (0.0226) | (0.0143) | (0.0198) | (0.0156) |
| School-aged × I(Year>2014) | -0.0504** | 0.0331 | -0.0497 | 0.0423 |
| | (0.0215) | (0.0264) | (0.0290) | (0.0279) |
| School-aged | 0.0321* | -0.0233 | 0.0457** | -0.0193 |
| | (0.0170) | (0.0167) | (0.0169) | (0.0186) |
| Coeff diff p-value | 0.000 | | 0.000 | |
| Observations | 10,296 | 13,812 | 10,296 | 13,812 |
| Adjusted R-squared | 0.175 | 0.146 | 0.291 | 0.253 |
| Household Control | Yes | Yes | Ye | Yes |
| City FE×Year FE | Yes | Yes | No | No |
| City FE×Year FE×Hukou Province FE | No | No | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| City Size Bandwidth | 3 | 3 | 3 | 3 |

*Notes*: The age bandwidth is two years. We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. "Coeff diff p-value" reports the p-value of a test of equality of the coefficient on "Above enrollment age ×I(Population>5 million) ×I(Year>2014)" between the female and male, using the Fisher's permutation test (following Cleary (1999), Brown et al. (2010) and Keys et al. (2010)). This bootstraps to calculate empirical p-values that estimate the likelihood of obtaining the observed differences in coefficient estimates if the true coefficients are, in fact, equal. The city size bandwidth is 3 million, and thus we only include cities with baseline population between 2 and 8 million in the city central district area. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

we explore why daughters are more likely to be left behind.

This could be an entirely economically rational choice: the market returns to education may be lower for females than males, or sons might be relatively more productive in cities. Or, perhaps, parents invest more time and money in sons because they are expected to support parents in their old age. But we find the clearest evidence in favor of an entirely different mechanism: that the decision to send daughters back is related to son-biased preferences.

### 7.1 *Hukou* Restrictions Exacerbate Pre-existing Son Preference

We first assess whether our empirical pattern is driven by the interplay between parental son preference and *hukou* restrictions. Table 12 limits households with at least two children and assigns girls (of migrant households) into two groups, based on whether they have male siblings who will compete with them for limited educational resources in cities. We find that our main empirical result – daughters being left behind when they reach middle-school-age in restricted *hukou* cities – is more evident and statistically precise for those with male siblings. The empirical patterns we document therefore appear related to unequal intra-household allocation of resources between boys and girls.

We conduct another heterogeneity test to explore whether gender-biased social norms explain the empirical patterns we report. We construct an index to measure the extent of gender discrimination in each province based on survey questions in China's Women Social Status Survey 2000. And we define an indicator for whether the index in one's home province is above the national average. We then re-estimate our specification from Table 10 and additionally interact our independent variable of interest – girls above enrollment age in restrictive *hukou* cities – with the indicator for the above-average level of gender discrimination in migrant parents' provinces of origin. Table 13 shows that this triple interaction term is significantly positive for the sample of female children, which implies that the main result from Table 10 (middle-school-aged girls left behind when migrants are in restrictive-*hukou* cities) is significantly more pronounced for migrants who come from regions featuring a high level of gender discrimination. The results are in accordance with the literature showing that, when people migrate, their beliefs and values on gender roles move with them, even though their external environment has changed (Alesina et al., 2013).

### 7.2 Other Mechanisms

**Differential Returns to Education or City Life?** Men and women are likely to have heterogeneous returns to education, and one may expect that parents leave their female children in villages if females have a lower rate of return to education and therefore should be allocated less educational resources. In Table A20, we use individual-level data to perform Mincer wage regressions, and study whether the returns to high school education differ between men and women.[21] Table A20 shows that girls actually have a *higher* rate of return

[21] We use individual-level pooled cross-sectional data by combining CLDS 2012, 2014, 2016 and 2018 to perform Mincer wage regression, because CLDS has a sample period similar to our baseline analysis and

Table 12: Heterogeneity by Whether Having Male Siblings

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | |
| | Has male siblings | | Doesn't have male siblings | |
| School-aged × Highly restricted cities (=1) | 0.0363** | 0.0369** | 0.0210 | 0.0207 |
| | (0.0172) | (0.0160) | (0.0229) | (0.0230) |
| School-aged | 0.00518 | 0.00636 | -0.0232 | -0.0283 |
| | (0.0219) | (0.0217) | (0.0207) | (0.0217) |
| Observations | 17,323 | 17,323 | 10,211 | 10,211 |
| Adjusted R-squared | 0.176 | 0.215 | 0.170 | 0.210 |
| Household Control | Yes | Yes | Yes | Yes |
| City FE×Year FE | Yes | No | Yes | No |
| City FE×Year FE×Hukou Province FE | No | Yes | No | Yes |
| Cohort FE | Yes | Yes | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| City Size Bandwidth | Linear | Linear | Linear | Linear |

*Notes*: Columns 1-2 show estimates for girls without male siblings, and columns 3-4 show estimates for girls with male siblings. The bandwidth is two years. We limit the sample to female children who are two years older or younger than the enrollment age of junior middle school and have at least one sibling. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. We use a local linear control function for the running variable. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** significant at 1%; **significant at 5%; * significant at 10%.

to education - both among rural families and urban families, and among migrants.

A related possibility is that working in cities offers boys larger marginal returns compared to girls, and it is therefore economically rational for parents to give their sons greater exposure and access to cities. We use information on individual incomes in CLDS surveys to estimate gender-specific returns to migration for a sample of rural *hukou* in Table A21. The returns to working in cities is actually significantly larger for girls compared to boys.[22]

These correlations make it highly unlikely that the stronger propensity to leave daughters behind in rural areas (which undermines their educational attainment and future work opportunities in cities) stems from sons producing greater returns from education or from

allows us to look at the pattern of gender-specific returns to education for people with different migration status and *hukou* types (rural or urban *hukou*).

[22] A concern with that test is endogenous selection into migration, or the "Roy sorting bias". We apply the Dahl (2002) selection correction procedure in Table A21 columns 5 and 6 to address this, and find that the returns to migration remains significantly larger for girls compared to boys.

Table 13: Heterogeneity by the Level of Gender Discrimination in Original Provinces

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | |
| School-aged×Highly restricted cities (=1) × High Level of Gender Discrimination (=1) | 0.0515** | 0.0499** | 0.0606*** | 0.0587*** |
| | (0.0222) | (0.0221) | (0.0204) | (0.0205) |
| School-aged×Highly restricted cities (=1) | 0.00496 | 0.00638 | 0.00456 | 0.00599 |
| | (0.0188) | (0.0186) | (0.0180) | (0.0179) |
| Observations | 30,959 | 30,959 | 30,959 | 30,959 |
| Adjusted R-squared | 0.173 | 0.174 | 0.207 | 0.207 |
| Household Control | Yes | Yes | Yes | Yes |
| City FE×Year FE | Yes | Yes | No | No |
| City FE×Year FE×Hukou Province FE | No | No | Yes | Yes |
| Cohort FE | No | Yes | No | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear |

*Notes*: We limit the sample to girls two years older or younger than the enrollment age of junior middle school (2-year bandwidth). We use a local linear control function for the running variable. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. In China's Women Social Status Survey 2000, there are several survey questions reflecting women's socioeconomic status. 1.Men should be society-oriented and women should be family-oriented (1=strongly agree,.., 4=strongly disagree). 2. Men are inherently more capable than women (1=strongly agree,.., 4=strongly disagree). 3.Doing well is not as good as marrying well (1=strongly agree,.., 4=strongly disagree). 4. A woman without children is not a complete woman (1=strongly agree,.., 4=strongly disagree). 5. Women should not have a higher social status than their husbands (1=strongly agree,.., 4=strongly disagree). 6.Generally speaking, appearance is more important than ability when women are looking for a job (1=strongly agree,.., 4=strongly disagree). 7. At least 30% of senior government leaders should be women (1=strongly agree,.., 4=strongly disagree). 8. Men should do half of the housework (1=strongly agree,.., 4=strongly disagree) 9. Do you think you are treated equally as men in society (1=strongly agree,.., 4=strongly disagree)? Based on these questions, we construct an inverse-covariance weighted summary index to measure the level of gender discrimination in each province. And high level of gender discrimination is an indicator for whether the index of *hukou* province is above the national average. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

remaining in the city.

**Sons are More Valuable for Old-Age Support?** If sons (but not daughters) are expected to support elderly parents, migrants may rationally respond by keeping their sons with them, and leave daughters behind. To test this hypothesis, we use the CFPS data to create an indicator for whether the share of old people (aged 60 years or above) that are supported by their sons in the origin province is above the national mean. Table A22 shows that the effect of *hukou* restrictions on the propensity to separate from daughters is not meaningfully affected by the strength of the social norm that sons provide old-age support.

**Girls are Just Different Than Boys?** If parents fear that the uncertainties created by migrant restriction policies that cities have been adopting would have a more detrimental effect on girls, or that cities are more dangerous for girls, or that it is easier for grandparents to raise girls than boys in the rural hometown, then they may be more likely to leave daughters behind. Our identification strategy – where we show that parents' propensity to send daughters back from *hukou* restricted cities once they enter middle-school age – suggests that fixed differences between boys and girls are unlikely to explain the patterns we document. Parents do not always treat girls differently; only when and where their child becomes more expensive and difficult to keep.

In sum, the interaction between pre-existing son-biased preferences and migration restrictions provides the most credible, concise explanation for Chinese migrant parents' propensity to leave daughters behind when it becomes costly to keep their children with them in the city. Such son preference may itself be a result of historical gender gaps in earnings. But current *hukou* policies serve to perpetuate and exacerbate those gender inequities.

## 8 Conclusion

Joining the WTO was a massive positive shock to China's economy that spurred two decades of unprecedented growth. We document a surprising unintended consequence: the daughters of workers who benefited from the new economic opportunities suffered in the long run. We construct an argument that this was due to daughters being left behind in rural areas by migrant parents, whenever the government made it expensive or difficult for migrant workers to keep their children with them. If this explanation is correct, then we should observe those adverse health and socio-economic effects on daughters to be more pronounced in the parts of China where rigid *hukou* policies imposed more stringent migration restrictions. This is indeed what we see in Table 14: girls from rural areas near hukou-restrictive cities fare much worse following trade liberalization. It is an *interaction* between trade liberalization and migration restrictions that harms girls: yet another example of a pre-existing policy distortion that undermines the benefits of free trade (Atkin and Khandelwal, 2020).

When migration policies are designed to make it difficult for those parents to keep their children with them, the poor migrants fueling China's post-liberalization growth are forced into a difficult choice: is it worth the expense of keeping my child with me? If there is some pre-existing gender bias in the population, then the cost of these choices are disproportionately borne by girls. Using longitudinal data, we document that this perpetuates inter-generational gender inequities.

China's "Left-Behind Children" highlights the fact that restrictions on people's mobility also undermine long-term economic development. With millions of African and South and South-East Asian children growing up without parents who are forced to migrate away under restrictive conditions in search of better livelihoods, this is a problem of global magnitude.

Table 14: Differential Effects of Trade on Girls by *Hukou* Policy Restrictiveness

| Dep. Var. | Girls near Lax *Hukou* Cities | Girls near Stringent *Hukou* Cities | P-value of Difference | Mean of Dep. Var. |
|---|---|---|---|---|
| Enrolled in Full-time Precollege (=1) | -0.00916 | -0.0417*** | 0.152 | 0.151 |
| | (0.0229) | (0.00767) | | |
| Graduate from Full-time Precollege (=1) | -0.00973 | -0.0331*** | 0.251 | 0.140 |
| | (0.0221) | (0.00546) | | |
| Good Mandarin (=1) | 0.0118 | -0.0530*** | 0.076 | 0.298 |
| | (0.0389) | (0.0158) | | |
| IHS Hourly Income | -0.0348 | -0.151*** | 0.256 | 1.835 |
| | (0.124) | (0.0336) | | |
| Work in Non-agricultural Sector (=1) | 0.0354 | -0.014 | 0.187 | 0.765 |
| | (0.0415) | (0.0184) | | |
| Work in Urban Areas (=1) | 0.0951*** | -0.0130* | 0.000 | 0.444 |
| | (0.0153) | (0.00637) | | |
| Log Height | 0.00478 | -0.000658 | 0.081 | 5.124 |
| | (0.00349) | (0.00149) | | |
| Psychological Problem Index | -0.123 | 0.0687** | 0.012 | 0.001 |
| | (0.0728) | (0.0230) | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use GSCF 2015 to perform individual-level quadruple difference regressions. The specification is $Y_{icn,t} = \beta_0 + \beta_1(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times Female_i \times high\_hukou_c + \beta_2 NTR_c \times Female_i \times high\_hukou_c + \beta_3(\overline{Age}_{sch} - Age_{2002})_i \times Female_i \times high\_hukou_c + \beta_4(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times high\_hukou_c + \beta_5(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times Female_i + \beta_6 NTR_c \times (\overline{Age}_{sch} - Age_{2002})_i + \beta_7 NTR_c \times Female_i + \beta_8(\overline{Age}_{sch} - Age_{2002})_i \times Female_i + \beta_9 high\_hukou_c \times Female_i + \beta_{10} high\_hukou_c \times (\overline{Age}_{sch} - Age_{2002})_i + \beta_{11} Female_i + \beta_{12}(\overline{Age}_{sch} - Age_{2002})_i + \xi_c + \eta_n + \varphi_{num} + \varepsilon_{it}$. $high\_hukou_c$ is an indicator for whether the inverse distance weighted average of *hukou* index in nearby cities (wihin 400km) is above average. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and the interaction between import tariffs, female dummy, $(\overline{Age}_{sch} - Age_{2002})_i$, and the indicator for *hukou* policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences), female dummy and $(\overline{Age}_{sch} - Age_{2002})_i$. Column 2 reports $\beta_5 + \beta_6$ (the effect on girls near "lax *hukou*" cities). Column 3 reports $\beta_1 + \beta_4 + \beta_5 + \beta_6$ (the effect on girls near "stringent *hukou*" cities). Robust standard errors clustered at the level of birth location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

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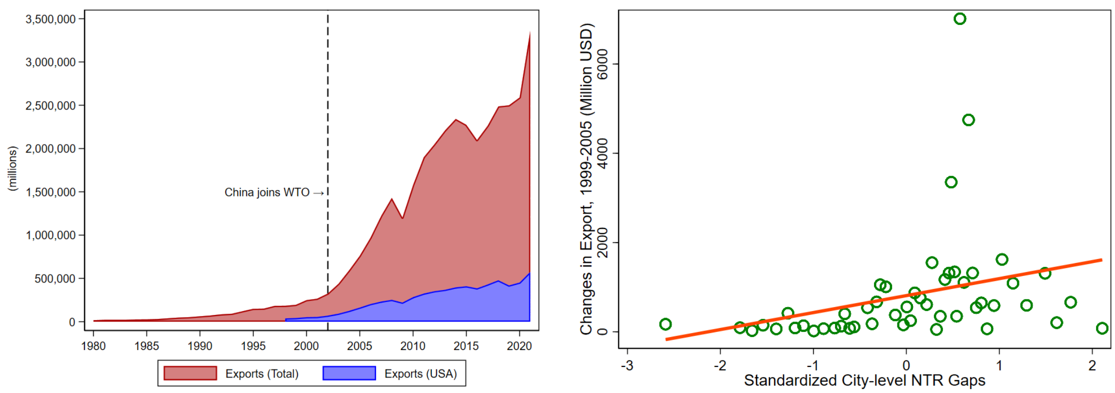
# Online Appendix

## Table of Contents

# A Additional Results, Figures and Descriptive Evidence

## A.1 Important Facts about Context and Educational System

Figure A1: The Rise of Chinese Exports in Response to the Accession to WTO

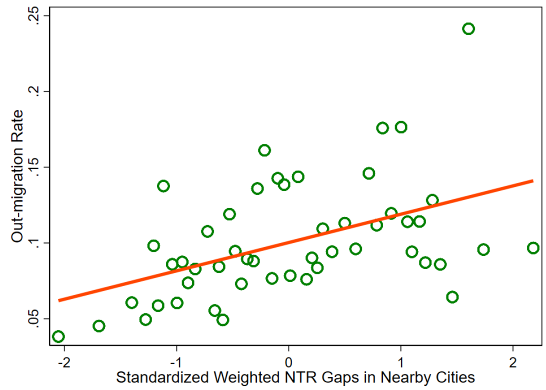



(a) Chinese Exports, 1980-2021

(b) NTR Gaps and Export Growth

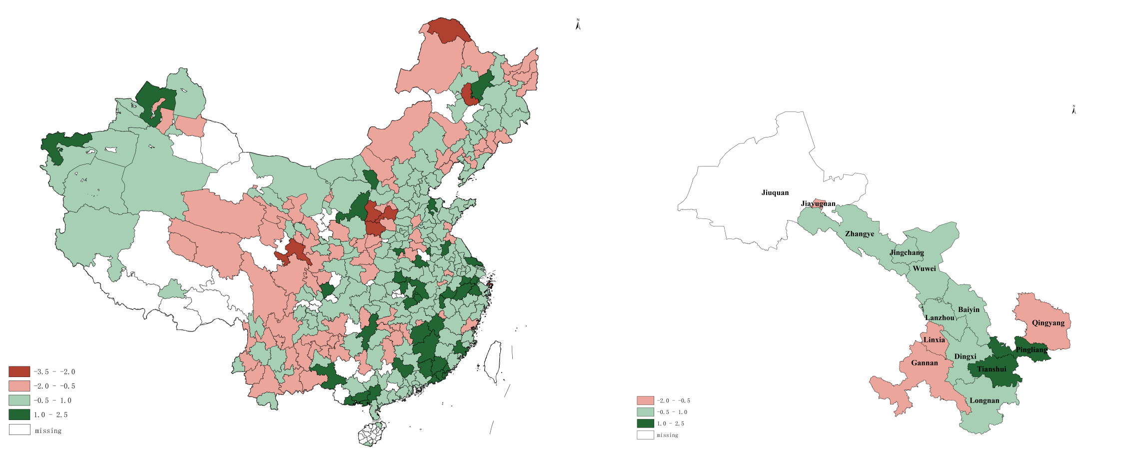
*Notes*: The left panel presents Chinese exports to the world as well as exports to the United States only. The right panel shows the correlation between city-level measures of NTR gaps and city-specific export growth between 1999 and 2005. China's entry to the WTO reduced the tariff uncertainty, which is defined as the difference between the non-NTR tariff and the NTR tariff. The NTR tariff gap varies substantially by products and sectors. We construct a city-level exposure measure that is the average gap between NTR and non-NTR rates across industries, weighted by the industry shares in the export basket of each city prior to China's accession to the WTO (as in Facchini et al. (2019); Pierce and Schott (2016)). We standardize city-level NTR gaps. Cities are grouped into fifty groups according to the quantile of the city-level NTR gaps. The vertical axis denotes the mean value of export growth and the horizontal axis denotes the mean value of the NTR gaps in each quantile. Data on exports are drawn from UN Comtrade. Data on NTR gaps come from Facchini et al. (2019); Pierce and Schott (2016).

Figure A2: Exposure to Trade Liberalization and Out-migration Responses




*Notes*: This figure shows the relationship between the share of rural parents moving out from their *hukou* location and the exposure to trade liberalization in nearby cities. The exposure to trade liberalization in nearby cities is measured as the inverse distance weighted average of city-level NTR gaps within 400 km of rural parents' *hukou* location (as measured in equation 1). We standardize the exposure to trade liberalization in nearby cities. Rural parents' *hukou* locations are grouped into fifty groups according to the exposure to trade liberalization in nearby cities. The vertical axis denotes the mean value of the share of rural parents leaving their *hukou* location and the horizontal axis denotes the mean value of the exposure to trade liberalization in nearby cities in each quantile. Data on out-migration rate come from the *China Population Census 2005*. Data on NTR gaps come from Facchini et al. (2019); Pierce and Schott (2016).

Figure A3: NTR Gaps across Cities in China




(a) All Cities in China

(b) Cities in Gansu

*Notes*: The left panel shows spatial distribution of city-level NTR gaps across all Chinese cities. The right panel shows city-level NTR gaps across cities covered in GSCF. Data on NTR gaps come from Facchini et al. (2019); Pierce and Schott (2016).

Table A1: The Share of Teachers by Education Levels

| | Master or above | College | Pre-college | High school | Below high school |
|---|---|---|---|---|---|
| | | **Panel A: Junior middle school** | | | |
| Urban | 0.031 | 0.830 | 0.135 | 0.003 | 0.000 |
| Rural | 0.004 | 0.657 | 0.328 | 0.011 | 0.000 |
| | | **Panel B: Primary school** | | | |
| Urban | 0.010 | 0.570 | 0.374 | 0.045 | 0.000 |
| Rural | 0.001 | 0.249 | 0.552 | 0.195 | 0.003 |

*Notes*: Data come from the Educational Statistics Yearbook of China 2013.

Table A2: The Share of Teachers by Professional Titles

| | Special Grade (Excellent) | Level-1 | Level-2 | Level-3 | No title |
|---|---|---|---|---|---|
| | | **Panel A: Junior middle school** | | | |
| Urban | 0.218 | 0.436 | 0.270 | 0.009 | 0.068 |
| Rural | 0.114 | 0.405 | 0.372 | 0.026 | 0.083 |
| | | **Panel B: Primary school** | | | |
| Urban | 0.578 | 0.302 | 0.022 | 0.003 | 0.095 |
| Rural | 0.508 | 0.360 | 0.041 | 0.002 | 0.089 |

*Notes*: Professional titles are designated to teachers based on their professionalism and progressive nature. The special grade teacher is the highest professional title, followed by Level-1 teacher, and then by Level-2 and Level-3 teacher. Data come from the Educational Statistics Yearbook of China 2013.

Table A3: Education Facilities per Student

| | Num of multi-media classrooms | Asset value of education equipment |
|---|---|---|
| | **Panel A: Junior Middle School** | |
| Urban | 0.053 | 0.511 |
| Rural | 0.036 | 0.358 |
| | **Panel B: Primary School** | |
| Urban | 0.081 | 0.653 |
| Rural | 0.036 | 0.293 |

*Notes*: Data come from the Educational Statistics Yearbook of China 2013.

## A.2 Summary Statistics of Key Variables

Table A4: Summary Statistics of Key Variables: GSCF

| Variable name | Mean | Std. dev |
|---|---|---|
| **Panel A: Children's Outcomes in GSCF 2015** | | |
| Graduated from Full-time Precollege (=1) | 0.140 | 0.347 |
| Enrolled in First-Tier College (=1) | 0.039 | 0.193 |
| Completed Junior Middle School (=1) | 0.834 | 0.372 |
| Drop off High School (=1) | 0.041 | 0.198 |
| Good Mandarin (=1) | 0.298 | 0.458 |
| Hourly Income | 6.820 | 11.75 |
| Work in Non-agricultural Sector (=1) | 0.765 | 0.424 |
| Have Formal Contract (=1) | 0.414 | 0.493 |
| Above Poverty Line (=1) | 0.645 | 0.479 |
| Psychological Problem Index | 0 | 1 |
| Height | 168.1 | 7.368 |
| Thin (BMI $<$18.5) | 0.107 | 0.309 |
| Work in Urban Areas (=1) | 0.444 | 0.497 |
| Move to Cities and Get Urban Hukou (=1) | 0.100 | 0.300 |
| Single Parents (=1) | 0.147 | 0.355 |
| Leaving Children Behind in 2015(=1) | 0.091 | 0.287 |
| **Panel B: Children's Outcomes in GSCF 2009** | | |
| Psychological Problem Index | 0 | 1 |
| Complete High School (=1) | 0.185 | 0.389 |
| Enrolled in Professional High School (=1) | 0.029 | 0.167 |
| Enrolled in key High School (=1) | 0.113 | 0.316 |
| Pass High School Entrance Exam (=1) | 0.379 | 0.485 |
| Good Academic Performance (=1) | 0.117 | 0.322 |
| Willing to Receive College/Precollege Education (=1) | 0.163 | 0.369 |
| Drop out of School due to Weariness (=1) | 0.194 | 0.396 |
| Smoke (=1) | 0.080 | 0.271 |
| **Panel C: Children's Outcomes in GSCF 2004** | | |
| Psychological Problem Index | 0 | 1 |
| Willing to Study in High School(=1) | 0.911 | 0.285 |
| Bad Math (=1) | 0.063 | 0.244 |
| Time on Housework Per Day | 0.525 | 1.076 |
| Time on Earning Money Per Day | 0.132 | 1.032 |
| Often Cut class (=1) | 0.011 | 0.105 |
| Often Be Distracted in Class due to Hunger (=1) | 0.015 | 0.123 |
| Drop out of School (=1) | 0.099 | 0.299 |

*Notes*: This table shows summary statistics for key variables. Data come from the Gansu Survey of Children and Families 2004, 2009 and 2015 (GSCF) .

Table A5: Summary Statistics of Key Variables: Other Datasets

| Variable name | Mean | Std. dev |
|---|---|---|
| **Panel A: Population Census** | | |
| Stay in Village with Children (=1) | 0.831 | 0.374 |
| Migrate and Leave Children Behind(=1) | 0.067 | 0.250 |
| Migrate and Bring Children to Cities(=1) | 0.101 | 0.302 |
| **Panel B: Migrant Sample in CMDS** | | |
| Leave Children Behind by Migrant Parents (=1) | 0.343 | 0.475 |
| Amount of Remittance (Chinese Yuan) | 5013 | 7282 |
| Age of Children | 9.154 | 3.422 |
| Age of Father | 36.520 | 5.300 |
| **Panel C: Children's Outcomes in CFPS** | | |
| Self-reported Unhappiness (=1) | 0.034 | 0.180 |
| Years of Education | 8.436 | 3.239 |
| Score in Word Test | 0.720 | 0.235 |
| Score in Math Test | 0.609 | 0.251 |
| Self-reported Bad Health (=1) | 0.026 | 0.160 |
| Blood Disease (=1) | 0.002 | 0.048 |
| Respiratory Disease (=1) | 0.027 | 0.162 |
| Bottom 10% income (=1) | 0.050 | 0.217 |
| **Panel D: NTR Gaps and *Hukou* Index** | | |
| Exposure to Trade Shocks, $NTR_c$ (Gansu) | 0.276 | 0.027 |
| Exposure to Trade Shocks, $NTR_c$ (China) | 0.339 | 0.044 |
| *Hukou* Index | 0.171 | 0.097 |

*Notes*: This table shows summary statistics for key variables. Data come from Population Census 2010 (Panel A), the China Migrants Dynamic Survey (CMDS) (Panel B), Facchini et al. (2019) and Pierce and Schott (2016)(trade shocks in Panel C) , and Zhang et al. (2019) (*hukou* index in Panel C). In panel C, we show that our measure of exposure to trade liberalization (defined as $NTR_c$ in equation 1) for rural regions covered by GSCF and for the whole China, repectively.

## A.3 Additional Results on Trade and Children's Outcomes

Table A6: Robustness of 2015 Children's Outcomes (Table 2): Bootstrapped Errors

| Dep. Var. | Effect on Boys | Effect on Girls | P-value of Diff. | Mean of Dep. Var. |
|---|---|---|---|---|
| **Panel A: Education Outcomes and Skills Later in Life** | | | | |
| Graduated from Full-time Precollege (=1) | 0.0464*** | -0.0323*** | 0.000 | 0.140 |
| | [0.002] | [0.004] | | |
| Enrolled in Full-time Precollege (=1) | 0.0194 | -0.0406*** | 0.000 | 0.151 |
| | [0.320] | [0.006] | | |
| Enrolled in First-Tier College (=1) | 0.0130 | -0.0360*** | 0.006 | 0.039 |
| | [0.194] | [0.000] | | |
| Completed Junior Middle School (=1) | 0.0641*** | -0.0269*** | 0.000 | 0.834 |
| | [0.006] | [0.002] | | |
| Drop off High School (=1) | -0.0185 | 0.00755*** | 0.058 | 0.041 |
| | [0.154] | [0.008] | | |
| Good Mandarin (=1) | 0.0357 | -0.0501* | 0.008 | 0.298 |
| | [0.174] | [0.054] | | |
| **Panel B: Labor Market Outcomes** | | | | |
| IHS (Hourly Income) | 0.185 | -0.150** | 0.000 | 1.835 |
| | [0.214] | [0.014] | | |
| Above Poverty Line (=1) | 0.0519 | -0.0650*** | 0.000 | 0.645 |
| | [0.120] | [0.000] | | |
| Work in Non-agricultural Sector (=1) | 0.0459*** | -0.0117 | 0.006 | 0.765 |
| | [0.000] | [0.554] | | |
| Have Formal Contract (=1) | 0.0472*** | -0.0130 | 0.000 | 0.414 |
| | [0.004] | [0.572] | | |
| **Panel C: Health and Welfare Status** | | | | |
| Psychological Problem Index | -0.0881*** | 0.0592* | 0.004 | 0.001 |
| | [0.006] | [0.0980] | | |
| Log Height | 0.00760*** | -0.000416 | 0.004 | 5.124 |
| | [0.000] | [0.832] | | |
| Height $<$Gender-specific Median | -0.0901*** | 0.0685*** | 0.000 | 0.451 |
| | [0.000] | [0.004] | | |
| Underweight (BMI $<$18.5) | -0.0439*** | 0.0257* | 0.000 | 0.107 |
| | [0.002] | [0.070] | | |
| Work in Urban Areas (=1) | 0.0573*** | -0.00904 | 0.000 | 0.444 |
| | [0.000] | [0.532] | | |
| Move to Cities and Get Urban Hukou (=1) | 0.0185*** | -0.00532 | 0.000 | 0.100 |
| | [0.000] | [0.306] | | |
| Single Parents (=1) | -0.0464** | 0.0141 | 0.000 | 0.147 |
| | [0.010] | [0.302] | | |
| Leaving Children Behind (=1) | -0.0237** | 0.0429*** | 0.008 | 0.091 |
| | [0.016] | [0.008] | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use GSCF 2015 to perform individual-level regressions. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and an interaction between import tariffs, female dummy, $(\overline{Age}_{sch} - Age_{2002})_i$, and an indicator for *hukou* policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences), female dummy and $(\overline{Age}_{sch} - Age_{2002})_i$. The effect on boys is measured by $\beta_2$ in equation 2, and the effect on girls is measured by $\beta_1 + \beta_2$ in equation 2. Above poverty line is a dummy variable for whether a particular individual earns more than 1.9 US dollars per day, which is the poverty line specified by the World Bank. The definition of underweight is based on WHO standard: https://www.who.int/europe/news-room/fact-sheets/item/a-healthy-lifestyle—who-recommendations. We construct an inverse-covariance weighted summary index of various psychological outcomes including depression, anxiety, loneliness, and self-dissatisfaction, and we standardize the psychological index. We report score based wild bootstrap cluster p-values in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

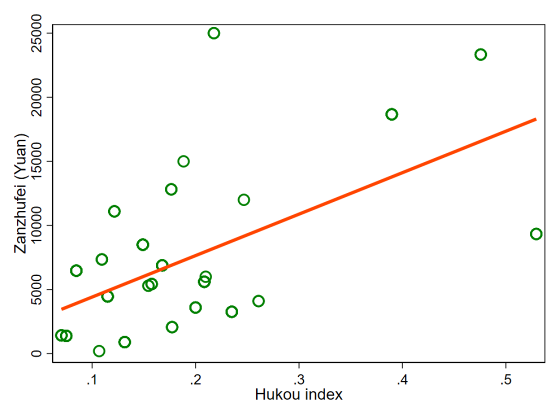
Table A7: Trade Liberalization and the Age of First Job

| Dep.Var. | Effect on Boys | Effect on Girls | P-value of Diff. | Mean of Dep. Var |
|---|---|---|---|---|
| Age of First Job <=16 | -0.00250 | 0.0124 | 0.427 | 0.135 |
| | (0.0108) | (0.0117) | | |
| Age of First Job <=15 | -0.000961 | -0.00354 | 0.745 | 0.058 |
| | (0.00377) | (0.00704) | | |
| Age of First Job <=14 | 0.00277 | -0.00574 | 0.160 | 0.025 |
| | (0.00289) | (0.00375) | | |

*Notes*: We estimate equation 2 to assess how exposure to trade liberalization affects the age of first job. Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use GSCF 2009 to perform individual-level regressions. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and the interaction between import tariffs, female dummy, $(\overline{Age_{sch}} - Age_{2002})_i$, and the indicator for *hukou* policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences), female dummy, and $(\overline{Age_{sch}} - Age_{2002})_i$. The effect on boys is measured by $\beta_2$ in equation 2, and the effect on girls is measured by $\beta_1 + \beta_2$ in equation 2. Robust standard errors clustered at the level of birth location are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

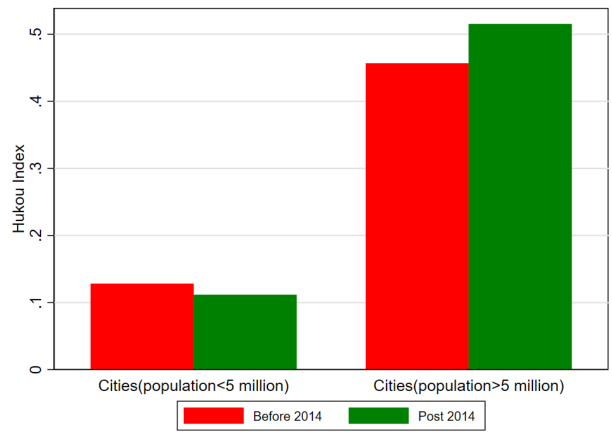
## A.4 Additional Facts on *Hukou* Restrictions and Left-Behind Children

Figure A4: *Hukou* Restrictions and *Zanzhufei* (Extra School Fee) for Migrants' Children



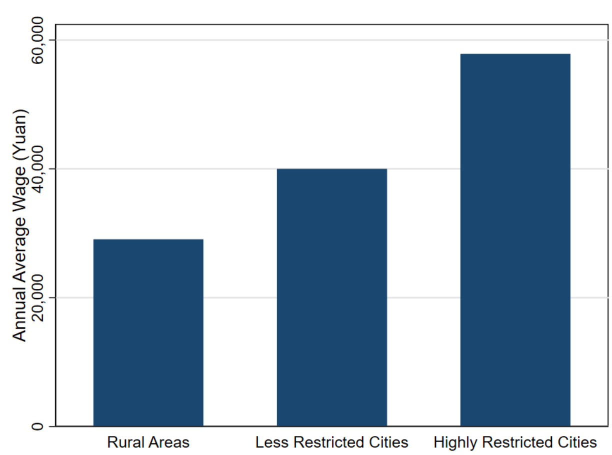

*Notes*: In China, migrant children without a local *hukou* have to pay *zanzhufei* (an extra fee imposed specifically on them) in order to go to a local school. This figure shows the relationship between the amount of *zanzhufei* and the stringency of *hukou* regulations in migrants' destination cities. Cities are grouped into fifty groups according to the quantile of the *hukou* index. The vertical axis denotes the mean value of the amount of *zanzhufei* and the horizontal axis denotes the mean value of the *hukou* index in each quantile. Data on *zanzhufei* children come from the *China Family Panel Studies (CFPS)*, and data on the *hukou* index come from Zhang et al. (2019).

Figure A5: 2014 Population Control Policy and *Hukou* Restrictions




*Notes*: We divide cities into two groups based on whether baseline population in the city central district area is above 5 million. The *hukou* index come from Zhang et al. (2019).

Figure A6: Wage Gains from Moving to Cites




*Notes*: We divide cities into two groups based on the stringency of *hukou* restrictions. Highly restricted cities are those in which the *hukou* index is above the national mean, and less restricted cities are those in which the *hukou* index is below the national mean. Wage data come form China Labor-force Dynamic Survey (CLDS). The *hukou* index come from Zhang et al. (2019).

Table A8: Beijing Closed Migrant Schools in Recent Years

| Year | Number of migrant children in Beijing (10,000) | Share of migrant children in migrant schools | Number of Migrant Schools |
|---|---|---|---|
| 2006 | 37.5 | 34.7 | 300 |
| 2007 | 40.0 | 36.5 | 268 |
| 2008 | 40.0 | 34.0 | 228 |
| 2010 | 43.4 | — | — |
| 2011 | 47.8 | 27.2 | 176 |
| 2012 | 41.9 | — | 158 |
| 2013 | 52.9 | 24.2 | 130 |
| 2014 | 51.1 | 18.2 | 127 |

*Notes*: Data come from the Annual Report on Education for China's Migrant Children (2016).

Table A9: Migrant Households' Spending on Education

| | Primary school | Junior middle school |
|---|---|---|
| *Zanzhufei* specific for migrant children | 1,432.005 | 2,198.48 |
| Total education expenditure (excluding *zanzhufei*) | 1,444.093 | 2,339.375 |

*Notes*: In China, migrant children without a local *hukou* have to pay *zanzhufei* (an extra fee specifically imposed on them) in order to go to a local school. Data come from the Chinese Household Income Project Survey (CHIPS) 2007 and 2008.

Table A10: Migrant Children in Guangzhou Disappear as They Enter Junior Middle School

| | | 2008 | 2012 | 2015 |
|---|---|---|---|---|
| Primary school | Num of migrant children | 376,963 | 434,473 | 458,216 |
| | Share of migrant children | 43.69% | 52.82% | 48.86% |
| Junior middle school | Num of migrant children | 86,089 | 121,426 | 127,815 |
| | Share of migrant children | 21.09% | 32.51% | 37.97% |
| High school | Num of migrant children | — | 23762 | 31969 |
| Entrance Exam | Share of migrant children | — | 20.06% | 28.87% |

*Notes*: Only a small fraction of migrant children without a local *hukou* are eligible to take local high-school entrance exams. Every year, the Guangzhou government sets a quota for the number of migrant children who can take local high-school entrance exams. Data come from the Annual Report on Education for China's Migrant Children (2016).

## A.5 Additional Results on Leaving Children Behind

Table A11: Average Marginal Effects: Alternative Controls and Bandwidths (Robustness of Multinomial Logit Estimates in Table 8)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Female | | | Male | |
| Dependent variable | All in Rural | Separation | All in City | All in Rural | Separation | All in City |
| **Panel A: Quadratic Control+2-year Bandwidth** | | | | | | |
| School-aged | -0.0215 | 0.0150 | 0.00646 | -0.000662 | -0.00890 | 0.00956 |
| | (0.0166) | (0.0110) | (0.0134) | (0.0156) | (0.00962) | (0.0119) |
| School-aged × Standardized weighted *hukou* index | -0.00355 | 0.0139** | -0.0104 | 0.00599 | -0.00109 | -0.00490 |
| | (0.00987) | (0.00607) | (0.00796) | (0.0142) | (0.00917) | (0.00901) |
| Observations | 9,020 | 9,020 | 9,020 | 10,640 | 10,640 | 10,640 |
| **Panel B: Quadratic Control+3-year Bandwidth** | | | | | | |
| School-aged | -0.0132 | 0.0116 | 0.00160 | -0.0103 | -0.00368 | 0.0139 |
| | (0.0125) | (0.00752) | (0.0104) | (0.0124) | (0.00770) | (0.0100) |
| School-aged × Standardized weighted *hukou* index | -0.00812 | 0.0133** | -0.00523 | 0.00486 | 0.00237 | -0.00723 |
| | (0.00754) | (0.00526) | (0.00602) | (0.00924) | (0.00672) | (0.00604) |
| Observations | 13,764 | 13,764 | 13,764 | 16,278 | 16,278 | 16,278 |
| **Panel C: Local Linear Control+3-year Bandwidth** | | | | | | |
| School-aged | -0.0158 | 0.0137 | 0.00214 | -0.0127 | -0.00117 | 0.0139 |
| | (0.0134) | (0.00874) | (0.0103) | (0.0128) | (0.00797) | (0.0106) |
| School-aged × Standardized weighted *hukou* index | -0.00811 | 0.0133** | -0.00522 | 0.00486 | 0.00237 | -0.00723 |
| | (0.00754) | (0.00525) | (0.00602) | (0.00924) | (0.00672) | (0.00604) |
| Observations | 13,764 | 13,764 | 13,764 | 16,278 | 16,278 | 16,278 |

*Notes*: We estimate the average marginal effects on the probability of parents' choices. All in rural represents the choice of staying in the village with children. Separation represents the choice of migrating to cities and leaving children behind. All in city represents the the choice of migrating to cities with children. Data come from 2010 Population Census. Robust standard errors clustered at the *hukou* prefecture level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Summary Statistics of Observables for Below and Above the Age Cutoff

| | (1)<br>Below age cutoff | (2)<br>Above age cutoff | (3)<br>Diff. in means | (4)<br>RD Estimates |
|---|---|---|---|---|
| **Panel A: Boys** | | | | |
| Household *hukou* transfer (=1) | 0.006 | 0.003 | -0.003 | -0.010 |
| | (0.078) | (0.054) | [0.003] | [0.007] |
| Father migrates (=1) | 0.014 | 0.009 | -0.005 | -0.027 |
| | (0.117) | (0.092) | [0.009] | [0.035] |
| Mother migrates (=1) | 0.018 | 0.018 | -0.000 | -0.008 |
| | (0.135) | (0.133) | [0.010] | [0.039] |
| Father income | 37,288.474 | 30,975.676 | -6,312.798* | -6,720.214 |
| | (23,504.601) | (23,217.771) | [3,253.024] | [13,079.839] |
| Mother income | 21,312.289 | 21,306.623 | -5.666 | 8,199.975 |
| | (14,738.911) | (17,566.470) | [2,327.219] | [10,131.891] |
| **Panel B: Girls** | | | | |
| Household *hukou* transfer (=1) | 0.003 | 0.002 | -0.001 | 0.006 |
| | (0.052) | (0.041) | [0.002] | [0.008] |
| Father migrates (=1) | 0.009 | 0.010 | 0.001 | 0.000 |
| | (0.096) | (0.101) | [0.008] | [0.032] |
| Mother migrates (=1) | 0.023 | 0.031 | 0.008 | -0.037 |
| | (0.149) | (0.173) | [0.013] | [0.052] |
| Father income | 35,217.738 | 35,613.582 | 395.844 | -7,051.000 |
| | (22,727.107) | (23,917.980) | [3,333.514] | [11,393.291] |
| Mother income | 21,669.966 | 19,778.509 | -1,891.457 | -9,579.064 |
| | (15,597.513) | (15,566.202) | [2,430.097] | [8,001.999] |

*Notes*: Household *hukou* transfer is an indicator for whether a particular household transfers their *hukou* location. Father migrates and Mother migrates are indicators for whether father and mother, respectively, move away from their *hukou* city. Columns 1 and 2 report the sample mean and standard deviation for children whose ages are above and below the age cutoff, respectively. Column 3 reports the raw difference between these sample means. Note that this statistic shows a simple difference between all children aged 6-15, which is not necessarily a discontinuous difference at the age cutoff. In column 4, we use our RD sample to investigate whether there is such a discontinuous difference. We use local linear regression to obtain RD estimates for the observables and report the standard errors in brackets. In columns 1 and 2, standard deviations are reported in parentheses. In columns 3 and 4, standard errors are reported in brackets. Data come from China Family Panel Survey (CFPS). *** p<0.01, ** p<0.05, * p<0.1.

Table A13: Data Manipulation Test

| | (1) | (2) | (3) |
|---|---|---|---|
| | All | Female | Male |
| T-stat | 0.352 | -0.384 | 0.795 |
| P-value | (0.725) | (0.701) | (0.427) |

*Notes*: This table reports the density test at the cutoff of school enrollment age using the method proposed by Cattaneo et al. (2020). T-statistics of the RD density test and corresponding p-values in parentheses are reported. Data come from China Migrants Dynamic Survey (CMDS).

Table A14: Robustness for Table 10: Alternative Controls and Different Bandwidths

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | | | | | |
| | Female | Male | Female | Male | Female | Male | Female | Male |
| **Panel A: Quadratic Control+2-year Bandwidth** | | | | | | | | |
| School-aged × Highly restricted cities (=1) | 0.0320** | 0.00356 | 0.0326** | 0.00469 | 0.0350** | 0.00904 | 0.0355** | 0.0102 |
| | (0.0145) | (0.0149) | (0.0144) | (0.0148) | (0.0145) | (0.0171) | (0.0144) | (0.0168) |
| Observations | 31,071 | 40,854 | 31,071 | 40,854 | 31,071 | 40,854 | 31,071 | 40,854 |
| Adjusted R-squared | 0.174 | 0.147 | 0.175 | 0.147 | 0.209 | 0.185 | 0.210 | 0.186 |
| **Panel B: Quadratic Control+3-year Bandwidth** | | | | | | | | |
| School-aged × Highly restricted cities (=1) | 0.0254* | 0.0163 | 0.0262* | 0.0169 | 0.0261* | 0.0190 | 0.0267* | 0.0196 |
| | (0.0147) | (0.0148) | (0.0145) | (0.0145) | (0.0145) | (0.0159) | (0.0143) | (0.0154) |
| Observations | 47,040 | 61,572 | 47,040 | 61,572 | 47,040 | 61,572 | 47,040 | 61,572 |
| Adjusted R-squared | 0.177 | 0.152 | 0.178 | 0.153 | 0.211 | 0.188 | 0.211 | 0.189 |
| **Panel C: Local Linear Control+3-year Bandwidth** | | | | | | | | |
| School-aged × Highly restricted cities (=1) | 0.0254* | 0.0164 | 0.0262* | 0.0170 | 0.0262* | 0.0191 | 0.0267* | 0.0197 |
| | (0.0147) | (0.0148) | (0.0145) | (0.0145) | (0.0145) | (0.0159) | (0.0143) | (0.0154) |
| Observations | 47,040 | 61,572 | 47,040 | 61,572 | 47,040 | 61,572 | 47,040 | 61,572 |
| Adjusted R-squared | 0.177 | 0.152 | 0.178 | 0.153 | 0.211 | 0.188 | 0.211 | 0.189 |
| Household Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| City FE×Year FE | Yes | Yes | Yes | Yes | No | No | No | No |
| City FE×Year FE×Hukou Province FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Cohort FE | No | No | Yes | Yes | No | No | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes*: Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A15: Triple Difference Regressions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | |
| Female × School-aged × Highly restricted cities (=1) | 0.0279* | 0.0276* | 0.0279* | 0.0276* |
| | (0.0151) | (0.0151) | (0.0151) | (0.0151) |
| Observations | 71,925 | 71,925 | 71,925 | 71,925 |
| Adjusted R-squared | 0.158 | 0.159 | 0.158 | 0.159 |
| Household Control | Yes | Yes | Yes | Yes |
| City FE × Year FE | Yes | Yes | Yes | Yes |
| Cohort FE | No | Yes | No | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Quadratic | Quadratic |

*Notes*: The bandwidth is two years. We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A16: Estimates using the Sample of First-born Children

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | | | | | |
| | Female | Male | Female | Male | Female | Male | Female | Male |
| School-aged × Highly restricted cities (=1) | 0.0368** | 0.00882 | 0.0374** | 0.00990 | 0.0420** | 0.0133 | 0.0424** | 0.0143 |
| | (0.0153) | (0.0158) | (0.0152) | (0.0156) | (0.0168) | (0.0187) | (0.0168) | (0.0184) |
| Observations | 27,370 | 34,234 | 27,370 | 34,234 | 27,370 | 34,234 | 27,370 | 34,234 |
| Adjusted R-squared | 0.174 | 0.141 | 0.175 | 0.142 | 0.205 | 0.176 | 0.206 | 0.177 |
| Household Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| City FE × Year FE | Yes | Yes | Yes | Yes | No | No | No | No |
| City FE×Year FE×HukouProvince FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Cohort FE | No | No | Yes | Yes | No | No | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear | Linear | Linear | Linear | Linear |

*Notes*: We use the sample of first-born children and limit the sample to children who are two years older or younger than the enrollment age of junior middle school. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. We use a local linear control function for the running variable. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A17: Controlling for the Effect of the OCP

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | | | |
| | Female | Male | Female | Male | Female | Male |
| School-aged × Highly restricted cities (=1) | 0.0348** | 0.00461 | 0.0358** | 0.00708 | 0.0359** | 0.00557 |
| | (0.0155) | (0.0150) | (0.0156) | (0.0146) | (0.0161) | (0.0148) |
| Observations | 31,071 | 40,854 | 30,590 | 40,094 | 30,590 | 40,094 |
| Adjusted R-squared | 0.183 | 0.157 | 0.180 | 0.156 | 0.181 | 0.156 |
| Household Control | Yes | Yes | Yes | Yes | Yes | Yes |
| City FE × Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Father Race FE×Year FE×Father Hukou Province FE | Yes | Yes | No | No | Yes | Yes |
| Mother Race FE×Year FE×Mother Hukou Province FE | No | No | Yes | Yes | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear | Linear | Linear |

*Notes*: We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. We use a local linear control function for the running variable. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A18: Migration Responses to 2014 Mega-City Population Controls

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable | Change city location Indicator | | |
| I(Population>5 million) × I(Year>2014) | -0.00128 | | |
| × Having a school-aged child (=1) | (0.00168) | | |
| I(Population>5 million) × I(Year>2014) | | -0.00271 | |
| ×Having a school-aged daughter (=1) | | (0.00327) | |
| I(Population>5 million) × I(Year>2014) | | | 0.000151 |
| ×Having a school-aged son (=1) | | | (0.000647) |
| Observations | 12,312 | 12,312 | 12,312 |
| Adjusted R-squared | 0.389 | 0.389 | 0.389 |
| Individual FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| City Size Bandwidth | 3 | 3 | 3 |

*Notes*: We employ individual longitudinal panel data from 2011 to 2016 constructed using China Labor-force Dynamic Survey. The dependent variable is an indicator for whether a particular migrant parent changed city location between year $t$ and $t-1$. I(Population>5 million) is an indicator for whether the migrant parent was in a mega city in year $t-1$. I(Year>2014) is an indicator for the post-treatment period. Having a school-aged child (=1) is an indicator for whether the parent had a child who had reached the middle-school age in year $t-1$. We control for the interactions between any two of the three indicators (in the triple interaction term) as well as the three indicators. The city size bandwidth is 3 million. We only include cities with baseline population within 2 million and 8 million. Robust standard errors clustered at the city level are reported in parentheses. *** significant at 1%; **significant at 5%; * significant at 10%.

Table A19: Robustness of 2014 Mega-City Policy (Table 11): Different Coming Years

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | |
| | Female | Male | Female | Male |
| **Panel A: Came before 2014** | | | | |
| School-aged ×I(Population>5 million) ×I(Year>2014) | 0.0720*** | -0.0413 | 0.0776** | -0.0300 |
| | (0.0210) | (0.0358) | (0.0303) | (0.0262) |
| Observations | 10,296 | 13,812 | 10,296 | 13,812 |
| Adjusted R-squared | 0.164 | 0.137 | 0.193 | 0.169 |
| **Panel B: Came before 2013** | | | | |
| School-aged ×I(Population>5 million) ×I(Year>2014) | 0.0927*** | -0.0622 | 0.0967*** | -0.0481 |
| | (0.0278) | (0.0379) | (0.0322) | (0.0300) |
| Observations | 9,629 | 12,815 | 9,629 | 12,815 |
| Adjusted R-squared | 0.164 | 0.135 | 0.194 | 0.171 |
| **Panel C: Came before 2012** | | | | |
| School-aged ×I(Population>5 million) ×I(Year>2014) | 0.0678*** | -0.0673 | 0.0713** | -0.0555 |
| | (0.0232) | (0.0419) | (0.0281) | (0.0324) |
| Observations | 8,626 | 11,466 | 8,626 | 11,466 |
| Adjusted R-squared | 0.160 | 0.130 | 0.188 | 0.166 |
| Household Control | Yes | Yes | Yes | Yes |
| City FE×Year FE | Yes | Yes | No | No |
| City FE×Year FE×Hukou Province FE | No | No | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| City Size Bandwidth | 3 | 3 | 3 | 3 |

*Notes*: We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. The city size bandwidth is 3 million. We only include cities with baseline population within 2 million and 8 million. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. We use a local linear control function for the running variable of age. Data come from China Migrants Dynamic Survey (CMDS). Robust standard errors clustered at the city level are reported in parentheses. *** significant at 1%; **significant at 5%; * significant at 10%.

## A.6 Tests of Alternative Mechanisms (from Section 7)

Table A20: Differential Returns to Education by Gender

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Dependent variable | Log individual income | | | | |
| | Full Sample | Rural *hukou* holders | Urban *hukou* holders | Migrants | Locals |
| High school (=1) | 0.217*** | 0.110*** | 0.300*** | 0.251*** | 0.195*** |
| | (0.0142) | (0.0185) | (0.0301) | (0.0243) | (0.0178) |
| High school (=1) × Female (=1) | 0.185*** | 0.118*** | 0.121*** | 0.148*** | 0.205*** |
| | (0.0196) | (0.0264) | (0.0403) | (0.0329) | (0.0249) |
| Observations | 30,021 | 21,860 | 8,161 | 9,740 | 19,842 |
| Adjusted R-squared | 0.381 | 0.360 | 0.297 | 0.378 | 0.354 |
| City FE × Year FE | Yes | Yes | Yes | Yes | Yes |

*Notes*: We use individual-level pooled cross-sectional data by combining CLDS 2012, 2014, 2016 and 2018. We control for an indicator for female, an indicator for rural *hukou*, age and age-squared. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A21: Differential Returns to Migrating to Cities by Gender

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variable | Log individual income | | | | | |
| Migrate to Cities (=1) | 0.242*** | 0.236*** | 0.224*** | 0.217*** | 0.224*** | 0.217*** |
| | (0.0276) | (0.0276) | (0.0288) | (0.0289) | (0.0288) | (0.0289) |
| Migrate to Cities (=1) × Female (=1) | 0.113*** | 0.117*** | 0.111*** | 0.116*** | 0.110*** | 0.115*** |
| | (0.0298) | (0.0298) | (0.0301) | (0.0301) | (0.0301) | (0.0301) |
| Observations | 16,046 | 16,046 | 16,046 | 16,046 | 16,046 | 16,046 |
| Adjusted R-squared | 0.364 | 0.367 | 0.366 | 0.369 | 0.367 | 0.369 |
| City FE × Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cohort FE | No | Yes | No | Yes | No | Yes |
| *Hukou* Location FE | No | No | Yes | Yes | Yes | Yes |
| Dahl Correction | No | No | No | No | Yes | Yes |

*Notes*: We use individual-level pooled cross-sectional data by combining CLDS 2012, 2014, 2016 and 2018 and restrict the sample to rural *hukou* holders. We control for an indicator for female, an indicator for high school and above, age and age-squared. To address the endogeneity of migration choices, we apply the Dahl (2002) semi-parametric selection correction approach in columns 5 and 6. We divide individuals into groups based on *hukou* regions, gender and education levels at baseline. Then, we define baseline selection probability $\omega_i$ as the fraction of the population in individual $i$'s cell that chooses to live in a particular destination city. Finally, we augment the Mincer equation by adding a quadratic function of $\omega_i$. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A22: Heterogeneity by the Norm that Aged Parents Are Supported by Sons

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Indicator for leaving the child in rural hometown | | | |
| School-aged × Highly restricted cities (=1) × | -0.00119 | -0.00254 | -0.0165 | -0.0187 |
| Strong norm that the aged are supported by sons (=1) | (0.0213) | (0.0215) | (0.0228) | (0.0229) |
| Observations | 29,336 | 29,336 | 29,336 | 29,336 |
| Adjusted R-squared | 0.163 | 0.163 | 0.192 | 0.193 |
| Household Control | Yes | Yes | Yes | Yes |
| City FE × Year FE | Yes | Yes | No | No |
| City FE×Year FE×Hukou Province FE | No | No | Yes | Yes |
| Cohort FE | No | Yes | No | Yes |
| Number of children FE | Yes | Yes | Yes | Yes |
| Age Bandwidth | 2 | 2 | 2 | 2 |
| Control function for the running variable | Linear | Linear | Linear | Linear |

*Notes*: We limit the sample to children who are two years older or younger than the enrollment age of junior middle school. Household controls include father's age and age-squared, an indicator for whether household income is above the median value among the migrant population in the city and an indicator for whether household consumption is above the median value among the migrant population in the city. We create an indicator for whether the share of people aged 60 years or above that are supported by their sons in a particular origin province is above the national mean. We use a local linear control function for the running variable. Robust standard errors clustered at the city level are reported in parentheses. *** significant at 1%; **significant at 5%; * significant at 10%.

## B Tests for Shift-share Variable

### B.1 The Distribution of Shocks and Exposure Weights

We first summarize the distribution of industry-specific shifters (NTR gaps at ISIC4 level), as well as the industry-level exposure weights $S_k$ (i.e. average exposure shares across locations for industry $k$). Table B1 column 1 shows that the distribution of shocks has an average of 0.34, a standard deviation of 0.15 and an interquartile range of 0.18. As in Borusyak et al. (2022), we calculate the inverse of its Herfindahl index (HHI) $1/\sum_k S_k^2$ to assess whether there is a high concentration of industry exposure. Although the largest exposure weight is 11.3% across industries, the inverse HHI of $S_k$ is 21.1 across industries, suggesting a sufficient and sizeable variation in exposure weights $S_k$. In Column 2, we regress industry-level NTR gaps on import tariffs (the factor that fails the industry-balance test) and use the regression residuals as the outcome variable. Conditional on industry-level import tariffs, the residual shocks have an approximately same standard deviation (0.15) and interquartile range (0.18).

Table B1: Shock-level (NTR Gap) Summary Statistics

| | (1) | (2) |
|---|---|---|
| Mean | 0.338 | 0.046 |
| Standard Deviation | 0.149 | 0.150 |
| Interquartile range | 0.183 | 0.184 |
| 1/HHI for exposure weight | 21.065 | 21.065 |
| Largest exposure weight | 0.113 | 0.113 |
| Number of Industries | 119 | 119 |

*Notes*: This table summarizes the distribution NTR gaps at ISIC4 level and as well as the industry-level exposure weights $S_k$. As in Borusyak et al. (2022), all statistics are weighted by the average industry exposure shares $S_k$.

### B.2 Falsification Tests

Borusyak et al. (2022) document that the orthogonality between a shift-share variable and an unobserved residual can be represented as the orthogonality between the underlying shocks and a shock-level unobservable (conditional upon observed confounding factors). We therefore implement falsification tests of the orthogonality of trade shocks, which provides a way of assessing the plausibility of the assumption of conditional quasi-random shock assignment in Borusyak et al. (2022). Following Borusyak et al. (2022), we do this in two ways. First, we regress potential proxies for the unobserved residual (i.e. any unobserved labor demand or labor supply shock) on the city-level shift-share variables. Second, we regress potential industry-level confounders directly on the shocks.

**Industry-level Balance Test:** We choose a set of potential confounders based on our research context. In particular, we examine the potential association between industry-level NTR gaps and potential confounders that impact trade between China and the rest of the world.

First, tariff rates imposed on imported inputs and final goods impact the labor demand and productivity in an industry. Thus, we construct two measures. The first, import tariff, is the industry-specific average of tariff rates across various products of final goods and imports' origin countries in the baseline year of 2000. The second measure, input tariff, is the industry-level average tariff rates across different inputs and imports' origin countries in the baseline year.[1]

Second, prior to China's joining the WTO, Chinese firms required export licenses to export directly, and those without licenses can only export through intermediaries. Since China's accession to WTO, these policy restrictions were gradually lifted and all Chinese firms can export directly by 2004 (Bai et al., 2017). The cancellation of this policy restriction may increase the demand for migrant workers in sectors that are concentrated by "indirect" exporters. To address the potential effect of this policy change, we use an industry-level measure of export license, which is the share of export revenues in total exports within an industry that is licensed to export directly in 2000[2].

Third, barriers to foreign direct investment may affect the demand for migrant workers across various industries in China. As in Facchini et al. (2019), we focus on imperfect contract enforcement, which an important barrier to FDI. As in Nunn (2007), we measure contract intensity as the fraction of intermediate inputs employed by firms that require relationship-specific investments by the supplier. It had been difficult for foreign firms to deal with imperfect contract enforcement before 2001. Driven by China's accession to WTO, these barriers were gradually removed, and industries with a higher contract intensity[3]may experience a greater increase in labor demand. Therefore, we assess balance with respect to industry-level contact intensity prior to China's joining WTO.

Last, we follow Khanna et al. (2023) to examine whether the industry-level shifters

[1]Data on import tariff come from the 2000 World Integrated Trade Solution, and data on input tariff come from the 2002 input-output table for China.

[2]Data on export licence come from Bai et al. (2017).

[3]Data on contract intensity come from Nunn (2007).

are systematically associated with baseline industry attributes, including ratio of labor to value-added, ratio of capital of value-added, average return on assets and return on equity[4]. These factors may reflect firm performance and labor demand at industry level.

Table B2 reports the results of industry-level balance tests. We regress each potential confounder on standardized industry-level NTR gaps, weighting by average industry exposure weights $S_k$ (as in Borusyak et al. (2022)). Except for import tariffs, all other baseline industry attributes do not have any statistically significant relationship with NTR gaps. Import tariffs are significantly positively associated with NTR gaps at the industry level. And one possible reason is that China imposed higher import tariffs to protect firms in industries facing a higher export tariff uncertainty, as this may reduce the competition from foreign firms in domestic market in these industries. Therefore, we control for observation-level exposure-weighted mean of import tariffs throughout our analysis on trade liberalization. And our identification assumption is that the industry-level shocks (NTR gaps) are exogenous, conditional on baseline import tariffs.

Table B2: Industry Balance Checks

| | |
|---|---|
| Contract intensity, 1997 | 0.0128 |
| | (0.0209) |
| Import tariffs, 2000 | 3.394** |
| | (1.494) |
| Input tariffs, 2002 | 0.00848 |
| | (0.0111) |
| Export licenses, 2000 | 0.0179 |
| | (0.0125) |
| Ratio of labor to value-added, 2000 | -0.0190 |
| | (0.0416) |
| Ratio of capital to value-added, 2000 | -7.143 |
| | (6.305) |
| Return on assets, 2000 | 0.000628 |
| | (0.00185) |
| Return on equity, 2000 | 0.0325 |
| | (0.0628) |
| Number of Industries | 119 |

*Notes*: We regress baseline industry attributes on standardized industry-level NTR gaps. Each row represents a separate regression, and column 1 shows the dependent variable for each regression. Robust standard errors clustered at the level of 3-digits industrial sectors are reported in parentheses.

[4]Data on these variables come from Annual Survey of Industrial Production in 2000.

**Regional Balance Test:** Table B3 reports our results of regional balance test. In Panel A, we assess balance with respect to baseline city-level demographic and education indicators. We again find no statistically significant relationships between our shift-share variable and baseline share of female population, share of females/males with high school education. We then examine pre-trends in regional demographic and education factors, including changes in the share of high school/college educated females, the number of colleges/middle schools, and the number of college/middle school students, prior to China's accession to WTO. After conditioning on city-level exposure-weighted average of import tariffs (column 2), changes in these factors cannot predict the exposure to trade demand shocks (i.e., city-level NTR gaps) driven by the entry to WTO.

Panel B examines balance with respect to baseline economic and employment indicators. We firstly examine the potential relationship with a range of baseline city-level variables, including gender-specific employment rate, GDP growth rate, employment shares in first/secondary/tertiary industries, industrial structure, and average wages. Conditional on city-level exposure-weighted average of import tariffs (column 2), none of these variables has any significant association with city-level NTR gaps. We next examine changes in a wide range of variables prior to China's joining WTO, including gender-specific employment, export, GDP, real-estate investment, FDI, share of skilled workers, and among others. Once again, changes in these variables have nothing to do with city-level NTR gaps.

Taken together, we provide evidence in support of our identification assumption. In particular, we document that conditional on city-level weighted average of baseline import tariffs, our shift-share variable is balanced with respect to city-level factors and their changes that may be associated with children's education achievement and socioeconomic outcomes later in life.

## Table B3: Regional Balance Checks

| | No Controls | Control for Exposure-weighted Mean of Import Tariffs | Number of Cities |
|---|---|---|---|
| **Panel A: Demographic and Education Indicators** | | | |
| Share of Female Population, 2000 | 0.000400 | 0.000162 | 310 |
| | (0.000433) | (0.000454) | |
| Share of High School Educated Females, 2000 | -0.00234 | -0.00150 | 310 |
| | (0.00282) | (0.00302) | |
| Share of High School Educated Males, 2000 | -7.80e-05 | 0.00108 | 310 |
| | (0.00277) | (0.00294) | |
| Gender Ratio for High School Graduates, 1990-2000 | 0.00830 | 0.00889 | 310 |
| | (0.00877) | (0.00959) | |
| Change in the Share of High School Educated Females, 1990-2000 | -0.000172 | 0.000393 | 310 |
| | (0.00208) | (0.00223) | |
| Change in the Share of College Educated Females, 1990-2000 | -0.000798 | -0.000784 | 310 |
| | (0.000522) | (0.000579) | |
| Log Change in Chinese Colleges,1997-2000 | 0.0157 | 0.0176 | 183 |
| | (0.0116) | (0.0122) | |
| Log Change in Chinese Middle Schools,1997-2000 | 0.00708 | -0.000565 | 218 |
| | (0.00749) | (0.00794) | |
| Log Change in Chinese Middle Schools,1997-2000 | 0.0165* | 0.0106 | 181 |
| | (0.00961) | (0.0109) | |
| Log Change in Chinese Middle Students ,1997-2000 | 0.0427** | 0.00148 | 243 |
| | (0.0178) | (0.0218) | |
| **Panel B: Economic and Employment Indicators** | | | |
| Female Employment Rate, 2000 | 0.0107 | 0.0110 | 310 |
| | (0.00657) | (0.00719) | |
| Male Employment Rate, 2000 | 0.00165 | 0.00173 | 310 |
| | (0.00302) | (0.00329) | |
| GDP growth rate ,1997 | 0.176 | 0.208 | 218 |
| | (0.132) | (0.132) | |
| GDP growth rate , 2000 | 0.101 | 0.138 | 251 |
| | (0.264) | (0.303) | |
| First Sector Employment Share, 1997 | 1.139 | 1.186 | 218 |
| | (0.713) | (0.856) | |
| Second Sector Employment Share, 1997 | -1.135** | -0.944 | 218 |
| | (0.566) | (0.659) | |
| Tertiary Sector Employment Share, 1997 | -0.00784 | -0.245 | 218 |
| | (0.238) | (0.278) | |
| Average Wages, 1997 | 184.3** | 137.2 | 218 |
| | (91.84) | (102.3) | |
| Average Wages, 2000 | 52.15 | 50.50 | 251 |
| | (123.1) | (145.4) | |
| Industrial structure (production ratio between tertiary and secondary sectors ), 1997 | 0.0168 | -0.0136 | 248 |
| | (0.0135) | (0.0121) | |
| Industrial structure (employment ratio between tertiary and secondary sectors ), 1997 | 0.0407** | 0.0259 | 218 |
| | (0.0198) | (0.0217) | |
| Change in Labor Force Gender Ratio, 1990-2000 | -0.00351 | -0.00222 | 310 |
| | (0.00508) | (0.00549) | |
| Change in Female Employment Rate, 1990-2000 | 0.00308 | 0.00340 | 310 |
| | (0.00429) | (0.00463) | |
| Change in Male Employment Rate, 1990-2000 | -0.000295 | -0.000441 | 310 |
| | (0.00248) | (0.00269) | |
| Change in Share of Skilled workers in Total Employment, 1990-2000 | -4.08e-05 | -0.000299 | 212 |
| | (0.000296) | (0.000284) | |
| Log Change in Exports,1997-2000 | -0.0279 | -0.0222 | 303 |
| | (0.0386) | (0.0417) | |
| Log Change in GDP.1997-2000 | -0.00554 | -0.00670 | 243 |
| | (0.00619) | (0.00651) | |
| Log Change in Employment, 1997-2000 | -0.0194 | -0.0116 | 218 |
| | (0.0215) | (0.0235) | |
| Log Change in Real Estate Investment, 1997-2000 | -0.0227 | -0.0162 | 216 |
| | (0.0180) | (0.0201) | |
| Log Change in FDI, 1997-2000 | -0.0629 | 0.0291 | 189 |
| | (0.0547) | (0.0576) | |

*Notes*: This table shows the effects of baseline city attributes and their changes on city-level average NTR gaps. Each row represents a separate regression. Column 1 shows the dependent variable for each regression, and the independent variable of interest is city-level average NTR gaps. We follow the two-step procedure proposed by Borusyak et al. (2022) to estimate exposure-robust standard errors. In the first step, we regress baseline city variables and the shift-share variable on controls and predict residuals for these city variables ($Y_d^{\perp}$) and for NTR gaps ($X_d^{\perp}$). We then calculate the exposure weighted average of $Y_d^{\perp}$ and $X_d^{\perp}$ at the shock level, which are $\overline{Y_d^{\perp}}$ and $\overline{X_d^{\perp}}$, respectively. And in the second step, we regress $\overline{Y_d^{\perp}}$ on $\overline{X_d^{\perp}}$, use industry-level NTR gaps as an IV for $\overline{X_d^{\perp}}$ and control for shock-level controls (import tariffs). We obtain exposure-robust standard errors clustered at the level of 3-digits industrial sectors from the second step estimation. Exposure-robust standard errors clustered at the level of 3-digits industrial sectors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

### B.3 Shock-level Equivalent Estimates using Exposure-Robust SEs

Based on Borusyak et al. (2022), the orthogonality between a shift-share variable and an unobserved residual can be represented as the orthogonality between the underlying shocks and a shock-level unobservable (conditional upon observed confounding factors). We show shock-level equivalence results for children's outcomes in 2015 and recast the conditional orthogonality of our shift-share variable at the shock level. This also allows us to perform statistical inference using exposure-robust SEs proposed by Borusyak et al. (2022).

Borusyak et al. (2022) develops a procedure by which region- or individual-level regressions with a single shift-share variable can be converted to shock-level regressions. In our baseline specification of equation 2, we combine male and female children and include three interaction terms incorporating our shift-share variable $NTR_c$, which is difficult to be converted to shock-level regressions. In order to apply the procedure proposed by Borusyak et al. (2022), we instead break our sample by the gender of children and use the following specification:

$$\begin{aligned} Y_{icn,t} =& \beta_0 + \beta_1(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c + \beta_2(\overline{Age}_{sch} - Age_{2002})_i + \xi_c \\ &+ \eta_n + \varphi_{num} + \varepsilon_{it} \end{aligned} \tag{B.1}$$

Equation B.1 only has a single interaction term incorporating the shift-share variable $NTR_c$. We estimate equation B.1 separately for boys and girls to assess how the effect of trade liberalization differs by gender. And we control for the interaction between exposure-weighted average of import tariffs for individual $i$'s birth location and $(\overline{Age}_{sch} - Age_{2002})_i$ and allow for the heterogenous effect of this interaction term by *hukou* policy restrictiveness of nearby cities.

Table B1 presents the results of individual-level subsample regressions of equation B.1, using robust standard errors clustered at the level of birth location. The empirical pattern is very similar to our baseline results (combining the sample of girls and boys) reported in Table 2. While trade liberalization benefits boys in various ways, it negatively impacts girls' educational achievement, phycological health, labor market outcomes and other welfare outcomes in 2015.

We can consider the interaction term $(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c$ as a shift-share variable, so the individual-level regression of equation B.1 can be converted to a shock-level regression.[5] The shifter is industry-specific NTR gap ($NTRGAP_k$), and the exposure shares of industry $k$ for child $i$ who was born in rural location $c$ is:

$$S_{ki} = \frac{(\frac{1}{dist_c^d})(\frac{EX_{k,d}}{\sum_j EX_{j,d}})}{\sum_{d,within400km}(\frac{1}{dist_c^d})(\frac{EX_{k,d}}{\sum_j EX_{j,d}})} \times (\overline{Age}_{sch} - Age_{2002})_i \tag{B.2}$$

Borusyak et al. (2022) document that when the sum of exposure shares does not equal 1, one needs to control for the sum of these shares. The sum of exposure shares of equation 2 is now $(\overline{Age}_{sch} - Age_{2002})_i$, and we have controlled for it in equation B.1. We then conduct a two-step estimation. In the first step, we regress individual-level outcome variables ($Y_{icn,t}$) and our primary variable of interest $(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c$ on all the controls (including trade controls and demographic controls) and fixed effects in equation B.1, and predict residuals for outcome variables ($Y_i^{\perp}$) and for the primary variable of interest ($X_i^{\perp}$). We then calculate the exposure weighted average of $Y_i^{\perp}$ and $X_i^{\perp}$ at the shock level:

$$\begin{aligned} \overline{Y_k^{\perp}} &= \frac{\sum_i S_{ki} Y_i^{\perp}}{\sum_i S_{ki}} \\ \overline{X_k^{\perp}} &= \frac{\sum_i S_{ki} X_i^{\perp}}{\sum_i S_{ki}} \end{aligned} \tag{B.3}$$

$\overline{Y_k^{\perp}}$ reflects the average revisualized outcome of observations most exposed to the $k$ th shock, while $\overline{X_k^{\perp}}$ is the same revisualized treatment. Finally, in the second step, we perform an equivalent industry-level regression using the following specification:

$$\overline{Y_k^{\perp}} = \alpha + \beta_1 \overline{X_k^{\perp}} + \overline{v_k^{\perp}} \tag{B.4}$$

We instrument $\overline{X_k^{\perp}}$ using the shock-level shifter ($NTRGAP_k$) and weight the regression by the each shock's average exposure across observations ($\frac{\sum_i S_{ki}}{N}$). Here, $N$ denotes the number of children in our regression sample. We also control for import tariff at the industry level, which fails the shock-level balance test. Note that the industry-level control (import tariffs) is included, even after city-level weighted average of import tariffs are partialled

[5] We thank Peter Hull's suggestions on the procedure on how to convert our individual-level regressions to equivalent industry-level regressions.

out when predicting residuals $Y_i^{\perp}$ and $X_i^{\perp}$.

Table B5 demonstrates that the equivalent shock-level regressions yield exactly same coefficient estimates as individual-level regressions of equation B.1. This further consolidates that the exogeneity of a shift-share instrument can be represented as the orthogonality between the underlying shocks and a shock-level unobservable (conditional upon observed confounding factors) (as documented by Borusyak et al. (2022)).

The shift-share variable inference may be complicated by the fact that the observed shocks and any unobserved shocks at the shock level induce dependencies in the shift-share variable and residuals across observations with similar exposure shares. In other words, observations with overlapping shares may have correlated shift-share variables and residuals, which may bias the estimated standard errors. Nevertheless, we can directly obtain valid ("exposure-robust") standard errors by estimating equivalent shock-level regressions of equation B.4 using the conventional robust standard error (as in Borusyak et al. (2022)). In other words, conventional shock-level standard errors from equation B.4 yield valid asymptotic inference on $\beta_1$. Moreover, recall that our research design exploits NTR gaps (shifters) at the 4-digit ISIC level. Thus, we cluster the standard errors of equation B.4 at the level of ISIC3 groups to address the mutual correlation of within ISIC3 groups (as in Acemoglu et al. (2016)) . As shown in Table B5, if anything, our results are more precisely estimated using the exposure-robust standard errors clustered at the level of ISIC3.

Table B4: Children's Outcomes in 2015: Subsample Regressions by Gender

| Dep. Var. | Effect on Boys | Effect on girls |
|---|---|---|
| **Panel A: Education Outcomes and Skills Later in Life** | | |
| Graduated from Full-time Precollege (=1) | 0.0521** | -0.0338*** |
| | (0.0225) | (0.00930) |
| Enrolled in Full-time Precollege (=1) | 0.0238 | -0.0413*** |
| | (0.0270) | (0.0128) |
| Enrolled in First-Tier College (=1) | 0.0106 | -0.0327*** |
| | (0.00931) | (0.00783) |
| Completed Junior Middle School (=1) | 0.0632*** | -0.0226 |
| | (0.0167) | (0.0164) |
| Drop off high school (=1) | -0.0140 | 0.00780 |
| | (0.0172) | (0.00440) |
| Good Mandarin (=1) | 0.0335 | -0.0406* |
| | (0.0332) | (0.0222) |
| **Panel B: Labor Market Outcomes** | | |
| IHS (Hourly Income) | 0.186 | -0.149*** |
| | (0.203) | (0.0463) |
| Work in Non-agricultural Sector (=1) | 0.0451** | -0.0106 |
| | (0.0166) | (0.0281) |
| Have Formal Contract (=1) | 0.0373** | -0.00820 |
| | (0.0157) | (0.0277) |
| Above Poverty Line (=1) | 0.0498 | -0.0673** |
| | (0.0485) | (0.0215) |
| **Panel C: Health and Welfare Status** | | |
| Psychological Problem Index | -0.107*** | 0.0737** |
| | (0.0237) | (0.0282) |
| Log Height | 0.00773*** | -0.000408 |
| | (0.00162) | (0.00188) |
| Height <Gender-specific Median | -0.0797* | 0.0636** |
| | (0.0368) | (0.0225) |
| Thin (BMI <18.5) | -0.0474*** | 0.0319* |
| | (0.00540) | (0.0168) |
| Work in Urban Areas (=1) | 0.0562** | -0.0112 |
| | (0.0217) | (0.0285) |
| Move to Cities and Get Urban Hukou (=1) | 0.0152** | -0.00274 |
| | (0.00640) | (0.00785) |
| Single Parents (=1) | -0.0442* | 0.00849 |
| | (0.0231) | (0.0127) |
| Leaving Children Behind (=1) | -0.0209** | 0.0415** |
| | (0.00799) | (0.0132) |

*Notes*: Each row represents two sub-sample regressions (for boys and girls, respectively) and column 1 shows the dependent variable for these regressions. We break the data by the gender of children. Column 2 shows the effect on boys, and column 3 shows the effect on girls. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and an interaction between import tariffs, $(\overline{Age_{sch}} - Age_{2002})_i$, and an indicator for hukou policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences) and $(\overline{Age_{sch}} - Age_{2002})_i$. Robust standard errors clustered at the level of birth location are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B5: Children's Outcomes in 2015: Equivalent Shock-level Estimates and Exposure Robust SEs

| Dep. Var. | Effect on Boys | Effect on girls |
|---|---|---|
| **Panel A: Education Outcomes and Skills Later in Life** | | |
| Graduated from Full-time Precollege (=1) | 0.0521*** | -0.0338*** |
| | (0.0105) | (0.00345) |
| Enrolled in Full-time Precollege (=1) | 0.0238 | -0.0413*** |
| | (0.0158) | (0.00645) |
| Enrolled in First-Tier College (=1) | 0.0106 | -0.0327*** |
| | (0.00954) | (0.00396) |
| Completed Junior Middle School (=1) | 0.0632*** | -0.0226* |
| | (0.0123) | (0.0126) |
| Drop off high school (=1) | -0.0140 | 0.00780 |
| | (0.00343) | (0.00126) |
| Good Mandarin (=1) | 0.0335 | -0.0406* |
| | (0.0124) | (0.0143) |
| **Panel B: Labor Market Outcomes** | | |
| IHS (Hourly Income) | 0.186* | -0.149*** |
| | (0.113) | (0.0178) |
| Work in Non-agricultural Sector (=1) | 0.0451*** | -0.0106 |
| | (0.0101) | (0.0158) |
| Have Formal Contract (=1) | 0.0373** | -0.00820 |
| | (0.00768) | (0.0199) |
| Above Poverty Line (=1) | 0.0498* | -0.0673*** |
| | (0.0276) | (0.0127) |
| **Panel C: Health and Welfare Status** | | |
| Psychological Problem Index | -0.107*** | 0.0737** |
| | (0.0118) | (0.0236) |
| Log Height | 0.00773*** | -0.000408 |
| | (0.00112) | (0.000769) |
| Height <Gender-specific Median | -0.0797*** | 0.0636*** |
| | (0.0258) | (0.0157) |
| Thin (BMI <18.5) | -0.0474*** | 0.0319** |
| | (0.000906) | (0.0128) |
| Work in Urban Areas (=1) | 0.0562** | -0.0112 |
| | (0.00766) | (0.0131) |
| Move to Cities and Get Urban Hukou (=1) | 0.0152*** | -0.00274 |
| | (0.00288) | (0.00733) |
| Single Parents (=1) | -0.0442*** | 0.00849 |
| | (0.00550) | (0.00929) |
| Leaving Children Behind (=1) | -0.0209*** | 0.0415*** |
| | (0.00535) | (0.0108) |

*Notes*: Each row represents two sub-sample regressions (for boys and girls, respectively) and column 1 shows the dependent variable for these regressions. We break the data by the gender of children. Column 2 shows the effect on boys, and column 3 shows the effect on girls. We control for birth location fixed effects, age cohort fixed effects, the number of children fixed effects, and an interaction between import tariffs, $(\overline{Age}_{sch} - Age_{2002})_i$, and an indicator for hukou policy restrictiveness in nearby cities. We also control for interactions between other trade controls (contract intensity, input tariffs, and export licences) and $(\overline{Age}_{sch} - Age_{2002})_i$. We follow the two-step procedure proposed by Borusyak et al. (2022) to estimate exposure-robust standard errors. In particular, we convert individual-level regressions to equivalent shock-level regressions, and this table shows equivalent shock-level estimates. Exposure-robust standard errors clustered at the level of 3-digits industrial sectors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

## B.4 Tests Based on Rotemberg Weights

Following Goldsmith-Pinkham et al. (2020), we calculate Rotemberg weights to measure the "importance" of each industry in driving the variation of shift-share variables. In our context, industries with higher Rotemberg weights drives the variations of exposure to trade liberalization across space. Table B6 lists top 30 ISIC4 industries regarding Rotemberg weights. Goldsmith-Pinkham et al. (2020) suggest examining the exposure shares of top 5 industries in terms of Rotemberg weights. We therefore re-estimate the effect of trade liberation on children's outcomes and control for interactions between an indicator for the gender of children and location-and industry-specific exposure shares for those top 2 and top 5 industries, respectively in Tables B7 and B8. Adding these additional controls account for any confounders that may be associated with exposure shares of these "important" industries. As reported in Tables B7 and B8, our empirical pattern remains similar.

Table B6: Rotemberg Weights by Industry, Top 30

| ISIC | Industry description | Rotemberg weight |
|---|---|---|
| 1810 | Manufacture of wearing apparel, except fur apparel | 0.48 |
| 1531 | Manufacture of grain mill products | 0.38 |
| 2320 | Manufacture of refined petroleum products | 0.28 |
| 2010 | Sawmilling and planing of wood | 0.20 |
| 2720 | Manufacture of basic precious and non-ferrous metals | 0.13 |
| 2710 | Manufacture of basic iron and steel | 0.10 |
| 1729 | Manufacture of other textiles n.e.c. | 0.09 |
| 1722 | Manufacture of carpets and rugs | 0.08 |
| 1721 | Manufacture of made-up textile articles, except apparel | 0.08 |
| 1730 | Manufacture of knitted and crocheted fabrics and articles | 0.05 |
| 2694 | Manufacture of cement, lime and plaster | 0.05 |
| 2424 | Manufacture of soap and detergents, cleaning and polishing preparations, perfumes and toilet prepara | 0.04 |
| 1514 | Manufacture of vegetable and animal oils and fats | 0.03 |
| 2893 | Manufacture of cutlery, hand tools and general hardware | 0.03 |
| 1551 | Distilling, rectifying and blending of spirits; ethyl alcohol production from fermented materials | 0.03 |
| 2919 | Manufacture of other general purpose machinery | 0.02 |
| 2430 | Manufacture of man-made fibres | 0.02 |
| 1520 | Manufacture of dairy products | 0.02 |
| 2924 | Manufacture of machinery for mining, quarrying and construction | 0.02 |
| 2413 | Manufacture of plastics in primary forms and of synthetic rubber | 0.02 |
| 1553 | Manufacture of malt liquors and malt | 0.02 |
| 2412 | Manufacture of fertilizers and nitrogen compounds | 0.01 |
| 3110 | Manufacture of electric motors, generators and transformers | 0.01 |
| 2610 | Manufacture of glass and glass products | 0.01 |
| 1512 | Processing and preserving of fish and fish products | 0.01 |
| 3699 | Other manufacturing n.e.c. | 0.01 |
| 3410 | Manufacture of motor vehicles | 0.01 |
| 3210 | Manufacture of electronic valves and tubes and other electronic components | 0.01 |
| 2429 | Manufacture of other chemical products n.e.c. | 0.01 |
| 2520 | Manufacture of plastics products | 0.01 |

Table B7: Children's Outcomes in 2015: Adding Interactions between Gender and Exposure Shares for Top 2 Industries

| Dep. Var. | Effect on Boys | Effect on girls | P-value of Diff. | Mean of Dep. Var |
|---|---|---|---|---|
| **Panel A: Education Outcomes and Skills Later in Life** | | | | |
| Graduated from Full-time Precollege (=1) | 0.0474** | -0.0326*** | 0.000 | 0.140 |
| | (0.0199) | (0.0100) | | |
| Enrolled in Full-time Precollege (=1) | 0.0196 | -0.0408** | 0.000 | 0.151 |
| | (0.0247) | (0.0132) | | |
| Enrolled in First-Tier College (=1) | 0.0103 | -0.0348*** | 0.000 | 0.039 |
| | (0.00886) | (0.00788) | | |
| Completed Junior Middle School (=1) | 0.0602*** | -0.0243* | 0.004 | 0.834 |
| | (0.0154) | (0.0111) | | |
| Drop off high school (=1) | -0.0145 | 0.00522 | 0.340 | 0.041 |
| | (0.0168) | (0.00436) | | |
| Good Mandarin (=1) | 0.0316 | -0.0467 | 0.022 | 0.298 |
| | (0.0322) | (0.0266) | | |
| **Panel B: Labor Market Outcomes** | | | | |
| IHS (Hourly Income) | 0.168 | -0.144** | 0.074 | 1.835 |
| | (0.197) | (0.0554) | | |
| Work in Non-agricultural Sector (=1) | 0.0439** | -0.0103 | 0.091 | 0.765 |
| | (0.0140) | (0.0273) | | |
| Have Formal Contract (=1) | 0.0331*** | -0.00594 | 0.086 | 0.414 |
| | (0.0100) | (0.0279) | | |
| Above Poverty Line (=1) | 0.0434 | -0.0616** | 0.008 | 0.645 |
| | (0.0466) | (0.0214) | | |
| **Panel C: Health and Welfare Status** | | | | |
| Psychological Problem Index | -0.0922*** | 0.0608** | 0.000 | 0.001 |
| | (0.0197) | (0.0265) | | |
| Log Height | 0.00771*** | -0.000429 | 0.002 | 5.124 |
| | (0.00133) | (0.00196) | | |
| Height <Gender-specific Median | -0.0846** | 0.0654** | 0.010 | 0.451 |
| | (0.0319) | (0.0207) | | |
| Thin (BMI <18.5) | -0.0503*** | 0.0282 | 0.001 | 0.107 |
| | (0.00483) | (0.0185) | | |
| Work in Urban Areas (=1) | 0.0502** | -0.00682 | 0.000 | 0.444 |
| | (0.0183) | (0.0274) | | |
| Move to Cities and Get Urban Hukou (=1) | 0.0143*** | -0.00404 | 0.043 | 0.100 |
| | (0.00399) | (0.00622) | | |
| Single Parents (=1) | -0.0475* | 0.0146 | 0.009 | 0.147 |
| | (0.0243) | (0.0141) | | |
| Leaving Children Behind (=1) | -0.0207** | 0.0413*** | 0.000 | 0.091 |
| | (0.00680) | (0.0124) | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use the same specification as that for Table 2. For each individual's birth location, we calculate the inverse distance weighted average of city-and industry-specific exposure shares for top 2 industries in terms of Rotemberg weights (for cities within 400km of the birth location). And we additionally control for the interaction between an indicator for children' gender and the inverse distance weighted average of exposure shares for these top 2 industries. Robust standard errors clustered at the level of birth location are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Children's Outcomes in 2015: Adding Interactions between Gender and Exposure Shares for Top 5 Industries

| Dep. Var. | Effect on Boys | Effect on girls | P-value of Diff. | Mean of Dep. Var |
|---|---|---|---|---|
| **Panel A: Education Outcomes and Skills Later in Life** | | | | |
| Graduated from Full-time Precollege (=1) | 0.0478** | -0.0330*** | 0.000 | 0.140 |
| | (0.0199) | (0.0102) | | |
| Enrolled in Full-time Precollege (=1) | 0.0201 | -0.0414** | 0.000 | 0.151 |
| | (0.0246) | (0.0135) | | |
| Enrolled in First-Tier College (=1) | 0.0108 | -0.0353*** | 0.000 | 0.039 |
| | (0.00880) | (0.00781) | | |
| Completed Junior Middle School (=1) | 0.0624*** | -0.0266* | 0.007 | 0.834 |
| | (0.0161) | (0.0143) | | |
| Drop off high school (=1) | -0.0146 | 0.00527 | 0.341 | 0.041 |
| | (0.0169) | (0.00437) | | |
| Good Mandarin (=1) | 0.0330 | -0.0481 | 0.013 | 0.298 |
| | (0.0316) | (0.0270) | | |
| **Panel B: Labor Market Outcomes** | | | | |
| IHS (Hourly Income) | 0.168 | -0.144** | 0.075 | 1.835 |
| | (0.197) | (0.0556) | | |
| Work in Non-agricultural Sector (=1) | 0.0421** | -0.00837 | 0.107 | 0.765 |
| | (0.0143) | (0.0265) | | |
| Have Formal Contract (=1) | 0.0346*** | -0.00744 | 0.069 | 0.414 |
| | (0.00974) | (0.0279) | | |
| Above Poverty Line (=1) | 0.0435 | -0.0616** | 0.008 | 0.645 |
| | (0.0466) | (0.0214) | | |
| **Panel C: Health and Welfare Status** | | | | |
| Psychological Problem Index | -0.0939*** | 0.0624** | 0.000 | 0.001 |
| | (0.0207) | (0.0257) | | |
| Log Height | 0.00770*** | -0.000421 | 0.002 | 5.124 |
| | (0.00133) | (0.00197) | | |
| Height <Gender-specific Median | -0.0858** | 0.0666** | 0.012 | 0.451 |
| | (0.0325) | (0.0218) | | |
| Thin (BMI <18.5) | -0.0499*** | 0.0279 | 0.001 | 0.107 |
| | (0.00470) | (0.0184) | | |
| Work in Urban Areas (=1) | 0.0514** | -0.00799 | 0.001 | 0.444 |
| | (0.0181) | (0.0277) | | |
| Move to Cities and Get Urban Hukou (=1) | 0.0152*** | -0.00499 | 0.065 | 0.100 |
| | (0.00431) | (0.00721) | | |
| Single Parents (=1) | -0.0482* | 0.0152 | 0.008 | 0.147 |
| | (0.0241) | (0.0150) | | |
| Leaving Children Behind (=1) | -0.0210** | 0.0417*** | 0.000 | 0.091 |
| | (0.00670) | (0.0125) | | |

*Notes*: Each row represents a separate regression, and column 1 shows the dependent variable for each regression. We use the same specification as that for Table 2. For each individual's birth location, we calculate the inverse distance weighted average of city-and industry-specific exposure shares for top 5 industries in terms of Rotemberg weights (for cities within 400km of the birth location). And we additionally control for the interaction between an indicator for children' gender and the inverse distance weighted average of exposure shares for these top 5 industries. Robust standard errors clustered at the level of birth location are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.