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 public schools is a factor. Triple difference research designs reveal that migrant parents become discontinuously more likely to leave daughters (but not sons) behind in rural areas 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. Millions of poor Asian and African workers separate from their children in search of better livelihoods in richer countries, so this is a problem of global magnitude.

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 and created new economic opportunities in those locations (Facchini et al., 2019; Erten and Leight, 2021). 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 unique longitudinal panel 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: similar negative effects of trade liberalization on girls are also evident in a nationally representative survey.

Next we explore 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 provide some clues. First, parents are more likely to emigrate and separate from daughters than sons. Second, daughters left behind receive 30% less remittance than sons left behind. Third, daughters generally do more housework than sons, but the gender gap in time allocation to household chores gets magnified when parents emigrate. In earlier rounds of the Gansu survey, girls whose parents benefited from better economic opportunities in cities report more psychological stress – a pattern consistent with psychology and sociology literature which establishes that girls' mental health is more vulnerable to parental absence than boys'.

The second part of our paper explore whether these gendered effects are connected to China's *hukou* policies, which erect 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 local 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 who migrated to *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 in 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 popular migration destinations); 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 A4). Using this alternative triple-difference identification strategy, we find that parents who had previously migrated to cities above the 5-million-population cutoff become 12 percentage points more likely to leave their middle-school-aged daughter behind *after* 2014 when the policy goes into effect. That same effect does not exist for boys. This analysis restricts 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 difficult 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, prevalence of sex-selective abortion technology, and the resulting imbalanced sex ratios (Qian, 2018), there are legitimate econometric 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. We show that our results hold even for the first child, where there is documented gender parity (Almond et al., 2019).

These results may have implications beyond China. 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. Vietnam has a similar *Hokhau* system which restricts migrants' access to services including schooling. Millions of internal migrants in India also find it difficult to access urban public services (Imbert and Papp, 2020).

Related Literature: A prominent 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 (Facchini et al., 2019; Erten and Leight, 2021; Li, 2018), and on gender inequality among adult workers (Keller and Utar, 2022; Tang and Zhang, 2021).

Our primary contribution to this literature is to carefully establish a new surprising fact by connecting this trade liberalization episode with a longitudinal panel survey that tracks children for 15 years: daughters (but not sons) are worse off later in life even when parents' earnings capacity improves. Second, we show how the *interaction* between China's accession to the WTO and its policy of internal migration restrictions can explain this counter-intuitive effect on girls. *Hukou* restrictions distort migrant parents' decision-making and leads to parental separation.

Closely related is the literature on the effects of trade shocks on children (e.g. Leight and Pan, 2020), which 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). Feng et al. (2023) shows a contemporaneous negative effect of trade on both boys' and girls' health in China. In contrast, we document both short-run *and* long-run health, education, economics effects and also analyze how migration restrictions can explain the gendered patterns of these effects.

We thereby add to the literature on the sources of gender disparities[1] by identifying a new

[1] Beaman et al. (2012); Blau and Kahn (2017); Card et al. (2016); Goldin (2014); Goldin et al. (2021); Barth et al.

mechanism by which disparities might emerge even if the underlying policy of mobility restrictions has no explicit gender dimension. We explain why such restrictions disproportionally hurt girls: parents in China are more likely to separate from girls than boys; they send less remittances back to daughters than sons; left-behind girls do more housework than boys; and girls' mental health suffers disproportionately. We also explore whether pre-existing son-preference explains these gender-asymmetric patterns.

Also related are papers on the effects of migration on children's educational outcomes (Zhang et al., 2014; Chen, 2013; Antman, 2011, 2012; Yang, 2008; Wong, 2023), as well as a voluminous literature on left-behind children in China that is summarized in Table F1. We specify the datasets and identification strategies used by each paper in Table F1, to clarify our distinctive contribution. In short, we are the first – to the best of our knowledge – to document how trade liberalization in the presence of mobility restrictions can have unintended consequences on children – and especially daughters – in the long run. To build a careful, rigorous argument, we use multiple sources of policy-relevant variation and a battery of sensitivity tests. For example, we exploit the plausibly exogenous variation in the difficulty and costs imposed on migrant parents for keeping their children with them in cities. Relatedly, Dang et al. (2016) points out the role that local school fees play in parents' decisions to leave children behind.

We thereby contribute to the migration literature in macroeconomics and in development economics 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.

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

(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); Gollin et al. (2014); McKenzie and Rapoport (2007); Facchini et al. (2019); Khanna et al. (2025); Imbert et al. (2022).

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 other cities, they cannot access many government-provided benefits at the destination without a local *hukou*. Most importantly, it is expensive and difficult for the children of migrants to enter local urban schools.

China has experienced a dramatic increase in rural-urban migration. The population of urban areas surged from 200 million in 1985 to 900 million in 2020 (Figure 1). Only a subset of those migrants were granted local *hukou*, so the number of urban residents without local *hukou* privileges also increased dramatically during this period. Obtaining a local *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., 2025). 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 2015, 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.

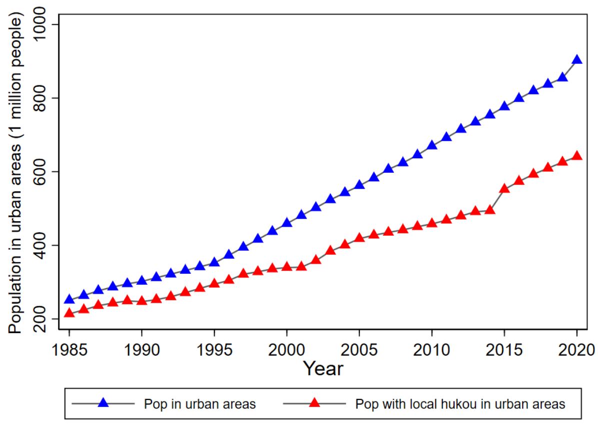
In summary, China's accession to the WTO created new economic opportunities that triggered large-scale migration into certain cities. But due to *hukou* policy restrictions, many of those migrants were forced to leave children behind in rural areas. These children are often raised by grandparents.

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

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*.

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. Survey attrition is unrelated with exposure to the trade shock (as defined below in Section 4.1), *hukou* policy restrictiveness in nearby cities, children's gender, experience of parental absence, or the interactions between these factors (Appendix Table A4).

3.2 Other Data on Migrants and Their Children

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 a particular city[3] 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.

[3]The geographic units of our analysis are Chinese prefecture-level cities, comprising both urban and rural areas. In the text, we use the terms cities and prefectures interchangeably. A prefecture can be regarded as a labor market area within commuting distance. Our analysis focuses on the migration of rural people from rural areas in their *hukou* prefecture to other prefectures. Different areas within the same prefecture are within commuting distance, so within-prefecture moves would not necessarily correspond to children being separated from parents. China Migrant Dynamic Survey (CMDS) 2010 shows that up to 82% of rural migrants migrate out from their *hukou* prefecture. Most of these rural migrants work in urban areas in their destination prefecture.

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.

In addition, we use the 2005 Population Census of China to assess the effects of trade liberalization on rural parents in Section 4.2, use the China Family Panel Studies (CFPS) 2010 survey to re-examine children's later-life outcomes in Appendix C, and employ the 2010 Population Census of China to conduct a multinomial logit analysis on parental migration choices in Appendix E.

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 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 local *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 A5-A6 report summary statistics of the key variables used in the analysis.

4 PART 1: The Effects of Trade Liberalization on Parents and 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 prefecture city is defined as the NTR gap for industry k ($NTRGap_k$) weighted by the importance of that industry to prefecture city d (as in Facchini et al., 2019), as measured by that city's pre-period (1997-1999) export share of that industry ($\frac{EX_{k,d}}{\sum_j EX_{j,d}}$),[4] 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 destination prefecture city d, to create the shift-share variable for rural region c:[5]

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

The exposure share for each sector k in equation 1 is $\sum_d \left(\frac{\frac{1}{dist_{dc}}}{\sum_m \frac{1}{dist_{mc}}} \right) \times \left(\frac{EX_{k,d}}{\sum_j EX_{j,d}} \right)$. The sum of this exposure share across sectors equals 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 this shift-share identification strategy to study the effects of trade liberalization across China and also on children born in 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.

[4] Like Facchini et al. (2019), we construct export shares at the level of entire prefectures because (a) data on location-and-sector level exports specifically for urban areas *within* a prefecture are not available in the baseline year prior to China joining the WTO, and (b) some in-migrants may work in factories located in rural areas within their destination prefectures. Like Facchini et al. (2019), we also use export weights to construct exposure shares because that approximates the true trade shock faced by a locality. This does a better job of predicting internal migration than employment weights (Appendix Table A7). Moreover, in our context, the validity of identification depends on the conditional exogeneity of shifters (sector-level NTR gaps) rather than that of exposure shares (Borusyak et al., 2022). Thus, potential correlates with export weights are unlikely to bias our estimates. Section 4.4 reports on robustness tests for our shift-share design.

[5] Rural region c denotes the rural area located within a given prefecture city where a particular rural child was born (and has local rural *hukou*). Our analysis focuses on inter-prefecture migration of rural people. In particular, we focus on internal migration of rural people from the rural area of their *hukou* prefecture c to a different prefecture city d. Rural and urban areas within the same prefecture would not necessarily correspond to children being separated from parents. Up to 82% of rural migrants migrate out from their *hukou* prefecture.

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 prefecture 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 nationally representative Census 2005 data, which unlike the 2000 census includes information on individuals' *hukou* prefecture and income. 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.[6]

Panel B uses 2000 and 2004 survey rounds of GSCF to examine economic outcomes for the parents of the tracked Gansu children soon after China joins 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 mar-

[6]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, ω_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. 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. In both Panels A and B, robust standard errors clustered at the level of *hukou* prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

gin, that one SD increase raises the probability of having a non-agricultural job by 19.6 percentage points. 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 other prefectures 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.

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

Exposure to trade liberalization and import demand shocks can either benefit or harm children. Children could benefit from enhanced economic opportunities for parents. Conversely, if children are left behind by migrant parents, that parental separation could adversely affect children's health and socioeconomic outcomes.

Empirical Specification: 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 compulsory schooling age, after China joined the WTO and the urban labor demand shock ensued. When surveyed in 2015, these "children" were 23-29 years old, and their entire schooling history therefore completed.

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

Y_{icn} 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.[7] Age_{2002} was child $i's$ age in 2002. As China joined WTO in December 2001, β_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. By law, children must remain enrolled in middle school before age 15 or 16. Equation 2 is therefore designed to study the effect of trade liberalization during the period when the child must attend school, and parental separation risk is high. ξ_c and η_n are birth prefecture fixed effects and age cohort fixed effects, respectively.

Our identification strategy is akin to a differences-in-difference model, where the first difference compares children in locations with higher or lower exposure to trade, and the second difference compares across children in the *same location* with more or fewer years of exposure depending on their age when China joined WTO. The second difference accounts for location-specific confounders that may be associated with trade exposure.

Our baseline regressions use robust standard errors clustered at the level of birth prefecture. Our results are robust to using wild cluster bootstrapping procedures and exposure-robust standard errors.[8] Due to China's One Child Policy (OCP), in some provinces, the local govern-

[7]The age at which a child completes junior middle school may vary slightly across children. Equation 2 controls for birth prefecture fixed effects which addresses any local variation in education policies. We adjust for child-specific factors (repeating or skipping grades, dropping out of school, temporary suspension) in Appendix Table A8 but our results are not sensitive to this.

[8]We use robust standard errors clustered at the level of birth prefecture in our baseline results of Table 2. Our

ment 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 (φ_{num}). The results are robust to not controlling for φ_{num} (Appendix Table A9). 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),[9] $Female_i$ 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 above-average *hukou* policy restrictiveness in nearby cities.

Results: 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 β_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). The pattern that emerges is that exposure to trade liberalization generally hurts girls, and the gender difference 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

results are robust to different error term structures that we assume for inference. In Appendix A.3 Table A10, 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.

[9] The industry balance test shows that baseline industry-level import tariffs appear to be significantly associated with NTR gaps (Appendix Table B2). We follow Borusyak et al. (2022) to control for exposure-weighted mean of import tariffs. Specifically, we construct a shift-share control that follows equation 1 but replaces $NTRGAP_k$ with industry-level baseline import tariffs.

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 benefiting from trade liberalization are significantly less likely to be underweight and short (as we study effects at ages when children are still growing), but we see no such effects on girls.

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 urban areas 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. This suggests that girls from Gansu migrate under greater economic pressure later when they become mothers, and problem of children-left-behind transmits inter-generationally.

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.

4.4 Robustness and Specification Tests

The Distribution of Ethnic Minorities: Gansu is home to several ethnic minority groups, which leads to a concern that their spatial distribution may confound the effects of trade liberalization. Only 1.3% of children are ethnic minorities in the GSCF data, and the rest are majority Han. Moreover, our shift-share trade exposure variable is well balanced with respect to the share of minorities in the baseline year, and also 1990 to 2000 pre-trends in minority population shares (see Appendix Table B3). Our estimated effects of trade exposure on children are robust to controlling for interactions between minority status *or* minority share, and the female dummy and $\overline{Age}_{sch} - Age_{2002}$ (Appendix Tables A11 and A12).

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 34 pp on average), a standard deviation of 0.15, and an inter-quartile range of 0.18. The inverse of its Herfindahl

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

Dep. Var.	Effect on Boys (β_2)	Effect on Girls ($\beta_1+\beta_2$)	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)		
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 β_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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

index (HHI) $1/\sum_k S_k^2$ is 21.1 across industries,[10] which indicates sufficient and sizeable variation

[10] We 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

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.[11] 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 perform regional balance tests for all prefectures (columns 1-2) as well as the sample of prefectures used in our analysis with Gansu children (columns 3-4). 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 baseline 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.

only shocks in a few industries drive the variation of shift-share variables.

[11] 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.

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).

Finally, in case the importance of exports to the local economy confounds our estimates. we follow Borusyak et al. (2022) and further control for the interaction between the share of exports in city GDP at baseline, the female dummy and $\overline{Age_{sch}} - Age_{2002}$ in Table B9. Our results are robust to this control.

4.5 Do the Gansu Results Replicate Nationwide?

Appendix C explores whether the long-run effects of trade liberalization we document with the rich longitudinal panel data from Gansu also replicates in nationally representative data. We use the 2010 wave of the China Family Panel Studies (CFPS) because it records the exact birth location of each individual and contains 2616 rural children from across 24 provinces aged < 16 in 2002.

Results reported in Appendix Table C1 are similar to our Gansu results: girls with greater exposure to trade demand shocks report being unhappier and in worse physical health, complete fewer years of education, and perform worse in word and math tests administered by surveyors. But unlike the positive effects of the trade shocks on boys in Gansu, there isn't much of an effect on boys in the rest of China. One potential explanation is that both parents (especially mothers) out-migrating is less common in Gansu compared to other Chinese provinces.

5 Mechanisms: Why Trade Liberalization Disproportionately Hurt Girls

The various rounds of the rich set of longitudinal surveys tracking the Gansu children from childhood into adulthood shed light on the mechanisms by which girls fare worse when parents receive new economic opportunities in other cities.

A. Girls Are More Likely to Be Left Behind: It is 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 3 tests this formally using regression analysis.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. Appendix Table A13 shows that these results are robust to controlling for individual minority status and the minority share of population.[12]

Table 3: The Effects of Trade Liberalization on Parental Absence

	(1)	(2)	(3)	(4)	(5)
Dep. Var.			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 columns 2 and 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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

B. Girls' Mental Health Suffers More with Parental Absence: Literature in psychology and sociology suggests that young girls' mental health is more vulnerable to parental absence than young boys' (Wu et al., 2019; Zhao and Yu, 2016; Culpin et al., 2013), and mental disorders in adolescence are more likely to carry over into young adulthood for girls than for boys (Patton et al., 2014). Appendix D uses 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. Indeed, an index of psychological problems that combines questions about unhappiness, depression, bad temper, combativeness, tiredness, and self-dissatisfaction shows that 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 psycho-

[12]Column 1 controls for prefecture-tier fixed effects, since rural areas located in Lanzhou may differ from rural areas in 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. 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.

Table 4: Weekly Hours of Housework by gender

Dep. Var.	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)
Coef. 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.

logical problem index for girls in 2004 and a 0.1 increase in 2009. This also hurts girls' human capital acquisition: they are more likely to cut class, and they perform worse in school.

C. Girls have to do More Housework when Parents Leave: Observed gender gaps could emerge if girls are treated more poorly at home than boys when parents leave children behind with grandparents or other caretakers. We use the 2004 round of the GSCF survey to examine gender differences in housework responsibilities. Results in Table 4 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. These results are consistent with grandparents expressing even more traditional views of gender roles.[13]

D. No Effects on Child Labor: 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 compared to boys.[14] 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 A15 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.

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

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

E. Left-Behind Girls Receive Less Remittances than Boys: 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. Unlike the Gansu survey, a survey focused specifically on migrants (the China Migrants Dynamic Survey - CMDS 2011-2012) reports detailed remittance information. So we use CMDS to study remittance patterns, focusing on the sub-sample of migrant families with children left behind in rural areas of China. Table 5 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 ($p<0.002$).[15] 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.[16]

Table 5: Remittance Sent to Rural Children by Gender

	(1)	(2)	(3)	(4)
Dep. Var.	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)
Coef. 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

[15] 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 gender attitudes between migrants from different areas), or age cohort fixed effects.

[16] Appendix Table A14 shows that these results are robust to controlling for potential confounders that may be correlated with the number of children left behind including educational attainment of parents, household migration income, and the level of gender discrimination in the *hukou* location.

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

6 PART 2: Migration Restrictions – Why Daughters are Left Behind

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 other 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. It's helpful to lay out some factual background before proposing formal empirical tests.

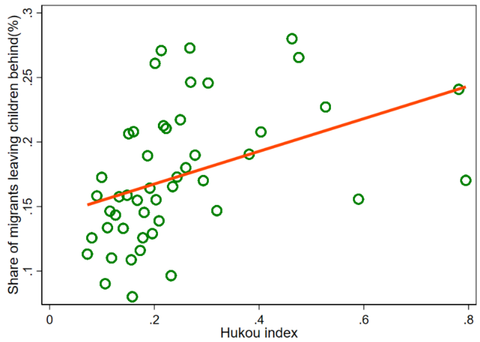
Accessing Urban Schools without a Local *Hukou* is Costly: To keep their school-aged children with them in the city, migrant parents who have not secured a local urban *hukou* either have to pay an extra fee called *zanzhufei*, or send their children to "migrant schools" set up in cities specifically for poor migrant children without a local *hukou*. Migrant schools are of poorer quality than urban public schools.[17] They also charge fees that are expensive for migrants, but these are lower than *zanzhufei* charged by urban public schools for migrant children. Many cities have closed migrant schools in recent years (Table A16) forcing parents to pay steeper *zanzhufei* if they want to keep their children with them.

School access and *zanzhufei* are important components of the *hukou* policy restrictions that induce migrants to leave their children behind in rural areas, but they are not the only relevant consideration. How easily migrant workers can obtain a local urban hukou is also obviously relevant for their decision-making. We therefore 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 A5). There is a 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: China's Compulsory Education Law requires parents to enroll their children in primary school if they turn six by September 1 in a given year and enroll them in junior middle school if they turn 12 by that date. Primary school covers grades 1–6 and junior middle school provides the last 3 years of the 9-year compulsory education required for all Chinese citizens. Junior middle schools charge a substantially higher *zanzhufei* than primary schools (Table A17), and the number of available school seats

[17] 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



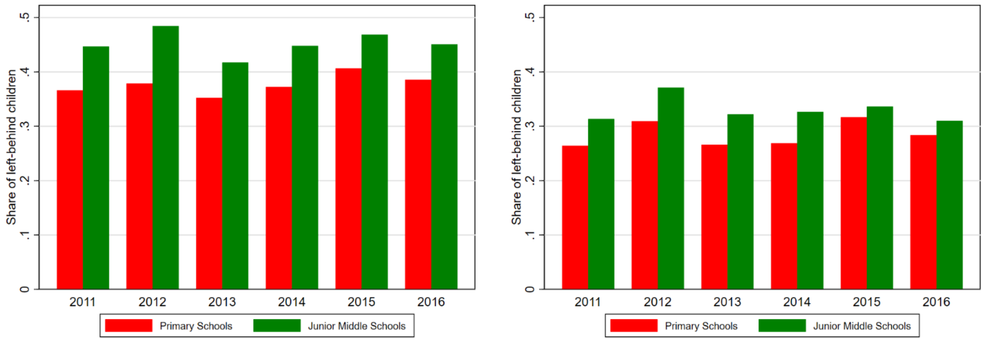
Notes: This figure shows the relationship between the share of migrants leaving children behind [Data from the *China Migrants Dynamic Survey*] and the stringency of *hukou* regulations in migrants' destination cities [Data from Zhang et al. (2019)]. Cities are grouped into fifty groups according to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

is also more limited, so *hukou* restrictions become a bigger constraint for migrants after their children turn 12. 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.[18] 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.

[18] 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 A18). 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.

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).

6.1 Effects of *Hukou* Policy on Parents' Decisions on When to Leave Children Behind

The China Migrant Dynamic Survey (CMDS) allows us to model migrant parents' decisions on whether to travel with their children. It becomes more expensive for parents to keep children with them in the city when the child turns 12 and enters middle-school, especially for those parents who migrate to cities with restrictive *hukou* policies. Our difference-in-differences model therefore compares primary versus middle school-aged children (when schooling costs increase discontinuously) of migrants, children whose parents are in lax-*hukou* versus *hukou*-restrictive cities. Studying migrant parents' choices separately for male and female children creates the triple difference:

$$\begin{aligned} Left\ behind_{ijt} = &\psi_0 + \psi_1 SchoolAged_{it} \times High\ Hukou_{jt} \times Female_{it} + \\ &\psi_2 SchoolAged_{it} \times High\ Hukou_{jt} + \psi_3 SchoolAged_{it} \times \\ &Female_{it} + \psi_4 High\ Hukou_{jt} \times Female_{it} + \psi_5 SchoolAged_{it} + \\ &\psi_6 Female_{it} + \psi_7 T_i \times SchoolAged_{it} + \psi_8 T_i + \lambda_{jt} + \eta_n + \varphi_{num} + v_{ijt} \end{aligned} \tag{3}$$

where $Left\ behind_{ijt}$ is an indicator for whether child i (whose parents work in city j and do not have a local *hukou* in their place of residence) are left behind in a rural area in year t. $High\ Hukou_{jt}$ is a binary variable that equals one if the stringency level of *hukou* restrictions in city j and year t is above average.

We combine CMDS 2011-2016 to create an individual-level pooled cross-sectional dataset to estimate equation 3.[19] This model is designed to examine whether parents' propensity to leave children behind shifts exactly 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 (RD) flavor around the age threshold. As is standard in the RD literature, our model includes a running variable T_i, which is the gap between the child's age and the middle school enrollment age cutoff.[20] Our primary variable of interest is the triple interaction between $SchoolAged_{it}$, $High\ Hukou_{jt}$ and $Female_{it}$, which examines whether there is any differential gender-specific discontinuous shift in the probability of leaving children behind at the school enrollment age ($T_i = 0$), in cities with more restrictive *hukou* policies.

We add city-by-year fixed effects λ_{jt} to control for city-by-year characteristics such as industrial structure and economic development plans of local government that may be correlated with the city's *hukou* policies . We control for birth cohort fixed effects η_n to account for any changes in other policies (e.g. the One Child Policy) pertaining to child outcomes.[21]

Results: Table 6 displays results separately for boys and girls. The interaction of the above-enrollment-age indicator and the high-*hukou*-restriction indicator is always positive and statistically significant for daughters, 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. 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. Appendix Table A19 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.

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. Appendix Table A20 formally demonstrates that the school-age discontinuity in restrictive *hukou* cities is statistically larger for girls than it is for boys. Migrant parents appear to leave their daughters rather than their sons behind in their hometown exactly when it becomes more expensive to

[19]We exclude children whose parents migrate from rural to urban areas within the same prefecture because that would typically be within commutable distance.

[20]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.

[21]Some specifications control for a triple interaction between city-, year- and *hukou* province- fixed effects to further absorb any differences in attitudes towards boys' versus girls' education between migrants from different areas. We always control for fixed effects for the number of children (φ_{num}), in case child gender is correlated with family size.

keep their child in the city with them, but only in destinations with strict *hukou* restrictions. In contrast, there is no obvious gender bias for parents in cities with relaxed *hukou* policies.

Table 6: 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) (ψ_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 (ψ_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 $\psi_1 + \psi_2$	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.

Robustness checks: Results remain similar when we re-run these regressions limiting the sample to first-born children only (Table A21), given concerns about sex selection at higher birth orders.[22] 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 A22).

Another potential threat to causal inference is that parents change migration decisions or they try harder to obtain a local *hukou* when their children reach middle school enrollment age. Table A23 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. In addition, families may entirely disappear from the CMDS dataset if their migration choices change. 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 A24, we find no discontinuity in the sample distribution at the school-age cutoff for either male or female children. This further mitigates concerns about "sorting" (e.g. changing migration status) based

[22]Given son preference in China and the availability of sex selection technology, child gender may itself reflect parental choices. Almond et al. (2019) shows that sex selection is more common at higher birth orders, while the gender ratio of first-born children in China matches biological expectations.

on their child's school entry date.

An important concern is that CMDS – by design – selects a sample of migrants, which means that we are suppressing the decision of whether to migrate at all in modeling parents' decision on whether to leave their children behind using equation 3. In reality, parents have a third choice of remaining in the rural hometown with their child. Appendix Section E uses the 2010 Census and a multinomial logit model to analyze all three options of parents (stay in the village, migrate with a child, and migrate without a child). Appendix Table E1 shows that the census data reproduces the exact same pattern of results: migrant parents are more likely to leave their daughters (but not sons) behind exactly when and where it becomes more expensive to keep their child with them.[23]

6.2 Effects of Mega-city Migrant Population Control Policy on Leaving Children Behind

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:

$$\begin{aligned} Left\ behind_{ijt} = &\alpha_0 + \alpha_1 School\ Aged_{it} \times I(Pop \geq 5\ million)_j \times I(t \geq 2014) + \\ &\alpha_2 School\ Aged_{it} \times I(Pop \geq 5\ million)_j + \alpha_3 School\ Aged_{it} \\ &\times I(t \geq 2014) + \alpha_4 School\ Aged_{it} + g(T_i, P_j) + \lambda_{jt} + \chi_{j\tau} + \\ &\eta_n + \varphi_{num} + v_{ijt} \end{aligned} \tag{4}$$

where $School\ Aged_{it}$ is a dummy for children who have reached middle-school enrollment age by year t, $I(Pop \geq 5\ million)_j$ is a dummy for the mega-cities subjected to the new policy because their central district population at baseline exceeded 5 million, and $I(t \geq 2014)$ is a dummy for the post-treatment period. $g(T_i, P_j)$ is a function of two running variables T_i (the gap between a child's age and the school-age cutoff) and P_j (the city-specific difference between baseline population and 5 million).[24] To account for the self-selected migration of parents based

[23]Moreover, Appendix Table E1 shows that the decision on *whether* to migrate at all is unaffected by *hukou* policies and the middle school age cutoff. This rationalizes our decision to focus only on the leave-behind decision using the richer CMDS data in this section. CMDS provides a larger sample of migrant children, and allows us to employ a linear regression model where the difference-in-differences and RD setup and fixed effects are easier to interpret than in the non-linear multinomial logit setting.

[24]Specifically, we control for the age running variable T_i along with its interactions with $I(Pop \geq 5\ million)_j$, $I(t \geq 2014)$ and $School\ Aged_{it}$, allowing the coefficients on T_i to be different by the school age cutoff, the population

on city-specific *hukou* and education policies in the past, we also add city-by-arrival year (when parents arrived in city j) fixed effects ($\chi_{j\tau}$). We restrict the sample to cities with populations between 3 and 7 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 in response to the policy shock in 2014.

Columns 1 and 3 of Table 7 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 12 percentage points[25] more likely to leave daughters behind. The second row shows that parents were not exhibiting that behavior *before* the policy went into effect. Columns 2 and 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. Yet again, we are documenting the exact same gender-biased reaction from parents as in section 6.1 when it becomes expensive to keep their child with them. But now the reaction is based on pre-post mega-city variation, and does not use any information on the stringency of *hukou* restrictiveness in the destination.

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 A25 tests this directly, and the triple interaction condition does not predict relocation. Table A26 restricts the sample of migrant parents further to those who made destination choices before 2011 or 2010, and our results remain robust in these sub-samples. Figure A6 presents an event study of the treatment effect on the likelihood of girls being left behind. We find no evidence of differential pre-trends between those mega-cities and cities below the population cutoff.

6.3 Bringing the two parts of the paper together

The first part of our paper (section 4) documents one surprising unintended consequence of trade liberalization: the daughters of workers who benefited from the new economic opportunities suffered in the long run. In this section we argue that this was in part due to daughters being left behind in rural areas by migrant parents, whenever the government made it expensive and 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

cutoff (for mega cities), and before and after 2014. The population running variable P_j is absorbed by city-by-year fixed effects (λ_{jt}). However, we control for the interaction between the two running variables (T_i and P_j) and further interact this interaction term with $I(Pop \geq 5\ million)_j$, $I(t \geq 2014)$, and $School\ Aged_{it}$.

[25] This is calculated based on the coefficient on $School\ Aged \times I(Population \geq 5million) \times I(Year \geq 2014)$ plus the coefficient on $School\ Aged \times I(Year \geq 2014)$ in columns 1 and 3.

Table 7: Effects of Mega-City Population Control Policy on Leaving Children Behind

	(1)	(2)	(3)	(4)
Dep. Var.	Indicator for leaving the child in rural hometown			
	Female	Male	Female	Male
School-aged ×I(Population≥5 million) ×I(Year≥2014)	0.283**	-0.0232	0.280**	-0.0432
	(0.117)	(0.0754)	(0.116)	(0.0822)
School-aged ×I(Population≥5 million)	-0.0702	-0.0920	-0.0677	-0.0776
	(0.0787)	(0.0869)	(0.0783)	(0.0890)
School-aged × I(Year≥2014)	-0.152**	-0.0432	-0.161**	-0.00510
	(0.0573)	(0.0503)	(0.0603)	(0.0543)
School-aged	0.0187	0.0641	0.0171	0.0325
	(0.0411)	(0.0679)	(0.0432)	(0.0688)
Coef. Diff. P-Value	0.000		0.000	
Observations	5,454	7,564	5,454	7,564
Adjusted R-squared	0.211	0.169	0.211	0.172
Household Control	Yes	Yes	Yes	Yes
City FE×Year FE	Yes	Yes	Yes	Yes
Cohort FE	No	No	Yes	Yes
City FE× Arrival Year FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Age Bandwidth	2	2	2	2
City Size Bandwidth	2	2	2	2

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 2 million, and thus we only include cities with baseline population between 3 and 7 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.

restrictions. This is indeed what we see in Table 8: 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). This does not imply that *hukou* mobility restrictions are the only reason why girls suffer adverse consequences, but we have documented a number of ways in which parental separation is harmful for girls: daughters receive less time *and* less money from parents, their mental health is more vulnerable, and they are forced to do more housework.

7 Why do Parents Choose to Treat Daughters Differently?

New economic opportunities for migrant workers induced by trade liberalization benefit sons but harm daughters, because migrant parents choose to leave children behind in rural areas – especially their daughters. In this section we explore why daughters and sons are treated differently in response to the same economic conditions and government policies.

This could be an entirely economically rational choice: the 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

Table 8: 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 $Y_{icn} = \beta_0 + \beta_1(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times Female_i \times high\_hukou_c + \beta_2 NTR_c \times Female_i \times high\_hukou_c + \beta_3(\overline{Age}_{sch} - Age_{2002})_i \times Female_i \times high\_hukou_c + \beta_4(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times high\_hukou_c + \beta_5(\overline{Age}_{sch} - Age_{2002})_i \times NTR_c \times Female_i + \beta_6 NTR_c \times (\overline{Age}_{sch} - Age_{2002})_i + \beta_7 NTR_c \times Female_i + \beta_8(\overline{Age}_{sch} - Age_{2002})_i \times Female_i + \beta_9 high\_hukou_c \times Female_i + \beta_{10} high\_hukou_c \times (\overline{Age}_{sch} - Age_{2002})_i + \beta_{11} Female_i + \beta_{12}(\overline{Age}_{sch} - Age_{2002})_i + \xi_c + \eta_n + \varphi_{num} + \varepsilon_{icn}$. $high\_hukou_c$ is an indicator for whether the inverse distance weighted average of *hukou* index in nearby cities (within 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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

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

Table 9 takes the subset of the CMDS sample households that have a daughter and at least one other child, and compares girls who have male siblings who will compete with them for limited educational resources in destination cities, against those who do not. 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 6 and additionally interact our independent variable of interest – children 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 10 shows that this triple interaction term is significantly positive for

Table 9: Heterogeneity by Whether Having Male Siblings: Female Sample

	(1)	(2)	(3)	(4)
Dep. Var.	Indicator for leaving the girl in rural hometown			
	Has male siblings		Doesn't have male siblings	
School-aged × Highly Restricted Cities (=1)	0.0326*	0.0346**	-0.0287	-0.00942
	(0.0191)	(0.0171)	(0.0350)	(0.0433)
School-aged	0.0114	0.00538	-0.0315	-0.0617
	(0.0257)	(0.0240)	(0.0439)	(0.0586)
Observations	14,115	14,115	3,510	3,510
Adjusted R-squared	0.184	0.227	0.178	0.260
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 with male siblings, and columns 3-4 show estimates for girls without 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. *** p<0.01, ** p<0.05, * p<0.1.

the sample of female children (columns 1 and 2), which implies that the main result from Table 6 (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 triple interaction term is statistically indistinguishable from zero for boys (columns 3 and 4). 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 A27, we use individual-level data to perform Mincer wage regressions, and study whether the returns to high school education differ between men and women.[26] Table A27 shows that girls actually have a *higher* rate of return to education - both among rural families and urban families, and among migrants. So this explanation seems *a*

[26] We use individual-level pooled cross-sectional data by combining China Labor-force Dynamics Survey (CLDS) 2012, 2014, 2016 and 2018 to perform Mincer wage regression, because CLDS has a sample period similar to our baseline analysis and 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*).

Table 10: Heterogeneity by the Level of Gender Discrimination in Original Provinces

	(1)	(2)	(3)	(4)
Dep. Var.	Indicator for leaving the child in rural hometown			
	Female		Male	
School-aged×Highly Restricted Cities (=1) ×	0.0531**	0.0515**	0.0160	0.0153
High Level of Gender Discrimination (=1)	(0.0221)	(0.0220)	(0.0245)	(0.0245)
School-aged×Highly Restricted Cities (=1)	0.00423	0.00558	-0.00698	-0.00538
	(0.0187)	(0.0185)	(0.0207)	(0.0208)
Observations	30,959	30,959	40,746	40,746
Adjusted R-squared	0.175	0.175	0.147	0.148
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	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.

priori implausible, although it is possible that parents have systematically biased beliefs about their children's market prospects.

A related possibility is that migrating to other cities offers boys larger marginal returns compared to girls, and it is therefore economically rational for parents to migrate with their sons. In Table A28 we use information on individual incomes to estimate gender-specific returns to migration for a sample of people who have rural *hukou*. The returns to migration are actually significantly larger for girls compared to boys.[27]

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 migration.

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

[27] A concern with this test is endogenous selection into migration, or the "Roy sorting bias". We apply the Dahl (2002) selection correction procedure in Table A28 columns 5 and 6 to address this, and find that the returns to migration remains significantly larger for girls compared to boys.

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 A29 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 when cities adopt polices to restrict migrant have more detrimental effect on girls, or that certain 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. 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 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.

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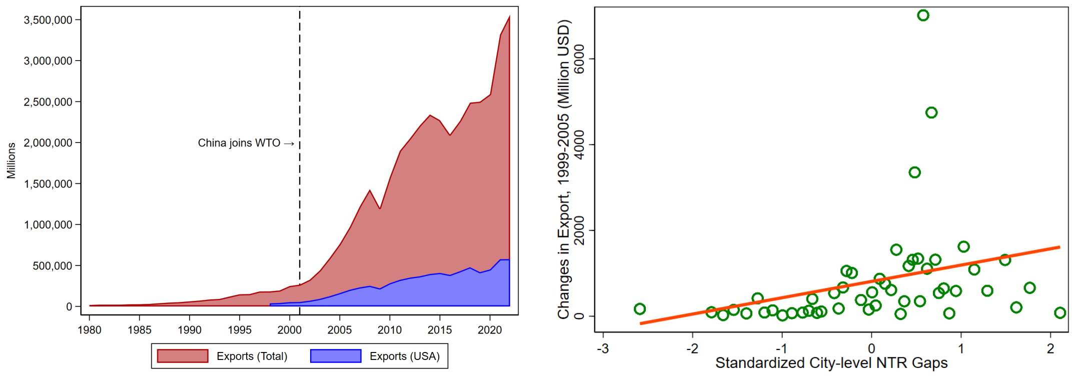
Supplemental Appendix

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

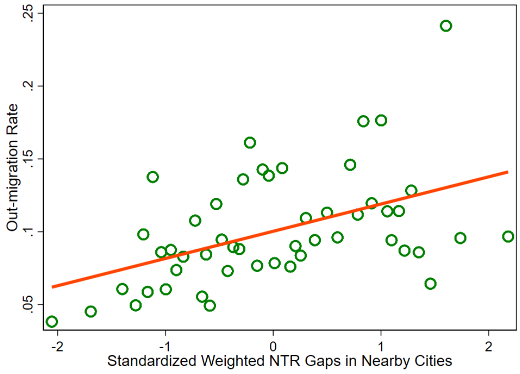


(a) Chinese Exports, 1980-2022

(b) NTR Gaps and Export Growth

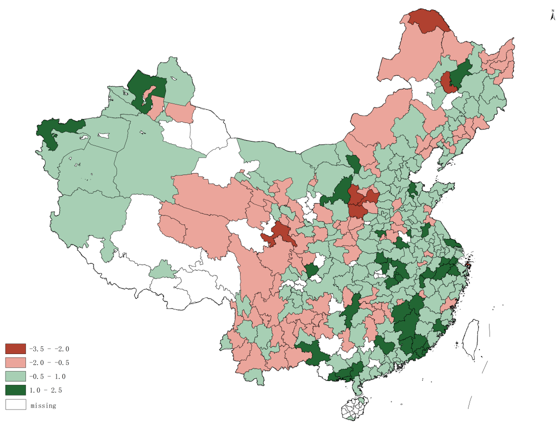
Notes: The left panel presents Chinese exports to the world as well as exports to the United States only. The right panel shows the correlation between city-level measures of NTR gaps and city-specific export growth between 1999 and 2005. China's entry to the WTO reduced the tariff uncertainty, which 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

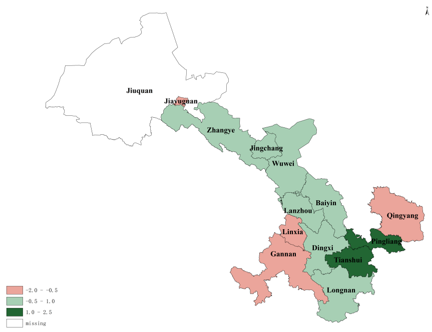


Notes: This figure shows the relationship between the share of rural parents moving out from their *hukou* location and the exposure to trade liberalization in nearby cities. The exposure to trade liberalization in nearby cities is measured as the inverse distance weighted average of city-level NTR gaps 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 Prefecture Cities in China



(a) All Prefecture Cities in China



(b) Prefecture Cities in Gansu

Notes: The left panel shows spatial distribution of city-level NTR gaps across all Chinese prefectures. The right panel shows city-level NTR gaps across prefecture 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: Attrition is Unrelated to Trade Exposure, Parental Absence, Gender or Hukou Policy Restrictiveness

	(1)
Dep. Var.	Attrition (=1)
Male (=1)	-0.0454
	(0.0393)
Being Left Behind (=1)	0.0113
	(0.0556)
Near Stringent *Hukou* Cities (=1)	-0.00995
	(0.0367)
Standardized NTR	0.00632
	(0.0243)
Male (=1) × Being Left Behind (=1)	0.0451
	(0.0734)
Male (=1) × Near Stringent *Hukou* Cities (=1)	0.00843
	(0.0491)
Male (=1) × Standardized NTR	0.0183
	(0.0314)
Being Left Behind (=1) × Near Stringent *Hukou* Cities (=1)	-0.0109
	(0.0729)
Being Left Behind (=1) × Standardized NTR	0.0482
	(0.0503)
Near Stringent *Hukou* Cities (=1) × Standardized NTR	-0.00744
	(0.0388)
Male (=1) × Being Left Behind (=1) × Near Stringent *Hukou* Cities (=1)	-0.0365
	(0.0960)
Being Left Behind (=1) × Near Stringent *Hukou* cities (=1) × Standardized NTR	-0.0555
	(0.0918)
Male (=1) × Being Left Behind (=1) × Standardized NTR	-0.0598
	(0.0646)
Male (=1) × Near Stringent *Hukou* Cities (=1) × Standardized NTR	0.0171
	(0.0530)
Male (=1) × Being Left Behind (=1) × Near Stringent *Hukou* Cities (=1)× Standardized NTR	-0.0170
	(0.126)
Observations	1,974
R-squared	0.006
F-value of Joint F test	0.750
P-value of Joint F test	0.730

Notes: Near stringent *hukou* cities is an indicator for whether the inverse distance weighted average of the *hukou* index in nearby cities (within 400km) is above average. Robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: 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
Underweight (BMI $<$18.5)	0.107	0.309
Work in Urban Areas (=1)	0.444	0.497
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 A6: 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 to a City with Children (=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 A7: The NTR Gaps Based on Export Weights Better Predict Migration Flows

	(1)	(2)
Dep. Var.	Share of Out-migrants	
Standardized NTR (Export Weights)	0.0289***	
	(0.00385)	
Standardized NTR (Employment Weights)		0.00399
		(0.00458)
Observations	337	337
Adjusted R-squared	0.135	0.006

Notes: The dependent variable is the share of out-migrants in *hukou* population at the prefecture level. Out-migrants are defined as those who had been away from *hukou* location. We use the 2005 Population Census. In column 1, the independent variable is the inverse distance weighted average of NTR gaps (constructed based on export weights) across potential destination cities (defined in equation 1). In column 2, the independent variable is the inverse distance weighted average of NTR gaps (constructed based on employment weights) across potential destination cities (i.e. a shift-share variable that follows equation 1 but replaces export weights with employment weights). We control for city-tier fixed effects. Robust standard errors are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table A8: Robustness of 2015 Children's Outcomes (Table 2): Control for Factors Affecting of the Age of Completing Junior Middle School

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)	-0.0465**	0.0329***	0.000	0.860
	(0.0174)	(0.0100)		
Enrolled in Full-time Precollege (=1)	0.0181	-0.0410**	0.000	0.151
	(0.0217)	(0.0130)		
Enrolled in First-Tier College (=1)	0.0143*	-0.0367***	0.001	0.039
	(0.00762)	(0.00886)		
Completed Junior Middle School (=1)	0.0715***	-0.0290*	0.005	0.834
	(0.0191)	(0.0151)		
Drop off High School (=1)	-0.00208	0.00818*	0.230	0.040
	(0.00496)	(0.00439)		
Good Mandarin (=1)	0.0304	-0.0494*	0.008	0.298
	(0.0314)	(0.0244)		
Panel B: Labor Market Outcomes				
IHS (Hourly Income)	0.139	-0.156**	0.066	1.835
	(0.176)	(0.0536)		
Above Poverty Line (=1)	0.0378	-0.0670***	0.007	0.646
	(0.0421)	(0.0205)		
Work in Non-agricultural Sector (=1)	0.0327**	-0.00937	0.283	0.765
	(0.0125)	(0.0287)		
Have Formal Contract (=1)	0.0339**	-0.0143	0.017	0.416
	(0.0119)	(0.0247)		
Panel C: Health and Welfare Status				
Psychological Problem Index	-0.0954**	0.0437*	0.005	-0.001
	(0.0377)	(0.0221)		
Log Height	0.00809***	-0.000361	0.001	5.123
	(0.00161)	(0.00205)		
Height <Gender-specific Median	-0.101**	0.0696**	0.011	0.451
	(0.0398)	(0.0234)		
Underweight (BMI <18.5)	-0.0462***	0.0262	0.001	0.108
	(0.00753)	(0.0187)		
Work in Urban Areas (=1)	0.0469***	-0.00620	0.018	0.443
	(0.0147)	(0.0281)		
Get Urban Hukou (=1)	0.0146***	-0.00588	0.032	0.101
	(0.00391)	(0.00621)		
Single Parents (=1)	-0.0349**	0.0136	0.003	0.147
	(0.0127)	(0.0143)		
Leaving Children Behind (=1)	-0.0279***	0.0411***	0.000	0.091
	(0.00684)	(0.0128)		

Notes: We create an indicator for factors that may change the age of completing middle school (repeating grades, skipping grades, dropping out of school, temporary suspension of school), and further control for interactions between the indicator, $Female_i$ and $(\overline{Age}_{sch} - Age_{2002})_i$. As in Table 2, 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 β_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 prefecture are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Robustness of 2015 Children's Outcomes (Table 2): Do Not Control for the Number of Children FEs

Dep. Var.	Effect on Boys (β_2)	Effect on Girls ($\beta_1+\beta_2$)	P-value of Diff.	Mean of Dep. Var.
Panel A: Education Outcomes and Skills Later in Life				
Graduated from Full-time Precollege (=1)	0.0448**	-0.0362***	0.000	0.140
	(0.0186)	(0.0105)		
Enrolled in Full-time Precollege (=1)	0.0177	-0.0440***	0.000	0.151
	(0.0236)	(0.0129)		
Enrolled in First-Tier College (=1)	0.0156	-0.0337***	0.001	0.039
	(0.00877)	(0.00813)		
Completed Junior Middle School (=1)	0.0699***	-0.0234	0.009	0.834
	(0.0179)	(0.0134)		
Drop off high school (=1)	-0.0191	0.0121***	0.163	0.041
	(0.0189)	(0.00242)		
Good Mandarin (=1)	0.0405	-0.0530*	0.001	0.298
	(0.0320)	(0.0253)		
Panel B: Labor Market Outcomes				
IHS (Hourly Income)	0.203	-0.153**	0.056	1.835
	(0.206)	(0.0553)		
Above Poverty Line (=1)	0.0567	-0.0704***	0.007	0.645
	(0.0513)	(0.0211)		
Work in Non-agricultural Sector (=1)	0.0496***	-0.0161	0.028	0.765
	(0.0141)	(0.0262)		
Have Formal Contract (=1)	0.0516**	-0.0128	0.000	0.414
	(0.0172)	(0.0254)		
Panel C: Health and Welfare Status				
Psychological Problem Index	-0.120**	0.0473*	0.005	0.000
	(0.0489)	(0.0213)		
Log Height	0.00758***	0.000	0.004	5.124
	(0.00128)	(0.00192)		
Height <Gender-specific Median	-0.0909**	0.0693***	0.010	0.451
	(0.0336)	(0.0215)		
Underweight (BMI <18.5)	-0.0407***	0.0292	0.000	0.107
	(0.00838)	(0.0181)		
Work in Urban Areas (=1)	0.0549**	-0.00838	0.000	0.444
	(0.0209)	(0.0240)		
Get Urban Hukou (=1)	0.0194***	-0.00531	0.025	0.100
	(0.00338)	(0.00731)		
Single Parents (=1)	-0.0474*	0.0202	0.002	0.147
	(0.0236)	(0.0140)		
Leaving Children Behind (=1)	-0.0238***	0.0438**	0.000	0.091
	(0.00524)	(0.0140)		

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 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 β_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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table A10: 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]		
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 β_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 A11: Robustness of 2015 Children's Outcomes (Table 2): Control for Whether a Child is a Minority (interacted with $Female_i$ and $(\overline{Age}_{sch} - Age_{2002})_i$)

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)	0.0471**	-0.0302***	0.000	0.140
	(0.0193)	(0.00945)		
Enrolled in Full-time Precollege (=1)	0.0199	-0.0389**	0.001	0.152
	(0.0245)	(0.0128)		
Enrolled in First-Tier College (=1)	0.0130	-0.0360***	0.000	0.039
	(0.00821)	(0.00834)		
Completed Junior Middle School (=1)	0.0640***	-0.0281*	0.006	0.835
	(0.0168)	(0.0136)		
Drop off High School (=1)	-0.0188	0.00648	0.270	0.041
	(0.0188)	(0.00424)		
Good Mandarin (=1)	0.0354	-0.0501*	0.007	0.298
	(0.0325)	(0.0274)		
Panel B: Labor Market Outcomes				
IHS (Hourly Income)	0.184	-0.157**	0.065	1.837
	(0.204)	(0.0569)		
Above Poverty Line (=1)	0.0519	-0.0663***	0.010	0.646
	(0.0512)	(0.0205)		
Work in Non-agricultural Sector (=1)	0.0465***	-0.00967	0.057	0.765
	(0.0142)	(0.0267)		
Have Formal Contract (=1)	0.0472**	-0.014	0.000	0.415
	(0.0162)	(0.0248)		
Panel C: Health and Welfare Status				
Psychological Problem Index	-0.116**	0.0498**	0.004	0.000
	(0.0435)	(0.0176)		
Log Height	0.00759***	-0.000469	0.004	5.124
	(0.00132)	(0.00204)		
Height <Gender-specific Median	-0.0894**	0.0720**	0.010	0.451
	(0.0330)	(0.0234)		
Underweight (BMI <18.5)	-0.0440***	0.0254	0.000	0.107
	(0.00842)	(0.0177)		
Work in Urban Areas (=1)	0.0571**	-0.00902	0.000	0.444
	(0.0220)	(0.0271)		
Get Urban Hukou (=1)	0.0183***	-0.00645	0.023	0.100
	(0.00370)	(0.00747)		
Single Parents (=1)	-0.0455*	0.0168	0.003	0.147
	(0.0231)	(0.0141)		
Leaving Children Behind (=1)	-0.0235***	0.0429***	0.000	0.091
	(0.00580)	(0.0119)		

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 $(\overline{Age}_{sch} - Age_{2002})_i$. Compared to Table 2, we additionally add interactions between an indicator for whether a child is a minority, female dummy, and $(\overline{Age}_{sch} - Age_{2002})_i$. The effect on boys is measured by β_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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table A12: Robustness of 2015 Children's Outcomes (Table 2): Control for Share of Minorities in Population (interacted with $Female_i$ and $(\overline{Age}_{sch} - Age_{2002})_i$)

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)	0.0475**	-0.033***	0.000	0.140
	(0.0196)	(0.0103)		
Enrolled in Full-time Precollege (=1)	0.0209	-0.0416**	0.000	0.151
	(0.0248)	(0.0135)		
Enrolled in First-Tier College (=1)	0.0134	-0.0362***	0.000	0.039
	(0.00815)	(0.00813)		
Completed Junior Middle School (=1)	0.0670***	-0.0289	0.010	0.834
	(0.0179)	(0.0163)		
Drop off High School (=1)	-0.0168	0.00640	0.291	0.041
	(0.0179)	(0.00444)		
Good Mandarin (=1)	0.0360	-0.0503*	0.006	0.298
	(0.0324)	(0.0269)		
Panel B: Labor Market Outcomes				
IHS (Hourly Income)	0.181	-0.147**	0.070	1.835
	(0.202)	(0.0554)		
Above Poverty Line (=1)	0.0497	-0.0635**	0.009	0.645
	(0.0494)	(0.0210)		
Work in Non-agricultural Sector (=1)	0.0407**	-0.00805	0.123	0.765
	(0.0140)	(0.0266)		
Have Formal Contract (=1)	0.0452***	-0.0117	0.003	0.414
	(0.0138)	(0.0266)		
Panel C: Health and Welfare Status				
Psychological Problem Index	-0.131**	0.0397*	0.001	0.000
	(0.0547)	(0.0187)		
Log Height	0.00760***	-0.000413	0.003	5.124
	(0.00129)	(0.00199)		
Height <Gender-specific Median	-0.0904**	0.0687**	0.012	0.451
	(0.0340)	(0.0230)		
Underweight (BMI <18.5)	-0.0448***	0.0263	0.000	0.107
	(0.00710)	(0.0179)		
Work in Urban Areas (=1)	0.0585**	-0.00985	0.000	0.444
	(0.0216)	(0.0269)		
Get Urban Hukou (=1)	0.0198***	-0.00623	0.042	0.100
	(0.00497)	(0.00749)		
Single Parents (=1)	-0.0482*	0.0153	0.008	0.147
	(0.0240)	(0.0151)		
Leaving Children Behind (=1)	-0.0234***	0.0426**	0.000	0.091
	(0.00565)	(0.0129)		

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 licenses), female dummy and $(\overline{Age}_{sch} - Age_{2002})_i$. Compared to Table 2, we additionally add interactions between the share of minorities in population, female dummy, and $(\overline{Age}_{sch} - Age_{2002})_i$. The effect on boys is measured by β_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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table A13: Robustness of Trade and Parental Absence (Table 3): Control for Minority Status

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Being Left behind by Parents(=1)				
Panel A: Control for Individual Minority Dummy (interacted with Female)					
Standardized NTR × Female	0.0312***	0.0332***	0.0370***	0.0368***	0.0402***
	(0.00911)	(0.00895)	(0.0102)	(0.0105)	(0.00720)
Standardized NTR	0.0441**	0.0398*			
	(0.0149)	(0.0216)			
Female	0.0429**		0.0455***		
	(0.0145)		(0.0140)		
Panel B: Control for Share of Minorities in Population (interacted with Female)					
Standardized NTR × Female	0.0278**	0.0278***	0.0350***	0.0344***	0.0357***
	(0.00969)	(0.00815)	(0.0102)	(0.0104)	(0.00541)
Standardized NTR	0.0438**	0.0392			
	(0.0149)	(0.0224)			
Female	0.0546***		0.0567***		
	(0.0164)		(0.0164)		
City-tier FE	Yes	Yes	No	No	No
City 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	1,408	1,408	1,408	1,408	1,408
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. In Panel A, we additionally control for the minority dummy and its interaction with and the female dummy. In Panel B, we additionally control for the share of minorities in population and its interaction with the female dummy. Robust standard errors clustered at the level of birth prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Table A14: Robustness of Remittance Results (Table 5): Control for Correlates with Children Left Behind

	(1)	(2)	(3)	(4)
Dep. Var.	The Amount of Remittance			
Panel A: Full Sample				
Number of Boys	1,037***	1,025***	1,097***	1,085***
	(118.0)	(118.1)	(128.6)	(128.3)
Number of Girls	680.2***	671.9***	732.1***	726.9***
	(125.9)	(125.9)	(130.0)	(130.3)
Observations	35,310	35,310	35,310	35,310
Mean of Dep. Var.	7925	7925	7925	7925
Panel B: Junior Middle School Age				
Number of Boys	1,754***	1,755***	1,775***	1,774***
	(223.1)	(223.6)	(240.7)	(240.9)
Number of Girls	1,021***	1,021***	895.5***	897.4***
	(193.6)	(194.9)	(195.7)	(195.3)
Observations	7,188	7,188	7,188	7,188
Mean of Dep. Var.	8085	8085	8085	8085
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: We control for the educational attainment of parents (an indicator for whether the father has high school and above education and an indicator for whether the mother has high school and above education), household migration income, and an overall index for gender discrimination in the *hukou* province. 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. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

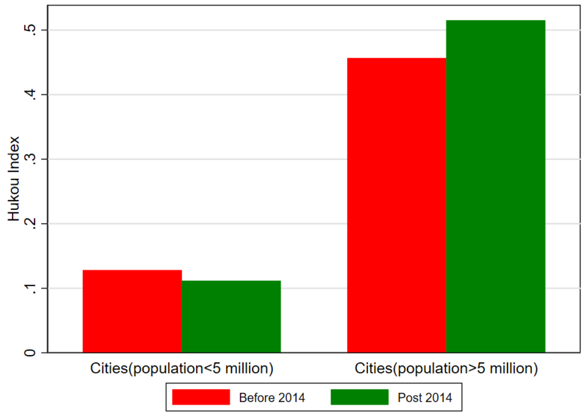
Table A15: Trade Liberalization and the Age of First Job

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 β_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.

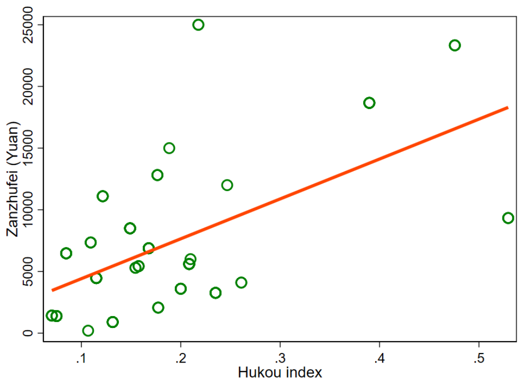
A.4 Additional Facts on *Hukou* Restrictions and Left-Behind Children

Figure A4: 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 A5: *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).

Table A16: 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 A17: 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 A18: 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 A19: Robustness for Table 6: Alternative Controls and Different Bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	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 A20: Triple Difference Regressions

	(1)	(2)	(3)	(4)
Dep. Var.	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 A21: Estimates using the Sample of First-born Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.	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 A22: Controlling for the Effect of the OCP

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	Indicator for leaving the child in rural hometown					
	Female	Male	Female	Male	Female	Male
School-aged × Highly restricted cities (=1)	0.0351**	0.00447	0.0363**	0.00695	0.0364**	0.00547
	(0.0157)	(0.0150)	(0.0159)	(0.0145)	(0.0164)	(0.0148)
Observations	31,071	40,854	30,590	40,094	30,590	40,094
Adjusted R-squared	0.185	0.158	0.183	0.157	0.183	0.157
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 A23: Summary Statistics of Observables for Below and Above the Age Cutoff

	(1) Below age cutoff	(2) Above age cutoff	(3) Diff. in means	(4) 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* location. 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 A24: 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 A25: Migration Responses to 2014 Mega-City Population Controls

	(1)	(2)	(3)
Dep. Var.	Change city location Indicator		
I(Population≥5 million) × I(Year≥2014) × Having a school-aged child (=1)	-0.00128		
	(0.00168)		
I(Population≥5 million) × I(Year≥2014) ×Having a school-aged daughter (=1)		-0.00271	
		(0.00327)	
I(Population≥5 million) × I(Year≥2014) ×Having a school-aged son (=1)			0.000151
			(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	2	2	2

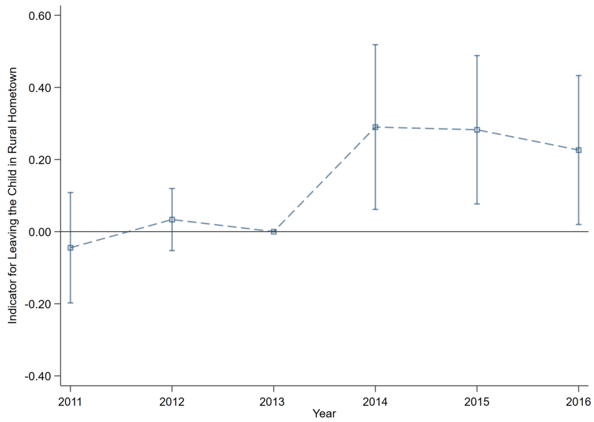
Notes: We employ individual longitudinal panel data from 2011 to 2016 constructed using China Labor-force Dynamic Survey. The dependent variable is an indicator for whether a particular migrant parent changed city location between year t and $t-1$. I(Population≥5 million) is an indicator for whether 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 2 million. We only include cities with baseline population within 3 million and 7 million. Robust standard errors clustered at the city level are reported in parentheses. *** significant at 1%; **significant at 5%; * significant at 10%.

Table A26: Robustness of 2014 Mega-City Policy (Table 7): Different Coming Years

	(1)	(2)	(3)	(4)
Dep. Var.	Indicator for leaving the child in rural hometown			
	Female	Male	Female	Male
Panel A: Came before 2011				
School-aged ×I(Population≥5 million) ×I(Year≥2014)	0.390***	0.0962	0.384***	0.0722
	(0.102)	(0.0916)	(0.103)	(0.0954)
Observations	4,746	6,523	4,746	6,523
Adjusted R-squared	0.201	0.158	0.201	0.160
Panel B: Came before 2010				
School-aged ×I(Population≥5 million) ×I(Year≥2014)	0.370***	0.000	0.363***	-0.0180
	(0.0950)	(0.0916)	(0.0967)	(0.0961)
Observations	3,945	5,491	3,945	5,491
Adjusted R-squared	0.189	0.147	0.188	0.148
Household Control	Yes	Yes	Yes	Yes
City FE×Year FE	Yes	Yes	Yes	Yes
Cohort FE	No	No	Yes	Yes
City FE× Arrival Year FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Age Bandwidth	2	2	2	2
City Size Bandwidth	2	2	2	2

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 2 million. We only include cities with baseline population within 3 million and 7 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%.

Figure A6: Event Study of Girls Being Left Behind



Notes: We test for pre-trends and dynamics of 2014 mega-city policy in an event study framework. Individual level regressions of children from 2011 to 2016. Outcome is an indicator for whether a rural girl was left behind by migrant parents at year t. Specifically, we run the regression: $Left\ behind_{ijt} = \beta_0 + \sum_{t=2011}^{2016} \beta_{1t} School\ Aged_{it} \times I(Pop >= 5\ million)_j \times I(Year = t) + \sum_{t=2011}^{2016} \beta_{2t} I(Pop >= 5\ million)_j \times I(Year = t) + \sum_{t=2011}^{2016} \beta_{3t} School\ Aged_{it} \times I(Year = t) + g(T_i, P_i) + \lambda_{jt} + \chi_{j\tau} + \eta_n + \varphi_{num} + v_{ijt}$. We plot β_{1t} (coefficients on the triple interaction term). Vertical bands represent 90% confidence intervals.

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

Table A27: Differential Returns to Education by Gender

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	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 A28: Differential Returns to Migrating to Other Cities by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	Log individual income					
Migrate to Other 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 Other 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 ω_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 ω_i. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A29: Heterogeneity by the Norm that Aged Parents Are Supported by Sons

	(1)	(2)	(3)	(4)
Dep. Var.	Indicator for leaving the child in rural hometown			
School-aged × Highly restricted cities (=1) ×	-0.00145	-0.00271	-0.0165	-0.0185
Strong norm that the aged are supported by sons (=1)	(0.0214)	(0.0216)	(0.0234)	(0.0235)
Observations	29,336	29,336	29,336	29,336
Adjusted R-squared	0.164	0.165	0.195	0.195
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 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

[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).

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

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. The first column on the left of the Table shows the dependent variable for each regression. Robust standard errors clustered at the level of 3-digits industrial sectors are reported in parentheses.

Regional Balance Test: Table B3 reports our results of regional balance test. We perform the test for all prefectures (columns 1-2) as well as the subsample of prefectures used in the analysis on children born in Gausu (columns 3-4). 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 (columns 2 and 4), 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, population growth rate, non-agricultural employment share, industrial structure, minority share of population, and average wages. Conditional on city-level exposure-weighted average of import tariffs (columns 2 and 4), 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 ratio of labor-force participation, female employment rate, share of skilled workers in employment, non-agricultural employment share, export, GDP, real-estate investment, FDI, minority population share, and among others. Once again, changes in these variables have nothing to do with city-level NTR gaps (columns 2 and 4).

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 Import Tariffs	No Controls	Control for Import Tariffs
	Total Sample		Gansu Sample	
	(1)	(2)	(3)	(4)
Panel A: Demographic and Education Indicators				
Share of Female Population, 2000	0.000400	0.000162	-0.000219	-0.000453
	(0.000433)	(0.000454)	(0.000728)	(0.000468)
Share of Highschool Educated Females, 2000	-0.00234	-0.00150	-0.00213	-0.00293
	(0.00282)	(0.00302)	(0.00735)	(0.00446)
Share of Highschool Educated Males, 2000	-7.80e-05	0.00108	-0.00208	-0.000549
	(0.00277)	(0.00294)	(0.00665)	(0.00450)
Gender Ratio for High School Graduates, 1990-2000	0.00830	0.00889	-0.00248	0.0106
	(0.00877)	(0.00959)	(0.0214)	(0.0108)
Change in the Share of High School Educated Females, 1990-2000	-0.000172	0.000393	0.000791	0.0106
	(0.00208)	(0.00223)	(0.00781)	(0.0108)
Change in the Share of College Educated Females, 1990-2000	-0.000798	-0.000784	-0.00127	-0.000891
	(0.000522)	(0.000579)	(0.000860)	(0.000882)
Log Change in Chinese Colleges,1997-2000	0.0157	0.0176	0.00130	0.0734
	(0.0116)	(0.0122)	(0.0536)	(0.0608)
Log Change in Chinese Middle Schools,1997-2000	0.00708	-0.000565	-0.0185	-0.0128
	(0.00749)	(0.00794)	(0.0299)	(0.0302)
Log Change in College Students,1997-2000	0.0165*	0.0106	-0.0183	0.0362
	(0.00961)	(0.0109)	(0.0305)	(0.0459)
Log Change in Chinese Middle Students ,1997-2000	0.00246	-0.00730	0.0342	0.0182
	(0.0214)	(0.0233)	(0.131)	(0.137)
Panel B: Economic and Employment Indicators				
Female Employment Rate, 2000	0.0107	0.0110	0.0116	0.0104
	(0.00657)	(0.00719)	(0.0147)	(0.0101)
Male Employment Rate, 2000	0.00165	0.00173	-0.00249	-0.00306
	(0.00302)	(0.00329)	(0.00279)	(0.00208)
Population Growth Rate ,1997	0.176	0.208	-0.667	-0.123
	(0.132)	(0.132)	(0.518)	(0.169)
Population Growth Rate , 2000	0.101	0.138	-1.389**	-0.675
	(0.264)	(0.303)	(0.576)	(0.579)
GDP Growth Rate , 1997	-2.092	-3.289	-1.081	1.059
	(3.044)	(3.568)	(8.875)	(7.837)
GDP Growth Rate , 2000	-0.0933	0.0198	-0.614	-0.412
	(0.180)	(0.214)	(0.952)	(1.019)
Non-agricultural Employment Share, 2000	-0.932**	-0.482	0.955	0.460
	(0.456)	(0.531)	(0.678)	(0.683)
Minority Share, 1990	-0.0110	-0.0174	-0.0137	-0.0180
	(0.0133)	(0.0143)	(0.0160)	(0.0150)
Average Wages, 2000	52.15	50.50	-356.9**	-220.9
	(123.1)	(145.4)	(151.2)	(144.9)
Industrial Structure (Production Ratio between Tertiary and Secondary Sectors), 1997	0.0168	-0.0136	0.141**	0.0933
	(0.0135)	(0.0121)	(0.0564)	(0.0605)
Change in Labor-force Gender Ratio, 1990-2000	-0.00351	-0.00222	-0.0121	-0.00491
	(0.00508)	(0.00549)	(0.0185)	(0.0186)
Change in Female Employment Rate, 1990-2000	0.00308	0.00340	-0.00812	-0.00903
	(0.00429)	(0.00463)	(0.00807)	(0.00832)
Change in Share of Skilled workers (college educated) in Total Employment, 1990-2000	-4.08e-05	-0.000299	0.000200	0.000692
	(0.000296)	(0.000284)	(0.00146)	(0.00145)
Change in Non-agricultural Employment Share, 1997-2000	0.301*	0.0614	0.754**	0.360
	(0.166)	(0.199)	(0.372)	(0.407)
Change in Second Sector Employment Share , 1997-2000	-1.159**	-0.273	-0.0710	0.916
	(0.556)	(0.525)	(1.402)	(1.338)
Change in the Proportion of the Secondary Industry in GDP,1995-2000	0.358	0.221	0.915	0.551
	(0.345)	(0.390)	(0.905)	(0.993)
Change in the Proportion of the Tertiary Industry in GDP,1995-2000	0.186	0.275	0.0901	0.244
	(0.279)	(0.304)	(0.839)	(0.882)
Log Change in GDP.1995-2000	0.00606	-0.00338	0.0758	0.0869
	(0.0206)	(0.0227)	(0.120)	(0.116)
Log Change in Exports,1997-2000	-0.0279	-0.0222	-0.0702	-0.124
	(0.0386)	(0.0417)	(0.137)	(0.111)
Log Change in Real Estate Investment, 1997-2000	-0.0227	-0.0162	-0.0841*	-0.0224
	(0.0180)	(0.0201)	(0.0511)	(0.0472)
Log Change in FDI, 1997-2000	-0.0629	0.0291	-0.446	0.159
	(0.0547)	(0.0576)	(0.315)	(0.360)
Change in Minority Share,1990-2000	-0.00416**	-0.00322	-0.0171	-0.0159
	(0.00189)	(0.00200)	(0.0109)	(0.0121)

Notes: We regress baseline city attributes and their changes on city-level average NTR gaps. Each row represents a separate regression. The column on the left of the Table shows the dependent variable for each regression, and the independent variable of interest is city-level average NTR gaps. We use all prefectures in columns 1-2 and use a sample of prefectures used in our analysis on Gansu in columns 3-4. We control for city-level exposure-weighted average of import tariffs in columns 2 and 4. 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:

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

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 B4 presents the results of individual-level subsample regressions of equation B.1, using robust standard errors clustered at the level of birth prefecture. 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, psychological 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:

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

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

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

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 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 β_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)
Underweight (BMI $<$18.5)	-0.0474***	0.0319*
	(0.00540)	(0.0168)
Work in Urban Areas (=1)	0.0562**	-0.0112
	(0.0217)	(0.0285)
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 prefecture 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)
Underweight (BMI <18.5)	-0.0474***	0.0319**
	(0.000906)	(0.0128)
Work in Urban Areas (=1)	0.0562**	-0.0112
	(0.00766)	(0.0131)
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)		
Underweight (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)		
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 prefecture 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)		
Underweight (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)		
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 prefecture are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B.5 Additional Tests Results

Table B9: Robustness of 2015 Children's Outcomes (Table 2): Control for the Share of Exports in Local GDP (interacted with $Female_i$ and $(\overline{Age}_{sch} - Age_{2002})_i$)

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)	0.0342	-0.0411**	0.000	0.140
	(0.0190)	(0.0130)		
Enrolled in Full-time Precollege (=1)	-0.00957	-0.0581***	0.002	0.151
	(0.0243)	(0.0144)		
Enrolled in First-Tier College (=1)	-0.0252***	-0.0390***	0.113	0.039
	(0.00309)	(0.00911)		
Completed Junior Middle School (=1)	0.0409*	-0.0557*	0.053	0.834
	(0.0202)	(0.0267)		
Drop off High School (=1)	-0.0336	0.0152**	0.076	0.041
	(0.0202)	(0.00577)		
Good Mandarin (=1)	0.0249	-0.0921***	0.007	0.298
	(0.0423)	(0.0258)		
Panel B: Labor Market Outcomes				
IHS (Hourly Income)	0.312	-0.198***	0.028	1.835
	(0.226)	(0.0593)		
Above Poverty Line (=1)	0.0772	-0.112***	0.001	0.645
	(0.0565)	(0.0177)		
Work in Non-agricultural Sector (=1)	0.0963***	-0.0544*	0.000	0.765
	(0.0145)	(0.0258)		
Have Formal Contract (=1)	0.0402*	-0.0627**	0.000	0.414
	(0.0191)	(0.0258)		
Panel C: Health and Welfare Status				
Psychological Problem Index	-0.263***	-0.0347	0.004	0.000
	(0.0443)	(0.0272)		
Log Height	0.0108***	0.00402*	0.053	5.124
	(0.00170)	(0.00214)		
Height $<$Gender-specific Median	-0.127**	0.0385	0.057	0.451
	(0.0477)	(0.0315)		
Underweight (BMI $<$18.5)	-0.0410***	0.0464*	0.001	0.107
	(0.00980)	(0.0217)		
Work in Urban Areas (=1)	0.0874***	0.00283	0.001	0.444
	(0.0213)	(0.0287)		
Get Urban Hukou (=1)	0.0139**	-0.0246**	0.018	0.100
	(0.00489)	(0.00915)		
Single Parents (=1)	-0.0468*	0.0229	0.007	0.147
	(0.0248)	(0.0251)		
Leaving Children Behind (=1)	-0.0264**	0.0101	0.000	0.091
	(0.0103)	(0.0143)		

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 $(\overline{Age}_{sch} - Age_{2002})_i$. Compared to Table 2, we additionally add interactions between an indicator for the share of exports in local GDP, female dummy, and $(\overline{Age}_{sch} - Age_{2002})_i$. The effect on boys is measured by β_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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

C Beyond Gansu: Effects of the Trade Shock on Children Nationwide

Section 4 tracks the effects of trade liberalization using unique longitudinal data from Gansu, and in this appendix we explore whether those Gansu-specific insights generalize to the rest of China. The China Family Panel Studies (CFPS) is a nationally representative survey of Chinese families and individuals. We extract from this dataset the subset of 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 is the only wave of the survey that records the exact birth location of each individual. We construct a measure of exposure to trade liberalization based on birth location. Our final sample includes 2616 rural children aged < 16 in 2002 from 24 provinces. While GSCF provides detailed long-run data on Gansu children, CFPS allows us to replicate results for all of China. We employ the same empirical strategy specified in equation 2 in Section 4.3.

Table C1: Trade Liberalization and Children's Outcomes: CFPS 2010

Dep. Var.	Effect on Boys (β_2)	Effect on girls ($\beta_1+\beta_2$)	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 estimate individual-level regressions based on equation 2 in Section 4.3. 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 β_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 prefecture are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

In Table C1, we use CFPS 2010 to revisit the association between trade liberalization and gender inequalities. We find very similar results on girls' outcomes as in Gansu. Girls with greater exposure to trade demand shocks report 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.[6]

Both GSCF results and CFPS results show that the effects of trade exposure on girls are always negative (Tables 2 and C1). Thus, the effects on girls observed in Gansu also hold up for other parts of China. Nevertheless, we acknowledge that the effects on boys seem to be sensitive to the data source. The GSCF results show that trade liberalization sometimes benefits boys, whereas the CFPS results show that trade exposure may not affect boys. One possible reason for the different results for boys is that one parent was away for most left-behind children in Gansu, while left-behind children in other provinces may be separated from both parents.[7] This however does not affect our gendered empirical pattern (reflected by both GSCF and CFPS) that girls are more adversely affected by trade liberalization (compared to boys).

Table C2: Gender Gaps Are Not Significantly Different between Gausu and Other Provinces

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Self-reported Good Health	Junior Middle School and Above	High school and Above	College and Above	Annual Income
Female (=1)	-0.0922***	-0.0391**	-0.0450	0.00302	-13,544***
	(0.0257)	(0.0147)	(0.0336)	(0.0187)	(3,788)
Female (=1) × Gansu (=1)	-0.00516	-0.0182	-0.0392	-0.0176	991.6
	(0.0257)	(0.0147)	(0.0336)	(0.0187)	(3,788)
Observations	2,962	2,962	2,962	2,962	1,799
Born Province by Cohort FE	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	0.555	0.848	0.459	0.106	31155
Adjusted R-squared	0.0381	0.0679	0.0870	0.00784	0.0270

Notes: We combine GSCF 2015 and CFPS 2016 and limit the sample to those who were born with a rural *hukou*. We exclude individuals born in Gansu provinces in CFPS 2016. CFPS 2010 records the exact birth location of each individual, whereas CFPS 2016 does not have this information for all individuals. We only use individuals that are included in CFPS 2010 and can be tracked in CFPS 2016, because we can obtain birth location information of these individuals based on their records in CFPS 2010. Gansu is an indicator for whether a particular individual was born in Gansu. We control for birth province by age cohort fixed effects. Self-reported good heath a dummy defined based on a continuous measure of health conditions reported by respondents (1=super healthy, 2=very healthy, 3=healthy, 4= unhealthy, 5=very unhealthy). The indicator equals one if an individual's response is 1, 2 or 3. Robust standard errors clustered at the level of birth province are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[6] We also compare the average gender gap between individuals born in Gansu and in other provinces. Specifically, we combine GSCF data and data on other Chinese provinces from CFPS. Appendix Table C2 shows that average gender gaps in self-reported health, education and earnings are not significantly different between Gausu and other provinces.

[7] 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.

D Mental Health and Other Early Life Outcomes for Gansu Children

In section 5, we explore why girls are disproportionately suffer when parents gain new economic opportunities from trade liberalization. According to literature in psychology and sociology, young girls' mental health is more vulnerable to parental absence than young boys', (Wu et al., 2019; Zhao and Yu, 2016; Culpin et al., 2013).[8] 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 D1 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 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 D1 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.

[8] 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.

Table D1: The Effects of Trade Liberalization on Early-Life Outcomes in 2004 and 2009

Dep. Var.	Effect on Boys (β_2)	Effect on girls ($\beta_1+\beta_2$)	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 β_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 bad temper, combativeness, tiredness, withdrawn, 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 prefecture are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

E A Multinomial Logit Analysis on Parents' Migration Choices

The 2010 Population Census China conducts its national population census every ten years. 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 2010 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. (2025), 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.

Effects of Hukou Policy on Parents' Migration Choices For a child i, parents 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 *hukou* location with the child, or (2) migrate to a city leaving the child behind in the rural area, or (3) migrate with the child.[9] We construct a multinomial 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 for whom 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.

[9] Our sample includes both children of first-time migrants and children of migrants who migrated years prior to the school-age cutoff. Since it becomes more difficult and costly to bring children to other cities as they become older, parents would start to bring their children at an early age rather than at middle school age (if they choose to migrate with children). Thus, these two cases (parents suddenly migrate to other cities when children reach middle school age, and parents migrated years prior and suddenly bring their children to their destination city when they reach middle school age) are unlikely to happen.

Indirect utility as defined below leads to a trichotomous choice regression we estimate:

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

$SchoolAged_i$ 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, $\sum_d \left(\frac{\left(\frac{1}{dist_{dc}}\right)}{\sum_m \left(\frac{1}{dist_{mc}}\right)} \times Hukou\ Index_d \right)$. 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 (φ_{num}). We estimate equation E.1 using the 2010 Census, which includes three groups of children—those staying in the village with parents, those migrating with parents, and those left behind by parents.

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 ($T_i = 0$)[10], 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.

Hukou policies in potential destination cities may be correlated with other city characteristics, 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 change 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 E1 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

[10]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 E1: 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	
Dep. Var.	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		
Dep. Var.	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 a city and leaving children behind. All in city represents the the choice of migrating to a city with children. We use the choice of migrating to a city 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.

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. Average marginal effects computed in Panel B [11] 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 E1 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

[11] Each rural child faces three outcomes (stay in the village with parents, stay in the village without parents, migrate to a city with parents). Table E1 panel B shows, *averaged* across all children, the marginal effect of the change in a regressor on the probability of a particular outcome.

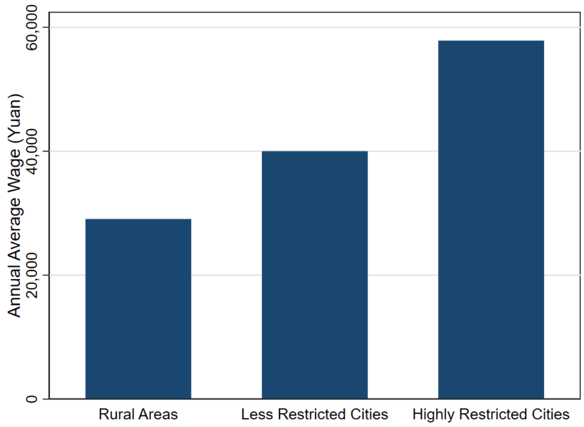
larger than the corresponding effect on sons. When faced with restrictions on children's educational opportunities in cities, rural parents appear more willing to separate from daughters than from sons. Although China's *hukou* regulations are not gender-specific in intent, they create a gendered inequity.

Panel A columns 1, 3, 5 in Table E1 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 E1). Instead, the decision on whether to bring their children with them or not is the margin that adjusts as schooling costs change.[12] *Hukou* policy restrictiveness may be correlated with economic prosperity and population in a city. To account for the effects of these potential confounders, we additionally control for the interactions between inverse distance-weighted sums of these potential correlates, i.e., $\sum_d \left(\frac{\left(\frac{1}{dist_{dc}}\right)}{\sum_m \left(\frac{1}{dist_{mc}}\right)} \times Correlates_d \right)$, $SchoolAged_i$ and the $Female_i$. Appendix Table E3 shows that adding these controls barely change our empirical pattern.

Are Children Left Behind without *Either* Parent Present? In Appendix Table E2 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 E1. This is relevant because the emotional toll and developmental burden on children are likely larger when both parents are absent (Zhang et al., 2014).

[12] Table E4 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.

Figure E1: 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 E2: Marginal Effects on the Probability that One versus Both Parents are Away

Dep. Var.	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 a city 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.

Table E3: Control for GDP per Capita, Population and Wages (Robustness of Multinomial Logit Estimates in Table E1)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Coefficient Estimates from Multinomial Logit Model					
	Sample of Daughters		Sample of Sons		Full Sample	
Dep. Var.	All in Rural	Separation	All in Rural	Separation	All in Rural	Separation
School-aged	0.293	1.874	0.347	1.389	0.326	1.542
	(1.184)	(1.455)	(1.680)	(2.121)	(1.154)	(1.389)
School-aged × Standardized weighted Standardized weighted *hukou* index	0.0737	0.555**	0.0120	0.137	0.0239	0.160
	(0.150)	(0.219)	(0.144)	(0.281)	(0.122)	(0.249)
School-aged × Standardized weighted *hukou* index × Female					0.0343	0.382*
					(0.113)	(0.201)
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		
Dep. Var.	All in Rural	Separation	All in City	All in Rural	Separation	All in City
School-aged	-0.0266	0.0185	0.00809	-0.0127	-0.00620	0.0189
	(0.0177)	(0.0133)	(0.0133)	(0.0175)	(0.0116)	(0.0134)
School-aged × Standardized weighted *hukou* index	-0.0135	0.0220**	-0.00850	-0.00438	0.00617	-0.00179
	(0.0158)	(0.00953)	(0.0121)	(0.0203)	(0.0144)	(0.0119)
Observations	9,020	9,020	9,020	10,640	10,640	10,640
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. We additionally control for interactions between inverse distance-weighted sums of the potential correlates of *hukou* policy restrictiveness (GDP per capita, population and wages) across potential destination cities within 400 km, $SchoolAged_i$ and the $Female_i$. All in rural represents the choice of staying in the village with children. Separation represents the choice of migrating to a city and leaving children behind. All in city represents the the choice of migrating to a city with children. We use the choice of migrating to a city 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E4: Average Marginal Effects: Alternative Controls and Bandwidths (Robustness of Multinomial Logit Estimates in Table E1)

	(1)	(2)	(3)	(4)	(5)	(6)
		Female			Male	
Dep. Var.	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 a city and leaving children behind. All in city represents the the choice of migrating to a city 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.

F A Summary of Other Studies on Left-Behind Children in China

Table F1: A Summary of Papers on Left-Behind Children in China

Paper	Main Findings	Data	Identification
Bai et al. (2018)	Parental migration has significantly positive effects on the academic performance (i.e. English test scores) of left behind children.	Data of 13,000 primary school students in Haidong, Hainan Province from 2013-2014, collected by the author	Difference in Difference based on changes in parental migration status+ Propensity score matching. The paper analyzes the short-term effects of parental migration.
Bai et al. (2022)	Maternal migration negatively affects the cognitive development of children.	A panel data of children from infancy to 63 months in 11 counties in the Qinba Mountain region.	Difference in Difference based on changes in maternal migration status. The paper analyzes the short-term effects of parental migration.
Démurger and Xu (2015)	Having a child in primary school (in rural hometown) induces migrant parents to delay their return. The interpretation is that parents need to work in cities to accumulate money for their children's education.	Data of 284 rural workers from Wuwei County in 2008.	No identification strategy.

Paper	Main Findings	Data	Identification
Démurger and Wang (2016)	Remittance income in rural China leads to an increased per capita consumption, an increased budget share of housing and other durable goods, and a decreased budget share of education expenditure.	Rural–Urban Migration in China (RUMiC) Survey 2008.	Propensity score matching: identification based on the comparison of households with and without remittance income.
Dong et al. (2021)	Parental migration increases the educational investment in children but does not have any effect on the years of schooling of children left behind.	China Rural Development Survey (CRDS) 2005, 2008, 2012 and 2016.	Fixed effects Model
Hu (2013)	Parental absence has a significantly negative effect on children's academic performance.	The Gansu Survey of Children and Families (GSCF), 2004.	The analysis constructs IVs for parental migration based on migration networks. IV1: the ratio of migrant households in the home village. IV2: a dummy for the presence of more than one migrant in a given household four years prior to 2004 The paper analyzes the short-term effects of parental migration.

Paper	Main Findings	Data	Identification
Li et al. (2015)	Children left behind in rural areas are more likely to get sick or develop chronic conditions than those living with their parents.	China Health and Nutrition Survey (CHNS)	Use parental peers' migration rates as IVs for the migration decisions of individual parents. The paper analyzes the short-term effects of parental migration.
Mao et al. (2020)	Parental absence in childhood is negatively associated with children's cognitive test score , academic test score and their likelihood of attending college later in life.	China Education Panel Survey (CEPS), 2013-2014. The China Family Panel Studies (CFPS) 2010 and 2018.	Use the share of left-behind children within the same school as an IV for the parental absence experienced by a particular child. The paper analyzes the short-term effects of parental migration.
Meng and Yamauchi (2017)	Exposure to cumulative parental migration has a sizable negative effect on children's education and health outcomes.	The Rural-Urban Migration Survey in China (RUMiC) data, 2009 and 2010.	The analysis instruments parental absence with weather changes in home villages when parents were aged 16–25, when they were most likely to initiate migration. The paper analyzes the short-term effects of parental migration.
Mu and De Brauw (2015)	Parental migration has no significant effect on the height of children, but it improves their weight.	The China Health and Nutrition Survey (CHNS), 1997, 2000, 2004, and 2006.	Use the interaction terms between gender-specific wage growth in provincial capital cities and initial village migrant networks as an IV for parental migration. The paper analyzes the short-term effects of parental migration.

Paper	Main Findings	Data	Identification
Shen et al. (2021)	Father's absence has a detrimental effect on children's educational attainment. At the same time, father's absence is associated with better family economic capital, which partially buffers the negative effect of father absence.	The Gansu Survey of Children and Families (GSCF), 2000 and 2015.	The Structural equation modeling (SEM) with inverse probability of treatment weighting (IPTW).
Wang et al. (2023)	Being left behind causes increased rates of norm disobedience for children. The effect is stronger for rural girls left-behind than other children.	The China Education Panel Survey (CEPS), 2013-2015.	The analysis uses propensity score matching (PSM) to compare the behaviors of children who share the same observable characteristics, differing only in whether they were left behind. The paper analyzes the short-term effects of parental migration.
Zheng et al. (2022)	Parental absence in childhood has significant negative effects on people's cognitive skills, personality traits, physical and mental health, and years of schooling later in life.	China Family Panel Studies (CFPS), 2010 and 2014.	The analysis uses the historical migration rate in birth county and historical rainfall shocks as IVs for parental absence in childhood.

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