Educational responses to local and migration destination shocks: Evidence from China*

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Abstract: This paper analyzes the effects of positive shocks to export-oriented industries following China's accession to the World Trade Organization on human capital investment in urban and rural areas. Exploiting cross-county variation in the reduction in export tariff uncertainty both locally and at plausible migration destinations, we find that youth reaching matriculation age post-accession in counties experiencing a larger export shock show a lower probability of enrolling in high school. The effects of local shocks are generally larger than the effects of shocks at migration destinations, but the opposite pattern is observed for rural youth.

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1 Introduction

In recent decades, the rising tide of globalization has had substantial effects on developing country economies (Goldberg and Pavcnik, 2007). Among the most important of these effects has been the contraction of agriculture and the associated shift of productive factors into non-agricultural production, a shift that frequently entails within-country migration to export-oriented urban areas. In addition to a substantial theoretical literature predicting a reallocation of workers from less income-elastic sectors such as agricultural production into more income-elastic sectors (e.g., manufacturing) in response to increased access to export markets (Matsuyama, 2009; Herrendorf, Rogerson and Valentinyi, 2014; Matsuyama, 2018), this prediction has been substantiated empirically in the context of China and Vietnam (Erten and Leight, 2020; McCaig and Pavcnik, 2013). There is also evidence of meaningful inmigration into urban areas experiencing positive export-driven shocks in China (Facchini et al., 2019).

Major sectoral shifts in the labor market stimulated by export shocks presumably have substantial implications for human capital investment, and the effects of trade liberalization on education have been previously explored in papers focused on Mexico, India, and China (Atkin, 2016; Edmonds, Topalova and Pavcnik, 2009; Edmonds, Pavcnik and Topalova, 2010; Li, 2018; Lin and Long, 2020). However, this literature has generally focused on the effects of local shocks (within a metropolitan area labor market), and has not systematically analyzed the effects of export shocks at plausible migration destinations. Given that the existing literature suggests that outmigration from rural areas is an important dimension of structural transformation, ignoring the relationship between youth educational decisions and destination shocks may meaningfully underestimate the role of export-oriented shocks in shaping human capital accumulation.

Our objective in this paper is to analyze the effects of positive shocks to export-oriented production on educational attainment among Chinese youth, analyzing shocks to both the local economy and the economies of migration destinations as predicted by past migration patterns. More specifically, we exploit a discontinuity generated by China's WTO accession and the associated reduction in tariff uncertainty in the U.S. market. Prior to WTO accession, China's Most Favored Nation (MFN) status in the U.S. required annual renewal by Congress, a process entailing considerable risk; if the renewal had failed, Chinese exports would have been subject to the much higher rates reserved for non-market economies. As of January 1, 2002, this uncertainty was reduced to zero as China became a WTO member, a positive shock that disproportionately benefited industries exposed to high uncertainty ex ante, and regions characterized by a high level of concentration in these industries. The magnitude of pre-accession uncertainty is captured by a measure denoted the Normal Trade Relations (NTR) gap, equal to the average difference between the lower tariffs provided to countries benefiting from NTR status, and the higher tariff imposed on non-NTR nations.

Our identification strategy then entails a difference-in-difference, comparing youth who reached the age of 16 — the age of matriculation into high school in China – before and after WTO accession, in counties differentially exposed to the reduction in tariff uncertainty. Here, we capture exposure using both the local shock and the shock observed in plausible migration destination counties, the latter proxied by counties where local residents have previously migrated. Intuitively, counties that were more exposed to a reduction in tariff uncertainty experienced a larger increase in export-driven manufacturing and associated labor demand post-2002 (Erten and Leight, 2020). Our measures capture this increase in labor demand in both the local and the migration destination markets.

This positive shock to labor demand in export-oriented industries may have several effects: household income may increase if parents access new employment in export-driven industries; higher demand for manufacturing labor, either locally or at migration destinations, may offer adolescents a more attractive outside option vis-a-vis education; there may be supply-side shifts in education if the trade shock has local general equilibrium effects; and the long-run returns to education may also change, plausibly increasing if export expansion is associated with an increase in the returns to skill (Goldberg and

Pavcnik, 2007). Utilizing micro-level data on educational attainment reported in the China Household Income Project Surveys in 2007, we identify the effects of this export-driven shock on enrollment in high school for a sample of youth in both urban and rural China and present evidence about the relevant channels. Importantly, this is the first paper to analyze the effects of this shock on a full sample of youth including permanent outmigrants, a sample structure that is crucial to accurately estimate the effects of migration destination shocks.

Our primary results suggest that youth reaching the point of high school matriculation post-2002 in counties exposed to higher NTR gaps either locally or at migration destinations show a significant decline in the probability of matriculation: the effect size suggests a one standard deviation increase in the NTR gap in either setting is associated with a decline in this probability of between two and six percentage points. This decline is observed even in specifications controlling for a range of individual covariates as well as province-year fixed effects, and accordingly does not reflect differential patterns in high school enrollment comparing across highly industrialized and less industrialized provinces. We also present evidence that high and low NTR gap counties were previously characterized by largely similar trends in high school enrollment, with the divergent pattern driven by differential labor demand emerging only post-2002. We also demonstrate that our main results are robust to a range of assumptions around violations of the parallel trends assumption (Roth and Rambachan, 2020).

The evidence of a decrease in high school enrollment in areas characterized by larger positive shocks to export-driven production is not consistent with a hypothesized positive income effect, assuming that education is a normal good, but rather suggests that the short-term opportunity costs of education in a context of increased labor demand are driving youths' enrollment decisions. We also explore some alternate mechanisms — shifts in the implied skill premium, a reduction in educational supervision by parents experiencing increased hours at work, or differential fiscal investments in education — and find no evidence that these mechanisms are operational in this context.

This is the first paper to our knowledge to analyze the effects of trade shocks on human capital accumulation exploiting both local and migration destination shocks, and adds to a very limited literature that has shown any response of potential migrants' human capital choices to economic shocks at migration destinations or shocks to the returns to potential migration (as distinct from shocks to migration costs). Using both micro-level survey data and county-level fiscal data, it is also among the first to identify the relevant household demand channels for the documented effects as well as possible supply-side explanations through general equilibrium effects.

Our paper also contributes to several related strands of literature. First, we add to the growing evidence base around the relationship between trade shocks and human capital accumulation. In this literature, Atkin (2016) finds that the growth of export-driven manufacturing in Mexico is associated with a reduction in high school employment in areas characterized by a more rapid pace of factory openings, a pattern consistent with our findings. In Bangladesh, by contrast, the growth of export-driven garment manufacturing is associated with increased educational attainment for girls (Heath and Mobarak, 2015). In India, Edmonds, Topalova and Pavcnik (2009) and Edmonds, Pavcnik and Topalova (2010) analyze the inverse case of trade reform — the reduction of protective import tariffs — and find that increased import competition is associated with a decline in school attendance. In general, however, these papers do not analyze shocks at potential migration destinations, and often exclude migrants entirely, despite the fact that internal migration rates are as high or higher in these contexts vis-a-vis China (Bell et al., 2015).[1]

In China, previous papers present evidence that reductions in external tariffs (Li, 2018; Li et al., 2019) or tariff uncertainty (Liu, 2018) or increases in exposure to globalization broadly defined (Lin and Long, 2020) are associated with differential effects on local educational attainment driven by the skill requirements of local industry. Our empirical strategy entails two important innovations that are novel in this literature: we use micro-level data that enables us to track all youth from a given household, includ-

[1]One recent paper analyzes the effects of shifts in the level of tariffs at migration destinations on educational choices, but uses only aggregate regional data (Cai, Shi and Xu, 2021).

ing outmigrants, and we separately identify the effect of trade liberalization shocks both locally and at migration destinations.

Second, our paper adds to a very limited literature that analyzes the response of potential migrants' human capital accumulation to economic shocks at migration destinations. One previous paper shows that rural human capital accumulation in India responds to regional variation in the urban returns to education, particularly for those with a higher propensity to migration (Kochar, 2004). Two papers analyzing international migration find evidence of an increase in educational attainment in Nepal associated with an increase in the returns to education in one potential migration destination (the United Kingdom, or more specifically its armed forces) (Shrestha, 2016), and an increase in the occupation-specific enrollment of college graduates in India in response to positive shocks to the informational technology sector in the U.S. (Khanna and Morales, 2020). Given meaningful migration rates across much of the developing world (Bell et al., 2015), this is an important and understudied channel for understanding human capital decision-making.

Finally, our paper connects to the literature on the relationship between the expansion of non-agricultural production in developing countries and human capital attainment, a relationship that is generally found to be positive. Evidence from Indonesia suggests manufacturing employment growth in the region modestly increases enrollment for both male and female youth (Federman and Levine, 2005). In India, the expansion of call centers (corresponding to more advanced positions in services, rather than manufacturing) is associated with increased enrollment of children in primary school (Oster and Steinberg, 2013), and experimental evidence suggests that dissemination of information about outsourcing opportunities similarly leads to increased educational attainment for young women (Jensen, 2012). A broader analysis of industrialization in Mexico finds evidence of small positive effects of industrialization on education, larger for domestic-oriented manufacturing vis-a-vis export-oriented manufacturing (Brun, Helper and Levine, 2011).

This paper proceeds as follows. Section 2 provides an overview of the institutional context and the conceptual framework. Section 3 describes the data, and Section 4 describes the empirical strategy and primary results. Section 5 concludes.

2 Background and Conceptual Framework

2.1 China's Export Expansion

China's accession to the WTO in 2001 entailed both new trade access benefits for the Chinese economy and a commitment to liberalizing domestic reforms. However, both the benefits and the reforms were largely phased in gradually, and did not result in any discontinuous jumps in 2001: in particular, both China's domestic tariffs and the tariffs imposed by external partners declined incrementally, and primarily in the 1990s.[2]

First, Chinese import tariffs had already been sharply cut prior to 2001 (from a weighted average of over 45% in 1992 to approximately 13%). WTO accession entailed further cuts, but these shifts were small in magnitude (Bhattasali, Li and Martin, 2004). Similarly, the level of tariffs imposed by the U.S. and other major trading partners (the European Union, Japan, Korea, and Taiwan) were largely stable in this period. Figure A1 in the Appendix shows the evolution of the average weighted domestic tariff rate and the average weighted NTR rate imposed in the U.S. market. These rates are calculated using industry-level tariffs and the share of each industry in total Chinese imports (for import tariffs) or total Chinese exports (for U.S. tariffs) as reported in 1996. There is no evidence of any dramatic shifts in tariff rates at the point of China's WTO accession.[3]

However, China did experience one discontinuous shock in 2002, a reduction in tariff uncertainty in the U.S market. Previously, China accessed NTR tariff rates in the U.S. subject to annual congressional renewals. In the absence of these renewals, Chinese

[2]Other gradual trade reforms implemented during this period included the loosening of restrictions on direct exporting, eliminated by 2004 (Bai, Krishna and Ma, 2017), and the reduction of requirements for foreign direct investment (Long, 2005). This description of the evolution of trade policy in this period draws heavily on Erten and Leight (2020).

[3]A similar pattern is evident if we examine the tariffs imposed in the other four major export markets; graphical evidence is provided in Erten and Leight (2020).

products would have faced much higher tariffs, originally set by the Smoot-Hawley Act in 1930, and designated for non-market economies. This regular approval process generated considerable uncertainty, despite the fact that the tariff imposed on imports did remain low. Using media and government reports, Pierce and Schott (2016) document that firms did not perceive the annual renewal of MFN status as guaranteed, particularly in periods of political tension in the early 1990s. The U.S. Congress passed legislation in October 2000 that granted permanent NTR status to China, effective as of January 1, 2002.

In this paper, we preferentially focus on analysis of the discontinuous shock induced by the reduction in tariff uncertainty for both conceptual and empirical reasons. Previous evidence suggests the effect of this shock was large in both the U.S. and China, and larger than the effect of other trade policy fluctuations in this period (Pierce and Schott, 2016; Handley and Limão, 2017; Erten and Leight, 2020). We can also verify this large effect in the subsample of counties examined here: Figure 1 shows the correlation between the estimated long-difference (2001–2011) for county-level exports and county-level GDP, both vis-a-vis the county-level NTR gap. It is evident that this correlation is significant and positive, suggesting that counties characterized by higher NTR gaps show more rapid export-driven growth in the post-WTO period. (By contrast, the corresponding correlation in the pre-WTO period, shown in Figure A2, is weakly negative and statistically insignificant, suggesting that the correlation subsequently observed does in fact reflect shocks linked to WTO accession.)

In addition, the data we utilize (as described in more detail in Section 3) is a cross-sectional survey that allows us to analyze high school matriculation as observed in a range of cohorts who reach the age of matriculation before and after 2002, exploiting the discontinuous shift in tariff uncertainty and thus labor demand observed at this point. In the absence of a full-scale panel, we do not focus on analyzing the effects of annual variation in trade policy (e.g., tariff fluctuations), though we will demonstrate that our results are controlling for variables capturing these additional policy fluctuations.

2.2 Conceptual Framework

Improving access to post-compulsory education has been an important educational policy goal across the developing world in the last two decades. Compulsory education in China consists of six years of primary and three years of junior secondary (middle school) education, followed by three years of non-compulsory high school; the transition to post-compulsory education is via an annual entrance exam. Although China has long achieved universal nine-year basic education, the transition rate to high school was modest at 52.9% in 2001, the final year prior to the shock of interest in this analysis.[4] The limited attractiveness of post-compulsory education has been attributed to both the substantial tuition fees[5] and the steady enhancement in employment prospects for less educated migrant workers (de Brauw and Giles, 2017). Given that meeting the increasing demand for skilled labor is crucial in this period of industrial upgrading in the Chinese economy, it is important to understand the role of factors including trade policy shocks in influencing students' education decisions.

The reduction of tariff uncertainty and the associated export expansion directly affect the post-compulsory schooling decisions of Chinese youth through two major direct channels. The expansion of export-oriented production may alter the skill premium in the labor market (we denote this as direct channel one), and the direction of this effect is unclear: if the expanding export sector sufficiently values skilled workers, middle school graduates may be encouraged to pursue high school education given the increased returns to education. However, if export-oriented industries disproportionately employ unskilled labor, the skill premium may decline, disincentivizing secondary school education. Separate from any shift in the longer-term returns to education, the availability of low-skilled positions immediately raises the short-term opportunity costs of staying in school, incentivizing youth to work directly upon middle school graduation (we denote this direct channel two). The sign of the net direct effect on high school enrollment is

[4]Source: China Statistical Yearbook, 2002.

[5]According to the China Education Expenditure Yearbooks, tuition fees for high school are 6 to 11 times that of middle school, depending on the high school type (i.e. academic or vocational).

thus theoretically ambiguous.

In addition to changes in returns and costs of schooling, the reduction in tariff uncertainty may also indirectly affect education through its impact on parental work and local economic conditions. First, the increase in labor demand in the labor market either locally or at plausible migration destinations may raise parental wage income. If high school education is a normal good, we would expect the demand for education to rise (indirect channel one). In addition, a higher level of parental income may render tuition fees more affordable for credit-constrained families, leading to a higher enrollment rate.

Second, an increase in the wage generated by higher labor demand may encourage parents to work for longer hours or induce non-working parents to enter the labor market, reducing the time available for them to invest in their children's educational performance.[6] This shift could also lead to increased demands for youth to care for younger siblings or engage in other household responsibilities; this channel may be particularly salient for girls (Morduch, 2000; Dammert, 2010; Qureshi, 2018). Both effects would reduce high school enrollment (indirect channel two).

Third, the reduction of tariff uncertainty affects the level of local education expenditure. It increases local GDP (Erten and Leight, 2020), generating higher levels of fiscal revenue. This may increase local spending on education, leading to high school expansion and increased high school enrollment rates. Alternatively, given positive shocks to the local export sector, local governments who seek to maximize growth may shift spending away from education to export-oriented projects (e.g., infrastructure construction), potentially adversely high school enrollment. We denote the fiscal channel as indirect channel three.

Our empirical analysis will provide evidence around a number of these channels. We focus on first identifying the net effect of the reduction of tariff uncertainty on high school enrollment. We will also provide additional evidence around heterogeneity with respect

[6] A number of papers have shown a positive relationship between parental time investment and children's educational outcomes (Datcher-Loury, 1988; Del Boca, Flinn and Wiswall, 2013; Bettinger, Hægeland and Rege, 2014; Gayle, Golan and Soytas, 2018), or a negative relationship between parental (especially maternal) employment and children's educational outcomes, including in China (Li et al., 2005).

to shifts in the local skill premium; heterogeneity in parental exposure to shocks in the local labor market; and local fiscal effects.

3 Data and Descriptives

3.1 Individual-level Data

The primary dataset employed in this study is the 2007 Chinese Household Income Project (CHIP). CHIP households constitute a random sample from the annual household income and expenditure surveys conducted by the National Bureau of Statistics in China (Kong, 2010), including 8000 rural and 5000 urban households residing in 179 counties in ten provinces in the eastern, central and western regions of China.[7] The CHIP survey collected detailed information about demographic characteristics, labor market performance and self-reported welfare of individuals and their families.

One unique feature of the CHIP survey is that it collected basic demographic information for all biological and adopted children of heads of sample households and their spouses. The sample of children thus includes not only child household members, but also children who have migrated for education and work purposes as well as those adult children who have departed the natal household to form their own family. This universal coverage ensures that our empirical analysis is not prone to sample selection biases resulting from migration, family splits or new household formation.

By contrast, this data structure is not found in other datasets such as the China population census, in which migrated youth cannot be linked to their birth households, rendering it challenging to analyze the effects of both local and migration shocks on human capital. More specifically, youth who have left their hukou households for more than six months (temporary migrants) and those who have changed their hukou residence (permanent migrants) cannot be linked to their birth households in the census. Though

[7]These provinces are Hebei, Shanghai, Jiangsu, Zhejiang, Guangdong, Anhui, Henan, Hubei, Chongqing and Sichuan.

the census clearly offers a larger sample, we preferentially use the CHIP data given our interest in exploring the role of migration in shaping youth educational choices. More details about data sources are provided in Appendix Section A1.

Our empirical analysis focuses on children born between 1980 and 1991 of the sample households. Cohorts born before 1980 reached the standard age of entry of primary school (six) prior to the passage of China's compulsory schooling law in 1986, and thus may have been less likely to initiate schooling on time, rendering it challenging to estimate the age at which they would make decisions around matriculation to high school. At the same time, cohorts born after 1991 are aged 15 or younger at the point of the survey, and thus may still be in middle school. Any children in the target cohorts that are still in middle school at the point of the survey are dropped from the analysis. We will subsequently demonstrate that our results are robust to alternate birth year cutoffs.

According to descriptive statistics reported in Table 1, the high school enrollment rate of the resulting sample is 54%. The sample of youth is 52% male, and 72.5% have at least one sibling.[8] Although demographic characteristics crucial for the main analysis were available for all children, certain information was not collected for children who were 16 years or older and were no longer household members. These variables are indicated in the last column of Table 1. In analyzing urban and rural dynamics, we utilize a definition of rural that is based on the reported hukou of the youth's household head at the age of 14, corresponding to the window in which matriculation decisions are made.[9] Importantly, this variable abstracts from any shift in hukou of the youth that may be induced by an educational choice (i.e., the pursuit of occupational high school or tertiary education allows rural individuals to convert to an urban hukou following enrollment). 75% of youth in the sample are from rural families.

[8]Although the One Child Policy was strictly enforced for urban households in the sample period, the one-child restriction was relaxed for rural households in 1984, allowing them to have a second child if the first born was a girl.

[9]The hukou system categorizes people as assigned to rural or urban residence at birth, according to their parents' status. Households report their hukou status as well as the date of any changes in hukou, allowing us to identify households' status in any given year.

3.2 Measurement of Trade Shocks

We analyze the effects of the reduction in NTR uncertainty experienced by China in the U.S. market following its accession to the World Trade Organization. The NTR gap is first defined at the subsector level for each of the 39 subsectors of tradable production reported in Chinese census data, and calculated as the linear difference between the higher tariff rate that would have applied in the case of revocation of China's NTR status and the lower NTR rate, $NTR\,Gap_i = Non\,NTR\,Rate_i - NTR\,Rate_i$. The NTR gap is weakly positive for all industries. Throughout the empirical analysis, we use the NTR gaps for 1999.[10] The highest NTR gaps are observed for textiles, garments, other manufacturing, medical and pharmaceutical products, and furniture manufacturing, while the lowest NTR gaps are observed for mining products and agricultural output.

The county-level NTR gap measure is then constructed as the weighted average of subsector gaps, using weights constructed from the baseline composition of tradable employment reported in the 1990 census. The census data allows us to calculate the share of tradable employment by industry in each county c in province p, interacting the NTR gap for subsector i with the subsector's county-specific employment share.

$$NTR\,Gap_{cp}^{Local} = \sum_i empshare_{icp}^{1990} \times NTR\,Gap_i \tag{1}$$

In the sample of interest for this analysis, the average NTR gap is 0.199 with a standard deviation of 0.102. Figure A3 in the Appendix shows a histogram of the NTR gap at the county level in the CHIP sample, comprising 179 counties.

The above variable $NTR\,Gap_c$ will be employed as a measure of the purely local shock to export production. However, we also use a second shock designed to capture shocks to the labor market at plausible migration destinations that is constructed as

[10] The industry-level NTR gap data are drawn from Pierce and Schott (2016), who constructed this data using ad valorem equivalent rates. The NTR gap for industry i is the average NTR gap across the four-digit ISIC Revision 3 tariff lines belonging to that industry. The NTR gaps in 1999 are almost identical to those in 2000 or 2001; accordingly, the results are robust to the use of data from other years. The ISIC industry categories were matched to the employment categories reported in Chinese data, and details of this matching are provided in Erten and Leight (2020).

follows. Data on migration from the 2000 census is used to estimate the share of prime age adults in a given county (individuals aged 16–59) who report migrating to (and thus currently residing in) each possible destination county d ($MigDest_{cp,d}$), relative to the total population of outmigrants from this origin county.[11] We then estimate the weighted average of the NTR gap at all destination counties; there are 2873 counties represented in the 2000 census, and thus each origin county has 2872 possible migration destinations.

$$NTRGap_{cp}^{MigDest} = \sum_{d=1}^{2872} MigDest_{cp,d} \times NTRGap_d \tag{2}$$

Given that migration patterns in rural China are heavily dependent on local networks, a pattern parallel to that observed elsewhere in the developing world (Du, Park and Wang, 2005; Chen, Jin and Yue, 2011; Mu and de Brauw, 2015; Munshi, 2003), past migration destinations reported as of 2000 are an informative proxy for plausible migration destinations for youth who are making matriculation and migration decisions in a multiyear window around the WTO accession period. More specifically, if we examine the persistence of migration destinations at the county level over a decade, we find that more than 80% of counties report at least one major migration destination that is consistent across this period; and more than 50% of counties report at least two major migration destinations consistent across this period.[12] The same census round in 2000 can also be used to roughly quantify the overall salience of migration: the average county in the CHIP data reports 7% of its prime-age population having outmigrated as of 2000, though this probability roughly doubles for the youngest cohorts that are of interest here (14% for individuals aged 16–29).

[11] In the 2000 census, individuals are identified as migrants if they report residing in the county of residence for more than six months while reporting an official household registration or hukou in a different county; or, residing in a county for less than six months while reporting a hukou in a different county, and reporting living outside of the hukou county for more than six months. The county of residence is thus identified as the migration destination, while the county of hukou is identified as the origin county.

[12] Migration destinations are reported in both the 2000 and 2010 census waves. We identify major migration destinations as those counties that are reported as one of the top five destinations for outmigrants (as measured by the number of reported outmigrants) from a given source county in each census round. We can then tabulate how many such destinations as identified in the 2000 census recur as major destinations for the same source county in 2010.

It may be helpful to clarify the overall choice of timing for information linked to the construction of the trade shocks. In general, we preferentially use information drawn from the 1990 census (sectoral weights as well as information about the skill concentration in employment) to minimize endogeneity linked to the rapid advance in global integration observed in the Chinese economy during the 1990s (i.e., prior to WTO accession). Insofar as local economies have already changed rapidly between the 1990 census and the subsequent shock, this may mean that our constructed shock measures are relatively weaker proxies for the realized shock in a particular county post-2002, in which case our results may be biased toward zero; however, we argue that the exclusion restriction (namely, that the initial sectoral and educational composition is uncorrelated with subsequent shifts in outcomes post-2002) is more plausible in using this earlier wave of data.

Importantly, implementing this strategy is not possible in the case of migration weights. Internal migration was relatively low in China prior to 1990, and was not reported in that census wave. The earliest large-scale data source in which we can identify migration sources and destinations is the 2000 census. Accordingly, we use later data for this variable only; potential endogeneity introduced by this choice is discussed further in Section 4.1.[13]

We also construct a second variable, the sectoral gap ($SectorGap_{cp}$), to capture the average gap in sectoral composition of employment comparing across origin counties and their migration destinations. Individuals may be more likely to migrate to counties with a similar mix of sectoral employment (i.e., a low sectoral gap) for at least two possible reasons: they may wish to find work using existing skills, or they may have superior informational networks in counties concentrated in similar industries. This variable is constructed as follows: using the same set of tradable subsectors described above, we

[13]For a related reason, we do not construct separate employment weights for the trade shock using subsamples of migrants only. This could be implemented, in principle, using the 2000 census; but we preferentially use the 1990 census round. In addition, given that the average cell size at the county level in the 1% sample of the population census available for analysis is only around 4000 individuals total (including all individuals, working and non-working), the median county reports only 34 working migrants. Using this small population of migrants to construct migrant-specific sectoral employment weights to estimate a shift-share shock is likely to introduce considerable noise.

calculate the sectoral shares in total tradable employment in the origin county, and the weighted average of sectoral shares across all migration destinations, the latter denoted $\overline{empshare}^{d}_{icp}$.[14] We then estimate the total quadratic gap as follows.

$$SectorGap_{cp} = \sum_{i}(empshare_{icp} - \overline{empshare}^{d}_{icp})^2 \tag{3}$$

The sectoral gap captures how similar the sectoral breakdown of employment is in origin counties compared to destination counties, on average. If, at the extreme, an origin county sent migrants only to counties characterized by an exactly similar sectoral composition of employment, the sectoral gap would be equal to zero.

In additional robustness checks, we also control for other trade shocks experienced during this period, including fluctuations in the effective applied tariff rate in the U.S. market (the NTR rate), the domestic tariff rate, and the quotas imposed by the Multifiber Agreement governing the textile industry. For each of these shocks, we construct a county-by-year level weighted average from the industry-level source data using employment weights from the 1990 census.[15] Data on MFA quotas is drawn from Khandelwal, Schott and Wei (2013), and we utilize the same methodology to construct a measure of the degree to which industries' quotas were binding under the MFA by calculating the import-weighted average fill rate. Using data on the universe of county-level shocks, we again construct a migration-augmented shock for each of these variables.

We link the trade data with the individual-level data by the county of parental residence. Compared with children's current residence, county of parental residence is a better proxy for the location where the child attended middle school given the prevalence of migration.[16]

[14] More specifically, the migration destination sectoral shares are calculated as follows: $\overline{empshare}^{d}_{icp} = \sum_{d=1}^{2872} MigDest_{cp,d} \times empshare_{id}$

[15] Since the industry categories for the export licensing and contract intensity variables are available for SIC categories, these categories are manually matched to the census employment categories. The industry classification for the import tariff data is available in ISIC Revision 3, the same source utilized to construct the NTR gap variable. Again, details regarding the associated matching are provided in Erten and Leight (2020).

[16] We exclude children from households in which the heads themselves are temporary migrants who

4 Empirical Analysis

4.1 Baseline Specification

The primary objective of the empirical analysis is to identify the effect of the reduction in tariff uncertainty driven by WTO accession on the probability of matriculating into high school. The dependent variable is a binary variable for high school matriculation for child i in household h in county c in province p born in year t, $Enroll_{ihcpt}$. The primary independent variable is an interaction of individual-level treatment intensity defined based on the birth year, $Treat_t$, and the own county and migration destination gaps.

Treatment intensity measures the proportion of individuals who make decisions about matriculation into high school in 2002 and subsequent years (i.e., following the WTO shock) for each birth cohort. The variation in the age at the decision to attend high school is substantial in China. As shown in Figure A4 in the Appendix, the majority of students graduate from middle school and make decisions about matriculation into high school between the ages of 14 and 16.[17] Therefore, $Treat_t$ is a continuous measure of treatment intensity defined as follows. Youth born in 1985 and prior years (who reach the age of 16 in 2001 and earlier) are defined as $Treat_t = 0$, or unexposed to the trade shock, as they make decisions about matriculation prior to WTO accession. Youth born in 1988 and subsequent years (who reach the age of 14 in 2002 and subsequent years) are defined as $Treat_t = 1$, or fully exposed to the trade shock. Youth born in 1986 and 1987 are assigned a continuous variable capturing partial treatment exposure, defined to capture the proportion of a particular birth-year cohort who makes decisions about high school enrollment prior to the date of the shock; we follow Pan (2017) in this definition.[18] We will also subsequently demonstrate that the primary results are robust to the use of a simpler binary treatment variable, excluding those cohorts that are partially exposed.

do not report a hukou (56 children or 0.6% of the main sample), because the location in which these children attended middle school cannot be identified.

[17] This figure draws on data from the China Health and Nutrition Survey, as the CHIP survey employed in this analysis does not provide detailed data about the age of decision-making around high school matriculation.

[18] Based on the numbers in Figure A4, $Treat_{1986} = 0.3646$ and $Treat_{1987} = 0.7201$.

As noted above, the NTR gap is time-invariant and captures the level of tariff uncertainty faced ex ante, prior to WTO accession. The primary specification can thus be written as follows.

$$\begin{aligned} Enroll_{ihcpt} &= \beta_1 Treat_t \times NTR\,Gap_{cp} + \beta_2 Treat_t \times NTR\,Gap_{cp}^{MigDest} \\ &+ \kappa_{cp} + \gamma_{pt} + \chi_{ihcpt} + \epsilon_{ihcpt} \end{aligned} \tag{4}$$

The relationship of interest is estimated conditional on county fixed effects κ_{cp}, province-year fixed effects γ_{pt}, and individual-level controls χ_{ihcpt} (gender, birth order, a binary variable for minority status, a binary variable for any siblings, and continuous variables capturing the years of schooling attained by each parent).[19] Standard errors are clustered at the county level, yielding 179 clusters.

The primary results of estimating equation (4) are presented in Panel A in Table 2. Column (1) reports our preferred, primary specification. In Columns (2) and (3), we report two additional specifications including differential trends for manufacturing-intensive counties (in Column 2) and both manufacturing-intensive and agricultural-intensive counties (in Column 3). Manufacturing- and agriculture-intensive counties are identified as counties characterized by an above-median (local) concentration of employment in the secondary and primary sectors, respectively, in the 1990 census.[20] Column (4) includes control variables for additional trade policy shocks as described above; and Column (5) includes more flexible controls for baseline variation in the primary employment share (deciles of primary employment share interacted with cohort fixed effects).

It is clear that the coefficients of interest β_1 and β_2 are consistently negative: youth who reach the age of matriculation into high school post-2002 in counties exposed to larger NTR gaps ex ante either locally or at plausible migration destinations are significantly less likely to enroll. The magnitude of the coefficients are fairly stable and larger for the own-

[19]If parental schooling data is missing, schooling is coded as zero, and an additional binary variable for missing data is included.

[20]The binary variables for manufacturing-intensive and agricultural-intensive counties are not inverses of each other, given that there are also counties heavily concentrated in services (tertiary employment).

county vis-a-vis the migration destination shock, but the hypothesis that the effects are equal cannot be rejected in any specification. The magnitude conservatively suggests that a one standard deviation increase in the own-county NTR gap (an increase of 0.102) is associated with a decline in the probability of enrollment of 4.1 percentage points, relative to a probability of enrollment for pre-shock cohorts of 49.3%. A one standard deviation increase in the migration destination NTR gap (an increase of 0.043) is associated with a decline in the probability of enrollment of (conservatively) 2.1 percentage points, an effect that is about half the size of the own-county effect.[21]

As previously noted, all county-level employment weights used to construct trade shocks are extracted from the 1990 census, with the exception of migration destinations; given the time patterns of migration and data limitations, these are necessarily identified from the 2000 census. Migration patterns in the 1990–2000 decade already reflect a disproportionate flow of migrants to areas experiencing rapid export-driven growth as China initiated liberalization (including tariff reductions) prior to WTO accession; unsurprisingly, the most common migration destinations at the provincial level include, in order, Guangdong, Shanghai, Jiangsu, Zhejiang and Beijing.[22] At the county level, popular migration destinations show a somewhat higher NTR gap vis-a-vis non-popular destinations, consistent with the fact that more industrialized areas generally have higher NTR gaps; accordingly, if those popular destinations also disproportionately benefit from WTO accession, the true flow of migrants to those destinations post-2000 may be even higher than our migration weights would imply. Overall, it is appropriate to be cautious in assessing potential bias induced by endogeneity of migration destinations, but plausible to argue that our coefficient may be biased toward zero.

We do not estimate any causal effects on the probability of outmigration per se by the target youth, for several reasons. First, this variable is only imperfectly observed in the

[21]In both cases, these calculations use the estimated coefficients that are lowest in magnitude, as reported in Column (5) of Panel A in Table 2.

[22]We identify these locations as follows: for each origin county in the CHIP data, we identify the top five destination counties, yielding a sample of 875 (non-unique) destinations. We then calculate the provincial composition within those popular migration destinations.

CHIP data. Second, the primary source of longitudinal variation that we exploit (cohorts that reach the age of matriculation before or after WTO accession) has no clear analogue in the case of a migration decision, given that this decision can in principle be made at any time. Third, our primary causal argument does not entail any claim that WTO accession necessarily affects the probability of outmigration, but rather that conditional on the ex ante probability of outmigration, the WTO-induced shocks at possible destinations may shift educational choices at migration origins. While the existing literature provides fairly clear evidence that areas with higher NTR gaps are characterized by higher in-migration post-accession (Facchini et al., 2019; Erten and Leight, 2020), there is no analogous evidence around the effect of NTR gaps at the origin on the probability of outmigration, and our research design does not allow us to provide any such evidence.

4.2 Alternate specifications and heterogeneity by subsample

Panel B of Table 2 reports some additional robustness checks on the primary results. In Column (1), we use a simpler, binary measure of treatment exposure; cohorts born in 1986 and 1987, identified above as subject to intermediate exposure to the NTR shock, are excluded from the analysis. In Column (2), we estimate the main specification using household fixed effects; the remaining source of identification in this specification is variation across siblings who reach the age of matriculation before and after WTO accession. Both specifications are consistent, though the destination gap index is noisily estimated in Column (2).

In Column (3), we include an additional interaction term between the migration destination shock index and the sectoral gap index (comparing migration origins and destinations) described above. If individuals are more likely to migrate to counties with a similar mix of sectoral employment (in order to find employment that uses existing skills, for example, or because of superior informational networks), one interpretation of the main results would be that migration origins and destinations are experiencing correlated shocks, and we are simply identifying the response to this same shock as experienced at two locations. In that case, the response to the destination shock should be significantly larger when the destination and origin are characterized by more similar sectoral compositions. However, the coefficient on the sectoral gap index interaction is insignificant, suggesting there is no evidence of this pattern.

Finally, in Column (4) we report a placebo test using height as the dependent variable in equation (4). The potential source of bias that we seek to assess here is as follows: if low NTR gap counties also experienced a sharp shift in preferences for human capital investment coinciding with the years in which the treated cohorts were born, those cohorts may have been exposed to higher investments in early life and be characterized (for example) by both greater height and potentially, higher cognitive ability. This could be reflected in a shift in high school enrollment around 2002 in which those low NTR gap counties disproportionately gain in high school enrollment, consistent with the empirical pattern we observe. However, the observed coefficient is small in magnitude and insignificant, suggesting that there is no evidence of any such shift in human capital investment around the birth of the treated cohorts.[23]

Table A1 in the Appendix further reports results for heterogeneity with respect to child and parent-level characteristics. In Panel A, there is little evidence of heterogeneity with respect to gender, sibship size, or birth order, other than a somewhat attenuated response to the migration shock for female youth and youth who are not first born. (Given the split samples, there is limited power in some specifications to estimate the effect of the migration destination shock.)

In Panel B, Columns (1) and (2) report results for urban and rural samples, respectively. It is evident that the response for urban youth is primarily driven by a response to local shocks; the coefficient on the destination shock is roughly half the magnitude of the response to the local shock, and statistically insignificant. By contrast, the coeffi-

[23]Given that the average age in the sample is 22 at the point of the survey, we follow other papers analyzing the effect of early life shocks on adult height in China and use simple linear height (Chen and Zhou, 2007; Meng and Qian, 2011; Gørgens, Meng and Vaithianathan, 2012). Height-for-age is generally considered an appropriate measure only for young children, or for children and adolescents (the World Health Organization charts allow for calculation of height-for-age only for youth up to age 19).

cient estimating the response of rural youth to the local shock is statistically insignificant and around half the magnitude observed for the urban sample, while the response to the migration shock is large and statistically significant. (In both cases, however, the standard errors do not allow us to reject the hypothesis that the effects of interest are equal comparing across the urban and rural samples.) In Columns (3) through (6), we analyze subsamples defined by education levels of both mothers and fathers. There is some evidence that the effect of the shock is attenuated for youth whose mothers possess a high school education (but not for children of more educated fathers), though the coefficients are statistically different only for the own-county shock.

4.3 Robustness Checks

Assessing robustness of the shift-share instrument A growing literature in recent years has probed the robustness of Bartik shift-share instruments such as the NTR gap constructed here. Goldsmith-Pinkham, Sorkin and Swift (2020) presents evidence that the validity of these research designs depends crucially on the exogeneity of the estimated shares relative to potential growth in the dependent variable, and recommends identifying control variables measured in the same year as the employment shares (in this case, 1990) and interacting these time-invariant controls with year fixed effects.[24] We have already reported specifications including the interaction between time-invariant county characteristics (control variables capturing variation in the baseline primary and secondary employment share) and time trends, as included in Columns (2), (3), and (5) of Table 2.

In Panel A of Table A2 in the Appendix, we report a series of alternate specifications that include more restrictive controls for initial conditions. In Column (1), we add inter-

[24] We preferentially use the framework of Goldsmith-Pinkham, Sorkin and Swift (2020) rather than Borusyak, Hull and Jaravel (2018) given that the latter relies on a large number of shocks, or in our case, assuming that the number of NTR gap shocks at the subsector level approaches infinity. Our analysis utilizes data from 39 subsectors, and thus this assumption is arguably not appropriate. In addition, given that we use subsector shares from 1990 considerably prior to the shock of interest, we argue that exogeneity of the employment shares is plausible.

actions between an urban dummy and province-year fixed effects. In Column (2), we add interactions between variables capturing deciles of baseline secondary employment shares and cohort fixed effects. In Column (3), we construct deciles of the baseline percentage of county population reporting secondary education and interact these binary variables with birth year fixed effects. In Column (4), we construct deciles for the initial employment share in the five industries characterized by the highest NTR gaps, and interact these deciles with birth year fixed effects. The estimated coefficients remain uniformly negative and significant, and of roughly consistent magnitude.

Panel B of Table A2 then replicates the primary results (reported in Panel A of Table 2) constructing the NTR gap using employment data as reported in the 2000 census. The use of 2000 employment weights may increase precision, by using employment data more proximate to the shock; however, it introduces bias associated with strategic industrialization by counties seeking to expand manufacturing in anticipation of shocks induced by WTO accession. There is ultimately no meaningful difference comparing across the estimates constructed with 1990 and 2000 data weights. We also explore the robustness of the primary results to broadening the birth year cutoff used to identify the sample. The primary sample includes birth cohorts born between 1980 and 1991, inclusive; we expand this window first by two years (1979—1992) and then by four years (1978—1993).[25] The results remain consistent, as reported in Table A3 in the Appendix.

We can also identify whether pre-trends in high school enrollment are correlated with other covariates reported in 1990 that are themselves correlated with employment shares: particularly, total county population, the primary / secondary school enrollment rate (the ratio of the number of reported students to the total reported youth population ages five to 15), the unemployment rate (the ratio of the number of individuals reported actively searching for work to the total labor force), and the fertility rate (average children per woman for women age 15–64). Each of these variables is highly correlated with employ-

[25]This entails the inclusion of older cohorts who were not subject as children to compulsory schooling laws, and thus who may have followed a different timeline for their schooling trajectory, as well as the inclusion of younger cohorts; again, however, any children still in middle school at the point of the survey are excluded from the analysis.

ment shares: on average, a higher share in non-agricultural employment is observed for larger counties, characterized by lower fertility and higher school enrollment.

Following the recommended methodology in Goldsmith-Pinkham, Sorkin and Swift (2020), we use data from earlier cohorts observed in the CHIP survey to calculate the average matriculation rate for cohorts born in 1980 and 1985 and construct a short-difference for matriculation rates for these pre-WTO cohorts (the 1985 cohort would reach the age of matriculation by 2001, at latest). The results reported in Table A4 in the Appendix suggest that none of the cited variables are significantly correlated with this county-level difference conditional on provincial fixed effects, consistent with the hypothesis that these omitted variables are not a meaningful source of bias.

Assessing robustness to differential trends We can also demonstrate that our results are robust to controlling for existing pre-trends, building on recent methods developed by Roth and Rambachan (2020). First, we re-estimate the relationship of interest in a county-year panel framework, rather than the individual-level regression that constitutes our primary specification. While we observe each individual only once and thus cannot construct an individual-level panel, we link each individual (and his/her reported matriculation status and other covariates) to a county-birth year cell and collapse to a county-year panel. We then estimate the following specification at the county level, employing for concision a single NTR gap that is the weighted average of the home-county (destination) shock, weighted by the share of non-migrants (migrants).[26] Each county-year cell is weighted relative to its number of observations, and the NTR gap is interacted with the same continuous measure of treatment used in equation (4). χ_{cpt} are the same

[26]More specifically, this measure is constructed as follows, where $NonMig_{cp}$ is the share of non-migrants in this population, and $AllMig_{cp}$ is the population share of outmigrants (to all possible destinations).

$$NTR\,Gap_{cp}^{Full} = NonMig_{cp} \times NTR\,Gap_{cp} + AllMig_{cp} \times NTR\,Gap_{cp}^{MigDest} \tag{5}$$

individual-level covariates used in the main analysis and collapsed to a county-year mean.

$$M_{cpt} = \beta Treat_t \times NTR\,Gap_{cp}^{Full} + \mu_t + \phi_{cp} + \chi_{cpt} + \epsilon_{icp} \tag{6}$$

We can verify that the primary coefficient β remains comparable to that estimated in the individual-level specification: $\beta = -0.779$, $p < 0.001$. We also generate a county-level event study plot, Figure 2 in the Appendix, to visually inspect pre-trends at the county level.

Second, we use the methodology estimated in Roth and Rambachan (2020) to evaluate the sensitivity of this estimate, reporting fixed-length confidence intervals that are robust to a violation of the parallel trends process that is either linear (notated as $M = 0$) or characterized by non-linearities parameterized by positive M (defined as a differential trend whose slope changes by no more than M in consecutive periods). We focus on the dynamic treatment effect estimated for the cohort born in 1991, the final treatment year observed in this analysis, as compared to the cohort born in 1985, corresponding to the final purely untreated pre-WTO cohort (matriculating into high school at latest in 2001). We then report the FLCIs in a sensitivity plot, Figure 3 in the Appendix; Figure 3a shows the 95% confidence intervals, and Figure 3b shows the 90% confidence intervals. It is evident that the confidence intervals are consistently negative and statistically significant, and generally robust to even a non-linear differential trend.

4.4 Mechanisms

Our primary results suggest that the rise in the short-term opportunity costs of education for youth given the increase in local and migration destination non-agricultural labor demand dominates any shift in the returns to education or any potential positive income effect induced by the same shock. This generates a decline in the probability of high school matriculation.

To further probe the hypothesized mechanism, we can also test the other channels identified in the conceptual framework. First, we analyze heterogeneity with respect to the implied skill premium in the relevant labor market — whether local or at plausible migration destinations — affected by the export shock. As noted in the conceptual framework (direct channel one), the effect of a reduction in tariff uncertainty on the export premium is theoretically ambiguous, on average. However, in areas where export-oriented industries are more likely to demand skilled labor, this would imply an increase in the skill premium as export-oriented production expands, thus dampening any negative educational effect induced by an increase in the short-term opportunity costs of education. In areas where export-oriented industries are more likely to demand unskilled labor, a decline in the skill premium may amplify the impacts of an increase in short-term opportunity cost.

In order to analyze this channel, we calculate the industry-level share of employees reporting high school or higher education in the 1990 census, and calculate the weighted average of high school skill intensity for the manufacturing sector in each county. We use this to construct a measure of average skill intensity for the local labor market and the average destination labor market in each county, and denote each county as characterized by a high-skilled versus low-skilled labor market at home, and a high-skilled versus low-skilled labor market at migration destinations. We then re-estimate the primary specification, equation (4) for youth in counties facing a high-skill vis-a-vis a low-skill shock in the home county, as well as in destination counties.[27]

In Table 3, Columns (1) and (2) report the results for high-skill home counties versus low-skill home counties: the response to the own-county shock is significantly lower in high-skill counties, where a one standard deviation increase in the NTR gap is associated with an only eight percentage point decline in the probability of matriculation, vis-a-vis 17 percentage points in low-skill counties. (There is no differential response to the migration

[27]As previously noted in the discussion of shock construction, using measures of sectoral composition and skill derived from the 1990 census may render our shocks a weak proxy for the true local skill or sectoral composition as extant around the point of WTO accession; however, we preferentially use this earlier wave of census data in order to minimize endogeneity induced by globalization-related shocks during the period of the 1990s. In addition, any bias induced by using the earlier wave should bias our effects toward zero.

destination shock comparing across these two subsamples, though the migration shock coefficient is noisily estimated in the first column.) Columns (3) and (4) report the results for high-skill destination counties versus low-skill destination counties, and we observe the same pattern: the response to the destination county shock is close to zero for youth linked to destination counties with high skill levels, while the response to the own-county shock is roughly consistent. (However, the wider confidence intervals for the estimated coefficients on the migration shock render it impossible to reject the hypothesis that the effects are equal comparing across these two subsamples.) These findings suggest that while the higher short-term opportunity costs of education seem to dominate educational choices, the first hypothesized direct channel (in which variation in the skill premium simultaneously affects educational choices) is also operational.

Second, we jointly evaluate the first and second indirect channels described: an expansion in labor demand generates a positive income effect for parents, or induces previously non-working parents (or minimally working parents) to enter the local labor market, requiring older children to invest more in the care of younger siblings and thus reducing their propensity to enter high school. The main effect detected is prima facie inconsistent with a positive income effect, and the second hypothesized indirect channel is inconsistent with several points of evidence: the heterogeneous effects presented in Table A1 suggest that the effect of the shock is not significantly different for first-born versus higher parity children or boys versus girls, while in general first born and female children would be expected to be more burdened by childcare responsibilities. Data directly reported by the sample households also suggests that care-taking responsibilities by siblings are minimal.[28] To further explore this hypothesis, we split the sample to compare the effects of the shock for households where the household head reports some change in employment post-2002, versus households where no change in employment is reported; the results reported in Columns (5) and (6) of Table 3 show that there is no statistically significant

[28] Only one pre-school child was identified as having an older sibling as a caregiver, and even this report may be unreliable given that the household otherwise reported the presence of only a single child.

evidence of heterogeneity comparing across these samples.[29] This suggests that variation in domestic responsibilities linked to shifting parental employment patterns are unlikely to be salient.

Finally, we evaluate the third hypothesized indirect channel to assess whether a reduction in educational supply could explain these patterns (i.e., if counties growing rapidly due to export-driven shocks redirect public investment away from education). Here, we use county-level data drawn from the Fiscal Statistical Compendium for All Prefectures and Counties (Quanguo Dishixian Caizheng Tongji Ziliao). Data is available from 1998 to 2007 and includes reported county fiscal revenue, reported total expenditure, and reported expenditure on education; we restrict the sample to the same counties observed in the CHIP survey, and regress these dependent variables in logs on the county-level NTR gap, a dummy for post-2002, and the interaction between the two.

The results reported in Panel B of Table 3 show coefficients that are positive and significant, suggesting that as expected, counties that are growing more rapidly due to positive export shocks expand their level of public investment, including in education. Figure A5 in the Appendix provides additional evidence about trends over time, regressing educational expenditure on the interaction of a series of year dummy variables interacted with the NTR gap. It is evident trends are relatively similar pre-2002, and educational expenditure increases steadily following the shock. Accordingly, we can reject the hypothesis that counties characterized by larger NTR gaps are directing resources away from education in the post-WTO period.

5 Conclusion

This paper presents new evidence about the effect of positive export shocks both locally and at plausible migration destinations on human capital attainment in China. Com-

[29] We cannot rule out that even parents who reported no change in employment experienced a change in compensation, or in hours. However, a marginal shift in parental hours at the same position of employment is unlikely to generate a substantial shock to time allocation of children.

paring youth who reached the age of high school matriculation before and after China's accession to the WTO in counties more or less exposed to reduced tariff uncertainty, we find evidence that youth reaching matriculation age in counties characterized by positive export shocks show a lower probability of enrolling in high school.

Our paper is consistent with several prior papers that have analyzed the effect of trade access shocks (tariff cuts or WTO accession), and have found a decline in enrollment rates in response to the shocks (Liu, 2018; Li et al., 2019; Lin and Long, 2020).[30] However, our analysis generally finds more meaningful effects on educational attainment compared to the previous literature. This difference reflects two key differences in our empirical strategy that we argue render our estimates more credible. First, our analysis includes all youth born into a household, even those who are permanent outmigrants; since youth who exit education and enter the labor market are disproportionately likely to migrate, excluding these youth will systematically underestimate the effect of trade access shocks. Second, we analyze the response to both local export-driven shocks and shocks at migration destinations. Our evidence suggests that examining the response to local shocks only may significantly underestimate the effect of export-driven growth on human capital accumulation, particularly in rural areas.[31]

This paper also joins a very limited literature analyzing the effect of demand shocks at migration destinations for the developing world more broadly, and suggests youth respond to shocks both locally and at these destinations. Understanding the relationship between shocks to non-agricultural growth, migration and education is an important dimension for future exploration.

[30]One additional paper, Li (2018) separately analyzed the effect of shocks in high-skill and low-skill sectors, but did not analyze the aggregate effect.

[31]Li (2018) uses census data from 2005 and finds that shocks to high-skill industries raise education, while our findings suggest these shocks still reduce education, but by a smaller margin vis-a-vis shocks to low-skill industries; this difference presumably reflects the analysis's exclusion of youth who have permanently departed their birth household. Liu (2018) uses the same census data as Li (2018) but finds the effect of shocks in high-skill industries is positive and insignificant; again, this is plausibly an underestimate of the effect of the trade shock on educational attainment. Lin and Long (2020) find that the effect for urban youth is insignificant, unlike our findings, but uses a very different region-level shock, and also excludes youth who have exited their birth households.

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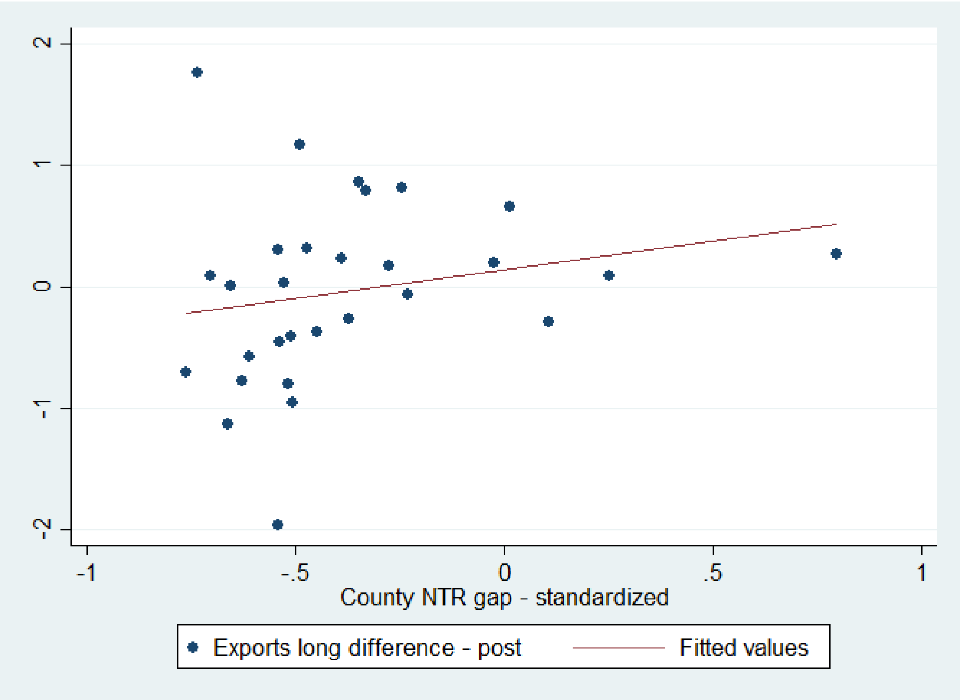
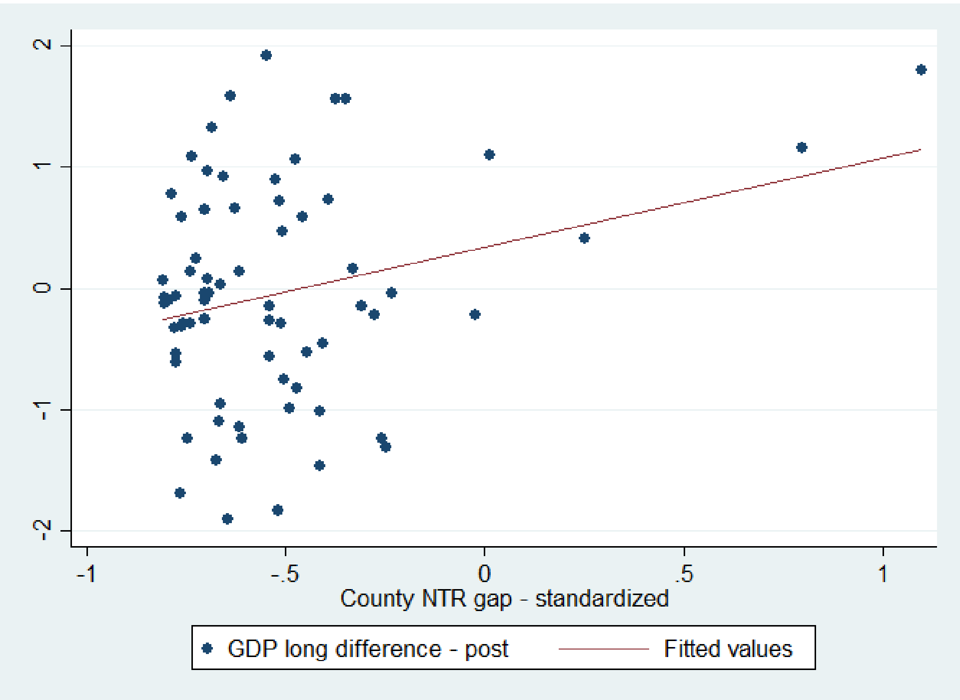
Figure 1: Long-difference post WTO: Local exports and GDP

(a) Exports

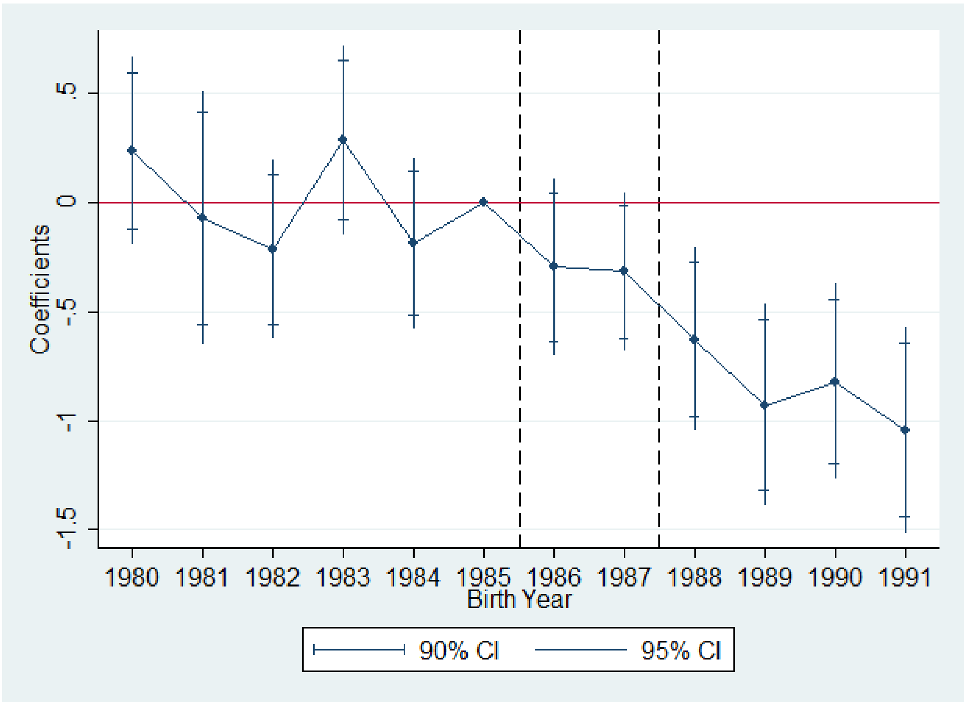


(b) GDP



Notes: Each graph shows a scatter plot capturing the correlation between the county-level long-difference in log exports and log GDP during the first decade following WTO accession (2001—2011) and the county-level NTR gap. All variables are standardized to have mean zero and standard deviation one.

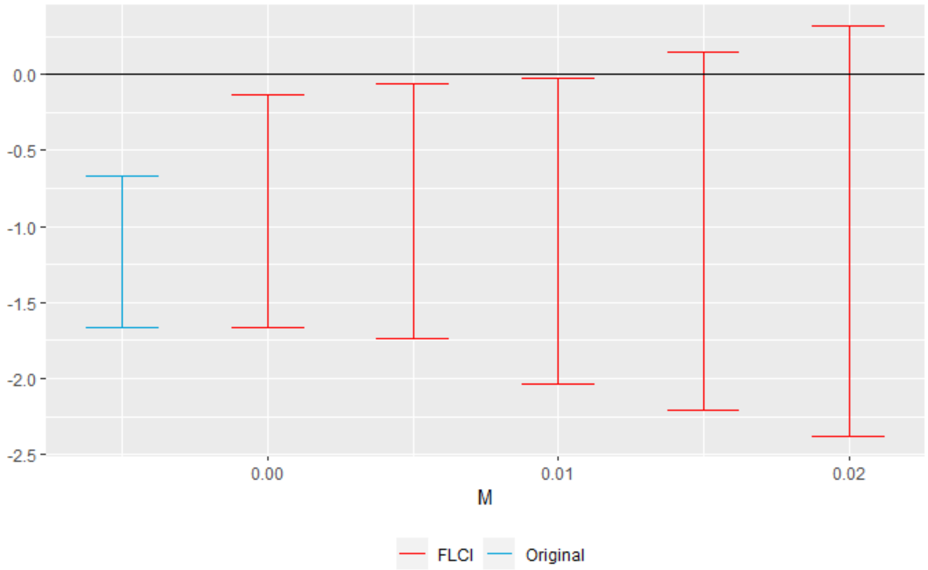
Figure 2: Effects of trade shock on high school enrollment over time: County-level panel



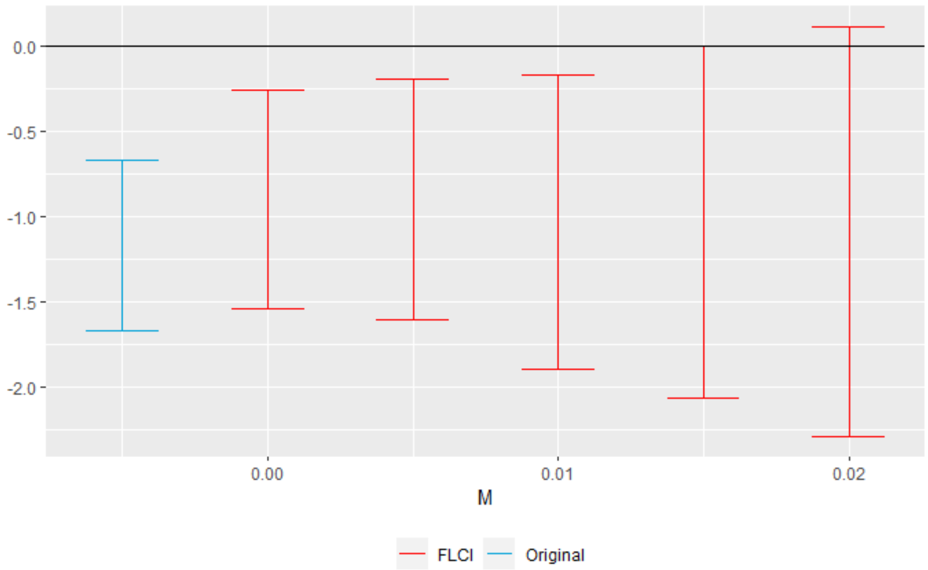
Notes: The figure shows estimated coefficients and confidence intervals for the effect of the full migration-augmented NTR gap on high school matriculation for each birth cohort using a county-year level panel analysis. Cohorts to the rights of the second dashed line are fully treated. Cohorts between the dashed lined are partially treated. The 1985 birth cohort is the omitted group.

Figure 3: Difference-in-difference sensitivity plots

(a) Sensitivity plot: 95% FLCIs



(b) Sensitivity plot: 90% FLCIs



Notes: The graphs show fixed length confidence intervals corresponding to the estimate of the effect of the full migration-augmented NTR gap on the county-level matriculation rate for cohorts born in 1991 relative to 1985, using a county-year level panel. The FLCIs are estimated following the honest dif-in-dif methodology described in Roth and Rambachan (2020).

Table 1: Summary statistics

Variable	Sample Mean	Standard Dev.	N	Full Sample
Panel A: Human capital measures				
High school enrollment rate	0.540	(0.498)	9,019	Yes
Height (cm)	166	(7.56)	7,465	No
Panel B: Individual and household characteristics				
Gender (male=1)	0.524	(0.499)	9,473	Yes
Ethnic minority	0.012	(0.111)	9,437	Yes
Having siblings	0.725	(0.446)	9,473	Yes
Birth order	0.161	(0.872)	9,473	Yes
Father years of schooling	8.11	(2.88)	9,150	Yes
Mother years of schooling	6.51	(3.51)	9,196	Yes
Household head with rural hukou	0.750	(0.434)	9,463	Yes
Panel C: Labor market indicators				
No-agricultural employment	0.744	(0.436)	6,957	No
High-skilled occupations	0.171	(0.376)	6,957	No
Low-skilled occupations	0.539	(0.499)	6,957	No
Log of monthly wage	7.14	(0.606)	3,796	No
Work hours per week	52.4	(13.2)	3,802	No

Notes: This table presents summary statistics for the CHIP sample. The final column indicates whether the variable is reported for the full sample of youth analyzed.

Table 2: Primary results

Panel A: Main specifications

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Treatment X NTR Gap	-0.803***	-0.761***	-0.662***	-0.761***	-0.400*
	(0.111)	(0.136)	(0.165)	(0.139)	(0.218)
Treatment X Destination Gap	-0.556**	-0.555**	-0.541**	-0.523**	-0.495**
	(0.217)	(0.218)	(0.216)	(0.225)	(0.196)
Observations	8,850	8,850	8,850	8,850	8,850

Panel B: Robustness checks

	(1)	(2)	(3)	(4)
	High school enrollment			Height
Post-2002 X NTR Gap	-0.867***			
	(0.114)			
Post-2002 X Destination Gap	-0.552**			
	(0.230)			
Treatment X NTR Gap		-1.209***	-0.647***	-0.553
		(0.345)	(0.135)	(2.494)
Treatment X Destination Gap		-0.517	-0.776**	2.779
		(0.355)	(0.298)	(3.748)
Treatment X Destination Gap X Sectoral Gap			0.106	
			(1.124)	
Observations	7,126	8,850	8,850	7,311

Notes: This table presents the results from regressing a binary variable for high school enrollment on a continuous measure of treatment capturing the relative exposure of different cohorts to post-WTO shocks at the point of matriculation interacted with the county-level local and migration-destination NTR gap. In Panel A, Column (1) includes county and province-year fixed effects and individual controls; Column (2) includes differential trends for manufacturing-intensive counties; Column (3) includes differential trends for both manufacturing-intensive and agricultural-intensive counties; Column (4) includes control variables for additional trade policy shocks; and Column (5) includes the interaction of deciles of baseline primary employment share and cohort fixed effects. In Panel B, Column (1) uses a simpler, binary measure of treatment exposure excluding 1986 and 1987 cohorts; Column (2) adds household fixed effects; Column (3) includes an additional interaction term between the destination NTR gap variable and the origin-destination sector gap index; and Column (4) reports a placebo test using height as the dependent variable. Standard errors are clustered at the county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Mechanisms

Panel A: Household-level evidence

	(1)	(2)	(3)	(4)	(5)	(6)
			High school enrollment			
Treatment X NTR Gap	-1.317***	-0.723***	-0.941***	-0.587**	-0.894***	-0.408
	(0.233)	(0.134)	(0.121)	(0.269)	(0.117)	(0.352)
Treatment X Destination Gap	-0.323	-0.546*	-0.623***	-0.0876	-0.603***	-0.913
	(0.343)	(0.316)	(0.229)	(0.543)	(0.225)	(0.814)
Sample	Low-skill (own)	High-skill (own)	Low-skill (dest.)	High-skill (dest.)	No emp. change	Emp. change
Cross-spec. tests (local shock)		0.023		0.209		0.114
Cross-spec. tests (dest. shock)		0.623		0.340		0.629
Observations	5,417	3,433	7,185	1,665	7,828	1,022

Panel B: County fiscal evidence

	(1) Log total fiscal revenue	(2) Log total fiscal expenditure	(3) Log education expenditure
Post-2002 X NTR Gap	2.518***	0.480**	0.596***
	(0.361)	(0.187)	(0.228)
Observations	1,695	1,695	1,695

Notes: Panel A presents results from regressing a binary variable for high school enrollment on a continuous measure of treatment capturing the relative exposure of different cohorts to post-WTO shocks at the point of matriculation interacted with the county-level local and migration-destination NTR gap. The sample is restricted as specified in each column, and the cross-specification tests report tests of equality of the coefficients estimated across complementary subsamples. All columns include county and province-year fixed effects and individual controls. Panel B presents results from regressing variables capturing county fiscal outcomes on a binary measure equal to one for the post-2002 period interacted with the county-level NTR gap. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Appendix

A1 Data sources

As previously noted, one of the unique features of the CHIP 2007 survey is that it tracks all adult children of the household, including those who are and are not still household members, and provides information about their educational and employment status. In this section, we briefly outline some other micro-level surveys that could be used to analyze the question of interest in this paper and discuss why we believe the survey employed is the most appropriate.

First, there are multiple waves of the population census. The census is constructed at the household level and does include some questions about migration of the primary adults as well as educational attainment. However, it does not allow for tracking of adolescents or adults who have permanently outmigrated from their birth county.

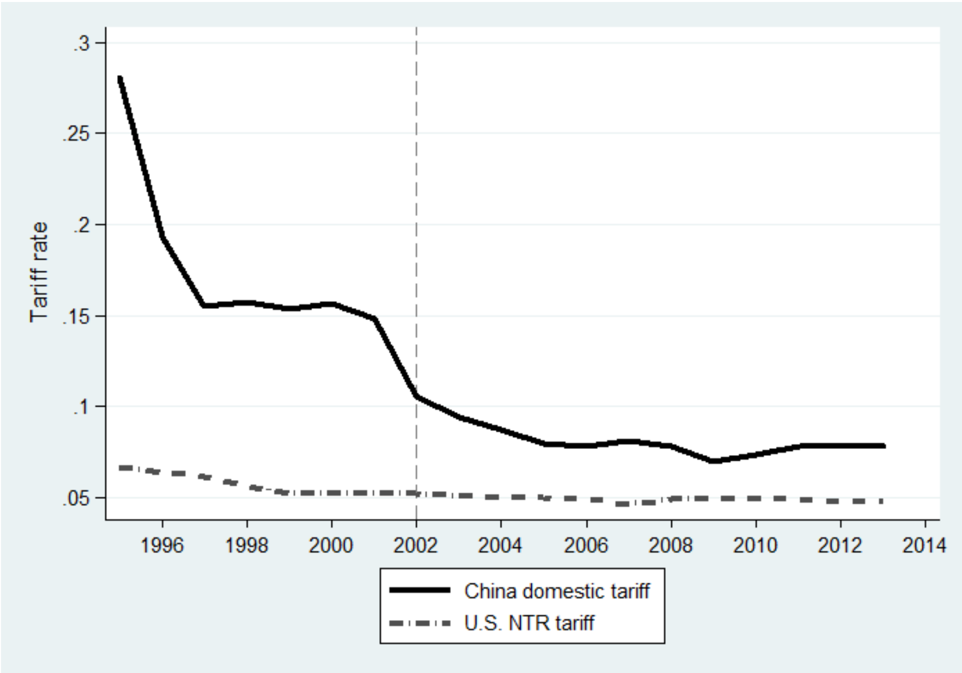
Second, there is a later wave of the CHIP survey (2013), a survey that includes a broader geographic coverage (15 provinces) and does include hukou conversion history in order to categorize each individual surveyed as characterized by a urban or rural hukou at age 14. However, in this dataset it is very challenging to construct a sample of households including the full set of children. If household heads are treated as parents and we seek to analyze the education of their children, children who are no longer household members are excluded. This is a serious challenge given that children born between 1980 and 1991 — the target cohorts in this analysis — were 23 to 34 years old in 2014, when the CHIP 2013 survey was conducted. Given the average rural marriage age of 25, a substantial fraction of them would have formed new households by the time of the survey, and thus would not be included in the survey.

A related option using the same survey is to treat survey respondents (90% are either household heads or their spouses) as "children" and focus on the education of themselves and their siblings. However, although education for all respondents' siblings are reported, hukou location is only reported at the time of the survey. Therefore, is it not possible to identify the county of origin or county of parental hukou for migrants, rendering it impossible to link the sample to the NTR gap in their counties of origin.

A third candidate dataset is the China Family Panel Survey, with multiple waves conducted since its launch in 2011. This survey generally includes all children of rural households, including migrants who retain economic ties with the survey family; however, it does not include youth who have formed new families and no longer retain economic ties. (The latter youth still report their birth year and educational level.) The survey also reports hukou status and hukou location at age 14. However, the survey does not permit merging with external administrative datasets, and thus it is not feasible to link this data to estimates of the county-level NTR gap. An analysis at the prefecture level is possible, but we preferentially utilize county-level data given that this analysis may have more statistical power for estimating the effect of very local shocks.

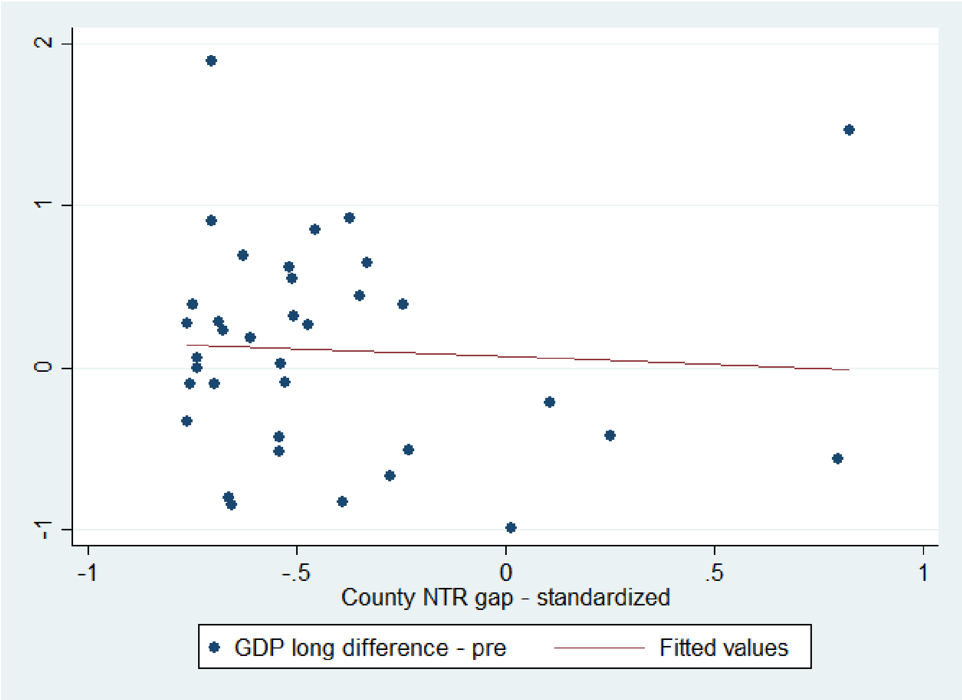
A2 Figures and Tables

Figure A1: Variation in Tariff Policy Over Time



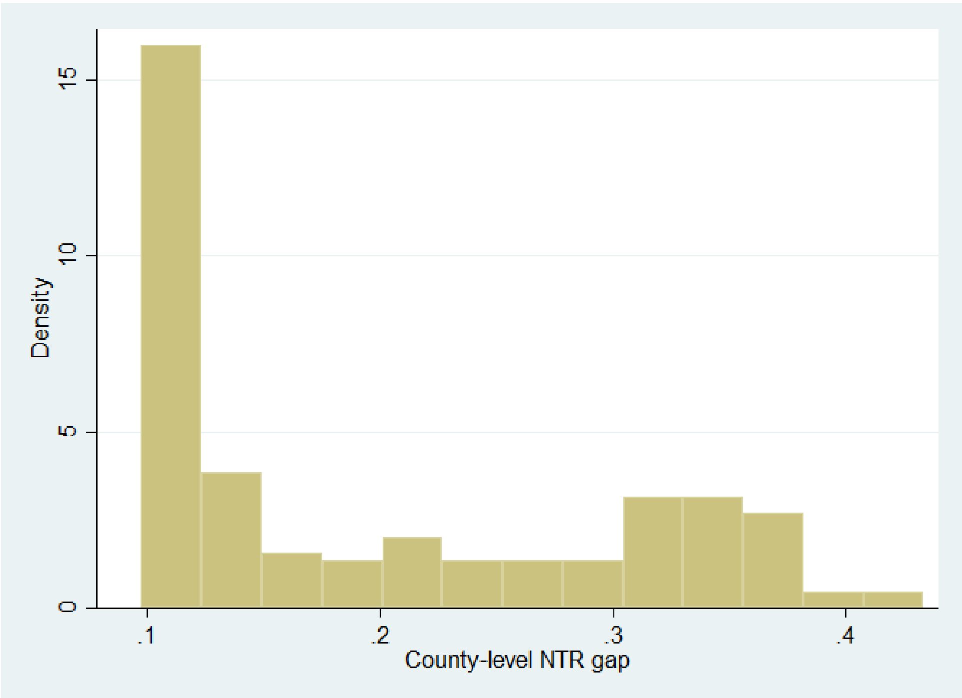
Notes: The figure shows the average domestic import tariff and the mean tariff rate (NTR or Normal Trade Relations rate) imposed on Chinese exports in the U.S market. The mean domestic import tariff is calculated as the weighted average of industry-level tariffs, utilizing as weights the share of total Chinese imports constituted by each industry's imports in 1996. The mean NTR tariff is calculated the weighted average of industry-level tariffs, utilizing as weights the share of total Chinese exports constituted by each industry's exports in 1996. Tariff data is obtained from the WITS-TRAINS database.

Figure A2: Long-difference pre WTO: Local GDP



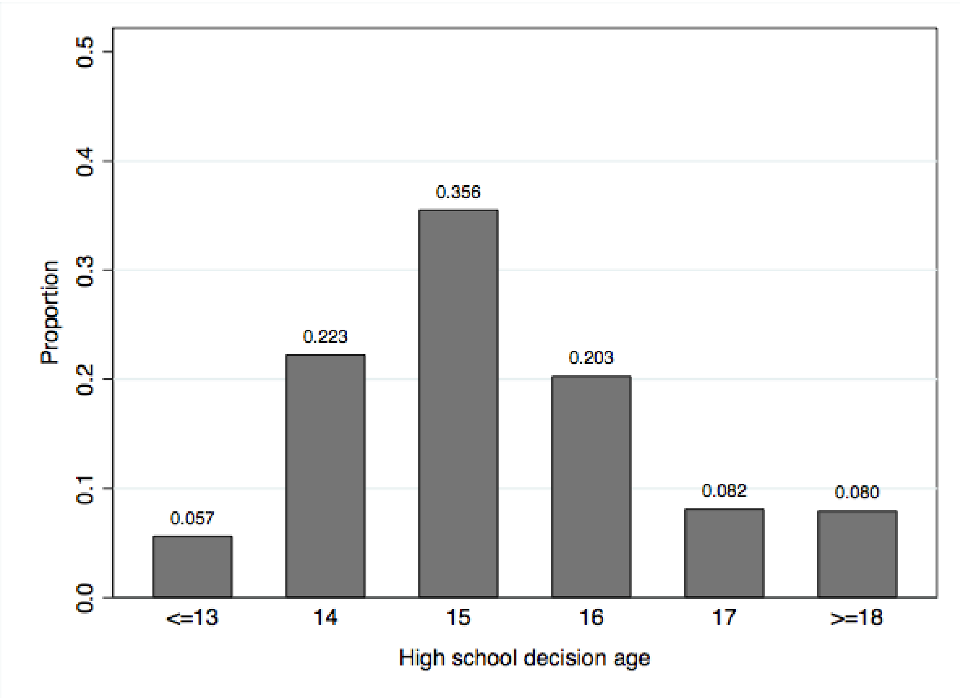
Notes: The graph shows a scatter plot capturing the correlation between the county-level long-difference in log exports and log GDP during the pre-period before WTO accession (1996–2001) and the county-level NTR gap. All variables are standardized to have mean zero and standard deviation one.

Figure A3: County-level NTR gap



Notes: The graph shows a histogram of the county-level NTR gap for the 179 counties in the CHIP sample.

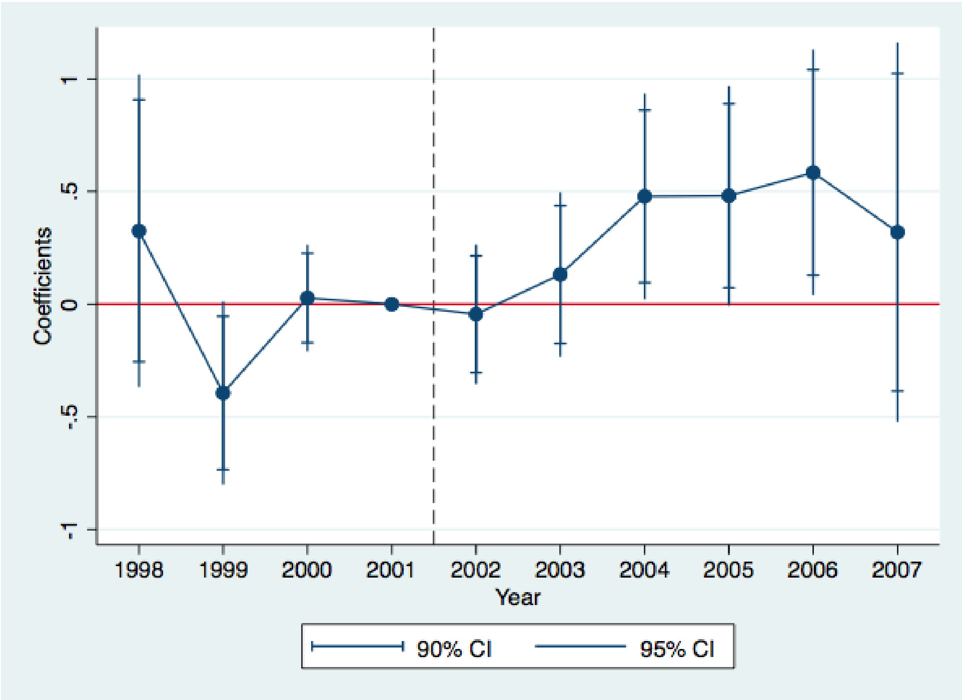
Figure A4: Distribution of high school decision age



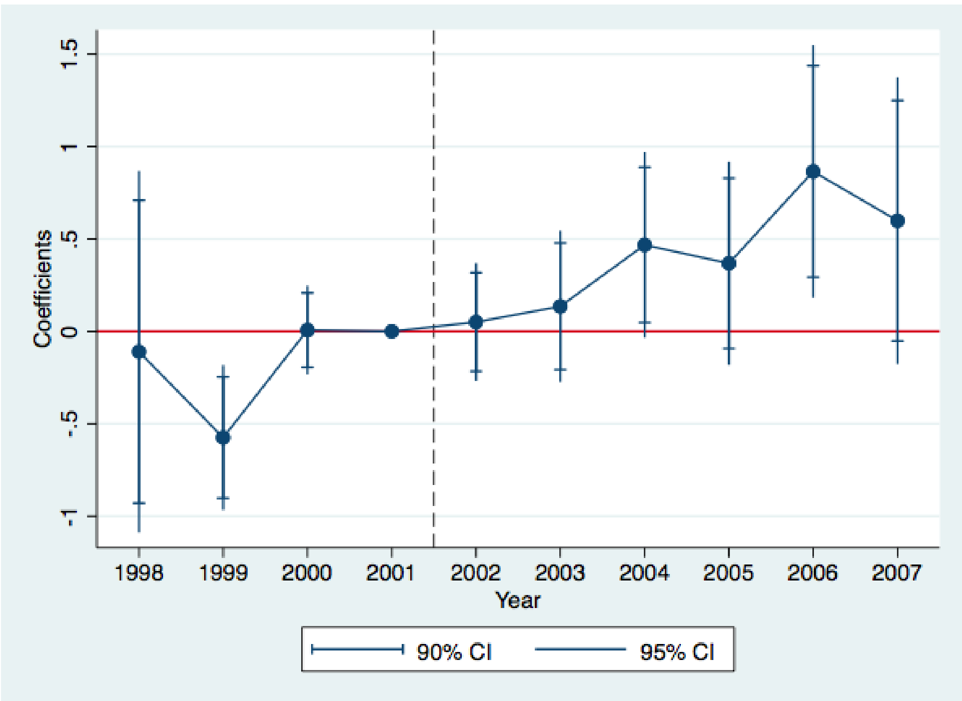
Notes: This figure captures the percentage of the sample who reports decisions about matriculating into high school at each specified age, using data from the China Health and Nutrition Survey.

Figure A5: Effects of trade shock on county education spending over time

(a) Base specification with county and birth year FE



(b) Specification including county, birth year and province-year FE



Notes: The graph shows the estimated coefficients and standard errors obtained from regressing reported county educational spending in each year on the local NTR gap. The data source is Fiscal Statistical Compendium for All Prefectures and Counties, 1998 to 2007. Year 2001 is the omitted group.

Table A1: Heterogeneous effects: individual and parental characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			High school enrollment			
Panel A: Individual characteristics						
Treatment X NTR Gap	-0.850***	-0.811***	-0.480**	-0.968***	-1.000*	-0.997*
	(0.158)	(0.171)	(0.195)	(0.328)	(0.535)	(0.530)
Treatment X Destination Gap	-0.378	-0.749**	-0.841**	-0.484*	-1.028**	-0.162
	(0.300)	(0.361)	(0.403)	(0.278)	(0.433)	(0.416)
Sample	Female	Male	No sibling	Any sibling	Firstborn	Non-firstborn
Cross-spec. tests (own-county shock)		0.855		0.184		0.996
Cross-spec. tests (dest. shock)		0.420		0.421		0.110
Observations	4,186	4,665	2,396	6,455	2,755	3,700
Panel B: Parental characteristics						
Treatment X NTR Gap	-0.636***	-0.345	-0.724***	-0.672***	-0.873***	-0.147
	(0.223)	(0.475)	(0.171)	(0.159)	(0.191)	(0.269)
Treatment X Destination Gap	-0.322	-0.664***	-0.477*	-0.799*	-0.728***	-0.326
	(0.407)	(0.240)	(0.260)	(0.424)	(0.253)	(0.441)
Sample	Urban	Rural	Fathers: no high school	Fathers: high school	Mothers: no high school	Mothers: high school
Cross-spec. tests (own-county shock)		0.568		0.800		0.022
Cross-spec. tests (dest. shock)		0.344		0.498		0.388
Observations	2,215	6,626	5,984	2,591	6,974	1,646

Notes: This table presents the results from regressing a binary variable for high school enrollment on a continuous measure of treatment capturing the relative exposure of different cohorts to post-WTO shocks at the point of matriculation interacted with the county-level local and migration-destination NTR gap. The sample is restricted as specified in each column, and the cross-specification tests report p-values corresponding to the tests of equality of the estimated coefficients across complementary subsamples (e.g., female versus male). All columns include county and province-year fixed effects and individual controls. Standard errors are clustered at the county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A2: Additional robustness checks

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Panel A: Alternate specifications					
Treatment X NTR Gap	-0.657***	-0.501***	-0.837***	-0.605***	
	(0.181)	(0.177)	(0.182)	(0.196)	
Treatment X Destination Gap	-0.483**	-0.422**	-0.409*	-0.393*	
	(0.231)	(0.205)	(0.221)	(0.206)	
Observations	8,850	8,850	8,850	8,850	
Panel B: Employment weights using 2000 census data					
Treatment X NTR Gap	-0.863***	-0.800***	-0.701***	-0.777***	-0.639***
	(0.106)	(0.124)	(0.157)	(0.134)	(0.193)
Treatment X Destination Gap	-0.522*	-0.509*	-0.531**	-0.497*	-0.457
	(0.269)	(0.267)	(0.265)	(0.289)	(0.294)
Observations	8,796	8,796	8,796	8,796	8,796

Notes: This table presents the results from regressing a binary variable for high school enrollment on a continuous measure of treatment capturing the relative exposure of different cohorts to post-WTO shocks at the point of matriculation interacted with the county-level migration-augmented NTR gap. In Panel A, Column (1) includes interactions between an urban dummy and province-year fixed effects; Column (2) includes interactions between deciles of baseline secondary employment share and birth year fixed effects; Column (3) includes interactions between deciles of baseline percentage of county population reporting secondary education and birth year fixed effects; Column (4) include interactions between deciles for initial employment share in the five industries characterized by the highest NTR gaps and birth year fixed effects. In Panel B, the NTR gaps are calculated using employment weights constructed from 2000 census data; all columns include county fixed effects, province-year fixed effects, and individual-level controls, and additional controls are as specified in Table 2. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Primary results using alternate cohort samples

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Panel A: Birth cohorts born between 1979 and 1992					
Treatment X NTR Gap	-0.881***	-0.807***	-0.715***	-0.784***	-0.692***
	(0.119)	(0.142)	(0.163)	(0.152)	(0.203)
Treatment X Destination Gap	-0.556**	-0.552**	-0.540**	-0.575***	-0.538**
	(0.215)	(0.215)	(0.212)	(0.219)	(0.227)
Observations	9,641	9,641	9,641	9,443	9,443
Panel B: Birth cohorts born between 1978 and 1993					
Treatment X NTR Gap	-0.871***	-0.791***	-0.725***	-0.789***	-0.691***
	(0.123)	(0.144)	(0.163)	(0.152)	(0.203)
Treatment X Destination Gap	-0.536**	-0.534**	-0.526**	-0.581**	-0.544**
	(0.225)	(0.223)	(0.222)	(0.231)	(0.238)
Observations	10,317	10,317	10,317	9,973	9,973

Notes: This table presents results from regressing a binary variable for high school enrollment on a continuous measure of treatment capturing the relative exposure of different cohorts to post-WTO shocks at the point of matriculation interacted with the county-level local and migration-destination NTR gap, using the specified cohort window. All columns include county and province-year fixed effects and individual controls, and additional controls are as specified in Table 2. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Analyzing pre-trends for county-level covariates in 1990 census

	(1)	(2)	(3)	(4)
		Enrollment rate short difference		
Total pop.	-9.76e-07			
	.00002			
Enrollment rate		-.260		
		1.146		
Fertility rate			.050	
			.061	
Unemp. rate				-2.243
				2.774
Obs.	175	175	160	175

Notes: This table presents the results from regressing a short difference at the county level constructed by calculating the difference in average matriculation rates for cohorts born in 1985 vis-a-vis cohorts born in 1980 on select county covariates as reported in the 1990 census. The covariates include total county population; the primary / secondary school enrollment rate (the ratio of the number of reported students to the total reported youth population ages five to fifteen); the unemployment rate (the ratio of the number of individuals reported actively searching for work to the total labor force); and the fertility rate (average children per woman for women age 15–64). All regressions are estimated conditional on province fixed effects.