Educational responses to migration-augmented export 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 variations 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. In urban areas, this effect is driven by local shocks, while in rural areas, it is primarily driven by shocks at migration destinations. Urban youth show evidence of a deterioration in labor market outcomes linked to declining matriculation rates, while there is no evidence of significant labor market effects for rural youth.

Keywords: Export Shock, Human Capital Attainment, Urban-rural Inequality, China

JEL Classification: F14, F16, J24, O15, O18, O19

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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, whether locally or via internal 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 confirmed 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 educational attainment 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 to the exporting sector (defining local with respect to 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, especially for rural youth.

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 plausible 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 that were more or less exposed to the reduction in tariff uncertainty. Here, we capture exposure as the weighted average of the local shock and the shock in plausible migration destination counties, the latter proxied by counties where 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: our measures captures this increase in labor demand in both the local and the migration destination market (Erten and Leight, 2020).

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.

Our primary results suggest that youth reaching the point of high school matriculation post-2002 in counties exposed to higher NTR gaps show a significant decline in the probability of enrolling in high school. In our preferred specification, a one standard deviation increase in the augmented county-level NTR gap (incorporating both local and migration destination shocks) is associated with a decline in the probability of enrollment in high school of nine percentage points, relative to a mean probability of enrollment of 49.3% for cohorts matriculating prior to the WTO-driven shock; this is a proportional effect of 20%. Importantly, this response is somewhat underestimated in specifications using the simple local shock that fails to capture shocks at migration destinations: the decline in matriculation rates is only about 16% in response to the local shock.

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 for the observed effect — a reduction in educational supervision by parents experiencing increased hours at work, systematically different preferences around human capital accumulation in high and low NTR gap counties, or differential fiscal investments in education — and find no evidence that these mechanisms are operational in this context.

We present further evidence that this pattern is substantially different for urban and rural youth. The educational response to local shocks is concentrated among urban youth, while the educational response to migration destination shocks is large and statistically significant for rural youth and virtually zero for urban youth; the difference between the urban and rural response to migration shocks is also significant. We also present some suggestive evidence around the medium-term labor market effects of the shift in high school matriculation patterns for affected cohorts as observed five years later. These effects in fact seem largely negative in urban areas, indicative of some divergence between the perceived short-term returns to terminating education to seek employment and the medium-term returns. Urban youth exposed to a substantial NTR shock are less likely to be employed in non-agricultural occupations, and particularly show a large decline in the probability of high-skilled employment. In rural areas, by contrast, the labor market effects are essentially null, though these results must be interpreted cautiously given some sample truncation (generated by youth still pursuing education at the point of the survey).

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 parts of the 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 China, previous papers use aggregate prefecture-level data and present evidence that reductions in external tariffs (Li, 2018) 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. 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 take into account shocks at potential migration destinations, and often exclude migrants from the analysis 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]

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 analyzing data from India shows that rural human capital accumulation responds to regional variation in the urban returns to education, particularly for those individuals characterized by a higher migration propensity (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 employment 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.

Third, our study also contributes to the literature examining the effects of trade shocks on inequality, though this work generally does not focus on urban-rural heterogeneity. Previous papers analyze the effect of trade liberalization on wage inequality in Mexico

[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).

(Verhoogen, 2008), Brazil (Pavcnik et al., 2004), and Colombia (Goldberg and Pavcnik, 2005), but this analysis necessarily excludes rural households who are not engaged in wage employment. Porto (2006) analyzes the effect of trade liberalization on both income and consumption inequality in Argentina, but does not analyze urban-rural heterogeneity. Topalova (2010) finds that rural districts more exposed to trade liberalization experienced a slower decline in poverty in India, using heterogeneity at the district level rather than the household level. In the literature focusing on China, only one paper analyzes the effects of trade on inequality, analyzing inequality in urban wages (Han, Liu and Zhang, 2012).[2] Our analysis enables us to provide some analysis of the comparative effects of export-oriented shocks on urban and rural youth, and suggests that the effects on human capital attainment are generally comparable (i.e., these shocks are not widening the pre-existing educational gap) — with the caveat that the educational effects for rural youth are observed to operate primarily through a migration channel.

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 the overall effects for high school enrollment in a pooled sample. Section 5 presents evidence on the implications of the shock for human capital attainment and labor market outcomes in urban vis-a-vis rural areas, and Section 6 concludes.

[2] Other analyses have looked broadly at cross-provincial inequality or the urban-rural income ratio at an aggregate level (Li and Coxhead, 2011; Wei and Wu, 2001; Zhang and Zhang, 2003).

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 additional, 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 only gradually, and primarily prior to accession to the WTO in the 1990s.[3]

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.[4]

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 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

[3]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).

[4]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).

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 high school entrance exam. Although China had 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.[5] The limited attractiveness of post-compulsory education has been attributed to both the substantial tuition fees[6] and the steady enhancement in employment prospects for migrant workers with lower levels of education (de Brauw and Giles, 2017). Given that meeting the increasing demand for skilled labor is crucial in this period of structural transformation and 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 offsetting chan-

[5]Source: China Statistical Yearbook, 2002.

[6]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).

nels. On the one hand, 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. On the other hand, the availability of low-skilled positions immediately raises the opportunity costs of staying in school, incentivizing youth to work directly upon middle school graduation. The sign of the net direct effect on high school enrollment is thus theoretically ambiguous.

In addition to changes in returns and costs of schooling, the reduction in tariff uncertainty may also indirectly affect education of youth 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 following the reduction in tariff uncertainty raises parental wage income. If high school education is a normal good, we would expect the demand for education to rise. In addition, a higher level of parental income may render tuition fees more affordable for credit-constrained families, leading to a higher high school enrollment rate.

Second, an increase in the wage induced by higher labor demand following the reduction of tariff uncertainty may encourage parents to work for longer hours, reducing the time available for them to invest in their children's educational performance.[7] Alternatively, a higher wage may motivate parents who were not previously working to enter the labor market, requiring youth to look after younger siblings or engage in other household responsibilities; this channel may be particularly salient for girls, who are often substantially involved in the care of younger siblings (Morduch, 2000; Dammert, 2010; Qureshi, 2018). Both effects would reduce high school enrollment.

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. The enhanced fiscal position 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 such as construction of infrastructure and manufacturing factories. These latter changes in the composition of fiscal expenditure are expected to lower high school enrollment.

Importantly, educational responses to the reduction of tariffs may differ between urban and rural youth. Urban youth may have better access to export-oriented industries that are clustered around cities; at the same time, rural youth may migrate to export-oriented destinations, or may shift from agricultural to non-agricultural employ-

[7] 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).

ment locally (Erten and Leight, 2020). As the increasing urban-rural educational gap has become a serious policy challenge for China (Zhang and Kanbur, 2005; Heckman and Yi, 2014; Zhang, 2017), it is crucial to empirically examine the net effect of the reduction of tariff uncertainty on education for urban and rural youth separately, and thus evaluate whether positive trade shocks have further widened existing educational gaps. Utilizing household-level data allows us to both analyze the net impact of the reduction of tariff uncertainty on education, and examine heterogeneous 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.[8] 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.[9] Though 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.

Our empirical analysis focuses on children born between 1980 and 1991 of the sample households, for the following reasons. Cohorts born before 1980 reached the standard

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

[9]More specifically, youth who left their hukou households for more than 6 month (temporary migrants) and those who changed their hukou residence (permanent migrants) cannot be linked to their birth households in the census.

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%. 52.4% of the youth sample are male, 75% are from rural families, and 72.5% have at least one sibling.[10] 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 due to questionnaire design. These variables are indicated in the last column of Table 1. Nevertheless, they are useful for robustness checks and for an analysis of the effects of the primary shocks on labor market indicators.

3.2 Measurement of Trade Shocks

In this analysis, 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.[11] 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 em-

[10] 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.

[11] 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).

ployment reported in the 1990 census. The census data allows us to calculate the share of tradable employment by industry in each county, interacting the NTR gap for subsector i with the subsector's county-specific employment share.

$$NTR\,Gap_c^{Local} = \sum_i empshare_{ic}^{1990} \times NTR\,Gap_i \tag{1}$$

We draw on two sources of census data to construct the 1990 county-level employment shares: an aggregated dataset that reports total employment at the level of the local jurisdiction, and a micro-level dataset reporting individual data for a 1% sample. For prefecture-level cities, the aggregate data reports employment only at the level of the entire city, not for its constituent county-level subunits. Accordingly, we use the aggregate data to construct the NTR gap for counties outside of prefecture-level cities, and use the micro-level data to construct county-level gaps for counties inside of prefecture-level cities. This allows us to maximize variation in measured exposure to trade shocks in urban areas, while minimizing any potential small-sample bias induced by use of the 1% micro sample. We will subsequently demonstrate that our results are robust to using a NTR gap constructed only using aggregate data.

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 in our analysis as a measure of the purely local shock to export production. However, in our main analysis we will preferentially use what we denote the migration-augmented NTR gap. Using data on the local NTR gaps estimated for all counties in China, we construct the migration-augmented gap as follows. Data on migration from the 2000 census is used to estimate the share of youth (individuals aged 16–29) in each CHIP county who have not migrated ($NonMig_c$) and the share of youth who report migrating to (and thus currently residing in) each possible destination county d (Mig_{cd}).[12] We then estimate the weighted average of the NTR gap at the origin county as well as all destination counties; there are 2873 counties represented in the 2000 census, and thus each origin county has 2872 possible migration

[12] 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.

destinations.

$$NTR\,Gap_c^{Mig} = NonMig_c \times NTR\,Gap_c + \sum_{d=1}^{2872} Mig_{cd} \times NTR\,Gap_d \tag{2}$$

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 future migration destinations. More specifically, if we examine the persistence of migration destinations at the county level between the 2000 and 2010 censuses, we find that more than 80% of counties report at least one major migration destination that is consistent across the two survey rounds; and more than 50% of counties report at least two major migration destinations consistent across rounds.[13]

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.[14] 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.[15]

[13]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.

[14]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).

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

4 Empirical Analysis

In this section, we present our primary empirical results, and discuss robustness checks as well as evidence around mechanisms.

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 county-level migration-augmented NTR gap $NTR\,Gap_{cp}^{Mig}$.

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 2, the majority of students graduate from middle school and make decisions about matriculation into high school between the ages of 14 and 16.[16] 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 into high school 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.[17] 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.

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.

$$Enroll_{ihcpt} = \beta_1 Treat_t \times NTR\,Gap_{cp}^{Mig} + \mu_t + \kappa_{cp} + \gamma_{pt} + \chi_{ihcpt} + \epsilon_{ihcpt} \quad (3)$$

[16] 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.

[17] Based on the numbers in Figure 2, $Treat_{1986} = 0.3646$ and $Treat_{1987} = 0.7201$.

The relationship of interest is estimated conditional on birth year fixed effects μ_t and county fixed effects κ_{cp}; additional specifications also include 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).[18] Standard errors are clustered at the county level, yielding 179 clusters.

The primary results of estimating equation (3) are presented in Panel A in Table 2. In Column (1), we estimate a simpler specification including only birth-year and county fixed effects; Column (2) includes individual-level controls, and Column (3) reports our preferred, primary specification including individual-level controls and province-year fixed effects. In Columns (4) and (5), we report two additional specifications including differential trends for manufacturing-intensive counties (in Column 4) and both manufacturing-intensive and agricultural-intensive counties (in Column 5). 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.[19]

It is clear that the coefficient of interest is consistently negative across all five specifications: youth who reach the age of matriculation into high school post-2002 in counties exposed to larger migration-augmented NTR gaps ex ante are significantly less likely to enroll. The magnitude of the coefficient is relatively consistent across specifications, ranging from -0.77 to -0.97, suggesting that a one standard deviation increase in the NTR gap (an increase of 0.102) is associated with a decline in the probability of enrollment of between 7.7 and 9.7 percentage points, relative to a probability of enrollment for pre-shock cohorts of 49.3%, a proportional effect of between 17% and 20%. (In Table A1 in the Appendix, we also present analogous results using 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. The results are entirely consistent, though slightly larger in magnitude.)

In Panel B of the same table, we present the analogous results using the local-only shock; the bottom row of the table presents a p-value comparing across the estimated coefficients for the local and migration-augmented shock. The estimated coefficients using the local only shock are consistently smaller in magnitude, and the difference is statistically significant at the five or one percent level. Using our preferred specification in Column (3), the estimated coefficient using the local only shock is around 20% smaller.

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

[19] 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).

This pattern is consistent with the hypothesis that the local shock does not fully capture the human capital response to export shocks in contexts where migration is salient, and we will subsequently demonstrate that this is particularly important in rural areas.

We now continue to explore these empirical patterns using only the migration-augmented gap. Figure 3 captures the primary result graphically. We regress the dependent variable of high school enrollment on a series of binary variables for each birth cohort interacted with the NTR gap, and present the estimated coefficients and confidence intervals. It is evident that there is no significant effect of the NTR gap on enrollment for cohorts born prior to (and including) 1985, who reached the age of 16 at latest in 2001 (prior to WTO accession). For cohorts born in 1986 and 1987, reaching the plausible age(s) of matriculation right around 2002, the effect is negative and noisily estimated, and it becomes robustly negative for cohorts born in 1988 and later. This pattern is evident in specifications both with and without additional control variables. In addition, the absence of any correlation between the NTR gap and high school enrollment variables for earlier cohorts is consistent with the hypothesis that there are no differential pre-trends in the outcomes of interest for high and low NTR gap counties. We reproduce these graphs showing a much longer pre-trend (including birth cohorts back to 1970) in Figure A4 in the Appendix, and find a consistent pattern.

Table A2 in the Appendix further reports results for heterogeneity with respect to child and parent-level characteristics. For concision, we focus on reporting the preferred specification, equation (3), including both individual-level control variables and province-year fixed effects. There is little evidence of heterogeneity with respect to gender, sibship size, or birth order (as reported in Panel A), but some evidence in Panel B 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)

To sum up, the evidence seems clear that the reduction in tariff uncertainty and the associated positive shock to labor demand both locally and in migration destinations is associated with a significant decline in the probability that youth choose to enroll into high school education, and this decline is observed consistently for youth with a range of individual characteristics. However, youth in households in which the mother has high school education do seem to be at least partly protected from the effects of this shock.

4.2 Robustness Checks

Table 3 reports a number of alternate specifications to explore the robustness of our results, building on our primary specification (3). Column (1) includes additional control variables for other trade shocks: the level tariff imposed by the U.S. (normal trade relations tariff or NTR rate), the level of import tariff imposed by China, and a variable capturing the degree to which quotas imposed under the Multi-Fiber Agreement were binding. All three variables are converted to weighted (migration-augmented) county-level shocks using the same industry and migration weights employed to construct the migration-augmented NTR gap. We then construct an estimated shock for each county-birth year cell corresponding to the average trade shock in the year of high school matriculation for children born in the target year, using the same weights described above. The estimated results are entirely consistent when including the additional controls.

In Column (2), we explore whether there is any evidence of an anticipation effect of the NTR shock for youth cohorts making choices around high school enrollment prior to the shock, constructing a treatment exposure variable that is parallel to the main variable $Treat_t$, but assuming cohorts are exposed post-2000, the year of legislative passage (as opposed to implementation) of China's permanent NTR status. It is evident that the coefficient on the alternate variable $Treat_t^{alt} \times NTR\,Gap_{cp}^{mig}$ is negative but significantly smaller in magnitude, and the hypothesis that the coefficients are equal can be rejected ($p = 0.024$). Column (3) estimates the primary specification conditional on household fixed effects for households with at least two children in the target age cohorts, to eliminate any possible bias associated with unobserved time-invariant heterogeneity across households; again, the results are consistent.

Finally, Columns (4) and (5) report two placebo tests in which other variables capturing human capital attainment — height, and educational performance — are regressed on the shock of interest. (Educational performance is reported on a scale from one to five for the current year of schooling for enrolled youth, or for the last year of schooling for those who have already finished schooling.) Analyzing these variables allows us to test the hypothesis that high and low NTR gap counties are characterized by significantly different trends over time in preferences for human capital investment, generating a gap between older and younger cohorts. Height is largely determined early in life (i.e., unlikely to be affected by the WTO shock), and thus analyzing this relationship allows us to identify whether there is a significantly different trend in early life investments in high versus low NTR gap counties; educational performance is largely determined contemporaneously, and allows us to analyze potential differences in cognitive status and parental investment (both of which determine performance), conditional on enrollment. There is no evidence of any significant relationship for either variable.[20]

In addition, Table A3 replicates the primary results (reported in Panel A of Table

[20]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).

2) using alternate strategies to construct employment weights, while employing identical migration weights to construct the migration-augmented gap. In Panel A, we construct the NTR gap by weighting each subsector with respect to total employment, assigning a zero weight to the tertiary (non-tradable) sector.[21] In Panel B, we construct 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 date of the shock; however, it introduces bias associated with strategic industrialization by counties seeking to expand manufacturing in anticipation of shocks induced by WTO accession. In Panel C, we use the shock variable constructed only from aggregate census data as reported in 1990. As noted above in Section 3.2, for prefecture-level cities, aggregate 1990 census data is reported only at the level of the prefecture, not for the constituent counties; this yields a sample of only 116 units characterized by unique NTR gaps, reducing power. However, the use of the aggregate data also reduces noise associated with small sample bias, particularly for county-industries characterized by low employment shares.

It is evident that the results are uniformly consistent in all three panels.[22] Accordingly, we conclude that the main effect of the WTO accession shock reported in the primary results does not reflect the specific construction of the NTR gap.

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 one year in each direction (1979—1992) and then by two years (1978—1993). 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. The results are presented in Table A4 in the Appendix. Relative to the primary sample, the sample expands by approximately 1100 observations in Panel A and by approximately 1800 observations in Panel B. At the

[21]In our main specification, we estimate the NTR gap without considering the relative size of the services sector, weighting employment with respect to tradable employment; this methodology is recommended by Kovak (2013), though earlier papers in the trade literature assign the non-tradable sector a weight of zero. More specifically, the alternate NTR gap is calculated as $NTRGap_c = \sum_i empshare_{ic}^{1990,total} \times NTRGap_i$, where $empshare^{1990,total}$ is the employment share for each tradable subsector relative to total county employment.

[22]The sample for the results reported in Panels B and C is slightly larger vis-a-vis the main specification in which the NTR gap is constructed using micro-level data. The reason for this is that seven CHIP counties that are part of prefecture-level cities had county names and codes that could not be matched to the micro-level data in the 1990 census. They can be matched to the 2000 census. Accordingly, these counties and associated households are excluded from the analysis using micro data-derived NTR gaps, but are included in the analysis using 2000 census data, and using 1990 census data at the level of the prefecture city.

same time, the primary results remain entirely consistent in both sign and magnitude.

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.[23] We have already reported specifications including the interaction between time-invariant county characteristics (binary variables for the county-level share of employment in the primary and secondary sectors falling above the median in 1990) and time trends, as included in Columns (4) and (5) of Table 2. We can further demonstrate that our results are robust to including interactions between a binary variable for a high primary employment share in 1990 and province-year fixed effects, and between a binary variable for a high share of the population reporting primary education in 1990 and province-year fixed effects.[24]

We can also identify whether pre-trends in high school enrollment are correlated with other covariates reported in the 1990 census 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 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). Each of these variables is highly correlated with employment 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 birth 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 pre-WTO cohorts (the 1985 cohort would reach the age of matriculation by 2001, at latest). The results reported in Table A5 suggest that none of the cited variables are significantly correlated with this county-level short-

[23]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.

[24]Results are not reported, but are available upon request.

difference, conditional on provincial fixed effects. This finding is consistent with the hypothesis that these omitted variables are not a meaningful source of bias in our primary analysis.

Finally, we draw on Adão, Kolesár and Morales (2019) to estimate alternate standard errors for our primary specification that are robust to arbitrary cross-regional correlations in regression residuals for regions characterized by similar sectoral shares, and find our primary results are consistent. For the primary specification reported in Column (3) of Table 2, the adjusted standard error suggests that $p < 0.001$.

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, weighting each county-year cell relative to its number of observations and interacting the county-level NTR gap with the same continuous measure of treatment used in equation (3). Xi_{cpt} are the same individual-level covariates used in the main analysis and collapsed to a county-year mean.

$$M_{cpt} = \beta Treat_t \times NTR_{cp} + \mu_t + \phi_{cp} + \chi_{cpt} + \epsilon_{icp} \tag{4}$$

We can verify that the primary coefficient β remains comparable to that estimated in the individual-level specification: $\beta = -0.812$, $p < 0.001$. We also generate a county-level event study plot that is parallel to Figure 3, reported in Figure A5 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 A6 in the Appendix; Figure A6a shows the 95% confidence intervals, and Figure A6b 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.

4.3 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 labor demand for non-agricultural employment dominates any perception of possibly higher returns to education or any potential positive income effect induced by the same shock, generating a decline in the probability of high school matriculation. This effect is observed consistently across a number of specifications and for a range of subsamples.

We present three additional sources of evidence that are consistent with this hypothesized channel. First, we analyze heterogeneity with respect to the required skill level of industries — whether local or at plausible migration destinations — affected by the export shock. If youth are primarily motivated by the short-term opportunity costs of schooling to forgo high school education, this effect will be smaller for youth entering labor markets where export-oriented industries are more likely to demand skilled labor: in that case, available opportunities for youth with only a middle school education are presumably scarcer. 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 migration-augmented average of skill intensity for the local and migration destination labor markets, generate a binary variable equal to one denoting a county characterized by above-average skill requirements, and re-estimate the primary specification, equation (3) for youth in counties facing a high-skill vis-a-vis a low-skill shock.

The results reported in Columns (1) and (2) of Table 4 show that the decline in reported high school matriculation is significantly lower in high-skill counties. In low skill counties, a one standard deviation increase in NTR gap is associated with a 17 percentage point decline in the probability of high school matriculation, while in high skill counties, there is only an eight percentage point decline, and this difference is significant at the five percent level ($p = 0.024$). This suggests that variation in the relative opportunity cost of education correlated is meaningfully informing youth decision-making.

Second, we evaluate a hypothesized alternate channel in which an expansion in labor demand 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. This pattern would be prima facie inconsistent with several points of evidence: the heterogeneous effects presented in Table A2 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, and data directly reported by the sample households suggest that care-taking responsibilities by siblings are minimal.[25] To further explore this hypothesis, we split the sample to compare the effects of the shock for households where either parent reports some change in employment post-2002, versus households where no change in employment is reported; the results reported in Columns (3) and (4) of Table 4 show that there is no evidence of heterogeneity comparing across these samples ($p = 0.199$).[26] This suggests that variation in domestic responsibilities linked to shifting parental employment patterns are unlikely to be a primary relevant channel.

Third, we evaluate 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, including all counties in the CHIP sample, and includes reported county fiscal revenue, reported total expenditure, and reported expenditure on education; we 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 Table 5 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 A7 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, it seems clear that we can reject the hypothesis that counties characterized by larger NTR gaps are directing resources away from education in the post-WTO period.

5 Urban-Rural Heterogeneity; Labor Market Effects

The previously presented evidence suggests that in general youth making decisions about human capital accumulation are responding to export-driven demand shocks both locally

[25] 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.

[26] We cannot rule out that even parents who reported no change in employment experienced a change 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.

and at migration destinations. Understanding whether these patterns differ for urban and rural youth may be informative given that processes of structural transformation are distinct in urban and rural areas.

In order to probe this pattern, we define two variables that capture the local and migration destination shock separately, denoted $NTRGap_c^{Local}$ and $NTRGap_c^{Dest}$. The local shock was defined in equation (1). The destination shock is estimated by setting the weight on the local shock to be zero and re-weighting each destination shock to use the share of destination-county migrants relative to total outmigrants ($AllMig_c$), as opposed to total county residents.

$$NTRGap_c^{Dest} = +\sum_{d=1}^{2872} Mig_{cd}/AllMig_c \times NTRGap_d \tag{5}$$

In analyzing urban-rural heterogeneity, 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.[27] 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).

Table 6 presents the results of analyzing the effect of both local and migration shocks separately for the two samples. In Panel A, it is evident in Column (1) that the response for urban youth is entirely driven by a response to local shocks; the coefficient on the migration shock is extremely small in magnitude and statistically insignificant. In Panel B, by contrast, the coefficient 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. While the hypothesis that the effect of local labor demand shocks is consistent across subsamples cannot be rejected given the imprecision in the rural coefficient ($p = 0.499$), the hypothesis that the effect of migration shocks is consistent can be rejected ($p = 0.081$). It seems clear that the rural human capital response is driven primarily by export shocks at migration destinations, while the urban response is driven by local shocks. One implication of this finding is that examining the response to local export-driven shocks only would significantly underestimate the effect of export-driven growth on human capital accumulation particularly in rural areas.

We can also estimate the net aggregate effect of export-driven shocks both locally and

[27] 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.

at migration destinations for urban and rural youth, using the standard deviation of each shock observed in each subsample. The net effect of a one standard deviation decline in both the local and migration destination shock on high school matriculation rates is 7.7 percentage points in urban areas, and 9.5 percentage points in rural areas. The hypothesis that these effects are equal cannot be rejected. Accordingly, it seems that not only do export-driven shocks expand non-agricultural employment for rural youth as part of a process of structural transformation, they also seem to have meaningful implications for the labor market in urban areas, reducing post-compulsory education. However, proportionally the effect is much larger in rural areas, where the mean probability of matriculation for pre-shock cohorts is only 36.4%, relative to urban areas in which the comparable probability is 85.3%.

Given that the primary survey used in this analysis surveyed youth five years following the 2002 shock, we are also able to provide some evidence around the medium-term labor market effects of the increase in dropout in both urban and rural areas. As of 2007, youth report their employment status, occupations, wages and weekly work hours; measures related to employment status, however, are reported only for a truncated sample excluding those youth who are still enrolled in school as of the survey date, and wage and hours are reported only for youth working outside of agriculture.

The employment variable constructed is a binary variable equal to one for non-agricultural employment, and zero for youth engaged in agriculture or unemployed / out of the labor force. While data limitations do not allow us to distinguish between the latter employment statuses, in practice rural youth who do not report non-agricultural employment are likely at least partially engaged in agriculture, while urban youth who report no such employment are plausibly unemployed or out of the labor force. We also report separate results for high-skilled and low-skilled employment defined as follows. We measure the skill requirement for each reported occupation category according to the percentage of workers who attended high school in the CHIP sample; based on this measure, high-skilled workers include professionals, technicians, and government employees, and low-skilled workers consist of agriculture, production, transport, and service workers.

In Panel A of Table 6, we observe evidence of substantial negative effects for urban youth. Youth who were affected by the NTR shock and thus were less likely into high school are also less likely to report non-agricultural employment, as observed in Column (2), and this decline is concentrated in high-skilled occupations, as observed in Column (3). The magnitude of these effects is sizeable: a one standard deviation increase in the NTR gap is associated with a 11 percentage point decline in the probability of non-agricultural employment, relative to a mean of 78%, and a 10 percentage point decline in the probability of high-skilled employment, relative to a mean of 44%. Thus proportionally, the probability of high-skilled employment declines by about a quarter. There is also evidence of a decline in low-skilled employment, wages and hours, but these coefficients are not significant.

Thus it seems that urban youth who reached a lower level of educational attainment due to a positive shock to local non-agricultural labor demand are in the medium-term outcompeted by youth with a higher level of attainment, despite their increased years of employment experience. Accordingly, the decline in high school enrollment in response to positive export shocks cannot be explained by the argument that the returns to on-the-job training exceeds the returns to schooling for urban youth; in this case, we would expect a positive impact of the trade shock on employment status and wages. Instead, the pattern of dropout among urban youth may reflect a high discount rate of future earnings (O'Donoghue and Rabin, 1999), misprediction of future returns, or credit constraints.

In Panel B, by contrast, it is evident in Column (2) that there is no significant effect on medium-term labor market for rural youth affected by the NTR shock; the coefficient on employment is close to zero and statistically insignificant. Thus even while youth show evidence of a decline in the probability of high school matriculation (driven primarily by positive shocks to migration destinations), there is no evidence of meaningfully adverse effects, though there is also no robust evidence that they benefited from their early entrance into the workforce. This pattern would be consistent with the hypothesis that the returns to education are lower for youth born with a rural hukou, and thus the associated penalty for non-high school graduates is minimal (Zhigang and Shunfeng, 2006).

The findings are broadly consistent with those presented in Atkin (2016) for Mexico, though in the Mexican sample the decline in income and wages for high school dropouts is statistically insignificant, and effects for urban and rural youth are not analyzed separately. In India, previous work suggests that adverse trade shocks (a reduction in domestic protection) are associated with a relative decline in schooling attainment for both urban and rural youth, but substitution into child labor is much higher for urban youth (Edmonds, Topalova and Pavcnik, 2009; Edmonds, Pavcnik and Topalova, 2010). Here, the findings around both education and labor market effects of the NTR shock suggest that in a context of rapidly growing urban-rural gaps in China, positive shocks to the export sector are not serving to further widen this gap, at least in the area of human capital accumulation. The adverse effect of the NTR shock on high school matriculation is consistent across sectors, and the labor market consequences of the decline in matriculation seem to be restricted to urban areas.

6 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. Comparing 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 the associated reduction in 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. A one standard deviation increase in the county-level NTR gap is associated with a 20% decline in the probability of enrollment in high school; this effect is primarily driven by local shocks in urban areas, and primarily driven by shocks at migration destinations in rural areas.

These micro-level findings are consistent with the evidence of structural transformation following WTO accession at the aggregate level. In particular, the labor market behavior of rural youth in some communities who forgo education to pursue new employment opportunities is consistent with the employment shift from primary to secondary sectors as a result of export expansions found by Erten and Leight (2020). More importantly, our results indicate that the trade shock has an even broader impact for the Chinese labor market than implied by these previous findings. The shock not only provides non-agricultural employment opportunities for rural workers and encourages structural transformation, but also substantially reshapes the labor market and alters the perceived returns to schooling for urban workers, discouraging youth from pursuing post-compulsory education.

This paper also joins a very limited literature analyzing the effect of demand shocks at migration destinations, and suggests youth respond to shocks both locally and at these destinations. Particularly for youth in rural areas or in high outmigration regions, analyzing only the role of local structural transformation may be meaningfully incomplete. Understanding the relationship between shocks to non-agricultural growth, migration and education is an important dimension for future exploration.

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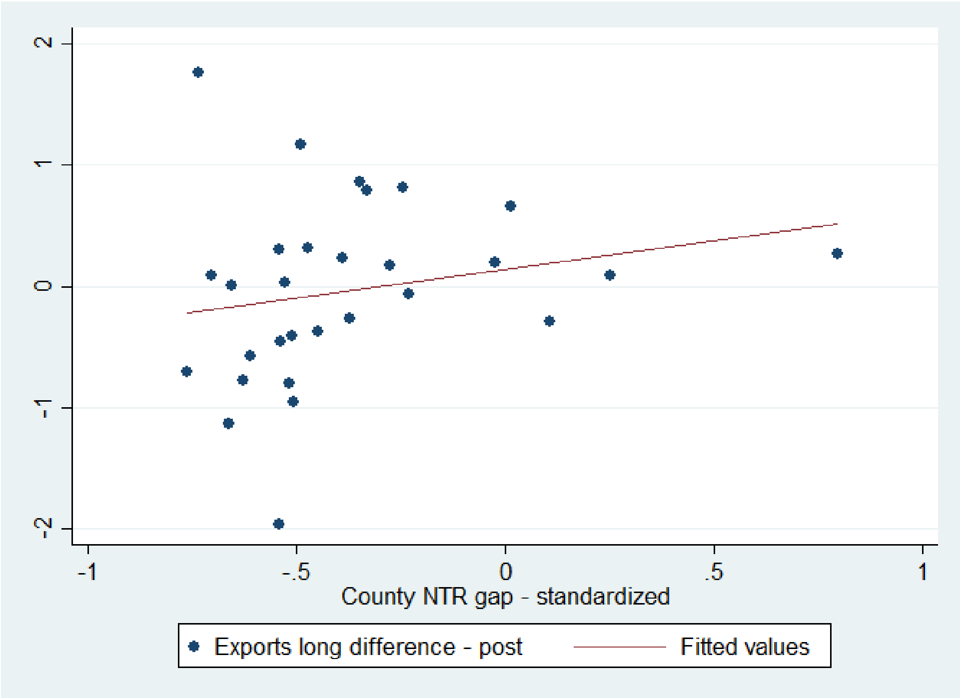
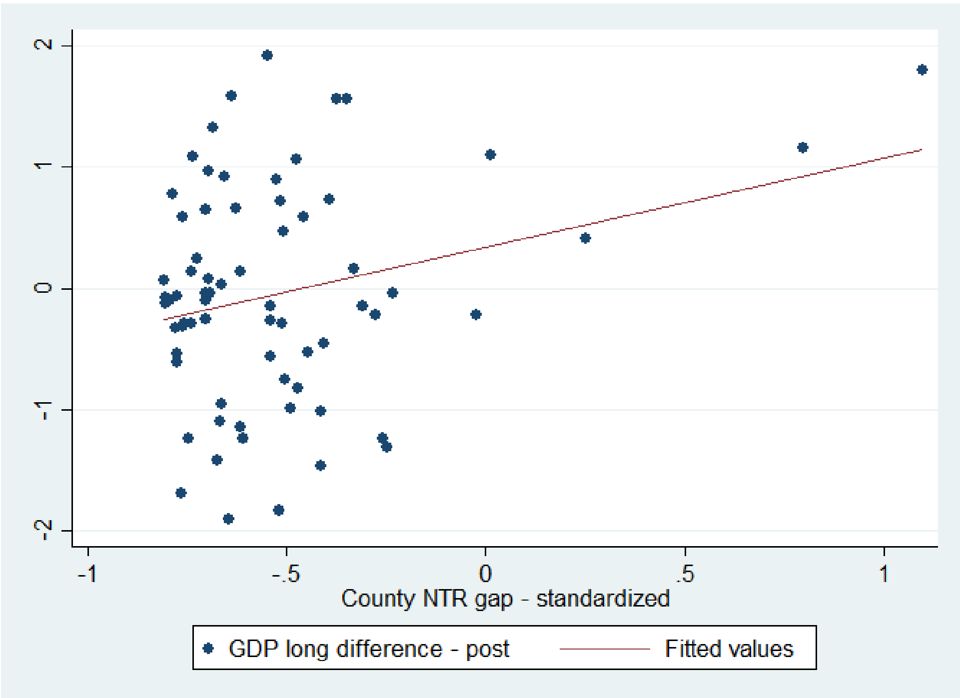
Figure 1: Long-difference post WTO: Local exports and GDP

(a) Exports

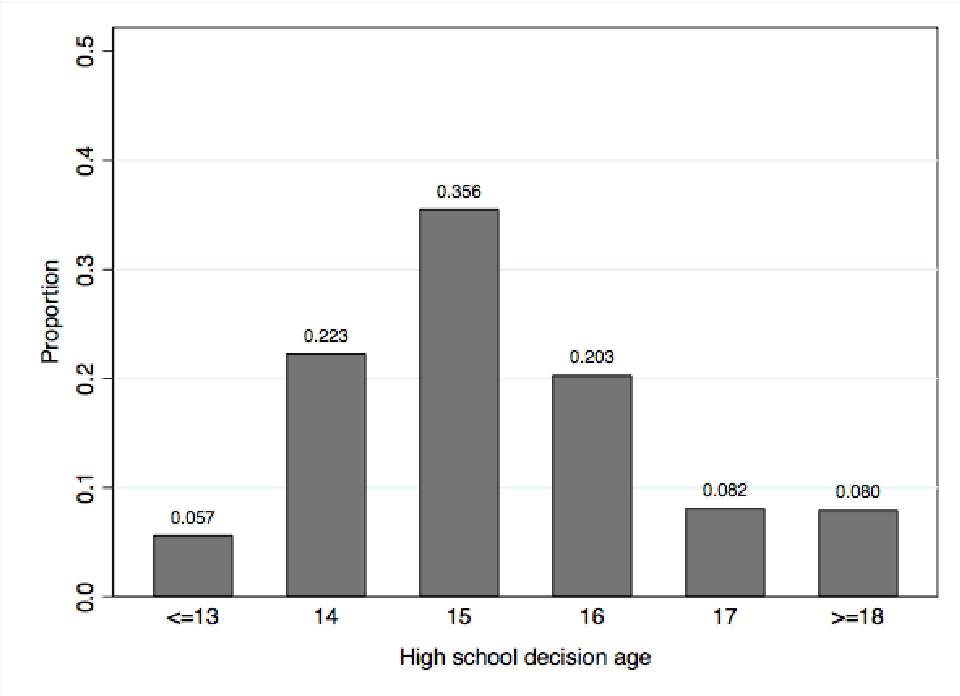


(b) GDP



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.

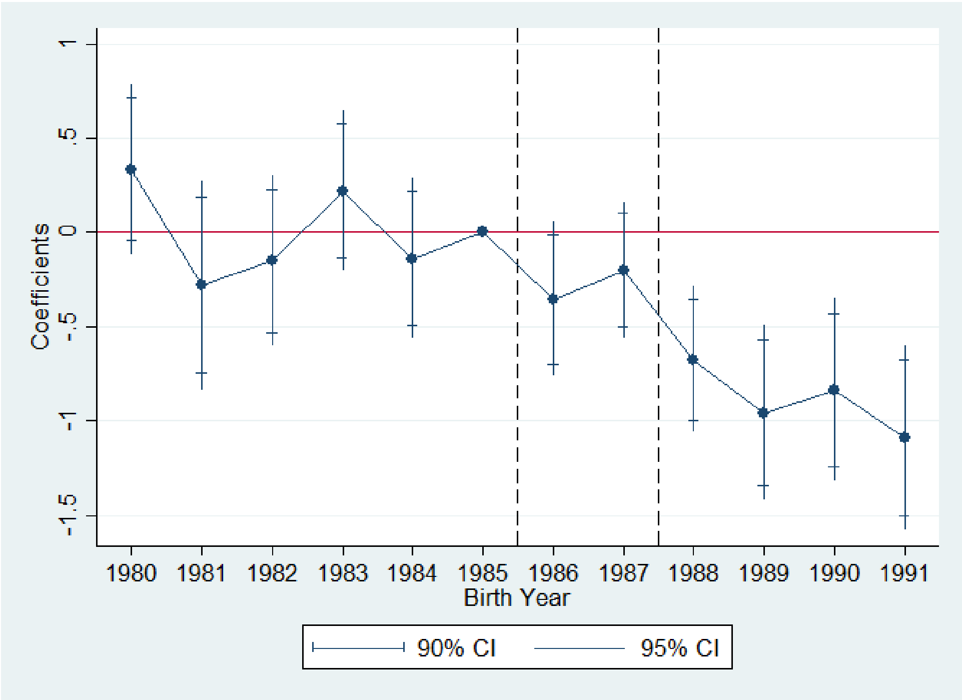
Figure 2: Distribution of high school decision age



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 3: Effects of trade shock on high school enrollment over time

(a) Base specification without controls



(b) Specification including controls

Notes: The figure shows estimated coefficients and confidence intervals for the effect of the migration-augmented NTR gap on high school matriculation for each birth cohort. 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.

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
Last grade school performance: 1 (very good) to 5 (very poor)	2.53	(0.732)	7,016	No
Panel B: Individual and household characteristics				
Gender (male=1)	0.524	(0.499)	9,473	Yes
Ethnic minority	0.012	(0.111)	9437	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

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Panel A: Migration-augmented NTR Gap					
Treatment Intensity	-0.774***	-0.829***	-0.974***	-0.924***	-0.799***
X Mig.$^{+}$ NTR Gap	(0.106)	(0.108)	(0.131)	(0.158)	(0.196)
Observations	8,850	8,850	8,850	8,850	8,850
Panel B: Local-only NTR Gap					
Treatment Intensity	-0.700***	-0.758***	-0.833***	-0.789***	-0.682***
X Local NTR Gap	(0.0907)	(0.0917)	(0.111)	(0.135)	(0.167)
Observations	8,850	8,850	8,850	8,850	8,850
Controls	County + birth year FE	+ Ind. level	+ Prov-year FE	+ Secondary trend	+ Primary trend
Test: $\beta^a = \beta^l$	0.043	0.050	0.000	0.007	0.039
Mean dep. var. (pre-shock cohorts)	0.493				

Notes: This table presents the results from regressing a binary variable for high school enrollment on the specified independent variable. In Panel A, a continuous measure of treatment capturing the relative exposure of different cohorts to post-WTO shocks at the point of matriculation is interacted with the migration-augmented county-level NTR gap; in Panel B, the Local NTR gap is employed. Additional control variables are as reported in the panel; individual-level control variables include 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. The final row of the table reports a test of the hypothesis that the estimated coefficient for the migration-augmented shock is equal to the estimated coefficient for the local shock. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Robustness checks

	(1)	(2)	(3)	(4)	(5)
	High school enrollment			Height	Edu. Perf.
Treatment Intensity	-0.924***	-0.897***	-1.406***	0.150	0.550
X Mig.$^{+}$ NTR Gap	(0.162)	(0.209)	(0.359)	(2.922)	(0.365)
Treatment Intensity (alt.)		-0.0888			
X Mig.$^{+}$ NTR Gap		(0.224)			
Spec.	Additional Controls	Alt. treatment	Household FE	Placebo	Placebo
Observations	8,850	8,850	8,850	7,311	6,883
Controls	County, birth year and province-year FE + ind. controls				
Mean dep. var. (pre-shock cohorts)	.493	.493	.493	166.961	2.488

Notes: Columns (1) and (2) present 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 Column (2), an additional treatment intensity variable coding cohorts matriculating post-2000 as exposed is added; in Column (3), household fixed effects are employed. In Columns (4) and (5), height and educational performance are regressed on the interaction of treatment exposure and the county-level migration-augmented NTR gap. All columns include the specified controls; individual-level 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 4: Mechanisms (education demand)

	(1)	(2)	(3)	(4)
	High school enrollment			
Treatment Intensity	-1.643***	-0.763***	-1.112***	-0.731**
X Mig.$^{+}$ NTR Gap	(0.376)	(0.135)	(0.157)	(0.293)
Sample	Low-skill	High-skill	No emp. change	Emp. change
Cross-spec. tests		0.024		0.199
Observations	4,982	3,868	6,931	1,919
Controls	County, birth year, prov-year FE + ind. level controls			

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 migration-augmented 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 the specified controls; individual-level 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 5: Mechanisms (education supply)

	(1) Log total fiscal revenue	(2) Log total fiscal expenditure	(3) Log education expenditure
Post-2002	2.518***	0.480**	0.596***
X Local NTR Gap	(0.361)	(0.187)	(0.228)
Observations	1,695	1,695	1,695
Controls	County, year and prov-year FE		

Notes: This table 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

Table 6: Urban and rural effects: education and labor market effects

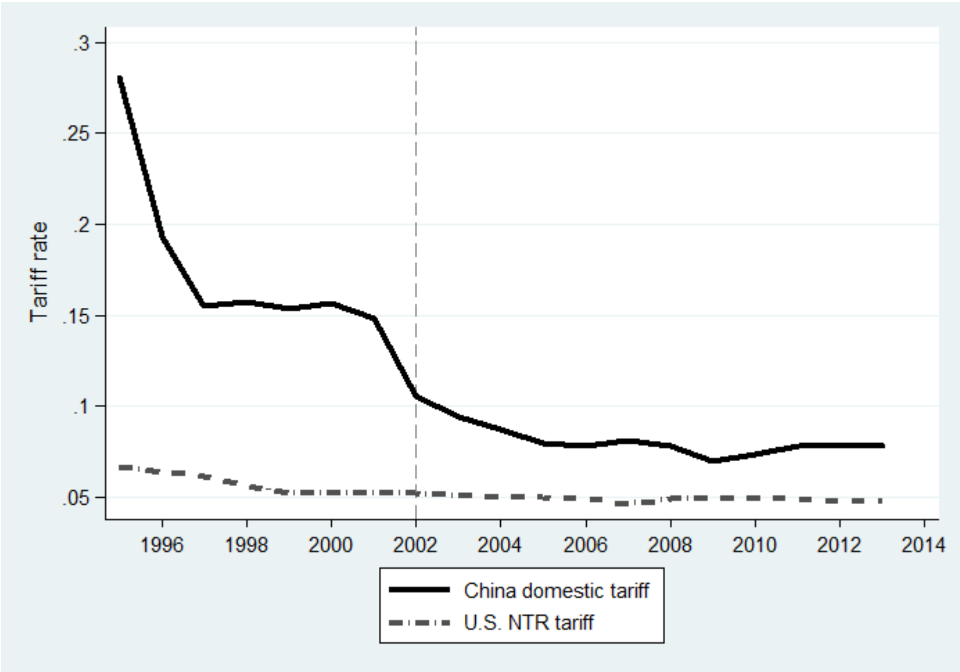
	(1) High school	(2) Non-ag. emp.	(3) High-skilled emp.	(4) Low-skilled emp.	(5) Log wage (month)	(6) Work hours (week)
Panel A: Urban youth						
Treatment Intensity	-0.645***	-1.069**	-1.026*	-0.170	-0.208	-4.076
X Local NTR Gap	(0.223)	(0.486)	(0.528)	(0.622)	(0.778)	(20.06)
Treatment Intensity	-0.00355	1.072	0.742	0.850	-0.680	19.24
X Mig. Dest. NTR Gap	(0.431)	(1.292)	(0.904)	(1.185)	(1.966)	(54.66)
Observations	2,215	1,401	1,401	1,401	804	809
Mean dep. var. (pre-shock cohorts)	0.853					
Panel B: Rural youth						
Treatment Intensity	-0.301	0.0393	-0.474	0.861	-1.112	13.03
X Local NTR Gap	(0.474)	(0.776)	(0.778)	(1.137)	(1.107)	(18.14)
Treatment Intensity	-0.705***	-0.777	-0.000497	-1.004	0.844	13.70
X Mig. Dest. NTR Gap	(0.237)	(0.514)	(0.391)	(0.715)	(1.161)	(16.94)
Observations	6,625	5,431	5,431	5,431	2,926	2,924
Mean dep. var. (pre-shock cohorts)	0.364					

Notes: This table presents the results from regressing a binary variable for high school enrollment as well as labor market indicators (a binary variable for any employment, binary variables for high- and low-skilled employment, and log wages and employment hours) 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 in Panel A is restricted to urban youth and in Panel B is restricted to rural youth. All columns include the specified controls; individual-level controls are as specified in Table 2. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Appendix

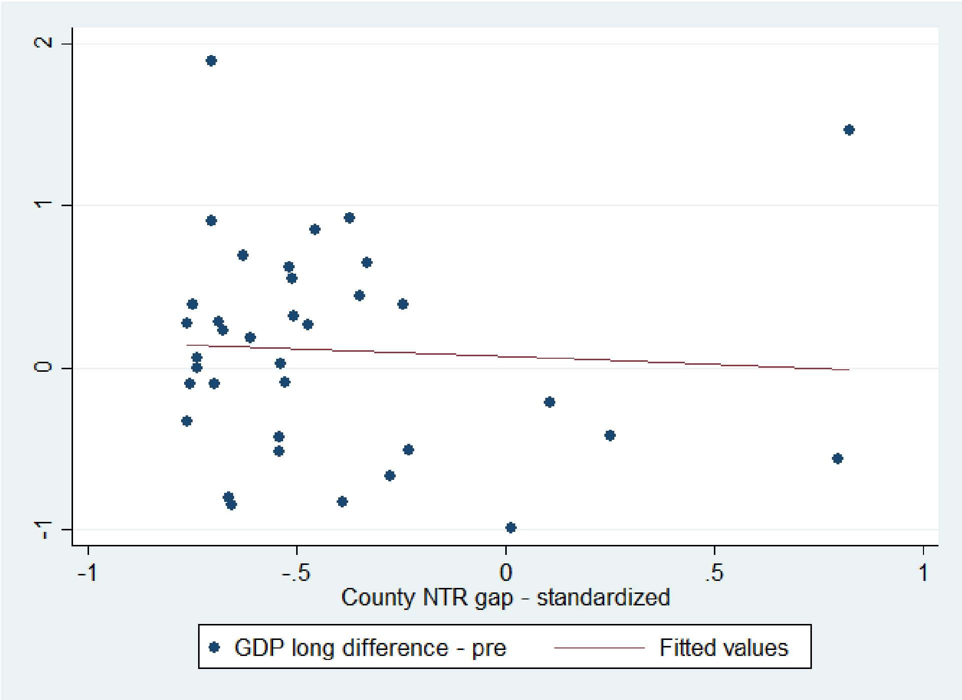
A1 Figures and Tables

Figure A1: Variation in Tariff Policy Over Time



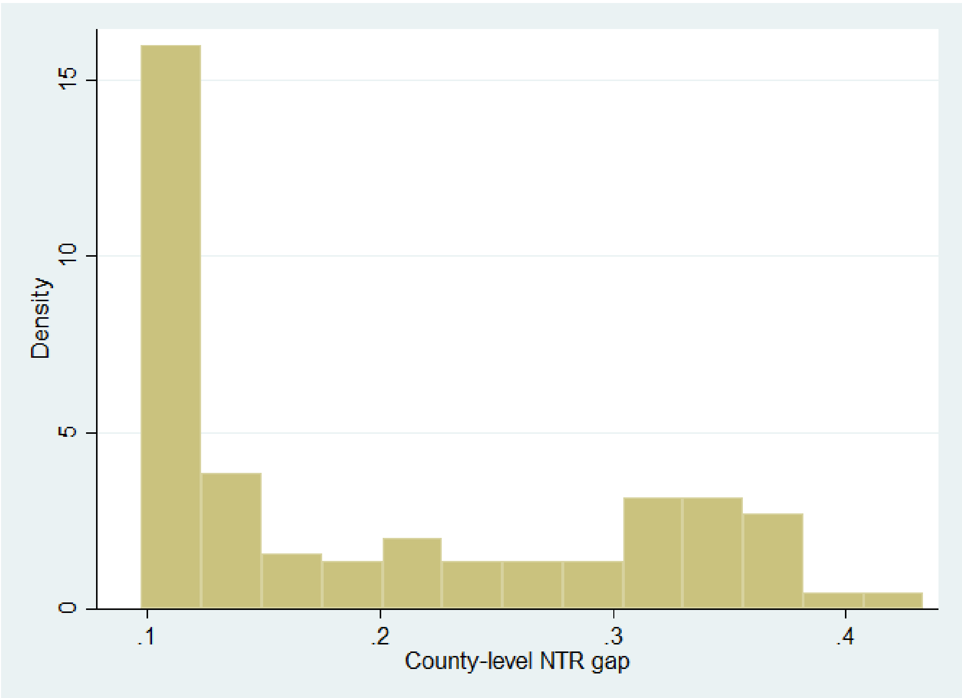
Notes: The figure shows the average domestic import tariff and the mean tariff rate (NTR or Normal Trade Relations rate) imposed on Chinese exports in the U.S market. The mean domestic import tariff is calculated as the weighted average of industry-level tariffs, utilizing as weights the share of total 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.

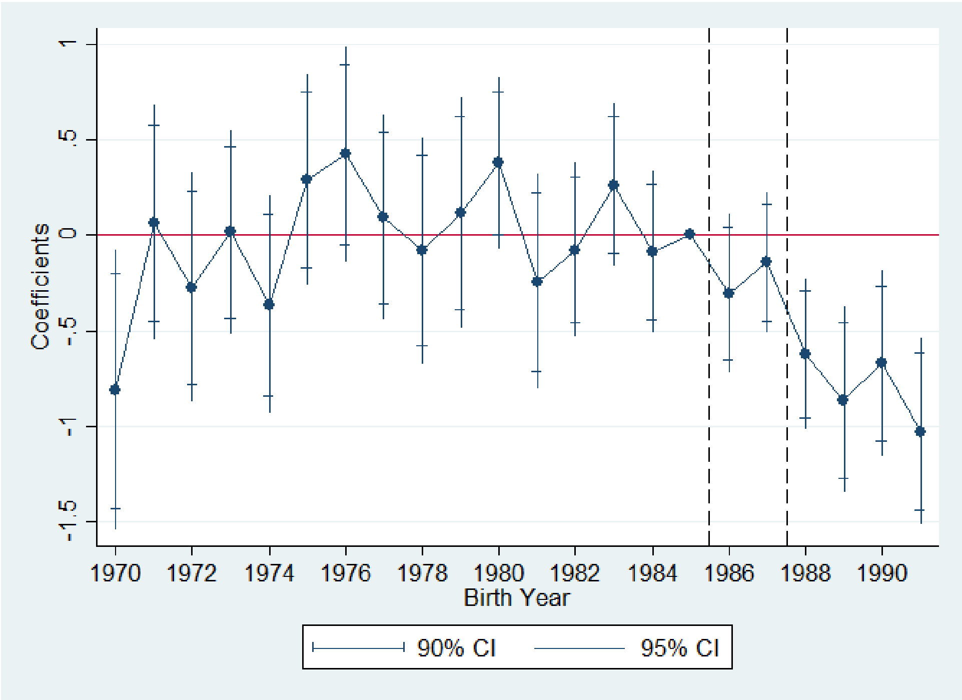
Figure A3: County-level NTR gap



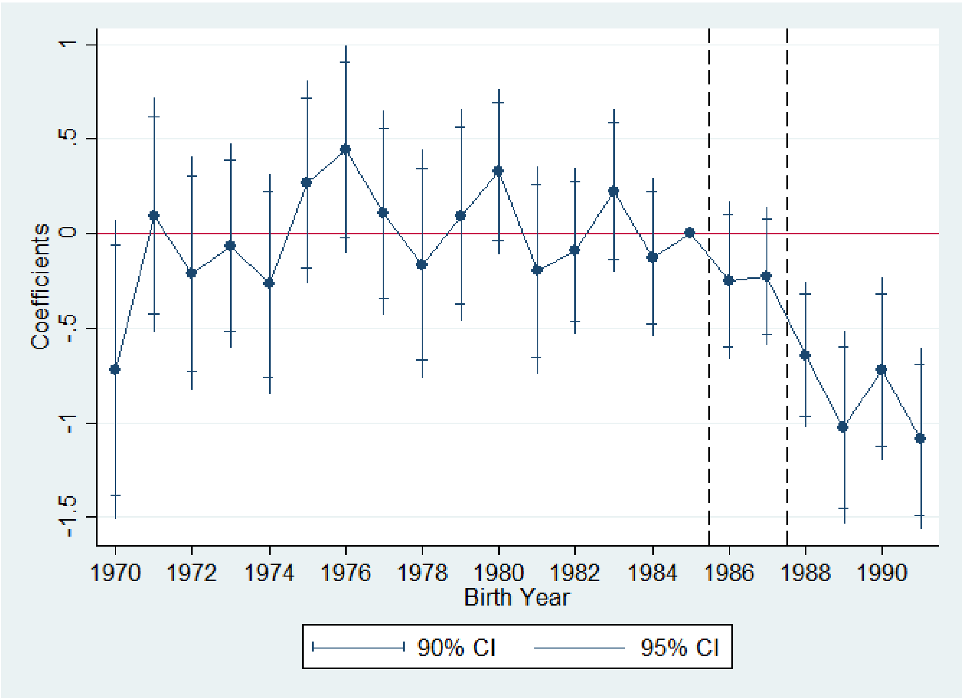
Notes: The graph shows a histogram of the county-level NTR gap for the 179 counties in the CHIP sample.

Figure A4: Effects of trade shock on high school enrollment over time: Long pre-trend

(a) Base specification without controls

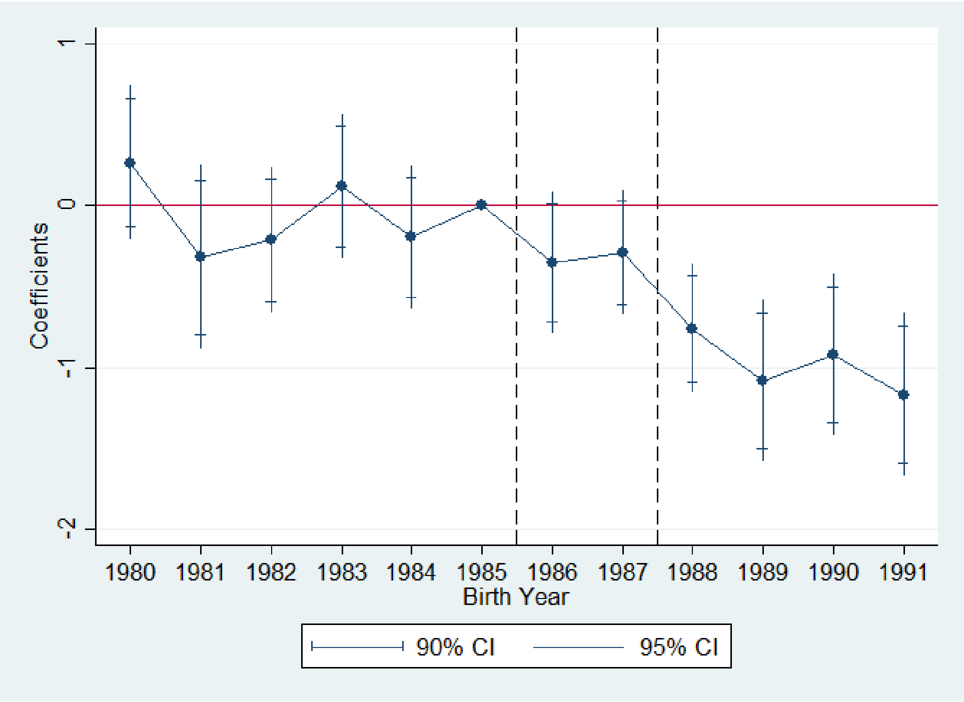


(b) Specification including controls



Notes: The figure shows estimated coefficients and confidence intervals for the effect of the NTR gap on high school matriculation for each birth cohort. 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.

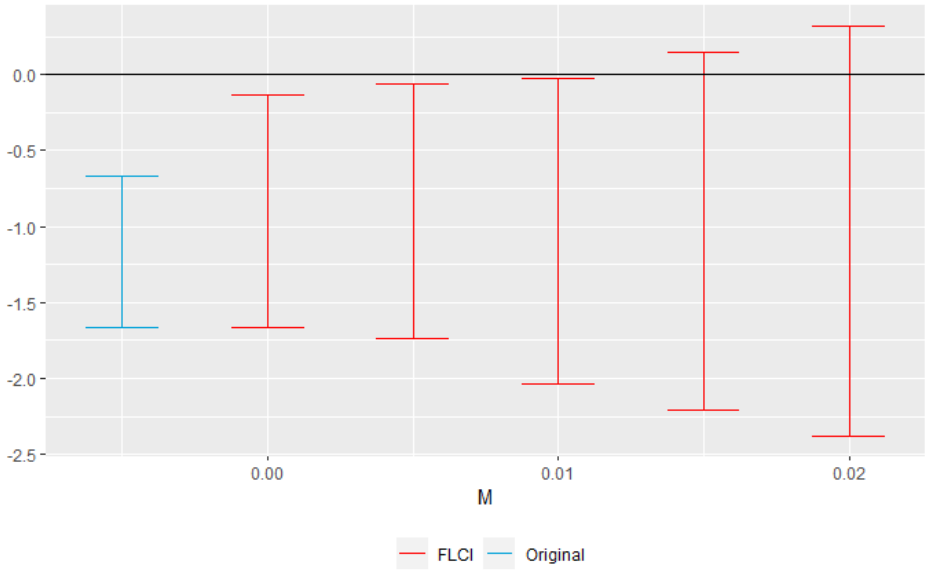
Figure A5: Effects of trade shock on high school enrollment over time: County-level panel



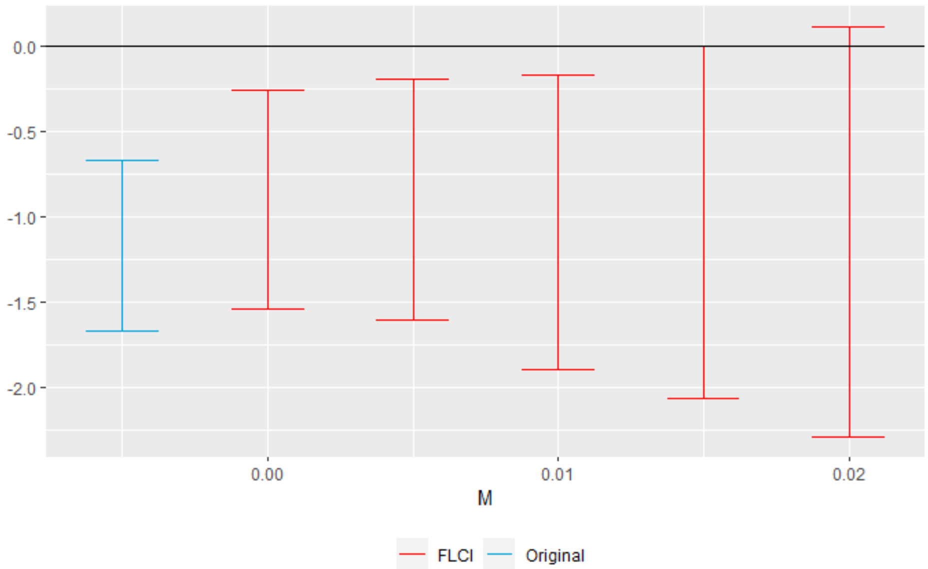
Notes: The figure shows estimated coefficients and confidence intervals for the effect of the migration-augmented NTR gap on high school matriculation for each birth cohort using a county-year level panel analysis. Cohorts to the rights of the second dashed line are fully treated. Cohorts between the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

Figure A6: Difference-in-difference sensitivity plots

(a) Sensitivity plot: 95% FLCIs



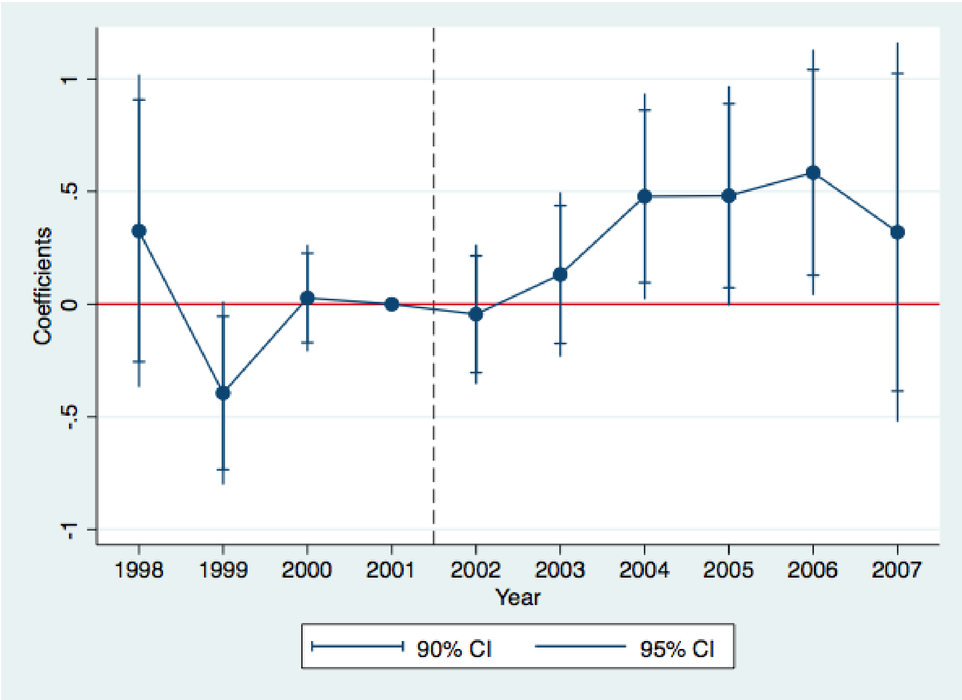
(b) Sensitivity plot: 90% FLCIs



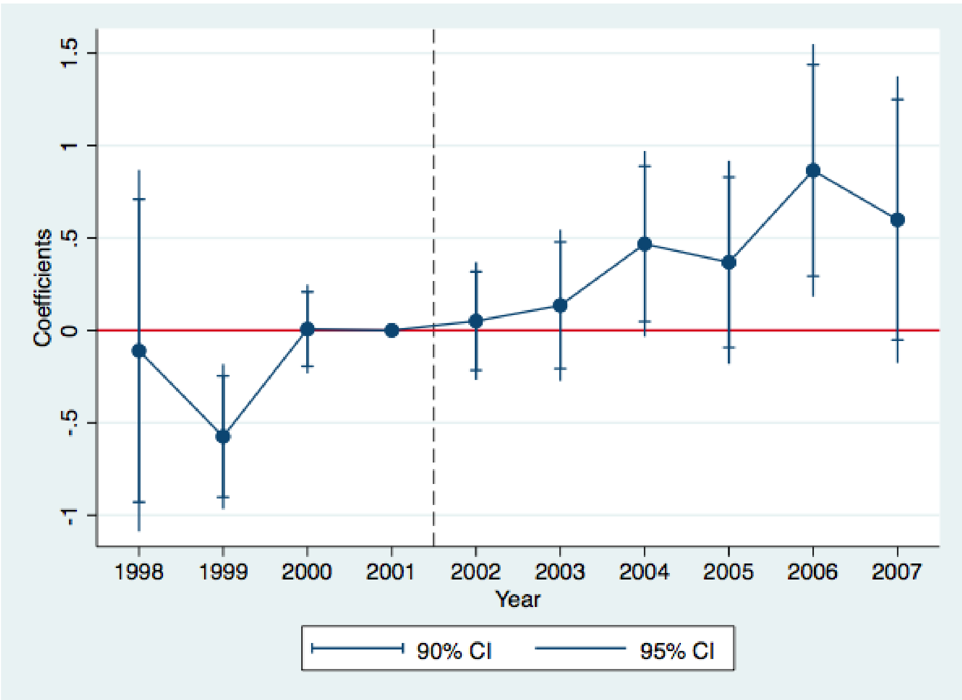
Notes: The graphs show fixed length confidence intervals corresponding to the estimate of the effect of the NTR gap on the county-level matriculation rate for cohorts born in 1991 relative to 1985, using a county-year level panel. The FLCIs are estimated following the honest dif-in-dif methodology described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

Figure A7: 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 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: Primary results using a binary treatment variable

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Treatment Binary	-0.837***	-0.894***	-1.054***	-1.018***	-0.892***
X Mig.$^{+}$ NTR Gap	(0.108)	(0.111)	(0.132)	(0.159)	(0.198)
Observations	7,238	7,238	7,238	7,238	7,238

Notes: This table presents the results from regressing a binary variable for high school enrollment on the specified independent variable. A binary measure of treatment (equal to one for cohorts matriculating post-2002) is interacted with the migration-augmented NTR gap, and partially treated cohorts are dropped. Additional control variables are as reported in the panel; individual-level control variables include 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. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Heterogeneous effects: individual and parental characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	High school enrollment					
Panel A: Individual characteristics						
Treatment Intensity	-0.995***	-1.037***	-0.580***	-1.201***	-1.546**	-1.135*
X Mig.$^{+}$ NTR Gap	(0.183)	(0.200)	(0.210)	(0.400)	(0.607)	(0.654)
Sample	Female	Male	No sibling	Any sibling	Firstborn	Non-firstborn
Cross-spec. tests		0.866		0.158		0.553
Observations	4,186	4,665	2,396	6,455	2,755	3,700
Panel B: Parental characteristics						
Treatment Intensity	-0.906***	-0.777***	-1.114***	-0.180		
X Mig.$^{+}$ NTR Gap	(0.199)	(0.185)	(0.223)	(0.325)		
Sample	Fathers: no high school	Fathers: high school	Mothers: no high school	Mothers: high school		
Cross-spec. tests	0.604	0.013				
Observations	5,984	2,591	6,973	1,647		
Controls	County, birth year and province-year FE + ind. controls					

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. 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 the specified controls; individual-level 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 trade shocks

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Panel A: Employment weights incorporating non-tradables					
Treatment Intensity	-0.863***	-0.962***	-1.137***	-0.862**	-0.613*
X Mig.$^+$ NTR Gap	(0.267)	(0.255)	(0.325)	(0.346)	(0.364)
Observations	8,850	8,850	8,850	8,850	8,850
Panel B: Employment weights using 2000 census data					
Treatment Intensity	-0.614***	-0.675***	-0.968***	-0.883***	-0.756***
X Mig.$^+$ NTR Gap	(0.139)	(0.137)	(0.130)	(0.153)	(0.195)
Observations	8,850	8,850	8,850	8,850	8,850
Panel C: Employment weights using 1990 census data in aggregate form					
Treatment Intensity	-0.484	-0.451	-0.814**	-0.773**	-0.985**
X Mig.$^+$ NTR Gap	(0.351)	(0.356)	(0.406)	(0.382)	(0.380)
Observations	8,760	8,760	8,760	8,760	8,760
Controls	County + birth year FE	+ Ind. level	+ Prov-year FE	+ Secondary trend	+ Primary trend

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, the migration-augmented NTR gap is calculated using employment weights constructed incorporating an estimate of total employment (including non-tradables); in Panel B, the migration-augmented NTR gap is calculated using employment weights constructed from 2000 census data; in Panel C, the migration-augmented NTR gap is calculated using employment data reported in aggregate form at the census level, rather than the 1% sample. All columns include the specified controls; individual-level 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: Primary results using alternate cohort windows

	(1)	(2)	(3)	(4)	(5)
	High school enrollment				
Panel A: Birth cohorts born between 1979 and 1992					
Treatment Intensity	-0.826***	-0.886***	-1.050***	-0.956***	-0.941***
X Mig.$^{+}$ NTR Gap	(0.113)	(0.114)	(0.135)	(0.160)	(0.164)
Observations	9,793	9,793	9,793	9,793	9,793
Panel B: Birth cohorts born between 1978 and 1993					
Treatment Intensity	-0.811***	-0.862***	-1.036***	-0.937***	-0.960***
X Mig.$^{+}$ NTR Gap	(0.120)	(0.120)	(0.140)	(0.164)	(0.168)
Observations	10,481	10,481	10,481	10,481	10,481
Controls	County + birth year FE	+ Ind. level	+ Prov-year FE	+ Secondary trend	+ Primary trend

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, the sample is expanded to include two additional birth cohorts in 1979 and 1992; in Panel B, the sample is expanded to include four additional birth cohorts, in 1978–79 and 1992–93. All columns include the specified controls; individual-level 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 A5: 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.