Export expansion, human capital investment and the urban-rural educational gap: 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 linked to WTO membership, 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; moreover, this effect is larger in urban than in rural areas. Our findings suggest that export-driven growth is plausibly narrowing the otherwise substantial urban-rural educational gap in China.

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. 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).

Major sectoral shifts in the labor market stimulated by export shocks presumably have substantial implications for human capital investment. 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). However, this literature has rarely explored the differential effects of trade liberalization on human capital attainment in urban and rural areas — despite the preceding theoretical and empirical evidence suggesting that the direct effect of the trade shock is plausibly very different in areas varying in their relative dependence on agriculture. In particular, the hypothesis of differential effects is prima facie consistent with the stylized facts observed in China, for which the trend in high school completion rates for cohorts born after the mid-1980s, and thus matriculating into high school following accession to the World Trade Organization in 2002, diverges for urban and rural areas (Figure 1).

Our objective in this paper is to analyze the effects of positive shocks to export-oriented production on educational attainment among Chinese youth and analyze heterogeneity in the effects across urban and rural youth, exploiting high-quality micro-level data from a sample of 179 counties in 10 provinces. 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. Intuitively, counties that were more exposed to a reduction in tariff uncertainty experienced a larger increase in export-driven manufacturing and associated labor demand post-2002 (Erten and Leight, 2020). We also verify this pattern in the subsample of counties examined here: Figure 2 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.

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 local demand for manufacturing labor 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 8,851 youth in both urban and rural China, present evidence about the relevant channels, and analyze heterogeneity in the response for urban and rural youth.

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 county-level NTR gap is associated with a decline in the probability of enrollment in high school of eight 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 16%. 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 patterns 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.

The evidence of a decrease in high school enrollment in areas characterized by larger positive shocks to export-driven industry is not consistent with a hypothesized positive income effect for households, 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 observed effect on high school enrollment is meaningfully smaller in rural areas, a pattern driven by a precise null effect observed in rural areas characterized by high outmigration. These are presumably areas in which the salience of shocks to the local non-agricultural sector is limited, and accordingly educational choices are not observed to be particularly responsive to these shocks. Rural areas characterized by low outmigration, however, show an effect comparable to urban areas. Overall, there is little evidence that enhanced access to export markets is widening urban-rural gaps in human capital accumulation, and if anything, the effect of the shock seems to be to narrow these gaps as evident in Figure 1: the urban-rural gap in high school enrollment rates shrinks from 40 percentage points for cohorts born in 1982 (matriculating into high school pre-WTO) to 34 percentage points for cohorts born in 1992 (matriculating into high school post-WTO).

Finally, we 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 welfare effects are essentially null, though these results must be interpreted cautiously given some truncation in the sample (generated by youth who are still pursuing education at the point of the survey).

This is among the first papers to analyze the differential impact of trade shocks on urban and rural human capital investment in a developing country. 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 contributes to a broader literature on 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, two previous papers use aggregate prefecture-level data and present evidence that reductions in external tariffs (Li, 2018) or tariff uncertainty (Liu, 2018) 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 both urban and rural areas. However, the urban and rural analyses are performed in two separate studies and do not analyze endogenous rural-to-urban migration, rendering it challenging to directly compare the estimated effects for urban and rural youth. Our work complements these studies by focusing on the differential response for youth attending school in urban and rural areas (prior to any migration choices), and documenting the role of migration opportunities as a driving force for these differences.

Our study also contributes to the literature examining the effects of trade shocks on inequality, though this work generally focuses on wage inequality within the urban sector rather than urban-rural inequality. Previous papers analyze the effect of trade liberalization on wage inequality in Mexico (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 inequality. 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).[1] None of these papers examine the effect of trade shocks on inequality in human capital attainment; evaluating this dimension has the advantage of focusing on implications for long-term inequality as well as intergenerational transmission, given the salience of human capital accumulation for long-term income trajectories.

[1] 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).

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 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 the 1990s.[2]

First, Chinese import tariffs had already been sharply cut prior to 2001 (from a weighted average of over 45% in 1992 to approximately 13%). WTO accession entailed further cuts, but these shifts were small in magnitude (Bhattasali, Li and Martin, 2004). Similarly, the level of tariffs imposed by the U.S. and other major trading partners (the European Union, Japan, Korea, and Taiwan) were largely stable in this period. Figure

[2]Other gradual trade reforms implemented during this period included the loosening of restrictions on direct exporting, eliminated by 2002 (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).

A2 in the Appendix shows the evolution of the average weighted domestic tariff rate and the average weighted NTR rate imposed in the U.S. market. These rates are calculated using industry-level tariffs and the share of each industry in total Chinese imports (for import tariffs) or total Chinese exports (for U.S. tariffs) as reported in 1996. There is no evidence of any dramatic shifts in tariff rates at the point of China's WTO accession.[3]

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

In this paper, we preferentially focus on analysis of the discontinuous shock induced by the reduction in tariff uncertainty for both conceptual and empirical reasons. Previous evidence suggests the effect of this shock was large in both the U.S. and China, and larger than the effect of other trade policy fluctuations in this period (Pierce and Schott, 2016; Handley and Limão, 2017; Erten and Leight, 2020). 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 completion as observed in a range of cohorts who reach the age of high school 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

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

modest at 52.9% in 2001, the final year prior to the shock of interest in this analysis.[4] The limited attractiveness of post-compulsory education has been attributed to both the substantial tuition fees[5] and the steady enhancement in employment prospects for 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 channels. 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 local labor demand following the reduction in tariff uncertainty raises parental wage income. If high school education is a normal good, we would expect demand for education to rise. 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.[6] 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

[4]Source: China Statistical Yearbook, 2002.

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

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

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. According to Erten and Leight (2020), the reduction in tariff uncertainty generated a shift in employment from the agricultural sector to non-agricultural (secondary and tertiary) sectors. Therefore, it is tempting to conclude that the labor market shock associated with the export expansion is larger in rural areas, leading to a stronger educational response for rural youth. However, such an inference ignores the fact that rural workers are more likely to migrate to other provinces, making their educational decisions less responsive to local labor market shocks. 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 et al., 2010), including 8000 rural and 5000 urban households residing in ten provinces in the eastern, central and western regions of China.[7] The CHIP survey collected detailed information about demographic characteristics, labor market performance and self-reported welfare of individuals and their families.

One unique feature of the CHIP survey is that it collected basic demographic information for all biological and adopted children of heads of sample households and their spouses. The sample of children thus includes not only child household members, but

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

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.

Our empirical analysis focuses on children born between 1980 and 1991 of the sample households. We choose these specific birth year cutoffs for the following reasons. Cohorts born before 1980 reached the standard age of entry of primary school (six) prior to the passage of China's compulsory schooling law in 1986, and thus may have been less likely to initiate schooling on time (and thus reach the age of matriculation into high school on schedule). 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 attendance 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.[8] Although demographic characteristics crucial for the main analysis were available for all children, certain information was not collected for children who were 16 years or older and were no longer household members 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 welfare.

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 industry 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 positive for all industries. Throughout the empirical analysis, we use the NTR gaps for 1999.[9] The highest NTR gaps are observed for textiles, garments, other manu-

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

[9]The 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

facturing, 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 industry gaps, using weights constructed from the baseline composition of industrial employment 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 industry i with the industry's county-specific employment share.

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

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

We draw on two complementary sources of census data to construct the 1990 county-level weights: 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 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.[10] Data on MFA quotas is drawn from Khandelwal, Schott and Wei (2013), and we utilize the same methodology to construct a measure of the

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).

[10]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).

degree to which industries' quotas were binding under the MFA by calculating the import-weighted average fill rate.

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. As previously noted, this yields a sample of 179 counties in 10 provinces.

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 NTR gap NTR_{cp}.

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 3, the majority of students graduate from middle school and make decisions about matriculation into high school between the ages of 14 and 16.[11] 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.[12]

[11] 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.

[12] Based on the numbers in Figure 3, $Treat_{1986} = 0.3646$ and $Treat_{1987} = 0.7201$.

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 by counties ex ante, prior to WTO accession. The primary specification can thus be written as follows.

$$Enroll_{ihcpt} = \beta_1 Treat_t \times NTR_{cp} + \mu_t + \kappa_{cp} + \gamma_{pt} + \chi_{ihcpt} + \epsilon_{ihcpt} \quad (2)$$

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}, the latter including 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.[13] Standard errors are clustered at the county level, yielding 179 clusters.

The primary results of estimating equation (2) 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), counties characterized by an above-median concentration of employment in the secondary sector as observed in the 1990 census, and differential trends for both manufacturing-intensive and agriculture-intensive counties (in Column 5), where the latter are defined as counties characterized by an above-median concentration of employment in the primary sector as observed in the same data source.[14]

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 NTR gaps ex ante are significantly less likely to enroll. The magnitude of the coefficient is relatively consistent across specifications, ranging from -0.70 to -0.83, 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 and 8.3 percentage points, relative to a probability of enrollment for pre-shock cohorts of 49.3%, a proportional effect of between 14% and 17%.

In Panel B of the same table, we present analogous results using a simpler, binary

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

[14] 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).

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; the benchmark specification in Column (3) suggests a one standard deviation increase in NTR gap generates a proportional decline of 19% in high school enrollment.

Figure 4 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 accession to the WTO, 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.

Table A1 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 (2), 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).[15]

To sum up, the evidence seems clear that the reduction in tariff uncertainty and the associated positive shock to local labor demand 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.

[15] These results contribute to a broader literature in which recent evidence has been mixed as to whether maternal or paternal education is more strongly associated with educational attainment of children in the U.S. and Europe (Oreopoulos, 2003; Chevalier, 2004; Black, Devereux and Salvanes, 2005; Maurin and McNally, 2008), and in China, evidence suggests that both parents' education is correlated with children's educational attainment (Dong et al., 2019). Other recent evidence from Pakistan finds significant correlations between maternal variation in education and children's learning even for very low levels of education (Andrabi, Das and Khwaja, 2012), and evidence that the children of more educated mothers are protected from the adverse effects of earthquake shocks (Andrabi, Daniels and Das, 2020), but the effect of paternal education was not evaluated.

4.2 Robustness Checks

Table 3 reports a number of alternate specifications to explore the robustness of our results, building on our primary specification (2). 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 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 county-level shocks using the same 1990 employment weights employed to construct the 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 $Treat_t^{alt} \times NTR_{cp}$ is negative but significantly smaller in magnitude, and the hypothesis that the coefficients are equal can almost be rejected at conventional levels ($p = 0.102$). 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.[16]

In addition, Table A2 replicates the primary results (reported in Panel A of Table 2) using alternate strategies to construct employment weights. 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.[17] 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 a low employment share and thus low levels of employment overall.

It is evident that the results are uniformly consistent in all three panels.[18] 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

[16]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).

[17]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.

[18]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.

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 A3 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 same time, the primary results remain entirely consistent in both sign and magnitude.

4.3 Mechanisms

Our primary results suggest that the increase in the short-term opportunity costs of education for youth given the increase in local labor demand for non-agricultural employment dominates any perception of potential increased 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 local industries 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 in areas where growing 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 then construct a binary variable equal to one denoting a county characterized by above-average skill requirements, and re-estimate the primary specification, equation (2) for high-skill and low-skill counties.

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 14 percentage point decline in the probability of high school matriculation, while in high skill counties, there is only a 7.9 percentage point decline, and this difference is significant at the ten percent level ($p = 0.097$). This suggests that variation in the relative opportunity cost of education correlated with the local industrial structure is in fact informing youth decision-making.

Second, we evaluate a hypothesized alternate channel in which an expansion in local 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 A1 suggest that the effect of the shock is not significantly different for first-born versus higher parity children or boys versus girls, while in general first born and female children would be expected to be more burdened by childcare responsibilities, and data directly reported by the sample households suggest that care-taking responsibilities by siblings are minimal.[19] 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 ($p = 0.285$).[20] 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 A4 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.

[19]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.

[20]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.

5 Differential Responses in Urban and Rural Areas

5.1 Education

As previously noted, evidence about the differential effect of trade policy shocks in urban and rural areas is extremely limited, both for China and for the broader developing world; existing evidence to date has largely focused on the effects of trade policy shocks on inequality across subsectors, firms or skill levels within the industrial sector. Directly analyzing the effects of trade shocks on urban-rural inequality in income or similar measures is extremely challenging given the absence of data sources that enable the construction of comparable income measures for urban, largely wage-earning households and rural households primarily engaged in independent agricultural production. However, analyzing the differential effect on educational outcomes enables us to generate an informative analysis about the effect of trade shocks on a uniformly measured outcome.

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.[21] 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 reports the results, following the same five specifications reported in Table 2. It is evident that the estimated coefficient for urban youth is consistently large and statistically significant, suggesting that a one standard deviation increase in the NTR gap leads to a seven percentage point decline in the probability of matriculation into high school (as reported in Column (3), our preferred specification), relative to a mean for pre-shock urban cohorts of 85%. By contrast, the estimated coefficients for rural youth are smaller in magnitude and statistically insignificant, though there is some variation in magnitude. The confidence intervals do not allow us to reject the hypothesis that the effects are equal across the rural and urban samples, however, as reported at the base of the table.

We then further probe this pattern by examining potential channels for the limited response in rural areas, focusing on two channels: heterogeneity with respect to village-level outmigration networks, and heterogeneity with respect to incentives linked to the hukou. First, 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

[21] 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.

Wang, 2005; Chen, Jin and Yue, 2011; Mu and de Brauw, 2015; Munshi, 2003); youth who benefit from a strong local network presumably have a higher probability of targeting employment opportunities in other provinces or cities, and thus will be less responsive to shocks to the local non-agricultural sector. This would be one possible channel for the limited effect observed in rural areas. Second, rural youth may be motivated to invest in education partly because of the opportunity to switch their hukou from rural to urban, an opportunity available upon enrollment in occupational high school and college (Zhao, 1997; Wu and Treiman, 2004; Pan, 2017). If hukou-switching is the primary benefit of education for rural youth, they may similarly be relatively unresponsive to fluctuations in the local employment-linked returns to high school education.

To evaluate these hypotheses, we first use data reported in a village-level CHIP survey on the percentage of the local labor force who have worked in another province as temporary migrant workers in 2007 and compare the effect in rural low-migration villages (defined as those below a threshold of 40%) and rural high-migration villages. The results are reported in Columns (1) and (2) in Table 7. It is evident that the effect is larger and statistically significant in the sample of respondents in rural low-migration villages, and even larger in magnitude than the estimated coefficient for the urban sample.[22] By contrast, the estimated effect in rural high-migration villages is positive, and the difference when comparing across the two subsamples is statistically significant ($p = 0.020$).[23] The effect on educational attainment in low-migration rural villages is particularly substantial given the low levels of high school matriculation observed on average in rural China: for pre-shock cohorts, the average probability of matriculation is only 36% for rural youth (roughly consistent in low- and high-migration villages), compared to 86% for urban youth as noted above.

To analyze the second channel, we use province-level variation in the timing of the replacement of the rural-urban dual hukou system with a uniform identity system that eliminated the need for rural youth to switch their hukou to urban status. Of the ten provinces in our sample, five had implemented these reforms prior to the CHIP survey (meaning that the prospect of a hukou switch is less important to the treated cohorts' matriculation decisions), and four had not implemented these policies prior to the survey. In the latter provinces, the treated cohorts in this analysis would have faced incentives to potentially pursue high school education as a means to update their hukou. We observe in Columns (3) and (4) of the same table that there is no meaningful heterogeneity comparing across these two sets of provinces. While this evidence may be considered

[22]The difference in the estimated coefficients when comparing across the rural low-migration and the urban sample is not significant.

[23]This pattern is also consistent when we use alternate thresholds to identify low migration vis-a-vis high migration villages.

tentative given potentially limited power, it suggests that the limited response to the NTR shock by rural youth does not reflect the salience of education as a pathway toward urban residence.

There are also other channels that would be consistent with the more minimal response by rural youth to the NTR shock. High school enrollment rates are much lower in rural areas, suggesting that youth enrolled in high school may have a strong preference for further education irrespective of enrollment constraints (e.g., high tuition and discretionary costs, and geographic inaccessibility of high schools). If this preference or an associated intrinsic motivation for more education dominates, then relative variation in non-agricultural opportunities may have a limited effect. In addition, the opportunities created by export-led expansion are disproportionately concentrated in non-agricultural production, and these opportunities in turn are disproportionately concentrated in urban areas within counties. As the majority of rural workers obtain employment opportunities through relatives and friends (76% in the CHIP sample), they may have limited access to newly created positions in urban areas.

However, both of these above phenomena are presumably equally salient in rural low-migration areas, where the response to the NTR shock is in fact larger than the response in urban areas. Moreover, even if relevant opportunities in export-driven subsectors are more rare in rural areas, if the earnings premium is larger for these opportunities vis-a-vis the plausible outside options (engagement in agriculture), then the opportunity cost of continued education may still be quite large. This is suggested by the very large response to local NTR shocks observed in rural low-migration areas, while for high-migration areas, the local NTR gap may simply be an un-informative proxy for the fluctuations in the opportunity cost of education.

To sum up, in general the available evidence is consistent with the hypothesis that the effect of the NTR shock is meaningfully attenuated for rural youth, and this appears to reflect the fact that youth in high-migration areas are not particularly responsive to local shocks to non-agricultural labor demand. There is little evidence that the role of education as a path toward an urban hukou, an intrinsic preference for education or limited access to newly created non-agricultural positions shape rural youth's response.

5.2 Labor Market Outcomes

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 welfare 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.

We then re-estimate the primary specification, equation (2), to analyze the medium-term effects of the NTR shock separately for urban and rural youth, and the results are reported in Table 8. In Panel A, we observe evidence of substantial welfare effects for urban youth. Youth who were affected by the NTR shock and thus failed to matriculate into high school are less likely to report non-agricultural employment, and this decline is concentrated in high-skilled occupations. The magnitude of these effects is sizeable: a one standard deviation increase in the NTR gap is associated with a 13 percentage point decline in the probability of non-agricultural employment, relative to a mean of 78%, and a 11 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.

In Panel B, by contrast, it is evident that there is no significant effect on medium-term labor market for rural youth affected by the NTR shock; the coefficient on employment is negative, but far from significant. We also further verify in Table A4 in the Appendix that these null effects is observed consistently in both low-migration and high-migration villages, despite the evidence previously presented that the effect on educational attainment following the NTR shock is large and significant in the former subsample.

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 educational 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 their 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, leading them to forgo post-compulsory education for work.

By contrast, in rural China even youth who did show evidence of a decline in the probability of high school matriculation (youth in low-migration villages) show no significant adverse welfare 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 meaningfully lower in rural vis-a-vis urban China, and thus the associated penalty for non-high school graduates is minimal (Zhigang and Shunfeng, 2006).

This evidence is broadly consistent with that presented in Atkin (2016) for Mexico, though in Mexico 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 growth 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 in general larger in urban areas, and the welfare consequences of the decline in matriculation seem to be restricted to urban areas. Rural areas, by contrast, show an attenuated effect on high school matriculation (concentrated in low-migration areas) but no substantial welfare effects.

6 Conclusion

This paper presents new evidence about the effect of positive export shocks on human capital attainment in China, using micro-level data on a sample of approximately 9,000 youth in urban and rural areas. 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 16% decline in the probability of enrollment in high school; this effect is highly salient in urban areas and relatively attenuated in rural areas, though rural youth living in communities characterized by a low intensity of migration do show a large response.

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 trade shocks on urban-rural inequality. In China, characterized by rapidly growing urban-rural gaps in both income and human capital attainment, understanding the effect of trade shocks on inequality is particularly important. The evidence presented here suggests that these shocks are not widening inequality gaps, but if anything may be narrowing these gaps. Future research may explore further the implications of export-driven growth for urban-rural divergence along other dimensions.

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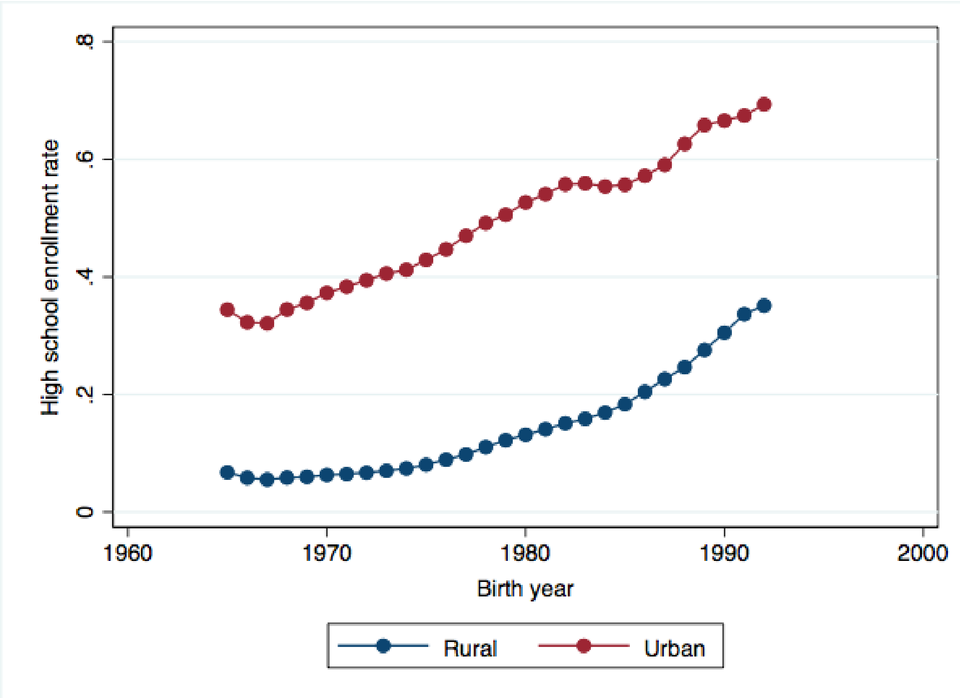
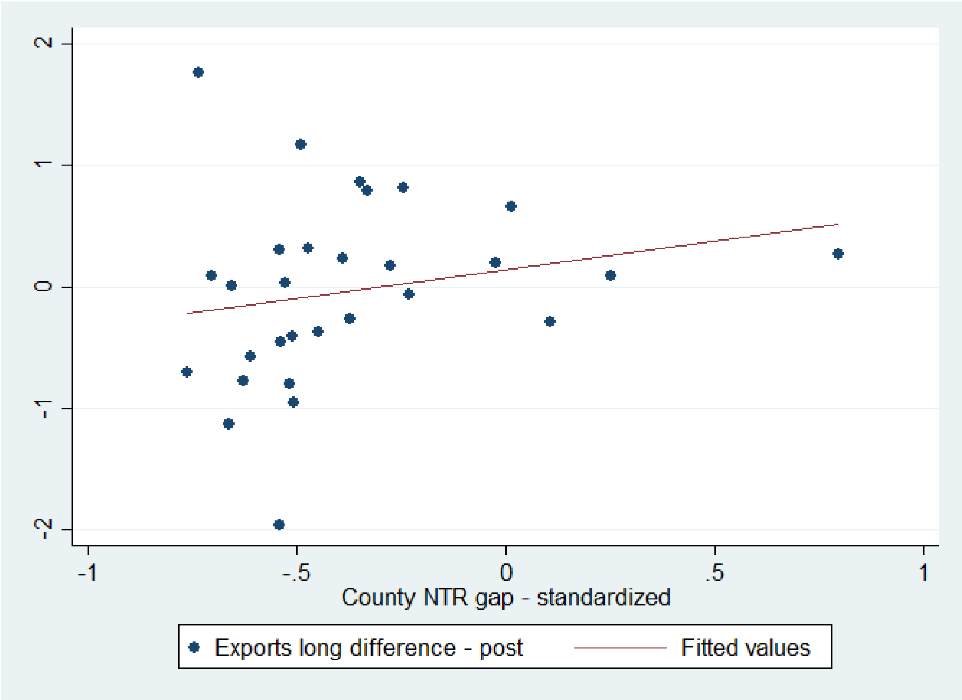
Figure 1: Trends in high school attainment over time



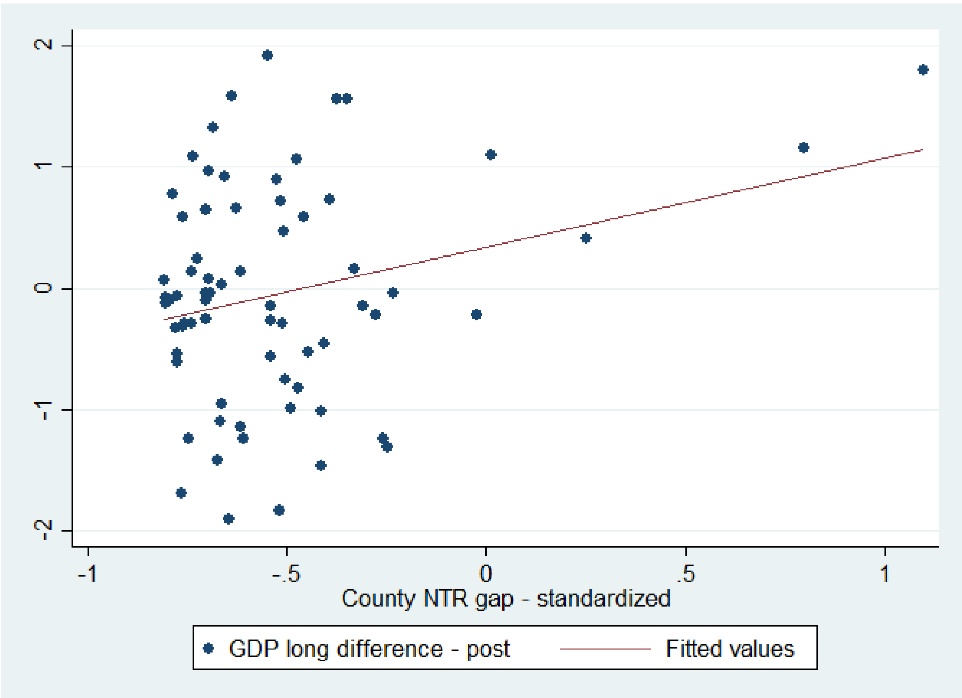
Notes: The figure uses data from the 2010 Population Census of China.

Figure 2: Long-difference post WTO: Local exports and GDP

(a) Exports

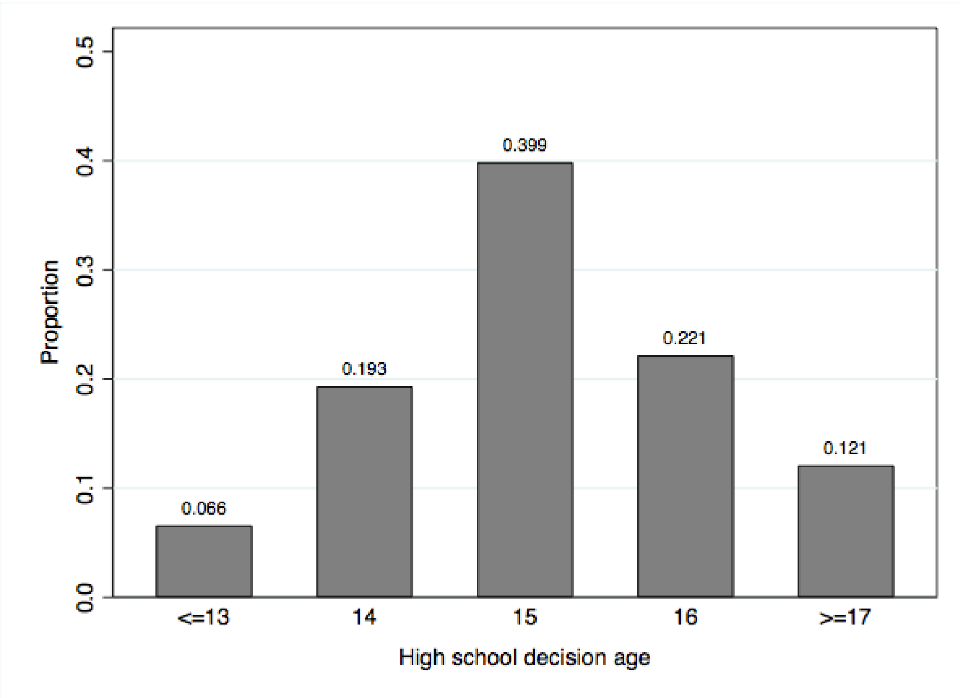


(b) GDP



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.

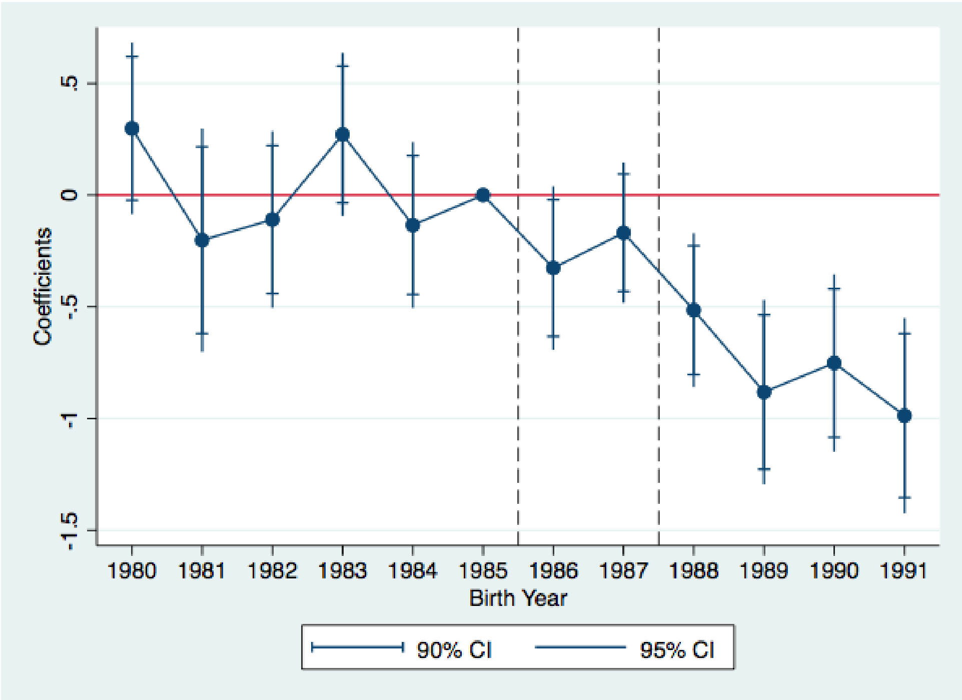
Figure 3: Distribution of high school decision age



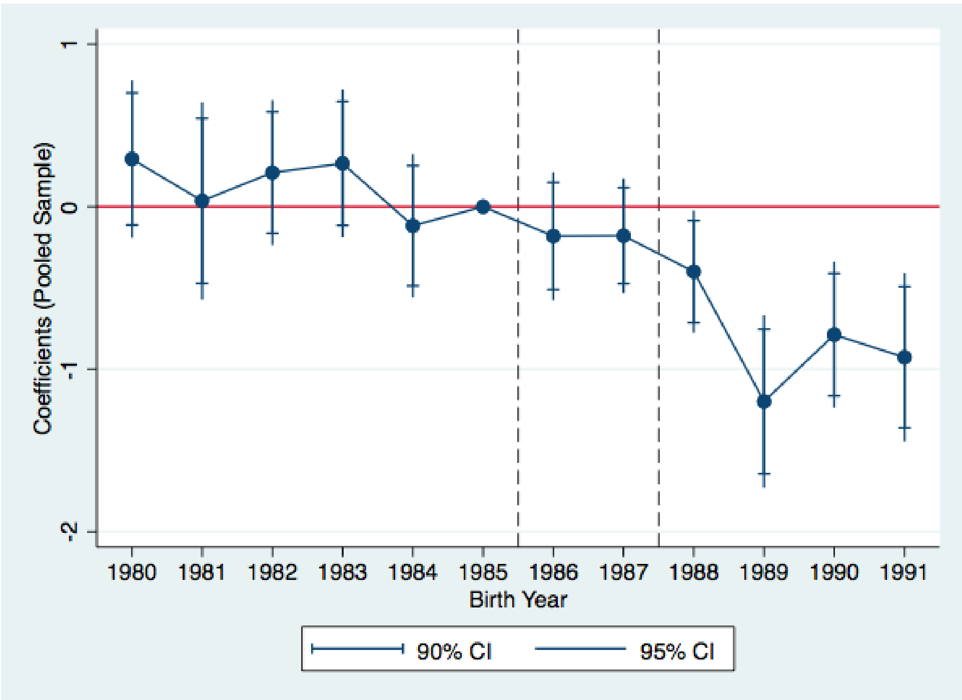
Notes: The figure uses data from the China Health and Nutrition Survey.

Figure 4: 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 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 attendance 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 and welfare 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 Attendance				
Panel A: Continuous treatment variable					
Treatment Intensity	-0.700***	-0.758***	-0.833***	-0.789***	-0.734***
X NTR gap	(0.0908)	(0.0918)	(0.111)	(0.135)	(0.145)
Observations	8,851	8,851	8,851	8,851	8,851
Panel B: Binary indicator for treatment					
Treatment Binary	-0.760***	-0.814***	-0.895***	-0.867***	-0.784***
X NTR gap	(0.0933)	(0.0955)	(0.114)	(0.139)	(0.150)
Observations	7,127	7,127	7,127	7,127	7,127
Controls	County + birth year FE	+ Ind. level	+ Prov-year FE	+ Secondary trend	+ Primary trend
Mean dep. var. (pre-shock cohorts)	.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 county-level NTR gap; in Panel B, a binary measure of treatment (equal to one for cohorts matriculating post-2002) is employed, 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 3: Robustness checks

	(1)	(2)	(3)	(4)	(5)
	High School Attendance			Height	Edu. Perf.
Treatment Int.	-0.844***	-0.721***	-1.259***	-0.415	0.493
X NTR gap	(0.185)	(0.184)	(0.343)	(2.506)	(0.312)
Treatment Int. (alt.)		-0.129			
X NTR gap		(0.195)			
Spec.	Additional Controls	Alt. treatment	Household FE	Placebo	Placebo
Observations	8,851	8,851	8,851	7,312	6,884
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 NTR gap. In Column (2), an additional treatment intensity variable coding cohorts matriculating post-2000 as exposed is added; in Column (2), household fixed effects are employed. In Columns (3) and (4), height and educational performance are regressed on the interaction of treatment exposure and the county-level 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 Attendance			
Treatment intensity	-1.312***	-0.792***	-0.935***	-0.667***
X NTR gap	(0.293)	(0.130)	(0.133)	(0.240)
Sample	Low-skill	High-skill	No emp. change	Emp. change
Cross-spec. tests		0.097		0.267
Observations	5,031	3,820	6,932	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 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 X NTR gap	2.518***	0.480**	0.596***
	(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 of the post-WTO shocks 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

	(1)	(2)	(3)	(4)	(5)
	High School Attendance				
Panel A: Urban youth					
Treatment Intensity	-0.565***	-0.593***	-0.646***	-0.586**	-0.441*
X NTR gap	(0.188)	(0.180)	(0.223)	(0.228)	(0.232)
Observations	2,216	2,216	2,216	2,216	2,216
Mean dep. var. (pre-shock cohorts)	.853				
Panel B: Rural youth					
Treatment Intensity	0.0862	-0.0977	-0.407	-0.318	-0.115
X NTR gap	(0.412)	(0.396)	(0.471)	(0.495)	(0.576)
Cross-spec. tests	.151	.254	.638	.609	.567
Observations	6,625	6,625	6,625	6,625	6,625
Controls	County + birth year FE	+ Ind. level	+ Prov-year FE	+ Secondary trend	+ Primary trend
Mean dep. var. (pre-shock cohorts)	.364				

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 NTR gap. The sample in Panel A is restricted to urban youth and in Panel B is restricted to rural youth, and the cross-specification tests report p-values corresponding to the tests of equality of the estimated coefficients comparing across parallel columns in Panel A and Panel B. 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 7: Rural effects: unpacking channels

	(1)	(2)	(3)	(4)
	High School Attendance			
Treatment Intensity	-1.058**	1.141	-0.276	-0.692
X NTR gap	(0.516)	(0.779)	(0.471)	(0.803)
Sample	Low mig.	High mig.	Uniform hukou	Rural/urban hukou
Cross-spec. tests		0.020		0.648
Observations	4,257	2,368	3,339	3,286
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 NTR gap. The sample for the table is restricted to rural youth and further restricted as specified in each column; the cross-specification tests report tests of equality across complementary subsamples (e.g., low-migration and high-migration rural villages). 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 8: Labor market effects

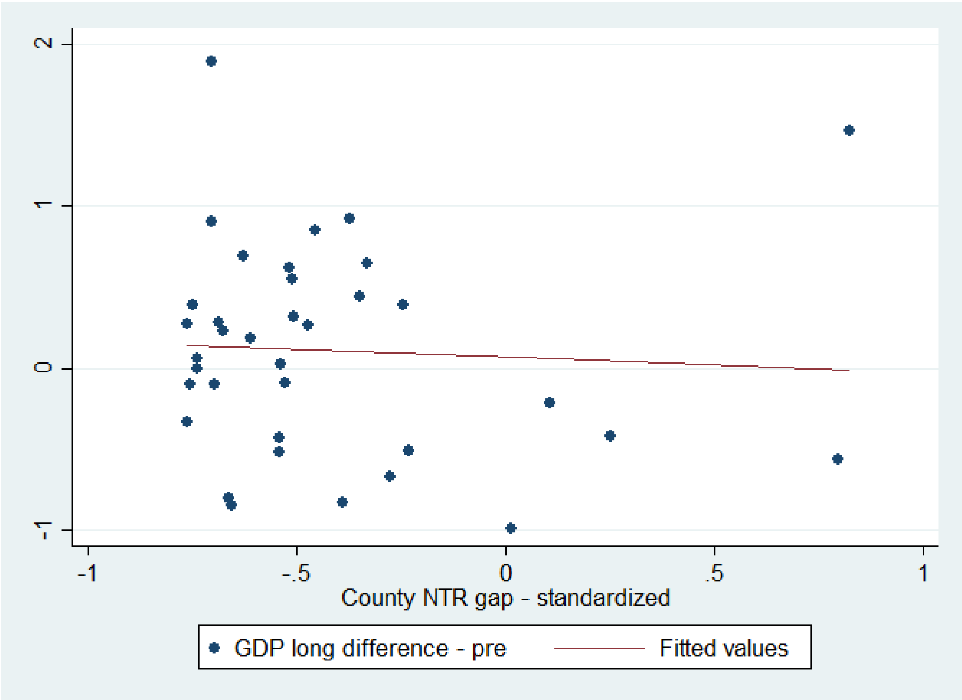
	(1) Non-ag. employment	(2) High-skilled occupation	(3) Low-skilled occupation	(4) Log wage (month)	(5) Work hours (week)
Panel A: Urban youth					
Treatment Int.	-1.264**	-1.056**	-0.225	-0.176	-4.817
X NTR gap	(0.527)	(0.509)	(0.627)	(0.754)	(19.20)
Observations	1,401	1,401	1,401	804	809
Mean dep. var. (pre-shock cohorts)	0.78	0.44	0.36	7.37	44.33
Panel B: Rural youth					
Treatment Int.	-0.0936	-0.477	0.686	-0.960	15.84
X NTR gap	(0.752)	(0.767)	(1.136)	(1.128)	(17.36)
Observations	5,431	5,431	5,431	2,926	2,924
Controls	County, birth year and province-year FE + ind. controls				
Mean dep. var. (pre-shock cohorts)	0.74	0.15	0.55	7.21	53.83

Notes: This table presents the results from regressing variables capturing labor market outcomes (a binary variable for non-agricultural employment, binary variables for high-skilled and low-skilled employment, and continuous measures of wages and 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 NTR gap. Note wages and hours are only reported for individuals reporting non-agricultural employment. The sample is restricted to urban youth in Panel A and rural youth in Panel B. 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

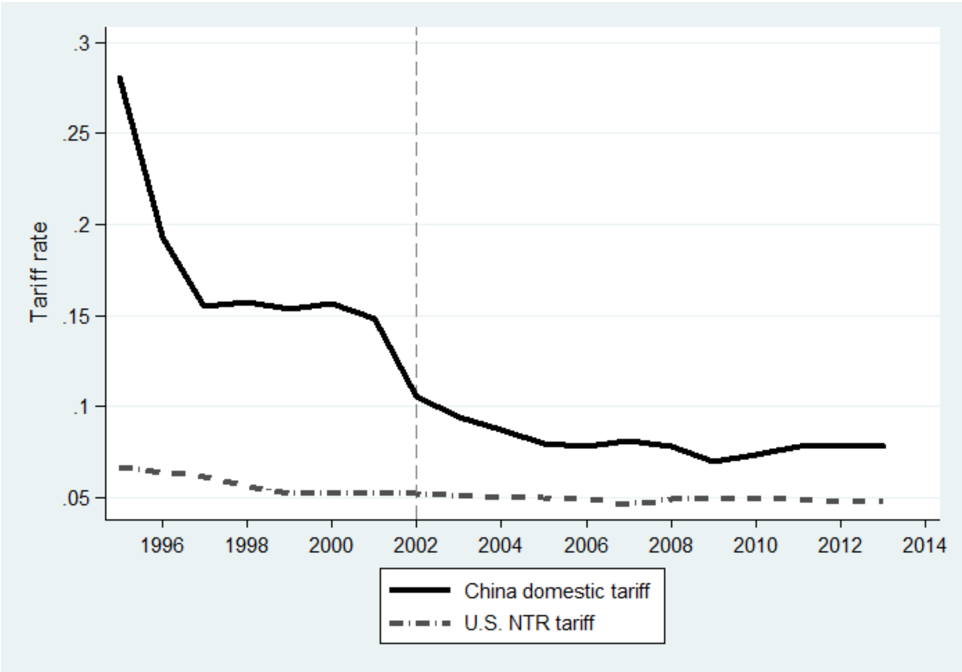
A1 Figures and Tables

Figure A1: Long-difference pre WTO: Local GDP



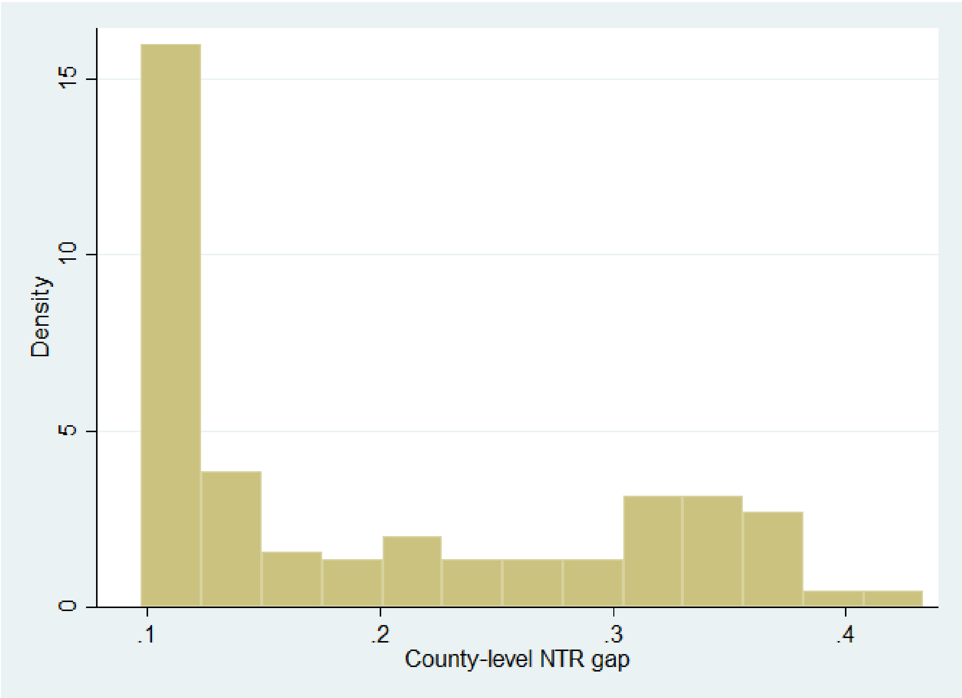
Notes: The graph shows a scatter plot capturing the correlation between the county-level long-difference in log exports and log GDP during the pre-period before WTO accession (1996–2001) and the county-level NTR gap. All variables are standardized to have mean zero and standard deviation one.

Figure A2: 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.

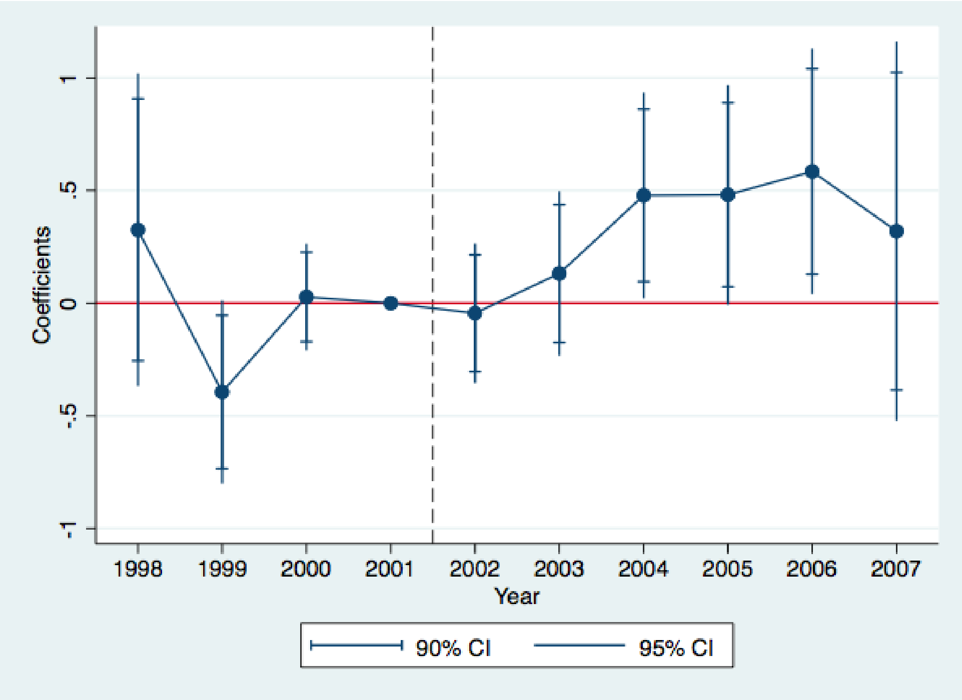
Figure A3: County-level NTR gap



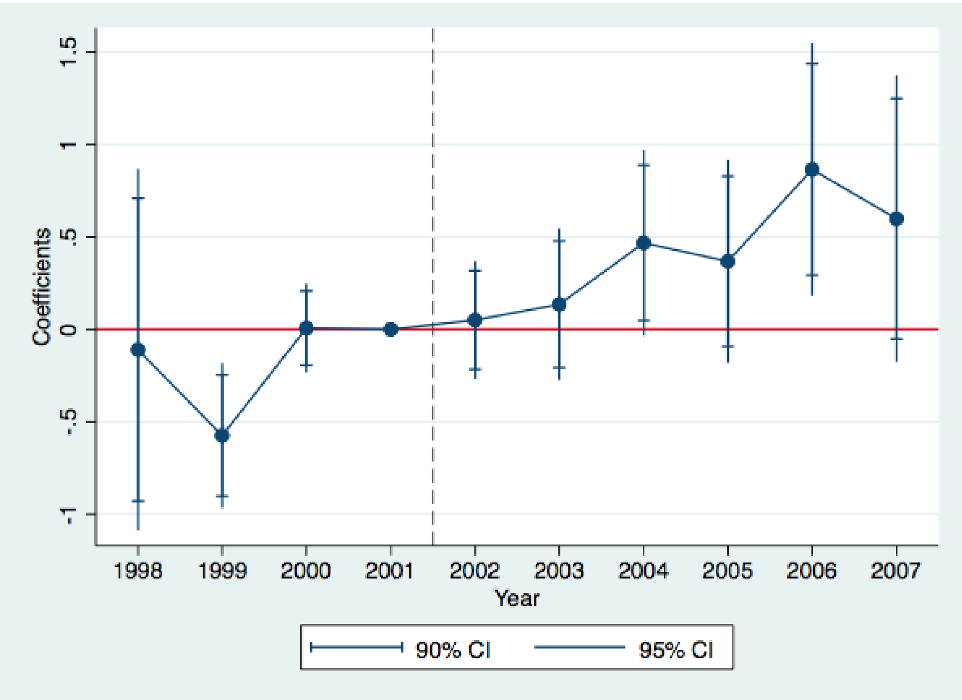
Notes: The graph shows a histogram of the county-level NTR gap for the 179 counties in the CHIP sample.

Figure A4: Effects of trade shock on 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: Heterogeneous effects: individual and parental characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	High School Attendance					
Panel A: Individual characteristics						
Treatment Intensity	-0.870***	-0.859***	-0.524***	-0.976***	-1.039*	-0.998*
X NTR gap	(0.158)	(0.166)	(0.193)	(0.332)	(0.546)	(0.529)
Sample	Female	Male	No sibling	Any sibling	Firstborn	Non-firstborn
Cross-spec. tests		0.955		0.226		0.942
Observations	4,186	4,665	2,396	6,455	2,755	3,700
Panel B: Parental characteristics						
Treatment Intensity	-0.758***	-0.691***	-0.930***	-0.153		
X NTR gap	(0.170)	(0.158)	(0.190)	(0.273)		
Sample	Fathers: no high school	Fathers: high school	Mothers: no high school	Mothers: high school		
Cross-spec. tests		.750		.014		
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 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 A2: Primary results using alternate trade shocks

	(1)	(2)	(3)	(4)	(5)
	High School Attendance				
Panel A: Employment weights incorporating non-tradables					
Treatment Intensity	-0.788***	-0.922***	-1.014***	-0.768**	-0.502
	(0.232)	(0.224)	(0.282)	(0.304)	(0.324)
Observations	8,851	8,851	8,851	8,851	8,851
Panel B: Employment weights using 2000 census data					
Treatment Intensity	-0.564***	-0.634***	-0.817***	-0.744***	-0.660***
	(0.119)	(0.116)	(0.107)	(0.126)	(0.161)
Observations	9,004	9,004	9,004	9,004	9,004
Panel C: Employment weights using 1990 census data in aggregate form					
Treatment Intensity	-0.759***	-0.832***	-0.947***	-0.877***	-0.844***
X NTR gap	(0.144)	(0.141)	(0.167)	(0.223)	(0.221)
Observations	9,004	9,004	9,004	9,004	9,004
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 NTR gap. In Panel A, the NTR gap is calculated using employment weights constructed incorporating an estimate of total employment (including non-tradables); in Panel B, the NTR gap is calculated using employment weights constructed from 2000 census data; in Panel C, the 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 A3: Primary results using alternate cohort windows

	(1)	(2)	(3)	(4)	(5)
	High School Attendance				
Panel A: Birth cohorts born between 1979 and 1992					
Treatment Intensity	-0.754***	-0.812***	-0.908***	-0.830***	-0.816***
X NTR gap	(0.0986)	(0.0999)	(0.121)	(0.142)	(0.145)
Observations	9,642	9,642	9,642	9,642	9,642
Panel B: Birth cohorts born between 1978 and 1993					
Treatment Intensity	-0.743***	-0.794***	-0.901***	-0.821***	-0.834***
	(0.103)	(0.104)	(0.124)	(0.145)	(0.147)
Observations	10,318	10,318	10,318	10,318	10,318
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 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 A4: Labor market effects: rural subsamples

	(1) Non-ag. employment	(2) High-skilled occupation	(3) Low-skilled occupation	(4) Log wage (month)	(5) Work hours (week)
Panel A: Rural youth in low-migration villages					
Treatment Int.	0.258	-0.562	0.490	-0.221	2.814
X NTR gap	(0.936)	(0.630)	(0.995)	(1.378)	(34.06)
Observations	3,425	3,425	3,425	1,786	1,782
Panel B: Rural youth in high-migration villages					
Treatment Int.	-1.120	1.479	-1.640	-3.033	65.35
X NTR gap	(1.388)	(1.678)	(1.770)	(2.189)	(48.21)
Observations	2,006	2,006	2,006	1,140	1,142
Controls	County, birth year and province-year FE + ind. controls				

Notes: This table presents the results from regressing variables capturing labor market outcomes 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 NTR gap. The variables are defined in Table 8; in Panel A, the sample is restricted to rural youth in low-migration villages, and in Panel B, the sample is restricted to rural youth in high-migration villages. 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