Does Enrollment Lead to Completion? The Link Between Increased High School Persistence and High School Graduation in Response to Trade Exposure*

Ramiro Burga
University of Virginia

Sarah Turner
University of Virginia & NBER

Janury 2020

Abstract

While adverse local labor market shocks such as those induced by increased exposure of local industries to trade have unambiguously negative effects on workers' employment prospects, the impact on high school enrollment and completion is ambiguous. Incentives to stay in school increase when employment prospects are weak; yet, public resources for local schools may also shrink, with cutbacks negatively impacting high school degree attainment. How large are the enrollment effects at the high school level? And, does increased time in school translate to high school degree attainment? This paper demonstrates that, while high school enrollment rates increase significantly, high school degree attainment does not show commensurate growth. Diploma counts relative to the population indicate only a modest increase, while the share of young adults with a high school degree in a community does not change. The correspondence between high school enrollment and diploma receipt of young adults reflects important measurement issues, as "outmigration" of young adults and changes in the timing of degree receipt may complicate measurement. Moreover, the negative impact of trade exposure on secondary school resources per student operates in the opposite direction of enrollment demand, likely attenuating gains in attainment and student achievement.

*For extraordinary research assistance, we are grateful to Dalton Ruh and Nathan Ruby. Other researchers including David Autor, David Dorn, Gordon Hanson, Andrew Greenland and John Lopresti have generously shared data or placed key empirical resources in the public domain. The authors are grateful for constructive exchanges and comments with John Bound, David Autor, Chris Foote, Karim Chalak, Jonathan Gruber, Bill Johnson, Andrew Greenland, and John Lopresti.

E-mail addresses: set5h@virginia.edu (S. Turner), rab9ze@virginia.edu (R. Burga)

Introduction

There is a growing body of evidence pointing to the transmission of local labor market shocks to other spheres including educational attainment, marital status and take up social services program. The widely-studied impacts of increased import exposure from growth of trade with China include direct effects on employment and earnings (Autor, Dorn and Hanson 2013), along with impacts on family formation (Autor, Dorn and Hanson 2019), internal migration (Greenland, Lopresti and McHenry 2019), educational attainment (Greenland and Lopresti 2016; Tuhkuri 2018), and funding for local government expenditures (Feler and Senses 2017). Taken together, this body of research suggests some puzzles as negative impacts on earnings, employment and family structure are juxtaposed against some quite marked reported increases in secondary educational participation. The focus of this paper is on the measurement of the impact of trade exposure on secondary school outcomes.

In the education market, declines in local labor market opportunities can be expected to increase demand for secondary education and reduce high school dropout behavior, as employment opportunities for low-skill workers decline. Yet, declines in local economic conditions also adversely affect the capacity of school districts to fund local schools, with resource effects having potentially adverse effects on attainment. Declines in local labor market conditions may also generate related behavioral responses such as migration among late teens and shifts in the type of secondary education or credentials (from private to public or from GED to regular diploma) which may affect the measurement of secondary enrollment and attainment effects. The existing research evidence does not align these different factors and, taken at face value, suggests that adverse local labor market events may generate gains in high school degree attainment as large as those produced by large and sustained increases in resources per student in the elementary and secondary school years (Jackson, Johnson and Persico 2016).

The most striking published finding that ties trade exposure to secondary attainment is the evidence presented in a 2016 paper by Greenland and Lopresti which shows that an increase of one standard deviation in import exposure from China leads to an increase in high school completion of at least 3.65 percentage points (about 5.6%). Other evidence such as Turner (2019) and Tuhkuri (2018) shows a reduction in dropout behavior among those ages 16-19 and increased enrollment at the secondary level, but magnitudes are much more modest. Local fiscal effects move in the opposite direction: evidence presented by Feler and Senses (2017) and in this work shows how expenditures on local public schools declined in response to increases in import exposure. The previously reported large gains in high school degree attainment which are accompanied by reductions in resources per student are hard to reconcile with broad-based evidence in the economics of education on enrollment demand elasticities and the impact of resources on secondary school attainment.

The magnitude of the impact of increased trade exposure (and local labor market shocks more generally) on educational attainment has a significant impact on the more general analysis of place-based policies and the plight of distressed labor markets (Austin, Glaeser, and Summers 2018). On the one hand, if job-loss for low-skilled workers is followed by substantial gains in educational attainment for youth, some of the adverse consequences of local labor market shocks would dissipate and intergenerational effects would likely be modest. If, on the other hand, educational attainment gains are anemic, then the economic rationale for place-based polices that provide increased support for local schools in the wake of labor market downturns is much stronger.

Evidence presented in this paper demonstrates that trade exposure increased enrollment demand at the high school level (particularly the 12th grade) while changes in high school degree completion were much more modest. Precise specification of outcomes is important in this context: counts of diplomas awarded by public schools from administrative data are not conceptually identical to age-specific measures of the population share with a high school degree. The combination of greater time to high school degree completion and out-migration of teens in response to increases in import exposure are two factors that contribute to the divergence between diploma outcomes (which are positive but small in magnitude) and the share of young people in their late teens with high school degrees (which are weakly negative). Moreover, while the identifying variation used by Autor, Dorn and Hanson (2013) and related papers has been broadly discussed by others (e.g. Goldsmith-Pinkham, Sorkin, and Swift 2019, Adao, Kolesár and Morales 2019), the choice of specification – particularly, the decision to include location fixed effects – impacts the extent to which the IV specification is subject to a weak instrument critique.

Our results examining effects of increased trade exposure on high school enrollment and completion focus on the educational production process and the correspondence between time in school (enrollment) and degree completion. This mapping is likely affected directly by individual decisions to persist along with school resources which impact attainment in the context of educational production function models. The reconciliation of our results on high school graduation and other educational outcomes with those in other published work yields a number of insights on econometric methods and measurement of educational outcomes. First, the basic Greenland and Lopresti (2016) results which report very large impacts of trade exposure on high school graduation suffer from specification problems resulting in biased estimates. While different pre-trends among local areas are plausible, the inclusion of local area fixed effects in a framework with two intervals of differences is an inefficient fix and introduces additional problems, as the remaining identifying variation is sufficiently limited to generate a weak instrument problem. Secondly, enrollment rates and high school graduation rates differ in temporal alignment, while the awarding of high school degrees in a geography is distinguished from persistence of high school graduates. Considering these measurement issues allows for the clarification of the differing impacts of trade exposure on enrollment and attainment. Local outmigration and changes in the age at high school degree receipt are two factors contributing to the difference. Finally, the evidence points to clear negative impacts of increased trade exposure on secondary school resources per student, with these effects magnified in localities with corresponding state-level trade effects. This result suggests that school funding effects work in the opposite direction of enrollment demand to attenuate gains in high school degree attainment in response to weak economic prospects.

The first section synthesizes the existing evidence related to the impact of local labor market shocks on secondary school enrollment and attainment in the context of a brief discussion of the theoretical framework governing these enrollment responses. The second section outlines the data sources that are used to measure educational attainment and the more general measurement challenge associated with tracking high school attainment at the level of local labor markets. The third section outlines the estimation strategy. The fourth section presents the results and illustrates the alignment among enrollment and high school degree attainment measures from different sources.

1 Evidence of the Impact of Trade Exposure on Labor Market and Educational Outcomes

The benefits and burdens of structural changes in U.S. economic activity have not been felt evenly across U.S. labor markets in the last four decades. Those areas in which manufacturing and mining predominated forty years ago have faced the greatest job losses induced by trade and technological change (Autor, Dorn and Hanson 2013; Austin, Glaeser, and Summers 2018).

Focusing on the impacts of plausibly unanticipated changes in the pattern of trade, Autor, Dorn and Hanson (2013) find substantial reductions in manufacturing employment in areas particularly affected by these trade shocks, with the impacts acute among those with a high school degree or less education. And, in many cases, there is evidence that these shocks have had long-lasting impacts on local labor markets rather than dissipating within a few years (Autor, Dorn, and Hanson 2019). The labor market impacts of increased trade exposure have differentially impacted workers without a college degree.[1] Compensatory increases in educational attainment among youth in affected localities could dissipate the persistence of these effects.

1.a Framework to Understand Local Labor Market Shocks and Educational Investments

From a theoretical perspective, declines in job opportunities associated with losses in manufacturing employment and the persistently high relative premium to college-level employment would be expected to put upward pressure on educational attainment. Such effects would be expected to be magnified when regional job loss is caused by structural shifts – the permanent loss of jobs – rather than short-turn cyclical fluctuations. In addition, structural changes would be expected to impact the educational trajectories of youth, beyond the more immediate choices made by young adults deciding whether to persist in (or return to) college based on immediate job opportunities.

But, the realization of educational gains as a result of changes in labor demand assumes that neither credit constraints nor limits in the supply-side of schooling opportunities impede attainment. Because public schools at the K-12 level rely on both local property taxes and state revenues, a basic concern is that declines in regional economic prosperity may place downward pressure on educational resources at the same time that an increased share of young people are seeking to change their prospects by persisting in school.[2] To fix ideas, suppose public secondary school enrollment increased by 10 percent as students stayed in high school longer rather than dropping out after reaching the age at which attendance was no longer compulsory. If the allocation of school funding (and

[1] For example, Autor et all (2013) show that $1,000 import exposure shock reduces the employment to population rate of college adults by 0.42 percentage points and of noncollege adults by 1.11 percentage points.

[2] The vast majority of education in the U.S. is provided by public institutions funded by state and local governments. Nearly 90% of students at the K-12 level are enrolled in public schools while 74% of students at degree-granting colleges and universities are at public institutions (Snyder, De Brey and Dillow 2018, Table 105.30).

resources to different grade levels) was fixed, one would expect class sizes to increase by 10 percent. This decline in resources per student would be compounded if school funding contracted as the number of teachers might well fall, even as student demand increased.

With about 45% of K-12 funding coming from the tax base of localities, it is natural to suggest that changes in labor demand that adversely affect local economies will concurrently impact the local tax base, effectively reducing resources for education, and, to the extent that enrollment demand is rising while resources are falling, effects are likely to be magnified. If local labor market shocks are weakly correlated with the overall level of state prosperity, state finance may serve as a compensatory role – essentially "filling in" for lost local tax revenues. However, when state and local economic circumstances are highly correlated, district resources can be expected to decline.

Because local labor market shocks affect both demand for education and the funding of public institutions (the supply), there is theoretical ambiguity regarding the size and the magnitude of impacts. In turn, empirical estimates of the impact of trade exposure or other labor market changes on educational attainment necessarily represent the net impacts of these two countervailing forces.

1.b Empirical studies

How mass layoffs, trade shocks and the more general place-based decline in employment opportunities impact educational investments of youth has received some attention in the education and public finance literature.[3] Researchers have examined enrollment and attainment effects to both positive and negative local labor market shocks, with the former hypothesized to decrease enrollment demand and the latter expected to increase enrollment demand. Perhaps the earliest paper to explicitly examine the link between

[3]The focus of this analysis is not on the direct effect through parental layoffs, but rather the more broadly defined place-based impact of changes in local economic opportunities on educational outcomes. Parental job loss has been shown to have unambiguously negative effects on children's education attainment in a number of dimensions including grade repetition (DeLeire and Kalil 2002), lower GPA (Rege, Telle and Votruba 2011), and lower collegiate attainment (Oreopolous, Page and Stevens 2008).

local labor market conditions and youth enrollment is the work of Black, McKinnish and Sanders (2005) who examine how the rise and fall of coal prices in communities in which coal mining is the primary industry affect the high school persistence of youth. Comparing high school enrollment rates in counties with and without coal reserves in the 1970s and 1980s, these authors a 10% increase in the earnings of low-skilled workers could decrease high school enrollment rates by as much as 5-7%. More recently, Cascio and Narayan (2015) examine the impact of fracking on youth enrollment and find a somewhat smaller negative effect of the fracking boom on youth dropout rates with estimates imply that, absent fracking, the teen male dropout rate would have been 1 percentage point lower over 2011-15 in the average labor market with shale reserves. Zuo, Schieffer and Buck (2019) follow a somewhat different identification strategy in their analysis of enrollment effects of the shale boom and find that intensive drilling decreased grade 11 and 12 enrollment, with the largest effects in states with low compulsory attendance ages.

More recently, authors have built on the variation in studies examining the wage and employment impacts of trade shocks (Autor, Dorn and Hanson 2013) and mass layoffs (Foote, Grosz and Stevens 2019) to examine educational outcomes like high school attainment, college enrollment and college degree attainment. Two studies examine variation in high school attainment in response to the effect of import penetration on manufacturing. Greenland and Lopresti (2016) find quite large (approximately 4 percentage points per $1,000 increase in import penetration) effects on high school diploma receipt measured as diplomas awarded by public schools relative to population. Using the somewhat longer interval from 1990 to 2010, Tuhkuri (2018) employs variation in changes in import exposure to examine how changes in manufacturing jobs impact high school drop-out rates and college access, measured as the cohort average of college attendance of children with parents at the 25th percentile in the national distribution. Tuhkuri (2018) finds smaller effects on dropout rates among those ages 16-19 (0.31 percentage points per \$1,000 increase in import penetration) and shows that these results are magnified in areas with high proportions of blacks and substantial income inequality.

In a recent paper for a Federal Reserve of Boston conference on geographic, Turner (2019) uses the ACS and the Census and reports estimates of secondary enrollment which are in line with those reported by Tuhkuri (2018), high school degree receipt estimates are appreciably lower than those presented by Greenland and Lopresti (2016). Turner (2019) also shows that increased import exposure leads to modest gains in college enrollment for those in their early 20s, but there is no evidence of gains in four-year degree completion. These results are consistent with Foote and Grosz (2019) who examine variation in college enrollment in response to year-to-year changes in mass layoffs and find substantial increases in enrollment at community colleges.[4]

In the context of an analysis on the broader public finance impacts of increased trade exposure, Feler and Senses (2017) highlight the extent to which local labor market shocks likely affect the local tax base and, in turn, public funding for education along with other public services including transportation and criminal justice expenditures. They find that total tax revenues decline about 1.9 percent for every \$1,000 increase in Chinese imports per worker, while expenditures on education decline by about 1 percent. In addition, Feler and Senses document increases in the student teacher ratios, which partially reflect enrollment growth. Feler and Senses (2017) also demonstrate the extent to which educational expenditure effects are magnified for localities in which the remainder of the state has similar shocks, with state level shocks producing effects more than twice as large.

Given the growing body of evidence that demonstrates the impact of resources on educational attainment (e.g. Jackson, Johnson and Persico 2016; Lafortune, Rothstein and Schanzenback 2018; Candelaria and Shores 2018), it is surprising to identify large

[4]There are other studies that investigate the link between labor market shocks and educational attainment outside the US. For example, Atkin (2016) finds that in Mexico school dropouts increases as a result of factory openings induced by trade.

gains in an outcome like high school degree receipt in the face of funding declines. To put this result in perspective, Jackson, Johnson and Persico (2016) find that an increase of 10% in per-pupil funding in all 12 school age years increases the probability of graduation by 7 percentage points (or 8%). It seems unlikely to think that a labor market shock which reduces school funding has an effect which is equivalent to an increase in per-student spending of 5% in all 12 years of education, particularly as the observed impact of increased trade exposure is actually to reduce expenditures in schools.

2 Measuring Secondary Educational Attainment in Local Markets

Measurement of educational attainment and enrollment follows from the individual-level ACS/Census surveys and administrative reporting from the U.S. Department of Education's Common Core of Data (CCD), which is the primary administrative database on public elementary and secondary education in the United States.[5] While the ideal data would allow researchers to follow individuals longitudinally, measuring attainment at different ages for multiple birth cohorts along with place of residence, the data available are more limited and generate challenges in measurement.

What is observed for each geographic area at annual intervals is repeated cross-sectional measures of enrollment and degree attainment. The ACS/Census data follow from a representative sample of U.S. households (see Ruggles et al 2019) which includes questions about each individuals' current enrollment status along with educational attainment. The CCD records enrollment by grade level and high school diplomas awarded based on administrative reporting by public school districts. For all measures, the commuting zone (CZ) represents the primary geographical unit of analysis. Following Dorn

[5]For 1990 and 2000, we employ the 5% sample of the U.S. Census and, for 2007, the 2008 3-year ACS (Ruggles et al, 2019). The ELSI table generator provided by the National Center for Education Statistics (NCES) is used to extract the CCD observations. The 2008 3-year ACS put together the yearly ACS from 2006, 2007 and 2008 and adjust the individual weights such that the estimated indicators estimated from this dataset are representative for this period.

(2009), county level data are aggregated to the CZ taxonomy (Tolbert and Sizer, 1996).[6] "Education markets" are not completely coincident with labor markets and, at the elementary and secondary level, there are multiple school districts within CZs though generally school districts aggregate to CZs and do not tend to cross county lines.

The ACS/Census and CCD measures are imperfectly aligned. While ACS/Census measures educational participation for the population living in a particular location at a given time, regardless of where individual obtained his education; CCD provides information about enrollment counts at a point in time and high school diploma receipt during a given year, but records neither age-by-grade nor location choice after school completion. What is more, the CCD is limited to the enumeration of public-school enrollment, which accounts for about 91.2 percent of secondary enrollment (Snyder, De Brey and Dillow 2018, Table 205.10).

The natural unit of analysis of in the ACS/Census is a rate of enrollment or the share of the population that has achieved a given level of attainment such as high school graduation or a contemporaneous measure of enrollment. Such measures include the share of 18 year olds with a high school degree or the proportion of 17 year olds enrolled in high school. While there is some distinction in more recent surveys, the ACS/Census measures do not distinguish between "regular" high school diplomas and GED degrees. Administrative data on high school degrees awarded reflect the count of diplomas (D_{jt}) awarded by public high schools in a geographic j unit in year t. Note that the adminis-

[6]For the case of the ACS/Census data, the aggregation at the county level was done using the crosswalk files provided by Autor and Dorn (2013). For the case of the NCES information, we extracted the number of diplomas and enrollment variables at the school district level as well as the county codes from the table generator for the periods 1988-1991, 1999-2001 and 2006-2008. The information we use covers only public schools (information for private schools is available only since 1997). The number of diplomas is reported by school districts themselves. The by-grade enrollment variables that we use are the result of aggregating enrollment across school within a district (NCES makes the aggregation). School report by-grade enrollment in a point in time, which is typically October. In order to create smoothed estimates of every variable for each year in our analysis (1990, 2000 and 2007), we averaged the extracted information over the three years of non-missing information we had within each period. After this, we kept those districts for which there was information in the three years so we constructed a balanced panel at the district level. Then we aggregated the information at the county level and collapsed it at the CZ level using Autor and Dorn (2013) crosswalk files.

trative data do not distinguish high school degree recipients by age and cohort of high school diploma recipients necessarily reflect a distribution of individuals in their teens.

Over recent years, important research has identified the more general problems and challenges in measuring and aligning high school graduation rates (Heckman and LaFontaine 2010; Murnane 2013).[7] To foreshadow the empirical results in subsequent sections, understanding the economic forces that differentiate between ACS/Census high school attainment rates and those based on administrative diploma counts is of first-order importance. One way to see the alignment is to write the survey-based measure in relation to the diploma counts. Begin with the conceptual (unobserved) disaggregation of diploma counts by the share of diploma recipients awarded to each age level "a" such that $r_a = \frac{D_a}{D}$ and $D_t = \sum_a r_a D_t$, where adding a t subscript to r_a would allow the age structure of high school diploma receipt to change over time while it may also vary across geography. Beyond the age structure of degree receipt, the survey measures differ in three other conceptual dimensions: 1) they include GED recipients, 2) they include diplomas awarded by private high schools, and 3) they reflect net migration of diploma recipients after degree receipt. To summarize, the age-specific share of high school graduates (HSG_{ajt}) can be expressed in terms of diplomas awarded (D_{jt}) as follows:

$$HSG_{ajt} = \frac{\text{Survey count HS degree recipients age a}}{\text{Survey count population age a}} = \frac{s_{jt}(\sum_{a' \leq a} r_{ajt} D_{jt} + GED_{ajt} + Private_{ajt})}{Pop_{(a-1)j(t-1)} \times m_{ajt}} \tag{1}$$

where m_{ajt} is the migration rate $\frac{Pop_{ajt}}{Pop_{(a-1)j(t-1)}}$ and s_{jt} is the likelihood that a high school graduate from j stays after getting a degree.[8] Thus, it should be self-evident that variation in the survey measure HSG_{ajt} may not mimic variation in a degree count measure

[7]A recent analysis by Murnane (2013) summarizes the determinants of the stagnant levels of graduation rates in the 1970-2000 period while also identifying the substantial increase in high school graduation rates in the first decade of the 21st century.

[8]This formulation assumes that D_{jt} does not change in the very short term (e.g between 2000 and 2001).

D_{jt} or such a measure relative to a single age population count $\frac{D_{jt}}{Pop_{ajt}}$.[9] While HSG_{ajt}, D_{jt} and Pop_{ajt} are outcomes that we are able to observe directly at the level of CZ, r_{ajt}, GED_{ajt}, and $Private_{ajt}$ are not elements observed directly at a fine level of geographic detail.[10] We return the consideration of the role of age of degree receipt and timing of degree receipt when we reconcile the ACS/Census and CCD empirical estimates.

Table 1 presents means for measures of enrollment and high school degree receipt at the intervals of 1990, 2000, and 2007, following the periodic observations of Autor, Dorn and Hanson (2013).[11] To permit comparisons of outcomes with other research, we consider measures for the broad 16-19 teen population and for specific ages (17,18, and 19); for the administrative data, we present diploma counts relative to both the age 17 population and relative to the lagged freshman enrollment (AFGR).[12] Not only have the measures of high school graduation risen (and dropout rates fallen), but there has been a striking convergence across geographies in high school attainment over the last two decades. These patterns are evident at both the local labor market and state levels, with convergence in high school graduation most apparent in the last decade (see also, Nunn, Parsons and Shambaugh 2018).

[9] We will refer to this ratio as high school graduation ratio.

[10] The Current Population Survey (CPS) has asked a question about the timing of receipt of the high school degree (or equivalent) since 1993 and we find that more than 90 percent of high school degrees were awarded by age 20. For 2007, the age distribution of individuals obtaining a degree in the current year was about 6.7% at age 17 or younger, 60% at age 18 and 21% at age 19 and there is indication that the age of degree receipt increased modestly between 1993 and 2007, with the share completing by age 18 falling from 74% to 71.6%. Capacity to measure variation in GED receipt by age, geography or point in time is more limited. Maralani (2011) documents that about 11% of people age 15 to 34 with a secondary credential held a GED and she uses data from the NLSY to distinguish the age of credential receipt for those receiving a traditional diploma relative to the GED. She finds that the median age at receipt of a regular diploma is age 18, with more than 90% of those with a regular diploma graduating by age 18; in contrast, the median age at GED receipt is 20, with only 1/3 having received this credential by age 18.

[11] Autor, Dorn and Hanson (2013, 2019) constructed a balanced panel with 722 CZs in each period. Because we trimmed values higher than 1 for the variable diploma/enrollment, in this version of the paper we work with an unbalanced panel of 1,342 observations comprising 690 CZs. In cases where the dependent variable allowed us to run specification with a balanced panel, we verified that the results were almost identical to those we obtained with our analytic sample.

[12] The averaged freshman graduation rate (AFGR) is an indicator presented by NCES and provides an estimate of the percentage of students who receive a regular diploma within 4 years of entering ninth grade. The rate uses aggregate student enrollment data to estimate the size of an incoming freshman class and aggregate counts of the number of diplomas awarded 4 years later.

At the secondary level, Figure 1 measures high school completion of those ages 19-21. This presentation illustrates convergence at the CZ level, with the horizontal axis showing the outcome measure in the base period of 1990 and the vertical axis showing the change in the outcome from 1990 to 2014. Convergence is marked, with a 10-percentage baseline gap in high school graduation narrowing by about 6.8 percentage points over the interval. A point to emphasize is that "convergence" has not occurred at a constant rate, but rather accelerated markedly after 2000 (Nunn, Parsons and Shambaugh 2018).

3 Estimating the Effects of Import Exposure on High School Enrollment and Graduation

The basic identification of the impact of trade exposure on high school attainment follows closely from Autor, Dorn and Hanson (2013, 2019), with these papers presenting a thoughtful and full motivation of the econometric setup. The aim in this analysis is to use plausibly exogenous changes that impact local labor markets to measure the response in education attainment. And, as such, the primary outcomes of interest are approximately decennial changes in educational outcomes (ΔE_{it}) as a function of the expected change in employment induced by unanticipated increased imports from China (ΔIPW_{uit}). The Chinese import penetration measure is constructed to reflect the change in U.S. imports of Chinese goods per worker in a local area, using the commuting zone (CZ) share in national industry employment as weights:

$$\Delta IPW_{uit} = \sum_j \frac{L_{ijt}}{L_{ujt}} \frac{\Delta M_{ucjt}}{L_{it}}$$

where L represents start of period employment and ΔM is the overall change in the value of U.S. imports from China, with i indicating CZ, j indicating industry, and t indicating time period. The primary specification form is as follows:

$$\Delta E_{it}^{m} = \gamma_t + \beta_1 \Delta IPW_{uit} + X_{it}^{'} \beta_2 + \epsilon_{it}$$

This setup is widely used to study how sectoral shocks affect labor market outcomes is known as shift-share analysis ("Bartik shocks"), where the national sectoral shocks are the shifts and the measures of exposure to sectoral shocks are the shares. Following Autor, Dorn and Hanson (2013), we instrument the imports measure with a measure based on the change in the value of imports to other high-income countries and lagged employment shares to mitigate the possibility of contemporaneous adjustment and that changes in sector-specific imports can be driven by endogenous demand changes in the US markets:

$$\Delta IPW_{oit} = \sum_j \frac{L_{ijt-1}}{L_{ujt-1}} \frac{\Delta M_{ocjt}}{L_{it-1}}$$

Following these specifications, changes are measured over the stacked intervals 2007-2000 and 2000-1990 at the level of the CZ. Further, our baseline results include the full set of CZ-level covariates measured at the beginning of each period used in Autor et al exposition to control for variables that can affect the dependent variable directly and also be correlated with the increments in trade exposure.[13] The key identification assumption in this instrumental variable analysis is that changes in import exposure at the CZ level predicted by the instrument are unrelated to unobservable changes in the CZ that could affect changes in high school attainment directly. In our main results, we clustered the standard errors at the state level in order to allow for spatial correlation of the error term across CZs within states. In the context of our analysis where a substantial

[13]These covariates are percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average "offshorability" index of occupations and census division dummies.

share of funding for education comes from state governments, the clustering at the state level reflects the common relationship among both residual education and labor market factors.

The validity of specifications such as this which rely on within-geography variation over time – effectively comparing CZs with high trade exposure to those with low trade exposure[14]– relies on the absence of residual pre-trends. Specifically, it is possible that the trade shocks have been more (or less) pronounced in areas that were already experiencing educational improvements prior to the increase in Chinese import penetration. This is certainly a possibility if we take into account that there has been a striking convergence over time in educational attainment across CZs (see section 2). One approach to addressing this this issue is to add CZ fixed effects (akin to adding geography-specific trends in approaches where the outcome is specified in levels), however this approach can be highly inefficient (see a more detailed discussion in Section 4). Rather, we add changes in dropouts share between 1980 and 1990 to the set of control variables (and show that our estimates are robust to this exercise), as data for diplomas are not available for 1980. This seems reasonable given a strong correlation between dropout and high school graduation rates.

More generally, recent research efforts have focused attention on the econometric properties of shift-share estimators, emphasizing the need to open the "black box" and provide explicit description of the underlying sources of identification. For example, Goldsmith-Pinkham et al. (2018) discuss the sources of the identification in this framework and show that when researchers rely on variation in the sectoral shares across locations as main source of identification, the shift-share estimator is a weighted average of the estimators that are obtained when each share is used separately as a just-identified instrument. They identify the industry shares that drive most of the predictive power in Autor, Dorn and Hanson (2013) instrumental variable strategy and show that their

[14]The specification is parallel to a traditional difference-in-differences specification, where the CZ level effects are removed by differencing.

results may be overestimated due to pre-existing trend in manufacturing employment in areas that have high presence of those industries. A further concern is that inference in the shift share context with the use of clustered standard errors (as in Autor, Dorn and Hanson 2013 along with Greenland and Lopresti 2016), can lead to over rejection of null effects since the error term can be correlated across regions with similar sector compositions.

4 Empirical estimates of enrollment and high school attainment impacts

Two questions motivate the empirical work on how the growth in trade exposure impacts high school attainment: First, does high school enrollment increase? And, as the answer is affirmative, the second question concerns whether increased enrollment results in growth in high school degree attainment. We show that there are gains in enrollment concentrated at the 12th grade (and in the late teen years) which align with some gains in public high school diploma receipt though the answer to the latter questions is nuanced given measurement challenges, but gains in the representation of high school graduates among the population persisting in a commuting zone (CZ) are indistinguishable from zero.

4.1 Enrollment Outcomes

We examine two types of enrollment indicators: enrollment by age measured in survey data and enrollment by grade (9th, 10th, 11th and 12th) relative to the number of 8th graders derived from CCD administrative data. Table 2 shows the enrollment outcomes for the broad 16-19 population and for each single-year age group. Panel A focuses on enrollment in high school (with dropouts, those enrolled in college and non-enrolled high school graduates coded as zero), and panel B presents the high school dropout rate (neither enrolled in school nor having received a degree). The top panel shows clear evidence of growth in high school participation, with an effect of about 0.9 percentage points for each $1,000 increase in import exposure for the 16-19 and the single age-group effect reaching a maximum of 1.2 percentage points (2.8%) for the 18 year old group.[15,16] High school persistence follow a similar pattern. The Panel B, which shows the decline in dropout behavior is focused among those ages 17 and 18.[17]

Turning to enrollment-based measures in Table 3, we find that the magnitudes of the relevant coefficients are larger for the more advanced grades and that they are particularly large for grades 11th and 12th. They imply that an increase of $1,000 in import exposure leads to an expansion in the 11th and 12th grade enrollment rates by 1.6 (1.8%) and 2.5 (2.9%) percentage points, respectively. Such evidence is consistent with the hypothesis that youth who might have left high school before the 12th grade may be induced to persist while some students may choose to repeat grades rather than leave to school without a diploma. In addition, increased enrollment at the 12th grade may be an indicator of increased grade repetition.[18]

Results from Tables 2 and 3 are consistent with each other, namely, they suggest that there is an increase in enrollment that is more pronounced in grade 11 and grade 12 and that, at least to some extent, these increases are driven by a greater persistence in the later years of high school, typically among those ages 17 and 18.

[15] Because the interquartile range in the CZ distribution of per worker import exposure growth between 2000 and 2007 in $1,006 (Autor et al 2013), an increase of $1,000 in per worker import exposure can be interpreted as the differential effect that a CZ at the 75 percentiles has experienced relative to the CZ that is at the 25 percentile in the distribution.

[16] Appendix Table A1 displays the effects of this analysis on a more comprehensive index of school enrollment, that is, college or high school enrollment. Results show a very similar patter to those shown in Pane A of Table 2.

[17] Suggestive evidence, presented in Appendix Table A2, shows that decline in dropout and the increase in enrollment is concentrated in those states with relatively low compulsory attendance ages (16-17). These patterns are more pronounced individuals that are 18 years old, age at which the low compulsory attendance age is no longer binding.

[18] Descriptive evidence calculated from CPS suggests that at the national level the share of students retaking grade 12 has increased during the period of analysis. While this ratio was 3.21% in 1992-1994, it was 4.08% and 3.26% in 1999-2001 and 2006-2008, respectively.

4.2 High School Degree Attainment

Increased high school persistence and enrollment need not translate to high school diploma receipt. Students who start the 12th grade may fail to persist through the year or they may fail to meet the requirements for graduation which often include passing particular course requirements and, in more recent years, statewide competency exams. In this subsection we show the effects of import exposure on two indicators of completion rates: total number of diploma recipients in year t as a share of the relevant population (high school graduation ratio) which is similar to Greenland and Lopestri (2016) primary specification, and the share of population (at certain age) that have obtained at least a high school diploma or equivalent degree in the CZ (share of with a high school degree). It is important to reiterate (see Section 2) the point that the construction of these two indicators differ in some fundamental ways and do not reflect identical economic outcomes.

Starting with the Census/ACS measures of educational attainment among young adults, Table 4 reports the instrumental variables estimates of the impact of increased import exposure on high school graduation rates. For students receiving traditional diplomas, one would expect Census/ACS measures to have fully captured attainment by age 20. While the modal age of high school receipt is age 18, the age at receipt of the traditional high school diploma include ages 17, 18 and 19. Table 4 reports estimates for single years of age (18-20) and several age group aggregates which are negative for each single year age, though including 0 in the confidence interval in most cases. The estimates for 18 year olds and 18-19 year olds are significant (-0.76 and -0.66, respectively), which may reflect increased time to high school degree receipt (recall from Table 2 than enrollment for this group concurrently increased, so this should not be interpreted as reflecting lifetime degree attainment.) Still, for those ages 19-20 and 20-21, we can rule out positive effects greater than 0.10 and 0.17 percentage points, respectively.

In Table 5, we turn from age-specific measures of the representation of high school graduates to diploma counts. We find significant and positive effects of import exposure on high school for our two definitions of high school graduation ratio. When we consider the number of diplomas awarded relative to prior cohort enrollment, effects are about 0.90 percentage points (1.4%) per $1,000 increase in import exposure and about 0.8 percentage points (1.3%) for the diploma measure normed by populations.

Two questions emerge from this analysis of the impact of import exposure on high school attainment: first, what factors drive the divergence of the age specific measures and those based on diploma counts? And, secondly, why are these point estimates presented in this analysis only about one-fourth those presented in Greenland and Lopresti (2016). We answer the questions in reverse order, as the "wedge" between the measures also appears in the Greenland and Lopresti analysis.

4.2.a *Greenland-Lopresti and a Weak Instrument Issue*

The preferred econometric specification employed by Greenland and Lopresti (2016) draws motivation from Autor, Dorn and Hanson (2013) but departs from the original specification by adding CZ level fixed effects. The motivating concern for Greenland and Lopresti is that pre-existing trends in high school completion at the level of localities are correlated with the trade exposure variation. As we discuss below, the concern is not without merit. However, the inclusion of locality fixed effects is an inefficient approach to accounting for these trends that weakens the estimation strategy. With the dependent variable defined as a difference measure, the inclusion of location-specific fixed effects amounts to identifying the import exposure coefficient from deviations from the location-specific change.

The Greenland-Lopresti main results, which we replicate in Table 6, produce estimates which imply that a $1,000 increase in import exposure would lead to a 3.6 percentage point increase in the graduation rate (see column 3). In the replication exercise, we use the outcome variable based on diploma counts related to the population age 17 shared by GL.[19,20] Their preferred estimates (and those presented in the Table 6 replication) are four times larger than those estimated using the same outcome variable without the locality fixed effects.[21]

The inclusion of locality fixed effects in GL specification dramatically reduces the identifying covariance between the instrument and the endogenous variable, generating a potential issue of a weak instrument, while also exacerbating finite sample bias. The difference in the IV estimates with and without CZ fixed effects is driven by the difference in the covariance between the endogenous variable and the instrument. It is straightforward to show that most of the covariance between "variation in the change in import exposure" and the instrument is between CZs, not within CZs over time. To provide a quantitative framing, while the overall correlation coefficient is 0.773, the conditional correlations after controlling for CZ fixed effect and for both CZs and time fixed effects are 0.42 and 0.158, respectively. Therefore, when the econometric specification includes CZ and time fixed effects, most of the initial variation is shut down, which then generates a weak instrument problem.[22] We further illustrate this point by adding CZ fixed effects to

[19] Greenland and Lopresti shared with us a dataset containing the main dependent variable in their analysis, that is, decennial changes in graduation rates. We did not receive the underlying diploma counts or population measures used to construct the measures; ultimately, our measures are correlated but not identical.

[20] In addition to presenting the OLS specification (see column 1 in Table 6), GL present an alternative that does not include the time fixed effect (see column 2 in Table 6 for an exact replication of this result). The result from this specification are significantly larger than Table 5 results (which do have year fixed effects) and reflect the notable convergence in graduation rates across localities.

[21] Greenland and Lopresti are thoughtful in reporting alternative specifications of the dependent variable in Table 7 of their paper, with columns (1) and (2) using alternative denominators, respectively the population ages 16-18 and 8th grade enrollment lagged by four years. Estimates in these specifications are 3.23 and 4.73 percentage points, respectively. In the final column of this table, they present estimates based on Census / ACS tabulations of the share of high school graduates in the population ages 18-25, which might be seen as most similar to our estimates presented in Table 5; these estimates are small in magnitude (0.78 percentage points).

[22] Alternatively, we can decompose the total covariance between the endogenous and instrumental variables. For the sake of illustration, let's assume that the first stage equation is: $\Delta I_{it} = \gamma \Delta Z_{it} + \mu_i + \sigma_t + \epsilon_{it}$ where μ_i and σ_t denote CZ and time fixed effects; and ΔI_{it} and ΔZ_{it} are the change in import exposure and the instrumental variable, respectively. It follows that: $COV(\Delta I_{it}, \Delta Z_{it}) = COV(\mu_i + \sigma_t, \Delta Z_{it}) + \gamma^2 VAR(\Delta Z_{it})$. Because $COV(\Delta I_{it}, \Delta Z_{it})$=2.004, $\hat{\gamma}^2$=0.394 and $VAR(\Delta Z_{it})$=2.418, the fraction of the total covariance explained by within CZ over time correlation is: $(0.394^2 \times 2.418)/2.004 = 19\%$

the estimates of the effect of changes in import exposure on manufacturing employment from the original Autor, Dorn, and Hanson (2013, see Table 3, page 2137 in the original) paper, which we present in Table A4. Panel A reproducing the original results and Panel B showing the results with the CZ fixed effects. It is apparent that the magnitude of all the estimated coefficients in the augmented specification doubles the magnitude of the coefficients obtained from the regressions without the fixed effects, which is consistent with the instability that characterizes estimation using weak instruments.

While in standard econometric references the rule of thumb to test for weak instruments is to check whether the F-statistic (which falls from 162 to 21 with the inclusion of CZ fixed effects) is higher than 10, the spatial correlation of the errors changes the validity of this rule. GL, along with Autor, Dorn and Hanson (2013), cluster errors at the state level (rather than assuming they are iid) and, as shown in Montiel-Olea and Pflueger (2013), departures from the iid assumption (e.g. spatial correlation) affect the asymptotic distribution of the 2SLS estimator and therefore the critical values of the weak instrument test. When we apply the weak instrument test developed by Montiel-Olea and Pflueger, in which the "new" rule of thumb is 37.5, we cannot reject the presence of weak instruments.[23] Moreover, previous specifications that do not include CZ fixed effects pass the modified weak instrument test since they have F-statistics higher than 37.5. Finally, the evidence described here highlights the need for caution when researchers add location/market fixed effects in scenarios where the length of the panel is short and the key variables are measured in differences, given the potential to reduce substantially the correlation between the endogenous regressor and the instrument.

Perhaps the most intuitive way of dealing with the presence of pre-existence trends

[23] This weak instrument test can be implemented only in situations where there is one endogenous variable. In the case of one instrument, the augmented F-statistic proposed in Montiel-Olea and Pflueger coincides with the conventional F. In this test the null hypothesis is that the estimator's asymptotic bias exceeds a fraction of a "worst-case" benchmark, being this benchmark a situation where the first stage is not informative and the errors terms in the first and second stages are perfectly correlated (Pflueger and Wang 2015). The critical values in the Montiel-Olea and Pflueger weak IV test for 5% significance level are 37.42, 23.11, 15.06 and 12.04 for the 5%, 10%, 20% and 30% of worst-case bias, respectively.

in high school graduation rates is by controlling for lagged decennial changes of this variable. Because CCD does not provide diploma information for years prior to 1986, an alternative is to control for other variables that are correlated with high school graduation rates. Our approach to deal with this issue is to include decennial change in dropout share for 16-19-year-old individuals in the preceding decades constructed from the decennial Census.[24] Column 4 in Table 5 shows the estimation results when the lagged decennial change in dropout rates is added to the set of covariates. Although there is a small decrease in the magnitude of the estimated effects of trade penetration, consistent with the presence of pre-trends, the effects remain economically similar and statistically significant at conventional levels. Indeed, as calculated previously they suggest that a $1,000 increase in trade exposure in the average CZ is associated with an increment of 0.818 percentage points 1.3% in the ratio of high school diplomas awarded to the population age 17.

4.2.b *ACS/Census and administrative data produce different impacts*

As noted in our discussion of Tables 5 and 6 (and as found in the original Greenland Lopresti analysis), estimates from high school diploma counts produce effects that are appreciably larger than those from survey measures such as the ACS / Census, with the former pointing to impacts of about 0.8 percentage points and the latter with null results. Following from equation 1, which highlights the conceptual differences between ACS/Census high school attainment and those based on administrative diploma, the answer to this question should be in the following aspects: (i) Migration effects which differ by educational attainment, (ii) changes in the composition of school degrees to-

[24]Figure A1 in the Appendix shows that the correlation between graduation rates (defined as number of diplomas divided by prior enrollment) and dropout rates for 16-19 years olds is -0.38 and -0.34 in levels and decennial first differences, respectively. Tuhkuri (2018) also uses observation of dropout rates from the 1970 and 1980 census to address concerns about pre-existing trends and also finds these controls have a minimal impact on estimates.

wards fewer private or less GED degrees, and (iii) changes in the age distribution among high school recipients.

First, to the question of the age of high school degree recipients, we find it plausible and strongly supported by the high school enrollment by age results (Table 2) that the age distribution of high school diploma recipients becomes "older", shifting mass from age 17-18 to 18-19. From equation (1), it is plausible that r_{17jt} falls relative to r_{19jt} , meaning that a smaller share of diplomas go to 17 year olds and a larger share go to 19 year olds, in those localities experiencing increased trade exposure and the evidence of increase secondary enrollment at ages 18 and 19, along with increased grade 12 enrollment (Table 3), is consistent with such an explanation. Yet, while this factor offers some explanation for the divergence between the diploma count measure and the share of high school graduates observed at ages 18-19, it does not explain the divergence for the ACS/Census measure at older ages. There is little evidence to suggest that traditional high school diplomas are awarded when individuals are older than 20, however. We believe that the only way this channel could have resonance is if some individuals who would have otherwise received GEDs in their 20s persist to get regular high school diplomas in response to increased trade exposure. While it is difficult to test for such effects directly, they are likely to be too small to account for the full divergence in estimates.

A second type of impact would follow from students shifting from private high school enrollment (and degree completion) to public enrollment. Plausibly, declines in local labor market conditions produce income effects which cause families to shift from paying tuition at private schools to public schooling which is provided at no charge. Table 7 shows the impact of the trade exposure on the share of individuals in the CZ enrolled in a private school for different age groups relative to the total population and to the population attending high school in the analyzed group. We do not find evidence of a shift toward public school in any of the specification.

Finally, high school graduates from areas affected by increased exposure to trade may migrate to other regions looking for better job opportunities, enroll in college away from home or enlist in the military.[25] To focus on this potential channel, consider the link between the two measures of high school attainment described in equation (1) when other channels are assumed to be unchanged:[26]

$$HSG_{ajt} = \frac{s_{jt} D_{jt}}{m_{ajt} \times Pop_{(a-1)j(t)}} \tag{2}$$

If s_{jt} and m_{ajt} respond to the trade shock at the same rate, we should not observe a divergence between the ACS/Census and the CCD measure and the trade shock would be not have differential migration effects by education.[27] We do not have data to test directly whether migration induced by the trade shock is educational neutral, however we can examine (i) whether there is some evidence of migration among graduation ages (in other words, effects on m_{ajt}) and (ii) whether the ratio of the stay-rate of high school graduates to the overall stay-rate $\frac{s_{jt}}{m_{ajt}}$ is negatively affected by the increase in import penetration, which would imply that recent high school graduate stay in the CZ less often than the general population.

To check for evidence of outmigration, we analyze whether as a result of the trade shock the size of the cohorts that are around or slightly beyond high school graduation ages have experienced more pronounced changes compared to the size of cohorts that are close to graduate from high school. In order to perform this analysis, we perform the instrumental variable analysis on two outcomes variables we constructed from ACS/Census data: log of the ratio between number of 19-20 and 16-17 years olds in the CZ and log of the ratio between number of 19-21 and 16-18 years old in the. Results displayed in Table 8 suggest that an increase of $1,000 in per worker import penetration

[25]Consistent with this, Greenland, Lopresti and McHenry (2019) document that local labor markets most exposed to import competition from China experienced reductions in population growth. They focus on broader age groups and our analysis examines individuals in their late teens and early 20s.

[26]We are imposing the assumptions of constant age (for example 18) at degree and no GED/Private shifts. In turn, because we do not have longitudinal data every variable is measured at t.

[27]A straightforward rearrangement of equation (2) lead us to $\frac{s_{jt}}{m_{jt}} = \frac{HSG_{ajt}}{\frac{D_{jt}}{Pop_{(a-1)j(t)}}}$.

in a CZ is associated with a reduction in the relative size of 19-20 and 19-21 cohorts of approximately 1.4% and 1.3%, with both results being statistically significant at 5%.

Columns 3, 4 and 5 from Table 8 report the effects on the stay-rate of high school graduates to the overall stay-rate. We do this analysis for several indicators of high school graduation from Census/ACS (HSG_{ajt}) in the numerator with $\frac{D_{jt}}{Pop_{(17)j(t)}}$ in the denominator. Although the relevant coefficients are not very precisely estimated, the sign of the estimates parameters is negative and suggest that high school diploma recipients outmigrate more often than other individuals in the same age group. The underlying causes of this outmigration are difficult to identify without longitudinal data; however, three pathways seem most plausible: first, high school degree recipients may migrate to other localities for improved employment opportunities; second, the rate at which students enroll in college outside of the "home CZ" may increase and, third, military enlistment may increase. The channel of military enlistment is particularly compelling given that manufacturing declines likely impact the prospects of male high school graduates, which is a group relatively likely to consider military service. While data on military enlistment at a fine level of geography are not available over an extended horizon, Murphy, Ruh and Turner (2020) show that when mass layoffs relative to workforce rise by 1 percentage point, Army enlistments rise by more than 2.8%.

In this section we have investigated several mechanisms that may be driving the differences between estimates that are obtained when high school graduation rates are measured from CCD and ACS/Census data. Although the available evidence for this divergence points to the importance of migration effects which differ by educational attainment, we cannot rule that other explanations (e.g. changes in the age distribution of diploma recipients) have played an important role also.

4.3 Education Production in Public Schools

The high school enrollment and attainment effects presented in this analysis show a substantial increase in late high school enrollment which is accompanied by a modest increase in diploma receipt and a null effect on the representation of high school students in local communities. A substantive concern is that schools may lack resources to help marginal high school students translate enrollment to diploma receipt. Analysis of school district spending provides some insight. Our analysis uses data from the CCD surveys of school district finances; these measures are parallel but not identical to the measures of local and state government expenditures employed by Feler and Senses (2017). Estimates presented in Table 9 show that an increase in trade exposure of $1,000 decreased 2015 constant dollar expenditures per student by about $300 (3.5%) per student. These effects are magnified by measures at the state level (effects of about $630 in association with a statewide increase in trade exposure of $1,000). When the dependent variable is represented in log form, the estimates show a local-level effect of about 2% with the effect magnified to about 5% when state level measures are included. In effect, this evidence clearly demonstrates that fiscal impacts of trade exposure are magnified by the lack of accommodation of public funding from state or federal sources.

5 Conclusion and Takeaways for Future Analysis

Local labor market shocks, including those that follow from manufacturing job loss related to increased trade exposure, have sustained impacts on communities. Evidence presented in this paper shows that, while late high school enrollment effects are substantial, effects on high school diploma attainment are modest. In turn, there is no net gain in the representation of high school degree recipients in affected communities. Because workers with low levels of education are often hit hardest by these disruptions, attention to whether the youth of affected communities are able to forge pathways to better outcomes through investments in education is particularly important. While large high school degree effects such as those presented by Greenland and Lopresti (2016) would indicate that educational attainment ameliorates labor market disruptions, the more modest effects presented here combined with the negative impacts on school resources suggest weaker long-term trajectories for affected labor markets and their populations.

Overall, the combination of the evidence assembled in this paper and the complementary analyses including Tuhkuri (2018) and Feler and Senses (2017) make clear a salient policy challenge associated with local labor market shocks and public education. The same factors that erode local employment opportunities both place downward pressure on school resources while also increasing student enrollment demand.

It is widely recognized that geographic disparities in the labor markets are increasingly persistent (Austin, Glaeser and Summers 2018). The substantial aggregate net gains from trade liberalizations produce benefits and costs have not been equally distributed across localities. To date, policies in the education sphere that aim to address the adverse consequences of trade shocks have focused quite narrowly on the individual workers who can demonstrate job loss as a direct result of policy induced changes in the patterns of trade and more generally on those who qualify for federal education assistance through programs like the Trade Adjustment Assistance. Speaking at a Federal Reserve Bank of Boston conference in 2019, former Treasury secretary Lawrence Summers raised the question of whether a major federal counter-cyclical program to invest in education in communities with eroding local labor markets might be warranted. Illustrating the fiscal challenges faced by cities that lost jobs to import competition, Summers quipped: "... we tell the people who live in Flint, MI that it is good for everyone to have lower-priced cars ... your plant is going to have to close... but when we go the next step to say there is not going to be calculus offered at your local high school because we can't afford to raise property taxes to prohibitive levels and the local property tax base is shrinking because house prices are going down ... that is not reasonable, that is not just, that is not fair."[28]

The connection between local labor market changes, including the loss of manufacturing jobs following from increased trade exposure, and education outcomes merits far greater investigation. Not only is there a need to measure the impact of increased trade exposure on a more comprehensive set of measures of academic and educational outcomes (including test scores), there is a need to better understand the interaction with school resources. A key policy counterfactual for which empirical evidence is largely absent concerns how increases in resources for education in response to local or regional labor market distress would affect the longer run trajectories of youth and the communities in which they reside.

[28] Remarks at the Federal Reserve Bank of Boston Conference, October 2019. https://www.bostonfed.org/housedivided2019.aspx

References

Adao, R., M. Kolesár, and E. Morales (2019). Shift-share designs: Theory and inference. *The Quarterly Journal of Economics 134*(4), 1949–2010.

Atkin, D. (2016). Endogenous skill acquisition and export manufacturing in mexico. *American Economic Review 106*(8), 2046–85.

Austin, B. A., E. L. Glaeser, and L. H. Summers (2018). Jobs for the heartland: Place-based policies in 21st century america. Technical report, National Bureau of Economic Research.

Autor, D. and D. Dorn (2013). The growth of low-skill service jobs and the polarization of the us labor market. *American Economic Review 103*(5), 1553–97.

Autor, D., D. Dorn, and G. Hanson (2013). The china syndrome: Local labor market effects of import competition in the united states. *American Economic Review 103*(6), 2121–68.

Autor, D., D. Dorn, and G. Hanson (2019). When work disappears: Manufacturing decline and the falling marriage-market value of young men. *American Economic Review: Insights 1*(2), 161–178.

Black, D. A., T. G. McKinnish, and S. G. Sanders (2005). Tight labor markets and the demand for education: Evidence from the coal boom and bust. *ILR Review 59*(1), 3–16.

Candelaria, C. A. and K. A. Shores (2019). Court-ordered finance reforms in the adequacy era: Heterogeneous causal effects and sensitivity. *Education Finance and Policy 14*(1), 31–60.

Cascio, E. U. and A. Narayan (2015). Who needs a fracking education? the educational response to low-skill biased technological change. Technical report, National Bureau of Economic Research.

DeLeire, T. and A. Kalil (2002). Good things come in threes: Single-parent multigenerational family structure and adolescent adjustment. *Demography 39*(2), 393–413.

Dorn, D. (2009). *Essays on inequality, spatial interaction, and the demand for skills*. Ph. D. thesis, University of St. Gallen.

Feler, L. and M. Z. Senses (2017). Trade shocks and the provision of local public goods. *American Economic Journal: Economic Policy 9*(4), 101–43.

Foote, A. and M. Grosz (2019). The effect of local labor market downturns on postsecondary enrollment and program choice. *Education Finance and Policy*, 1–50.

Foote, A., M. Grosz, and A. Stevens (2019). Locate your nearest exit: Mass layoffs and local labor market response. *ILR Review 72*(1), 101–126.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2018). Bartik instruments: What, when, why, and how. Technical report, National Bureau of Economic Research.

Greenland, A. and J. Lopresti (2016). Import exposure and human capital adjustment: Evidence from the us. *Journal of International Economics 100*, 50–60.

Greenland, A., J. Lopresti, and P. McHenry (2019). Import competition and internal migration. *Review of Economics and Statistics 101*(1), 44–59.

Heckman, J. J. and P. A. LaFontaine (2010). The american high school graduation rate: Trends and levels. *The review of economics and statistics 92*(2), 244–262.

Jackson, C. K., R. C. Johnson, and C. Persico (2015). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics 131*(1), 157–218.

Lafortune, J., J. Rothstein, and D. W. Schanzenbach (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics 10*(2), 1–26.

Maralani, V. (2011). From ged to college: Age trajectories of nontraditional educational paths. *American Educational Research Journal 48*(5), 1058–1090.

Murnane, R. J. (2013). Us high school graduation rates: Patterns and explanations. *Journal of Economic Literature 51*(2), 370–422.

Murphy, F. and S. Ruh, Dalton Turner (2020). Work boots to combat boots: Mass layoffs and military enlistment (in progress).

Nunn, R., J. Parsons, and J. Shambaugh (2018). The geography of prosperity. *Place-Based Policies for Shared Economic Growth*, 11–42.

Olea, J. L. M. and C. Pflueger (2013). A robust test for weak instruments. *Journal of Business & Economic Statistics 31*(3), 358–369.

Oreopoulos, P., M. Page, and A. H. Stevens (2008). The intergenerational effects of worker displacement. *Journal of Labor Economics 26*(3), 455–483.

Pflueger, C. E. and S. Wang (2015). A robust test for weak instruments in stata. *The Stata Journal 15*(1), 216–225.

Rege, M., K. Telle, and M. Votruba (2011). Parental job loss and children's school performance. *The Review of Economic Studies 78*(4), 1462–1489.

Ruggles, S., S. Flood, R. Goeken, J. Grover, E. Meyer, J. Pacas, and M. Sobek (2019). Ipums usa: Version 9.0 [dataset]. *Minneapolis, MN: IPUMS*.

Snyder, T. D., C. De Brey, and S. A. Dillow (2018). Digest of education statistics 2016, nces 2017-094. *National Center for Education Statistics*.

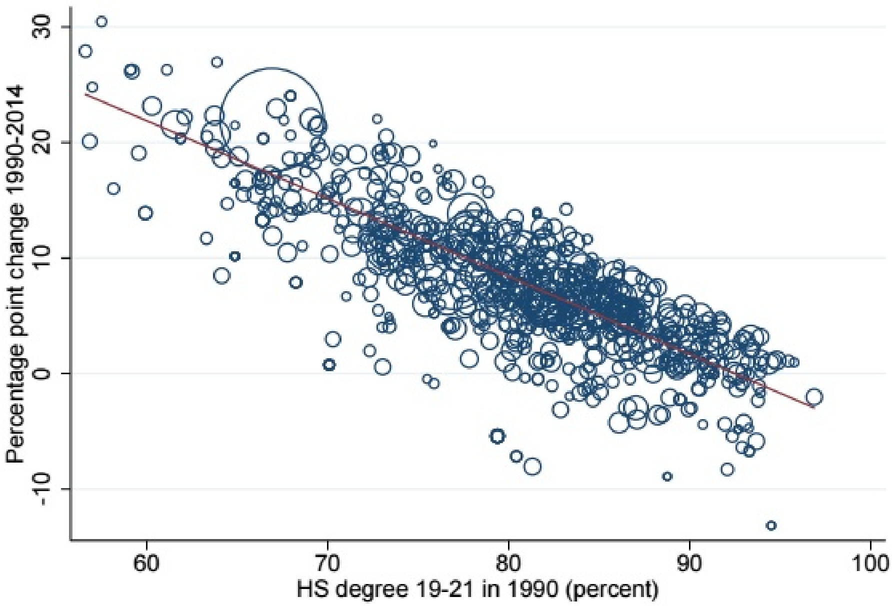
Tolbert, C. M. and M. Sizer (1996). Us commuting zones and labor market areas: A 1990 update. Technical report.

Tuhkuri, J. (2018). Children of crisis: The intergenerational effects of manufacturing decline. Technical report, MIT.

Turner, S. (2019). Convergence and divergence: The link between spatial labor market disparities and educational attainment. Technical report, University of Virginia.

Zuo, N., J. Schieffer, and S. Buck (2019). The effect of the oil and gas boom on schooling decisions in the us. *Resource and Energy Economics 55*, 1–23.

Figure 1. Convergence in high school (HS) Degree Attainment (16-19), 1990-2014



Notes: Data are from the ACS / Census enumerations and measured as a percent of the indicated population. Regression line indicated in red and initial CZ population represented by size of marker. Slope: 0.68

Table 1: Descriptive statistics

Variable	(1) 1990 Mean/SD	(2) 2000 Mean/SD	(3) 2007 Mean/SD
Chinese import exposure indicators			
(Imports from China to US)/(workers in 1990) (in kUS$)	0.29	1.32	3.56
	(0.32)	(1.18)	(2.79)
(Imports from China to US)/(workers in 2000) (in kUS$)	0.25	1.09	2.93
	(0.27)	(0.90)	(2.12)
American Community Survey (ACS)\Census variables			
Share of 16-19 population enrolled in high school (HS)	54.45	58.68	54.71
	(4.89)	(4.88)	(5.08)
Share of 16 population enrolled in HS	92.59	94.81	95.49
	(1.90)	(1.85)	(2.00)
Share of 17 population enrolled in HS	83.35	86.22	85.71
	(3.84)	(3.66)	(3.75)
Share of 18 population enrolled in HS	41.68	45.75	31.33
	(6.90)	(6.67)	(6.73)
Share of 19 population enrolled in HS	9.62	8.81	5.67
	(3.19)	(2.65)	(2.30)
Share of 18-19 population graduated from HS	60.99	58.20	72.05
	(7.05)	(6.47)	(6.10)
Share of 19-20 population graduated from HS	78.33	77.49	85.65
	(6.72)	(6.35)	(4.36)
Share of 20-21 population graduated from HS	81.63	80.35	87.17
	(6.66)	(6.37)	(4.15)
Share of 18-25 population graduated from HS	77.11	75.63	83.24
	(6.17)	(5.59)	(4.10)
Share of 18 population graduated from HS	44.93	41.41	60.67
	(7.59)	(7.01)	(7.99)
Share of 19 population graduated from HS	75.70	75.32	84.45
	(7.09)	(6.61)	(5.09)
Share of 16-19 population dropped out from HS	11.25	9.80	6.45
	(3.01)	(2.80)	(2.03)
Common Core of Data (CCD) variables			
Diplomas/17 population	64.67	62.73	66.02
	(12.64)	(11.85)	(12.73)
Diplomas/8th grade enrollment in t-4	77.16	74.33	76.48
	(12.41)	(12.83)	(12.42)
9th grade enrollment(t-3)/8th grade enrollment(t-4)	110.12	112.78	111.08
	(7.60)	(9.12)	(7.65)
10th grade enrollment(t-2)/8th grade enrollment(t-4)	101.97	100.11	100.02
	(7.98)	(6.57)	(5.76)
11th grade enrollment(t-1)/8th grade enrollment(t-4)	92.69	89.29	89.57
	(8.31)	(7.27)	(12.94)
12th grade enrollment(t)/8th grade enrollment(t-4)	84.01	81.47	85.11
	(9.06)	(9.89)	(13.46)

Note: 722 CZs. Statistics are weighted by 1990 CZ population. The variable diploma/8th grade enrollment hs 663, 695 and 684 observations in years 1990, 2007 and 2007, reflecting the trimming of ratios greater than one.

Table 2: Effect of local change in imports from China on high school enrollment and dropout rates by age groups, 1990-2007

	(1)	(2)	(3)	(4)	(5)
	Age group				
	16-19	16	17	18	19
Panel A: High school enrollment					
Δ Import exposure	0.859	0.332	1.034	1.211	0.176
	(0.230)***	(0.180)*	(0.264)***	(0.471)**	(0.137)
1990 mean (× 100)	54.4	92.6	83.2	41.6	9.6
Panel B: High school dropouts					
Δ Import exposure	-0.267	-0.152	-0.497	-0.448	0.175
	(0.143)*	(0.136)	(0.211)**	(0.242)*	(0.191)
1990 mean (× 100)	11.3	6.3	10.2	13.5	14.8
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational share measured from the ACS/Census enumerations. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Effect of local change in imports from China on high school enrollment by grade level, , 1990-2007

	(1) 9th grade(t-3)/ 8th grade(t-4) enrollment	(2) 10th grade(t-2)/ 8th grade(t-4) enrollment	(3) 11th grade(t-1)/ 8th grade(t-4) enrollment	(4) 12th grade(t)/ 8th grade(t-4) enrollment
Δ Import exposure	0.445	0.443	1.623	2.464
	(0.819)	(0.523)	(1.157)	(1.114)**
R-squared	0.191	0.163	0.303	0.218
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
1990 mean (× 100)	110.1	101.8	92.4	83.6

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational share measured from CCD. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates:Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Effect of local change in imports from China on the local share of high school graduates by age groups, 1990-2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age group						
	18-19	19-20	20-21	18-25	18	19	20
Δ Import exposure	-0.661	-0.286	-0.171	-0.151	-0.762	-0.351	-0.231
	(0.233)***	(0.199)	(0.176)	(0.134)	(0.407)*	(0.240)	(0.219)
R-squared	0.853	0.738	0.720	0.858	0.841	0.665	0.645
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1990 mean (× 100)	61.0	78.2	81.5	77.1	45.0	75.6	80.9

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational share measured from the ACS/Census enumerations. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of local change in imports on high school graduation ratio, 1990-2007

	(1)	(2)	(3)	(4)
	Diplomas/8th graders in t-4		Diploma/17 population	
Δ Import exposure	0.907	0.888	0.835	0.818
	(0.413)**	(0.421)**	(0.404)**	(0.409)**
R-squared	0.245	0.252	0.176	0.180
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Δ_{t-1} in dropout rates	No	Yes	No	Yes
First stage F	46.57	46.98	46.57	46.98
1990 mean ($\times$ 100)	64.45		64.45	

Note: N=1342 (unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. Diplomas and 8th grade enrollment measured from CCD and Pop17 is measured from ACS/Census enumerations. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational measure. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. The dependent variables are in percentages. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population.*** p<0.01, ** p<0.05, * p<0.1

Table 6: Replication of Greenland and Lopresti (2016) estimated effect of local change in imports from China on graduation ratio (diplomas divided by the number of 17 year-olds)

	(1)	(2)	(3)	(4)	(5)	(6)
	GL (2016) baseline results			BT replication		
	OLS	IV	IV	OLS	IV	IV
Panel A: Matching our observations to non-missing obs. in GL						
Δ Import exposure	1.34	2.47	3.55	0.93	1.77	3.07
	(0.35)***	(0.55)***	(1.65)**	(0.29)***	(0.45)***	(1.16)***
Observations	1,432	1,432	1,432	1,428	1,428	1,428
First stage F		162.3	21.78		162.3	21.78
Panel B: Same number of observations across both datasets						
Δ Import exposure	1.34	2.47	3.55	0.93	1.77	3.07
	(0.35)***	(0.55)***	(1.65)**	(0.29)***	(0.45)***	(1.16)***
Observations	1,428	1,428	1,428	1,428	1,428	1,428
First stage F		162.3	21.78		162.3	21.78
Year FE	No	No	Yes	No	No	Yes
Controls	No	No	No	No	No	No
CZ FEs	No	No	Yes	No	No	Yes

Note: In columns (1)-(3) we replicate GL (2016) using data they shared with us. In columns (4)-(6) we replicate Greenland and Lopresti (2016) main results using the variables we generated for this paper. In both cases the number of diplomas and 17 years-olds is obtained from CCD and Census/ACS, respectively. Standard errors clustered at the level of state and data are weighted by start of period share of national population. Although GL report using 2006-2008 ACS (as we do in this paper), we obtain closer estimates to theirs when we use the 2005-2007 period instead. These results are reported in Appendix Table A3. Our replication has 1,428 observations (more than the 1,342 we reported in other tables) because in this case we did not restricted our dependent variable to have the same number of observations than the trimmed variable (diploma/ 8th grade enrollment). The critical values in the Montiel-Olea and Pflueger weak IV test for 5% significance level are 37.42, 23.11, 15.06 and 12.04 for the 5%, 10%, 20% and 30% of worst case bias, respectively. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect of local change in imports from China on private enrollment by age groups, 1990-2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Private Enrollment/total population				Private Enrollment/School population			
	16-17	17-18	16-18	16-19	16-17	17-18	16-18	16-19
Δ Import exposure	-0.021	0.201	0.063	0.068	-0.095	0.130	-0.045	-0.040
	(0.120)	(0.106)*	(0.079)	(0.060)	(0.143)	(0.132)	(0.117)	(0.113)
R-squared	0.139	0.253	0.260	0.280	0.155	0.096	0.174	0.169
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
1990 mean (× 100)	7.8	5.4	6.3	4.8	8.7	8.4	8.6	8.5

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational share measured from the ACS/Census enumerations. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effect of local change in imports from China on migration outcomes, 1990-2007

	(1)	(2)	(3)	(4)	(5)
	Cohort relative size		Stay-rate of high school graduates to overall stay-rate		
	ln(Pop19-20/ Pop16-17)	ln(Pop19-21/ Pop16-18)	19 HSG/ (Diploma/Pop17)	18-19 HSG/ (Diploma/Pop17)	19-20 HSG / (Diploma/Pop17)
Δ Import exposure	-0.014	-0.013	-0.925	-1.660	-0.626
	(0.005)***	(0.005)***	(1.277)	(0.860)*	(1.444)
Observations	1,342	1,342	1,340	1,340	1,340
R-squared	0.356	0.398	0.120	0.419	0.109
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: Unbalanced panel with 690 CZs. Each column presents the results from the instrumental variable estimation described in Section 3. In columns (3)-(5) we excluded a CZ due to having outliers in the dependent variable. Population variables and high school graduation rates (HSG) measured from the ACS/Census, and diplomas measured from the CCD.The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated variable. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of local and state change in imports from China on K-12 expenditure per student, 1990-2007

	(1)	(2)	(3)	(4)	(5)	(6)
	Expenditure per student			ln(Expenditure per student)		
Δ Import exposure	-299.9		-317.2	-0.022		-0.022
	(116.7)**		(126.5)**	(0.008)**		(0.008)***
Δ Import exposure in state		-655.5			-0.051	
		(254.2)***			(0.017)***	
Δ Import exposure in rest of state			-77.4			-0.021
			(226.0)			(0.013)*
R-squared	0.360	0.411	0.369	0.197	0.265	0.244
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. Variables in 2015 constant dollars. Average expenditure in 1990: $8,623. K12 expenditure measured from CCD. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational measure. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Variables Import exposure in state and Import exposure in rest of state were obtained from Feler and Senses (2017). Standard errors clustered at the level of state and data are weighted by start of period share of national population.*** p<0.01, ** p<0.05, * p<0.1

Appendix

Table A1: Effect of local change in imports from China on school (high school or college) enrollment by age groups, 1990-2007

	(1)	(2)	(3)	(4)	(5)
	Age group				
	16-19	16	17	18	19
Δ Import exposure	0.729	0.233	0.718	1.056	0.490
	(0.185)***	(0.143)	(0.180)***	(0.322)***	(0.298)*
R-squared	0.256	0.120	0.001	0.217	0.481
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
1990 mean (× 100)	77.2	93.1	88.2	73.5	57.4

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational share measured from the ACS/Census enumerations. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Heterogeneous effect of local change in imports from China by maximum compulsory attendance age, 1990-2007

	(1)	(2)	(3)	(4)
	Age group			
	16	17	18	19
Panel A: High school dropouts				
Δ Import exposure	-0.0521	-0.4517	0.2259	0.7056
	(0.110)	(0.201)**	(0.275)	(0.281)**
Δ Import exposure × Max CA age 16-17	-0.1531	-0.0679	-1.0235	-0.8038
	(0.153)	(0.191)	(0.240)***	(0.253)***
1990 mean (× 100)	6.3	10.2	13.5	14.8
First stage F	16.7	16.7	16.7	16.7
Panel B: High school enrollment				
Δ Import exposure	0.3094	0.8556	0.9508	0.1799
	(0.170)*	(0.514)*	(0.729)	(0.195)
Δ Import exposure × Max CA age 16-17	0.0364	0.2707	0.3952	-0.0064
	(0.187)	(0.586)	(0.685)	(0.241)
1990 mean (× 100)	92.6	83.2	41.6	9.6
First stage F	16.7	16.7	16.7	16.7
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: N=1,342 (Unbalanced panel with 690 CZs). Each column presents the results from the instrumental variable estimation described in Section 3. The dependent variable is the interval difference (2000-1990 and 2007-2000) for the indicated educational share measured from the ACS/Census enumerations. Import exposure, measured in millions of dollars per capita, is the focal explanatory variable. Max CA age 16-17 is a dummy variable that takes value of 1 for states that have 16 or 17 as maximum compulsory attendance age, and 0 for states in which the maximum compulsory attendance age is 18. Covariates follow Autor, Dorn and Hanson (2013) 2SLS estimates: Percentage of employment in manufacturing, percentage of college educated population, percentage of foreign-born population, percentage of employment among women, percentage of employment in routine occupations, average offshorability index of occupations and census division dummies. Standard errors clustered at the level of state and data are weighted by start of period share of national population. First-stage F-statistic: 46.57. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Replication of Greenland and Lopresti (2016) using 2005-2007 ACS instead of 2006-2008 ACS.

	(1)	(2)	(3)	(4)	(5)	(6)
	GL (2016) baseline results			BT replication		
	OLS	IV	IV	OLS	IV	IV
Panel A: Matching our observations to non-missing obs. in GL						
Δ Import exposure	1.34	2.47	3.55	1.16	2.04	3.25
	(0.35)***	(0.55)***	(1.65)**	(0.25)***	(0.38)***	(1.17)***
Observations	1,432	1,432	1,432	1,428	1,428	1,428
First stage F		162.3	21.78		162.3	21.78
Panel B: Same number of observations across both datasets						
Δ Import exposure	1.34	2.47	3.55	1.16	2.04	3.25
	(0.35)***	(0.55)***	(1.65)**	(0.25)***	(0.38)***	(1.17)***
Observations	1,428	1,428	1,428	1,428	1,428	1,428
First stage F		162.3	21.78		162.3	21.78
Year FE	No	No	Yes	No	No	Yes
Controls	No	No	No	No	No	No
CZ FEs	No	No	Yes	No	No	Yes

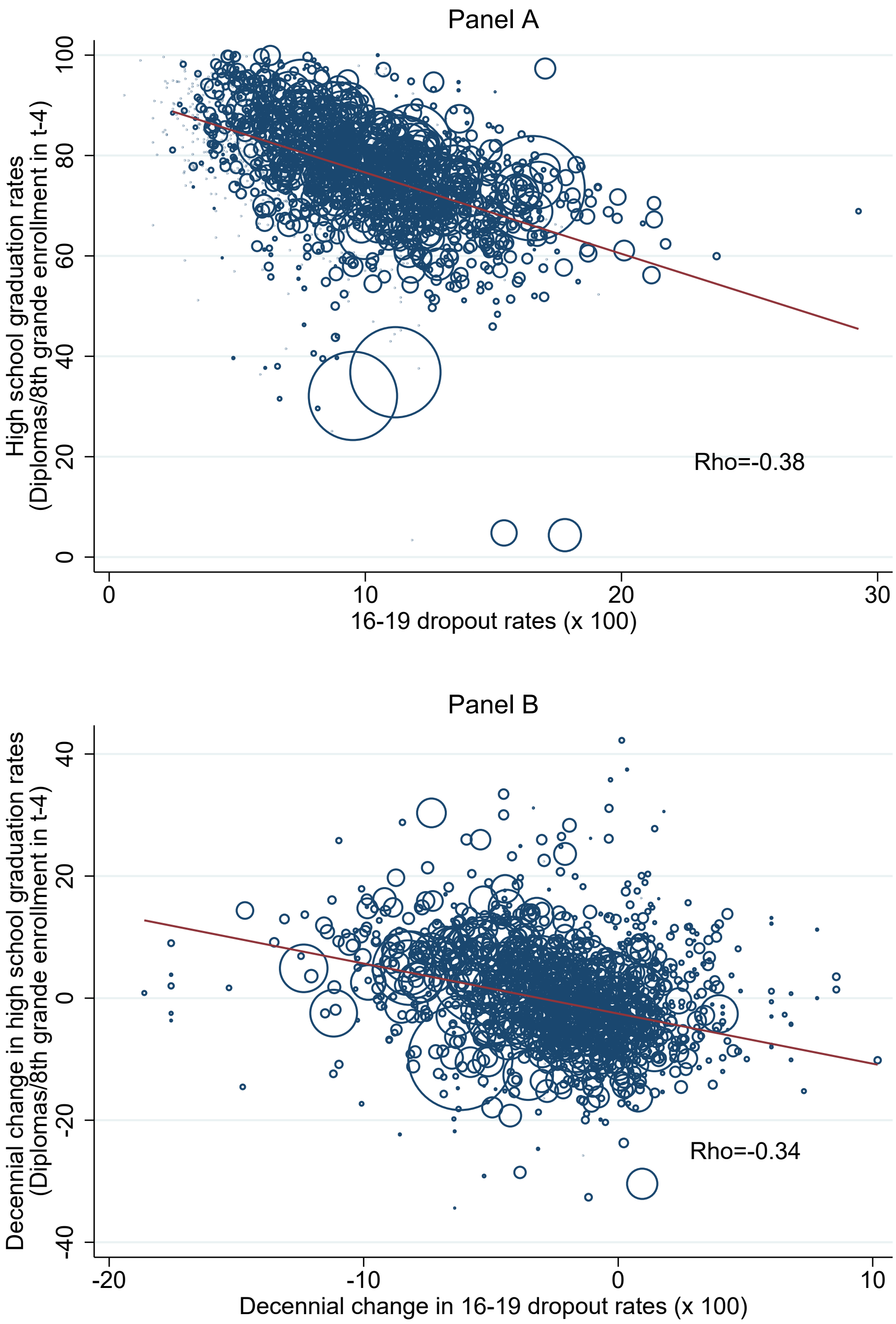
Note: In columns (1)-(3) we replicate GL (2016) using data they shared with us. The difference with Table 6 is that in this table we use 2005-2007 ACS to represent 2007, while in Table 6 (and the rest of the paper) we use 2006-2008 ACS. In columns (4)-(6) we replicate Greenland and Lopresti (2016) main results using the variables we generated for this paper. In both cases the number of diplomas and 17 years-old is obtained from CCD and ACS/Census, respectively. Standard errors clustered at the level of state and data are weighted by start of period share of national population.*** p<0.01, ** p<0.05, * p<0.1. The critical values in the Montiel-Olea and Pflueger weak IV test for 5% significance level are 37.42, 23.11, 15.06 and 12.04 for the 5%, 10%, 20% and 30% of worst case bias, respectively

Table A4: Replication of effects of Imports from China and on Manufacturing Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pure replication						
Δ Import exposure	-0.7460	-0.6104	-0.5376	-0.5080	-0.5625	-0.5964
	(0.068)***	(0.094)***	(0.091)***	(0.081)***	(0.096)***	(0.099)***
Percent. of employmentin manufacturing $_{-1}$		-0.0355	-0.0521	-0.0613	-0.0563	-0.0402
		(0.022)	(0.020)***	(0.017)***	(0.016)***	(0.013)***
Percent. of college educated population $_{-1}$				-0.0082		0.0131
				(0.016)		(0.012)
Percent. of foreign born population $_{-1}$				-0.0071		0.0304
				(0.008)		(0.011)***
Percent. of employment among women $_{-1}$				-0.0541		-0.0059
				(0.025)**		(0.024)
Percent. of employment in routine occupations $_{-1}$					-0.2301	-0.2449
					(0.063)***	(0.064)***
Avg. offshorability index of occupations $_{-1}$					0.2440	-0.0590
					(0.252)	(0.237)
Panel B: Adding CZ fixed effects						
Δ Import exposure	-1.0649	-1.1912	-1.1912	-0.9448	-1.1572	-0.9462
	(0.211)***	(0.257)***	(0.257)***	(0.218)***	(0.255)***	(0.217)***
Percent. of employmentin manufacturing $_{-1}$		-0.0870	-0.0870	-0.0391	-0.0661	-0.0452
		(0.046)*	(0.046)*	(0.048)	(0.050)	(0.045)
Percent. of college educated population $_{-1}$				-0.0562		-0.0581
				(0.068)		(0.064)
Percent. of foreign born population $_{-1}$				-0.0150		0.0118
				(0.082)		(0.066)
Percent. of employment among women $_{-1}$				-0.3331		-0.3347
				(0.095)***		(0.090)***
Percent. of employment in routine occupations $_{-1}$					-0.3639	0.1635
					(0.127)***	(0.156)
Avg. offshorability index of occupations $_{-1}$					-0.0584	-0.2476
					(0.798)	(0.664)
Number of czone	722	722	722	722	722	722
Census division dummies	No	No	Yes	Yes	Yes	Yes

Note: N=144 (722 commuting zones × 2 periods). Panel A replicates Autor, Dorn and Hanson (2013) Table 3. In Panel B we add Commuting Zone (CZ) fixed effects to the specification.

Figure A1: Correlations in level and decennial differences between high school graduation rates and 16-19 dropout rates (1990-2007)



Note: Every dot represents a CZ. Diploma and enrollment variables are constructed using CCD district level information. Panel A illustrates the unconditional correlation derived from pooled 1990-2007 data. Panel B stack differences for the two periods , 1990 to 2000 and 2000 to 2007. Dropout rates are constructed from ACS/Census. Variables constructed from CCD show fewer number of observations due to missing values in some districts.

Data Appendix

Census and American Community Survey (ACS)

For 1980, 1990 and 2000, we employ the 5% sample of the U.S. Census and, for 2007, the 2008 3-year ACS (Ruggles et al, 2019). The 2008 3-year ACS puts together the yearly ACS from 2006, 2007 and 2008 and adjust the individual weights such that the estimated indicators estimated from this dataset are representative for this period. The aggregation at the county and commuting-zone level was done using the crosswalk files provided by Autor and Dorn (2013).

Common Core of Data (CCD)

The ELSI table generator provided by the National Center for Education Statistics (NCES) is used to extract the CCD observations (to access the Elsi table generator go to https://nces.ed.gov/ccd/elsi/tableGenerator.aspx)

Diploma counts and enrollment information. We extracted the number of diplomas (Completers section) and enrollment (Enrollment section) variables at the school district level as well as the county codes from the table generator for the periods 1988-1991, 1999-2001 and 2006-2008. The information we use covers only public schools (information for private schools is available only since 1997). The number of diplomas is reported by school districts themselves. The by-grade enrollment variables that we use are the result of aggregating enrollment across schools within a district (NCES makes the aggregation). Schools report by-grade enrollment in a point in time, which is typically October. In order to create smoothed estimates of every variable for each year in our analysis (1990, 2000 and 2007), we averaged the extracted information over the three years of non-missing information we had within each period. After this, we kept those districts for which there was information in the three years of analysis so we constructed a balanced panel at the district level. Then we aggregated the information at the county level and collapsed it at the commuting-zone level using Autor and Dorn (2013) crosswalk files.

Expenditure per student. We used the "Total Current Expenditures per pupil" at the district level provided in the General Finance section for the years 1989, 1991, 2000 and 2007. Because there is no data for 1990, information for this year was approximated by interpolating information from 1989 and 1991. Then we aggregated the data at the county level and collapsed it at the commuting-zone level using Autor and Dorn (2013) crosswalk files.