Do Human Capital Adjustments Protect Youths from Structural Change?

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Abstract

Structural changes to labor demand can have lasting consequences on the employment and earnings of workers in affected industries and geographies. However, individuals coming of age may avoid similar fates if they internalize salient changes to the returns to education and adjust their human capital investments. This paper studies the effects of exposure to structural labor demand shocks during youth and adolescence on human capital accumulation and later-life earnings. I use student-level administrative data from Texas and a modified difference-in-differences design that compares changes in outcomes across cohorts of students living in areas that were more or less exposed to Chinese import competition. Students exposed to larger shocks were 4% more likely to enroll in college and 8% more likely to earn a bachelor's degree. I provide evidence that these adjustments, along with shifts of fields of study away from those directly exposed to import competition in both high school and college, shielded students from more than 90% of the shock's negative effects on later-life earnings. My results contribute a silver lining to the gloomy findings of prior work on the long-term effects of "the China shock" and other negative labor demand shocks: if individuals coming of age sufficiently adjust their human capital investments, they can emerge relatively unscathed.

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1 Introduction

Structural changes to an economy often create clear winners and losers. While substantially lowering consumer prices (Jaravel and Sager, 2019), trade liberalization with China caused lasting declines in employment and earnings for low-skilled workers in exposed industries and local labor markets (Autor et al., 2014; Pierce et al., 2022). Whether individuals coming of age avoided the same fate likely depended on their ability to make extensive-margin (i.e., attending college) and intensive-margin (i.e., field of study) adjustments to their human capital investments. As continued automation, decarbonization, and other looming changes to the U.S. economy threaten to bear asymmetric benefits and costs across workers and local labor markets, understanding the degree to which human capital adjustments can protect against adverse labor market outcomes is particularly important.

In this paper, I study the effects of exposure during youth and adolescence to structural declines in local labor demand generated by Chinese import competition (i.e., the "China shock") on human capital accumulation and later-life earnings. Using linked student-level administrative data from Texas, I find that students from counties exposed to larger local shocks were 4% more likely to enroll in college and 8% more likely to earn a bachelor's degree. I provide evidence that these adjustments, along with shifts of fields of study away from those directly exposed to import competition in both high school and college, shielded students from over 90% of the decline in adult earnings experienced by individuals that had already made key educational decisions prior to the onset of the shock. These results suggest a silver lining to the gloomy findings of prior work on the long-term effects of the China shock (e.g., Autor et al., 2021) and other structural changes (e.g., Choi, 2024): if individuals coming of age sufficiently adjust their human capital investments, they can emerge relatively unscathed.

My research design exploits quasi-random variation in exposure to changes in local labor demand based on a change in U.S. trade policy in October 2000 – formally, the establishment of "Permanent Normal Trade Relations" (PNTR) with China. PNTR exposed subsets of domestic manufacturing firms to increased competition from Chinese exporters (Pierce and Schott, 2016a). This unexpected policy change resulted in larger negative labor demand shocks in counties with more firms specializing in exposed subsets of manufactured goods (e.g., toys and games) compared to those with similar levels of manufacturing employment, but with firms producing less-exposed product specialties (e.g., processed foods) (Pierce and Schott, 2020; Greenland et al., 2019).

Building off of Pierce and Schott (2016a, 2020), I leverage this variation in a two-step "detrended" difference-in-differences specification (Goodman-Bacon, 2021). I compare changes in educational and labor market outcomes across cohorts of students that reached critical ages before and after the policy change (first difference) in counties that were more- and less-exposed to PNTR (second difference) relative to existing linear trends. My preferred specification controls for individual-level demographics and allows for baseline county characteristics to flexibly affect outcomes across cohorts, but my main results are robust to excluding covariates or controlling for exposure to subsequent labor demand shocks. Identification requires the assumption that differences in outcomes of students from counties that experienced large and small shocks would have continued to evolve along existing differential linear trends if both groups were exposed to small labor demand shocks (Callaway et al., 2021; Goodman-Bacon, 2021).[1] I provide support for causal interpretation by showing that differences in *fixed* student characteristics across counties that later experienced larger and smaller shocks continued along existing trends across cohorts after the policy change.[2] Moreover, I show robustness to relaxing this assumption by allowing potential outcomes to trend in parallel after the onset of the shock and by allowing smooth deviations in potential outcomes from existing linear trends (Rambachan and Roth, 2023).

To inform interpretation of my main estimates, I first confirm that Chinese import competition had similar effects on local labor markets in Texas as previous research finds nationwide.[3] Counties with above-median exposure to PNTR experienced a 3.0 percentage-point

[1]This essentially combines two assumptions: (1) outcomes of students from counties that later experience larger and smaller shocks would continue along existing differential linear trends in the absence of PNTR and (2) students from both groups of counties would respond similarly on average to a similarly sized labor demand shock (Callaway et al., 2021).

[2]This exercise is analogous to standard difference-in-differences placebo analyses (i.e., putting outcomes measured prior to treatment on the left-hand side) but adapted to my two-step specification. An alternative interpretation of the exercise is that it tests for selection on observables into treatment.

[3]Previous research on the local labor market effects of trade liberalization with China finds that more exposed localities experienced persistent decreases in employment and earnings, particularly among low-skilled workers and workers in exposed manufacturing subsectors (Autor et al., 2013, 2015; Pierce and Schott, 2016a; Greenland and Lopresti, 2016; Autor et al., 2021).

decline in their employment-to-population ratio relative to those with below-median exposure. On average, more-exposed counties saw an 8% decline in earnings for workers without a college degree and an 18% decline in earnings for workers between the ages of 15 and 24. These substantial declines in the opportunity costs of schooling, along with increases in the college earnings premium, likely incentivized marginal students to pursue a postsecondary education instead of entering the labor force. On the other hand, declines in family income may have inhibited their ability to do so. I find that greater exposure to the shock caused an 8% increase in student eligibility for free-or-reduced-price lunch, a proxy for low-income status. These effects are similar in size to estimates of earnings and employment losses across the country caused by the same shock and to those caused by a recession on exposed local labor markets.[4]

In my primary analyses, I first examine whether students made adjustments to their human capital investments in high school. Although I find that local shocks did not affect the likelihood students graduated high school – consistent with prior estimates using school-level graduation counts (Burga and Turner, 2022), I provide evidence of substantial intensive-margin human capital adjustments by forward-looking students along novel margins.[5] In high school, exposed students reduced their enrollment in manufacturing-aligned vocational elective courses and took more courses at local colleges through dual-enrollment. These responses suggest that students internalized both reductions in long-term earnings prospects in industries directly exposed to the labor demand shock and increases in the college earnings premium.

Following students beyond high school, I estimate that greater exposure to local shocks caused a 1.8 percentage-points (4%) increase in the likelihood of enrolling in a public college

[4] Autor et al. (2021) finds that the China shock reduced overall employment relative to population by nearly 2 percentage-points in exposed local labor markets, and Greenland and Lopresti (2016) find the shock decreased earnings for workers without a college degree by 6%. Moreover, Hershbein and Stuart (2024) show that each recession since the 1970s corresponded to approximately a 3 percentage-point employment decline in counties with above-median exposure.

[5] Greenland and Lopresti (2016) and Burga and Turner (2022) both examine the effects of the China shock on high school graduation rates using aggregated graduation counts but come to different conclusions. Greenland and Lopresti (2016) find that graduation rates increase by 3.6 percentage-points in local labor markets exposed to the China shock; however, Burga and Turner (2022) provide evidence that this result can be mostly explained by outmigration and weak instrument bias.

in Texas.[6] This magnitude is comparable to previously estimated enrollment effects of smaller elementary school classrooms (Chetty et al., 2011) or of a $1,000 increase in need-based financial aid (Castleman and Long, 2016).[7] By age 25, exposed students accumulated 5% more cumulative semesters and were 1.1 percentage-points (8%) more likely to have earned a bachelor's degree. Just as in high school, students adjusted their choice of fields of study away from those feeding into employment in industries directly exposed to import competition.

Evidence of the persistence of the China shock's negative effects on earnings of prime-aged workers (Autor et al., 2021) and of the lasting earnings losses for individuals coming of age during historical periods of structural change (Choi, 2024) raise the question of whether the above human capital adjustments translated into improved labor market outcomes for young adults exposed to the shock. I show that exposure to larger local shocks caused earnings losses averaging $1,248 annually over the next ten years for young workers already in the labor market. However, exposed students young enough to adjust their educational decisions experienced statistically significant relative earnings gains large enough to compensate for over 90% of these losses. This suggests that human capital adjustments nearly fully buffered against lasting harm from the shock, despite the persistence of its negative effects on overall per-capita earnings and employment rates in exposed local labor markets.

Estimates of the average effects of the shock on human capital accumulation and later-life earnings may mask substantial harm on "left-behind" students. Therefore, I examine heterogeneous responses by particularly vulnerable subgroups of students. I find that male students, students from low-income households, and students identifying as racial or ethnic minorities all adjusted their human capital investments in manners that yielded substantial protection against the labor demand shock.

[6] I find that exposed students were 1.2 percentage-points more likely to enroll in a two-year Texas public community or technical college and 1.6 percentage-points more likely to enroll in a Texas four-year public university by age 20. I do not observe if a student enrolls in a college outside of Texas, and the interpretation of my estimates as representing extensive effects on college enrollment would be threatened by substitution from out-of-state or private colleges into in-state public institutions. I provide evidence supporting that such substitution did not occur in Section 7.

[7] Chetty et al. (2011) find that assignment to small classes (averaging 15 students, rather than 22) for grades K-3 through the Tennessee STAR experiment caused a 1.8 percentage-point increase in college enrollment. Castleman and Long (2016) find that a $1,995 (2020$) increase in need-based aid in Florida caused a 3.2 percentage-point increase in enrollment at public four-year universities. Assuming linearity, my estimated effect on four-year enrollment of 1.6 percentage-points scaled by their coefficient corresponds to a $998 increase in aid.

This paper contributes to an existing literature on the effects of structural changes to labor demand on individuals coming of age. Previous work finds that increases in labor market opportunities for workers with less education cause students to obtain less schooling (Atkin, 2016; Kovalenko, 2023; Cascio and Narayan, 2022; Black et al., 2005; Charles et al., 2018), but the mixed evidence on whether students obtain more education in response to declines in such opportunities suggests potential frictions to “upward” adjustments of human capital investments (Choi, 2024; Burga and Turner, 2022; Greenland and Lopresti, 2016; Ferriere et al., 2018; Di Giacomo and Lerch, 2023). My results reveal adjustments along novel margins – in particular, high school vocational electives and dual-enrollment – that suggest students internalized salient changes in the returns to education across fields of study and attainment levels. Students may also have made similar adjustments in other settings where previous research finds limited responses on “extensive” margins of educational attainment (Burga and Turner, 2022; Ferriere et al., 2018). Thus, “null” results in these settings may still reflect meaningful changes in human capital accumulation. Moreover, my results support prior findings from this literature – notably, that the China shock had little effect on high school graduation (Burga and Turner, 2022) but increased college enrollment (Ferriere et al., 2018) and that industry-specific labor demand shocks affect college major choice (Acton, 2021; Weinstein, 2022).

My larger contribution, enabled by the administrative linkages to earnings records, is to provide evidence that educational adjustments in my setting translated into labor market benefits in adulthood. Stuart (2022) finds that children growing up in local labor markets hit hard by “douple-dip” recessions of the early 1980s obtained less education and earned less as adults and attributes these effects to declines in childhood human capital investments translating to lower educational attainment. Studying a similar structural shock to labor demand as the China shock, Choi (2024) finds that exposure to deindustrialization during the 1920s in the Northeast caused students to obtain more education; however, she finds no evidence that the additional human capital yielded labor market benefits. The ex ante “permanence” of the structural changes studied in Choi (2024) and in this paper likely provided more salient changes to the returns to education compared to the business cycle shock studied in Stuart (2022), potentially contributing to why only the former two find positive effects on educational attainment.[8] Moreover, the broader scope of labor market opportunities outside of manufacturing and the larger returns to education in the 21st- compared to 20th-century U.S. economy may explain the differences in the earnings results of Choi (2024) and this paper.[9] The robust earnings benefits to human capital adjustments that I find are in line with evidence of the labor market returns to re-training for older workers displaced by trade (Hyman, 2018) and of the high returns to a college education for marginal enrollees (Goodman et al., 2017). My findings also complement those of Kovalenko (2023), who shows that high school students in local areas benefiting from the fracking boom attained less education than their peers from nonfracking areas but experienced earnings gains through at least their mid-twenties. In both cases, the sustained earnings gains suggest adolescents made optimal adjustments to changes in local labor demand.

This paper also contributes to existing research on the China shock. In contrast to classical trade models, this literature finds substantial and persistent consequences of exposure to Chinese import competition on exposed local labor markets and workers (e.g., Autor et al., 2014, 2021; Pierce and Schott, 2020). I provide evidence that, at least in Texas, individuals coming of age largely avoided such harm. My results align with existing evidence that other household members adjusted more easily to the China shock than displaced manufacturing workers (Ahlquist and Downey, 2023).[10] More broadly, they suggest that the extent to which the costs of other skill-biased technological changes (e.g., automation) transmit to youths may also depend on the ability of students to adjust their human capital investments and the presence of policies to support these investments.[11]

[8]However, Hershbein and Stuart (2024) that local labor markets most-exposed to recessions exhibit *ex-post* sustained declines in employment and earnings, matching the local dynamics in areas affected by the China shock.

Other differences in my empirical setting and that of (Stuart, 2022) may contribute to differences in our respective estimates of the effects of exposure to negative labor demand shocks on college degree receipt and later-life earnings. These include the greater presence of need-based financial aid programs in my sample period and the presence of Texas' "Robin Hood" K-12 finance system, which prevented declines in local property values from manifesting in declines in school spending.

[9]Feigenbaum and Tan (2020) find a 4% annual earnings increase for an additional year of schooling in 1940, falling below the consensus range of 6 to 15% found by Card (1999) in more recent settings. Moreover, Autor et al. (2020) documents college earnings premia of more than 10 log points higher in the 2000s than in 1940.

[10]Ahlquist and Downey (2023) find that spouses and children of manufacturing workers exposed to the China shock were more likely to find work in education, social work, and health care.

[11]I discuss what such policies might resemble in Section 8

2 Background

My research design leverages variation in declines in local labor demand resulting from a change in U.S. trade policy toward China in 2000.[12] The U.S. has historically subjected goods imported from foreign countries to one of two sets of tariff rates. Goods imported from fellow members of the World Trade Organization (WTO) are subject to relatively low "column 1" rates (hereafter, "preferred" tariff rates), while goods from nonmarket economies are subject to relatively high "column 2" rates (hereafter, "punitive" tariff rates) set by the Smoot-Hawley Tariff Act of 1930. In 1999, preferred tariff rates averaged 4% and punitive rates averaged 37%. Moreover, the difference between the preferred and punitive tariff rates (the "tariff gap") varied widely by type of good, ranging from a 0 to 80 percentage-point difference.

The President may annually extend preferred tariff rates to nonmarket economies, although Congress can pass legislation to block such an extension. The U.S. first granted preferred tariff rates to Chinese imports in 1980; however, following the Tiananmen Square incident in 1989, Congressional approval of these preferred tariff rates became a politically contentious process.[13] The political uncertainty surrounding trade policy with China permeated into the operations of U.S. firms, and those producing goods subject to large tariff gaps were particularly unlikely to outsource production to China due to the prospect of reversal to punitive tariff rates in any given year (Pierce and Schott, 2016a).[14]

In October 2000, Congress passed a bill to establish "Permanent Normal Trade Relations" (PNTR) with China, permanently locking in preferred tariff rates for Chinese goods imported into the U.S., and the following December, China formally joined the World Trade Organization (WTO). In response to the differential change in incentives, the real value of U.S. imports from China increased for goods with high tariff gaps, such as toys, relative to those with low tariff gaps, such as processed foods (Pierce and Schott, 2016a). Intuitively,

[12] Much of this discussion follows (Pierce and Schott, 2016a).

[13] Legislation to block preferred tariff rates from being applied to Chinese goods was voted on in the U.S. House of Representatives annually from 1990 until 2001 and passed the House in 1990, 1991, and 1992, but never passed the Senate.

[14] The chilling effect of uncertainty over tariff rates on U.S. firms is evident by a 1993 letter from CEOs of 340 U.S. firms to President Clinton describing the annual tariff renewal process as creating "an unstable and excessively risky environment fo US companies considering trade and investment in China" (Rowley, 1993)

areas with larger shares of workers in industries with high tariff gaps experienced larger reductions in local labor demand due to import competition following the policy change: Pierce and Schott (2020) find that an interquartile shift in a county's tariff gap was associated with approximately a 1 percentage-point increase in the unemployment rate and a 1.5 percentage-point decline in labor force participation by 2007. Overall, the real value of Chinese imports nearly tripled by 2007; during this same period, U.S. domestic manufacturing employment fell by over 3 million workers.

Research on the China shock finds that exposed local labor markets experienced increases in plant closures and sharp and sustained reductions in employment, labor force participation, and income (Acemoglu et al., 2014; Autor et al., 2013, 2021). Employment and earnings losses were concentrated among low-skilled and less-educated workers (Autor et al., 2013, 2014; Acemoglu et al., 2014; Pierce and Schott, 2016a), and the existing literature provides mixed evidence as to whether such effects spilled over into industries outside of manufacturing.[15] These effects persisted through at least 2016 (Autor et al., 2021). Additionally, more exposed labor markets experienced reductions in housing prices and public spending (Feler and Senses, 2017) and increases in fatal drug overdoses (Pierce and Schott, 2020; Autor et al., 2019).

The above literature on the local labor market effects of the China shock suggest multiple channels through which exposure to the shock could affect human capital accumulation according to canonical human capital theory (Becker, 1962). First, diminished *contemporaneous* labor market opportunities for school-aged workers represent declines to the opportunity costs of schooling, incentivizing the extensive-margin adjustments of high school completion and college attendance. Second, the shock's ex ante "permanence" and incidence on workers

[15] Using a shift-share design and aggregate Census data, Autor et al. (2013) find that local labor markets (defined as commuting zones) exposed to the China shock experienced decreases in non-manufacturing employment relative to population, and this reduction statistically differs from zero for workers without a college education. Bloom et al. (2019) utilize the same design and establishment-level data from the Census Bureau's Longitudinal Business Database and find commuting-zone-level exposure to the China shock resulted in small increases in non-manufacturing employment. Both papers find reductions in manufacturing and overall employment in exposed local labor markets. Moreover, Ahlquist and Downey (2023) find that spouses and children of manufacturing workers exposed to the China shock were more likely to find work in education, social work, and health care. Pierce et al. (2022) show that workers employed outside of manufacturing even experienced earnings gains in counties with large clusters of employment "downstream" of exposed manufacturing industries, due to reductions in input costs.

with low levels of education and those in the manufacturing sector plausibly shifted expected *lifetime* earnings across education levels and fields of study. Such changes to earnings premia may cause both extensive-margin and intensive-margin changes to human capital investments. Decreased expected earnings for high school dropouts and high school graduates relative to those with a college education would increase the expected college earnings premium, further incentivizing college enrollment and – to the extent that students are forward-looking – intermediate changes to high school course-loads that support college preparation. Concurrently, decreases in expected earnings for workers in sectors directly exposed to the labor demand shocks, such as manufacturing, would reduce the expected returns for studying fields that lead to employment in these industries and incentivize students to adjust their course and major selection away from such fields.

Finally, the negative shock to local labor demand may tighten constraints on "external" investments in human capital by a student's family and school.[16] The institutional details of my setting allow me to examine the effects of a negative shock to local labor demand while holding constant – in relative terms – K-12 (kindergarten through 12th-grade) school spending.[17] Since 1993, Texas has employed a relatively unique school finance equalization system known as the "Robin Hood" plan. Under order by the Texas Supreme Court, the state education agency equalizes spending across districts by redistributing excess funding from property-wealthy districts toward property-poor districts.[18] As a result of this policy, even though K-12 education in Texas is largely funded through property taxes, school spending is nearly orthogonal to changes in *local* property wealth. I show in Section 4.2 that the Robin Hood formula shielded students in counties exposed to adverse local shocks from reductions in school spending that would typically accompany declines in local property wealth. However, I also provide evidence that students from more-exposed counties experienced declines in family income. This may particularly hinder students from adjusting by attending college,

[16]Both theoretical and empirical research provides evidence that investments in human capital during childhood can yield substantial gains in later-life skills and earnings through dynamic complementarities (Heckman and Mosso, 2014; Almond et al., 2018; Johnson and Jackson, 2019; Stuart, 2022)

[17]See Jackson (2018) for a review of the recent literature on the causal effects of school spending on student outcomes.

[18]The Robin Hood school finance equalization system was originally adopted in response to the Texas Supreme Court case *Edgewood Independent School District v. Kirby*. Appendix C.1 details the Robin Hood system.

which often requires a substantial monetary cost.[19]

Although unique in some manners, my research setting of Texas is inherently important. Nearly 11% of all kindergarten through 12th-grade (K-12) students and 10% of full-time college students in the U.S. attend school in Texas, making the state an inherently relevant setting to study the effects of labor demand shocks on human capital accumulation. Public K-12 schools in Texas differed from those of the U.S. as a whole in the years before the trade shock in terms of student demographics – 40% of students in public K-12 schools in Texas were Hispanic, as opposed to only 16% across the country overall – but received similar per-pupil funding levels and produced similar high school graduation rates. Students at both two- and four-year colleges in Texas also exhibited similar differences in demographics from U.S. college students as a whole. Although graduation rates trailed at Texas colleges, funding levels mirrored nationwide averages.[20]

3 Data

My primary dataset consists of administrative student-level data from the University of Houston Education Research Center (UHERC), featuring linked individual-level K-12, postsecondary, and workforce records from 1994 to present. K-12 data from the Texas Education Agency include standardized test scores, course schedules, graduation records, and demographic information for all students who attended public schools in Texas. Postsecondary data from the Texas Higher Education Coordinating Board include enrollment, declared majors, and degree and certificate records from all public colleges and universities in the state. Finally, wage reports from the Texas Workforce Commission include monthly earnings and employment records by employer for all Texas workers earning wages in positions covered by

[19]Existing research finds mixed evidence on the degree to which parental job loss affects college enrollment. Hilger (2016) analyzes 7 million fathers' layoffs from 2000 to 2009 in the U.S. and finds paternal layoffs during adolescence caused only a 0.5 percentage-point reduction in college enrollment. On the other hand, Coelli (2011) studies a sample of Canadian adolescents from 1993 to 2007 and finds that parental layoffs were associated with to a 10 percentage-point decline in the probability of university enrollment.

In addition to the household income channel, parental job loss may also affect human capital investments by shifting student preferences. Ahlquist and Downey (2023) provide evidence parental exposure to the China shock caused children to be less likely to work in manufacturing, and Huttunen and Riukula (2024) find that paternal job loss caused children in Finland to be less likely to choose their father's field of study.

[20]Appendix Table C1 compares K-12 and college student demographics, K-12 spending, postsecondary tuition and appropriations, and high school and college graduation rates for Texas and the U.S. overall.

state unemployment insurance.[21]

To build my analysis sample, I start with cohorts of students that entered ninth grade at Texas public high schools since Fall 1995. Two factors related to the nature of the administrative data determine further selection. First, I only observe students attending public schools, colleges, and universities in Texas; hence, students who permanently leave Texas attrit from the dataset. I discuss the implications of this for identification in Section 4. Second, student linkages across time allow me to define exposure to the trade shock based on a student's county of residence prior to 2000 to avoid bias due to endogenous migration out of counties exposed to larger shocks. Thus, I limit my analysis samples to students observed prior to the start of the shock, which also bounds the time horizon over which treatment effects can be estimated. The panel begins with students in ninth grade in Fall 1995 and ends with those in ninth grade in Fall 2008.[22]

For "first-stage" estimations of the effects of PNTR on local labor market conditions and K-12 funding, I use data on employment and earnings from the Census County Business Patterns and Quarterly Workforce Indicators datasets, income data from the Bureau of Economic Analysis Regional Economic Accounts database, and school district finance data from the National Center for Education Statistics. I provide additional details on these supplemental data sources in Appendix B.

4 Research Design

In this section, I describe my research design, identifying assumptions, and specification choices. I then provide empirical evidence supporting my identifying assumptions and characterize the "first-stage" effects of PNTR on local labor markets that may have affected students' educational choices.

[21]Self-employed workers, independent contractors, many federal employees, members of the military, and participants in the informal sector are not covered by the state unemployment insurance system.

The records report employer industry codes (6-digit NAICS) in all years but do not report county of residency or county of employment.

[22]My analysis sample ends with students in ninth grade in Fall 2008, because the youngest students that I can observe prior to the onset of the shock were kindergarteners in 1999, who would enter ninth grade in 2008. I further detail my sample selection in Appendix B.

4.1 Identification

My research design seeks to isolate the same changes in local labor demand caused by PNTR as Pierce and Schott (2020) and compare the difference in outcomes of students in cohorts who made high school and college decisions after vs. before the policy change in counties that were more vs. less exposed to the labor demand shock. I follow Pierce and Schott (2020) in measuring county c's exposure to the establishment of Permanent Normal Trade Relations with China ($Exposure_c$, i.e., their treatment "dose") as its employment-weighted average tariff gap using 1990 employment counts from the harmonized County Business Patterns Database (Eckert et al., 2020). I then discretize $Exposure_c$ into a binary treatment measure based on the population-weighted median tariff gap ($HighExposure_c$).[23] Figure 1 portrays variation in county-level exposure to the shock across the U.S. and Texas, and Table 1 presents descriptive statistics for samples of students from cohorts entering high school prior to 2000 across counties experiencing above- and below-median shocks.

Specifying treatment timing (τ) as when a cohort entered high school, I estimate the following event-study specification:[24]

$$y_{ict} = \sum_{\substack{\tau=1995 \\ \tau\neq1999}}^{2008} \pi_\tau^{ES} \mathbf{1}\{t=\tau\} * HighExposure_c + \alpha_c + \alpha_t + \Phi S_i + \sum_{\substack{\tau=1995 \\ \tau\neq1999}}^{2008} \Gamma_\tau \mathbf{1}\{t=\tau\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \tag{1}$$

In equation (1), I regress outcome y_{ict}, such as an indicator variable for graduating high school, for student i in county c and ninth-grade cohort t on $HighExposure_c$ interacted with

[23]Existing literature studying the local labor market effects of the China shock utilizes continuous treatment measures (e.g., Pierce and Schott, 2020), which impose a linear relationship between a county's employment-weighted tariff gap and student outcomes. However, the mechanisms through which exposure to import competition would affect educational decisions more plausibly conform with a threshold model. In other words, a *marginal* reduction in earnings for workers without a college degree may not induce additional students to enroll in college, but a *salient* decrease plausibly could. In Section 7, I present estimates using alternate thresholds to define $HighExposure_c$.

[24]I do not specify treatment centered around an earlier grade because the Texas Child Labor Law first allows children to work (subject to hour restrictions) at 14, the age at which students commonly enter ninth grade. Thus, there are no market-defined opportunity costs of schooling before this age. On the other hand, I do not specify treatment centered around a later grade, because I am interested in adjustment mechanisms that occur during high school: vocational, AP, and dual-credit course-taking, along with diploma receipt.

an indicator variable for belonging to a particular cohort τ, along with county (α_c) and cohort (α_t) fixed effects that absorb county-specific characteristics common across all cohorts and cohort-specific characteristics common across all counties, respectively. In addition to time-invariant student demographics (S_i), I control for two sets of county-level covariates.[25] First, I include a vector of pre-period measures of county economic and demographic characteristics (X_c) interacted with cohort dummies to account for time-varying shocks related to a county's economic profile; second, I control for a vector of other trade policy changes that may affect local labor demand (Z_{ct}).[26] Because X_c includes a county's baseline manufacturing share, my identifying variation consists of comparisons of students in counties with similar *overall* manufacturing presence but with differing shares of employment in firms specializing in *specific* products that were exposed to import competition. π_τ^{ES} (Event Study) represents the difference in outcomes between such students among cohort τ relative to this difference among the 1999 ninth grade cohort.

Although $HighExposure_c$ is a binary measure, the underlying exposure to import competition is inherently continuous. Equation (1) thus compares changes in outcomes across cohorts of students from counties exposed to exogenously "big" and "small" labor demand shocks without a group of "pure" control counties that are completely unaffected by PNTR. Such comparisons cannot identify the causal effect of a labor demand shock relative to the shock's absence (Callaway et al., 2021). However, they can identify the causal effect of exposure to a large local labor demand shock relative to a small one. Identification requires the assumption that trends in outcomes of students from high-exposure counties would on average evolve in parallel to those of students from low-exposure counties if all counties had

[25] S_i consists of indicator variables for student race and ethnicity, gender, Limited English Proficiency status, and free-or-reduced-price lunch eligibility. These characteristics are included as controls to reduce residual variation, but I show that results are robust to their exclusion in Section 7.

[26] X_c consists of 1990 measures of median household income, population share without a college degree, the foreign-born population share, and the share of employment in manufacturing, along with the per capita volume of shale oil and gas reserves within a county's borders. Z_{ct} includes the average import tariff rate associated with a county's goods, the county's exposure to the end of global restrictions on textiles and clothing imports from the phasing out of the Multi-Fiber Arrangements, and the county's exposure to changes in tariffs on imports into China and Chinese domestic production subsidies. Both sets of covariates are adopted from Pierce and Schott (2020), with the addition of shale oil and gas reserves to account for the fracking boom – a potential confounder specific to my setting of Texas that Kovalenko (2023) shows caused students to leave school early to enter newly robust local labor markets. I show in Section 7 that my results are robust to excluding both sets of controls from the specification.

instead experienced a small shock.[27] In other words, one must assume that a low treatment "dosage" (i.e., a small decrease in labor demand) would have the same effects on students from both treatment groups.

Estimates of equation (1) with outcomes proxying for labor demand on the left-hand side (e.g., per-capita labor income) inform the appropriate estimator for identifying the causal effects of exposure to negative local labor demand shocks on human capital accumulation and later-life earnings. Event studies in Section 4.2 show that labor demand grew faster in counties that were later more exposed to PNTR than in less-exposed counties leading up to the policy change.[28] These patterns suggest that outcomes of Texas students would not have trended in parallel across more- and less-exposed counties if both groups were exposed to a low treatment dosage, but more likely would have continued along existing differential trends.[29] Therefore, in order to isolate the changes in labor demand caused by PNTR from longstanding secular trends, I explicitly control for differences in existing trends in outcomes.

Specifically, I use the following two-step procedure proposed by Goodman-Bacon (2021), which is comparable to controlling for linear time trends in a standard two-way fixed effects estimation but is not biased by dynamic treatment effects:[30]

[27]Specifically, under this assumption, such comparisons identify the average causal effect of exposure to a large local labor demand shock relative to a small one for population of students from high-exposure counties – analogous to an Average Treatment Effect on the Treated parameter. Under the additional assumption that on average a high treatment dosage would have the same effects on students from both groups, equation (1) identifies the average causal effect of exposure to a large local labor demand shock relative to a small one for all Texas students – analogous to an Average Treatment Effect parameter (Callaway et al., 2021).

[28]This phenomenon was not unique to Texas. Figure A1a presents event study estimates of the relationship between exposure to PNTR and labor income using all U.S. counties. The estimates mimic a result from (Pierce and Schott, 2016b) and show that just as in Texas, high-exposure counties experienced sustained income growth relative to low-exposure counties leading up to the policy change.

[29]A particular threat to the modified parallel trends assumption is that continued differential growth of labor market opportunities in high-exposure counties relative to low-exposure counties could increase the opportunity costs of schooling, such that educational attainment would fall. On the other hand, this differential growth could increase educational attainment by increasing parental income. In either case, the standard difference-in-differences equivalent of equation 1 would not identify causal effects.

[30]Goodman-Bacon (2021) notes that unit-specific linear trends in a standard two-way fixed effects regression misidentifies time-varying treatment effects that grow over time as existing trends rather than attribute them as treatment effects. In contrast, the two-step de-trended difference-in-differences estimator is not biased by dynamic treatment effects because only pre-period data is used to identify the trends. The two-step approach and the similarly spirited "parametric event study" specification proposed by Dobkin et al. (2018) have been used to estimate causal effects of hospital admissions on financial health (Dobkin et al., 2018), bankruptcy flag removal on consumer spending (Gross et al., 2020), bankruptcy reform on credit behavior, school finance reforms on student achievement (Lafortune et al., 2018), abortion denial on financial health (Miller et al., 2023), opioid supply on local economic conditions (Beheshti, 2022), immigration reform on educational attainment (Kuka et al., 2020), and Paycheck Protection Program loans on employee retention

$$y_{ict} = \lambda t * HighExposure_c + \alpha_c + \alpha_t + \Phi S_i + \sum_{\tau=1995}^{1998} \Gamma_\tau \mathbf{1}\{t=\tau\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \quad (2)$$

$$\widetilde{y}_{ict} = \sum_{\substack{\tau=1995\\ \tau\neq 1999}}^{2008} \pi_\tau^{DTES} \mathbf{1}\{t=\tau\} * HighExposure_c + \alpha_c + \alpha_t + \Phi S_i + \sum_{\substack{\tau=1995\\ \tau\neq 1999}}^{2008} \Gamma_\tau \mathbf{1}\{t=\tau\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \quad (3)$$

In the first step (equation (2)), I estimate a linear pre-trend in outcomes by regressing the outcome of interest y_{ict} on a linear trend interacted with $HighExposure_c$, using only cohorts entering ninth grade prior to 2000. I then extrapolate the estimated differential pre-trend ($\hat{\lambda}$) beyond 2000 and construct $\widetilde{y}_{ict}$, the *de-trended* outcome variable, by partialing out $\hat{\lambda} t * HighExposure_c$.

In the second step (equation (3)), I estimate the same event study specification as equation (1) but with the de-trended outcome variable ($\widetilde{y}_{ict}$) on the left-hand side. π_τ^{DTES} (De-Trended Event Study) are the differences in outcomes between students from more-exposed and less-exposed counties among cohort τ relative to the extrapolated linear pre-trend.[31] Following Kuka et al. (2020), I construct standard errors using a degrees-of-freedom adjustment to account for utilizing a regression-adjusted outcome variable.

The two-step event study estimates are useful for showing visually whether a linear pre-trend fits the data and assessing dynamic responses to treatment. To summarize treatment effects across cohorts, I estimate a difference-in-differences specification as the second step, replacing equation (3) with the following equation:

$$\widetilde{y}_{ict} = \Pi^{DTDD} HighExposure_c * \mathbf{1}\{t \geq 2000\} + \alpha_c + \alpha_t + \Phi S_i + \Gamma_\tau \mathbf{1}\{t \geq 2000\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \quad (4)$$

where I interact $HighExposure_c$ (and control measures, X_c) with an indicator variable for

(Autor et al., 2013).

[31] An event-study plot of π_τ^{DTES} across cohorts intuitively resembles that of π_τ^{ES} with its pre-trend rotated toward the x-axis.

entering high school after the start of the shock instead of with individual cohort dummy variables. Identification of the causal effect of exposure to the local labor demand shock (Π^{DTDD}; De-Trended Difference-in-Differences) requires the assumption that differences in outcomes of students from more- and less-exposed counties would continue to evolve along the existing linear trend if they both were exposed to small labor demand shocks.

Greenland et al. (2019) and Burga and Turner (2022) find evidence of the China shock increasing outmigration from exposed local labor markets, particularly among young adults and the less-educated. Such out-migration among lower-ability students would yield selection into treatment in a naive framework based on contemporaneous residence, biasing estimates of the effects of exposure to the labor demand shock on educational attainment and later-life earnings upward. Therefore, I assign treatment status and county-level covariates to students based on their county of residence *prior* to the onset of the shock in an Intent-to-Treat (ITT) framework. This approach is made possible by my ability to link students across years and datasets in the individual-level UHERC data. Because I cannot observe students that leave Texas, I also test for differential attrition rates (Figure A3) and find no statistically significant evidence of differential attrition by treatment status at any age from 16 through 30.[32]

4.1.1 Empirical Support for Required Assumptions

I note two conditions that are sufficient for the required assumption for identification – that differences in student outcomes across more- and less-exposed counties would continue

[32]Figure A3 presents separate estimates of equation (4) with indicator variables equal to one if an individual is not observed in the data again from that age through age 30 as the outcomes. The 95% confidence intervals for every estimate include 0. If the negative point estimates are taken at face value, they suggest exposure to the labor demand shock caused students to be *less* likely to leave Texas. For such behavior to bias estimates of the effect of exposure to the shock on human capital accumulation and later-life earnings upward, these marginal "stayers" would need to be positively selected. A plausible story conforming with this notion would be that students that otherwise would have attended out-of-state colleges and found work outside of Texas instead attended in-state schools because of declines in family income caused by the shock. I provide evidence against this story in Section 7.

The increase in magnitude of estimates in A3 as the age of definition approaches 30 is largely a mechanical effect. Because I observe no workers past age 30, "attrition" at age 29 only reflects non-participation in the labor force or education system in Texas for 2 years, as opposed to non-participation for 10 years for attrition defined at age 20. To the extent that human capital adjustments increased labor force opportunities, increases in employment for a given year appear as reduced attrition for later ages, while more weakly relating to earlier defined measures.

along existing linear trends if both groups experienced small labor demand shocks – to hold and provide empirical support for each. First, one must assume that such differences in student outcomes would continue along existing trends for the rest of the sample period in the absence of the policy change (Goodman-Bacon, 2021). Consistent with this condition, I show that differences in *pre-measured* characteristics of ninth-grade students do not deviate from existing trends following the policy change. Table 2 presents estimated "effects" of exposure to larger shocks on baseline (pre-shock) characteristics using equation (4) and shows no evidence of local shocks "affecting" previously measured student race, ethnicity, gender, English-Language Learner status, free-or-reduced-price-lunch eligibility, or an index of later-life earnings predicted on these measures. Exposure to larger shocks correlates with a statistically insignificant $62 (2020$) decrease in earnings predicted on fixed student characteristics, suggesting that earnings would have continued along existing trends in the absence of the labor demand shock. Moreover, that differential growth in labor demand started 8 years prior to PNTR – as shown in Figure A2 – supports the notion that these differential trends could have continued for the length of the post-period in my sample – 8 successive cohorts.[33]

Second, one must assume a small shock to local labor demand would cause the same average deviations from existing differential trends for students from high-exposure and low-exposure counties (Callaway et al., 2021). Table 1 presents average characteristics of individuals from high-exposure and low-exposure counties and shows the groups only statistically differ from one another along 2 out of 19 dimensions. The balance in observable characteristics of individuals across both sets of counties suggests that members of each treatment group would respond similarly to the same sized labor demand shock.

In Section 7, I test the robustness of my main results to two relaxations of the above assumptions. First, I allow potential outcomes of students from high-exposure and low-exposure counties to trend in parallel in the post period. Second, I allow for smooth de-

[33]Figure A2 replicates the per-capita labor income event study in Figure 2 with an extended pre-period dating back to 1975. Earnings appear to have trended in parallel across high-exposure and low-exposure counties until 1991, when high-exposure counties start to experience differential growth. To empirically assess when the differential pre-trends began, I estimate linear trend break specifications for each possible year between 1975 and 1999 (Andrews, 1993); I find that setting the break at 1991 maximizes R-squared.

Event studies characterizing the effects of PNTR on local labor markets in Section 4.2 start at 1994 to match the starting point of the student-level data.

viations in potential outcomes from the existing linear pre-trends, rather than assuming exact linearity (Rambachan and Roth, 2023). I also show robustness to adding or dropping controls, using alternate trends-based estimators, adopting alternate thresholds to define *High-Exposure*, assigning treatment status based on where students attended ninth-grade, and dropping any single county from the sample.

4.2 Effects of PNTR on Local Labor Markets

Using data from the County Business Patterns Database (Eckert et al., 2020), Bureau of Economic Analysis Regional Economic Accounts, and Census Quarterly Workforce Indicators, I characterize the local labor demand shock caused by PNTR in Texas before examining student responses. This serves dual purposes of assessing which specification isolates changes in labor demand caused by PNTR and guiding interpretation of the channels through which the shock may have affected student behavior. In an initial example, I present results from three specifications that build toward causal identification: standard event-study estimates (equation 1), de-trended event-study estimates (equation 3), and de-trended difference-in-difference estimates (equation 4) – my primary measure of treatment effects. Afterward, I only present de-trended event studies and difference-in-difference estimates in the main text and present standard event studies in Appendix A.2.

I first examine how exposure to PNTR affected local wage and salary income, a proxy for overall labor demand. Panel (a) of Figure 2 presents estimates of π_τ^{ES} that represent the difference in per-capita labor income in high-exposure versus low-exposure counties relative to this difference in 1999. Income grew faster in counties that were later more exposed to PNTR than in less-exposed counties leading up to the policy change. The continuance of this dynamic if both groups were exposed to small labor demand shocks would violate the modified parallel trends assumption and bias the estimated effect of greater exposure to import competition on local income upward. Therefore, I explicitly control for the difference in existing trends using equation 3. Panel (b) presents estimates of π_τ^{DTES}, representing the difference in per-capita labor income between high-exposure and low-exposure counties in each year relative to the evolution implied by the differential growth before 2000. Coefficients for before 2000 hover around 0, confirming the fit of a linear trend and supporting the validity of the specification. Under the assumption that earnings in more-exposed counties would continue to grow relative to less-exposed counties along this existing trend, post-period coefficients suggest that greater exposure to import competition dramatically reduced income. The de-trended difference-in-difference estimate (π^{DTDD}) in Table 3 summarizes this effect, indicating that PNTR caused a statistically significant 18% decline ($p < 0.01$) in earnings in high-exposure counties.

In addition, Table 3 and Figure 3 provide evidence of the labor demand shock's effect on employment. Difference-in-differences estimates indicate that greater exposure to import competition caused the loss of an average of approximately 1,400 manufacturing jobs per county and reduced employment relative to population by 3.0 percentage-points. Both coefficients are statistically significant ($p < 0.01$), and the magnitudes are similar to previous estimates of the effects of the China shock on local labor markets using nationwide samples and nearly identical to the effects of a recession on more- exposed local labor markets.[34] Moreover, the point estimate in Column (3) suggests that the negative employment effects of import competition spilled over outside of manufacturing.[35]

Declines in local labor demand may enter into educational decisions by decreasing the opportunity cost of schooling. I define two proxies for opportunity costs: the average earnings for school-aged (15-24) workers and the average earnings for workers who never attended college. Table 4 shows that the labor demand shock significantly reduced both measures by 18% and 8%, respectively. To the extent that students expected the declines in labor market opportunities for workers without college experience to persist, the second estimate

[34] Autor et al. (2021) finds a 1.9 percentage-point decline in employment relative to population in exposed local labor markets. Hershbein and Stuart (2024) show that each recession since the 1970s corresponded to approximately a 3 percentage-point employment decline in counties with above-median exposure. Although Autor et al. (2021) shows that the effects of Chinese import competition persisted for nearly 20 years after the onset of the shock, much of the China shock literature examines effects on local labor market outcomes only through 2007. In Appendix Table A1, I show robustness to setting the end of my panel at 2007, 2012, and 2016.

[35] Existing literature on the China shock finds mixed evidence on how the shock affected employment outside of manufacturing. Using a shift-share design and aggregate Census data, Autor et al. (2013) find that local labor markets (defined as commuting zones) exposed to the China shock experienced decreases in non-manufacturing employment relative to population, and this reduction statistically differs from zero for workers without a college education. Bloom et al. (2019) utilize the same design and establishment-level data from the Census Bureau's Longitudinal Business Database and find commuting-zone-level exposure to the China shock resulted in small increases in non-manufacturing employment. Both papers find reductions in manufacturing and overall employment in exposed local labor markets.

also represents a decrease in the expected lifetime earnings associated with entering the labor market before or directly after high school completion. The estimate in Column (3) indicates that the decrease in earnings for workers without college experience translated to an increase in the college earnings premia. Both the decline in opportunity costs and the increase in the labor market return to a college education would incentivize marginal students to enroll in college.

On the other hand, local economic shocks also may affect human capital accumulation by decreasing family income and school funding (Stuart, 2022; Burga and Turner, 2022). If property values fell in exposed counties, as shown with a nationwide sample by Feler and Senses (2017), accompanying reductions in school spending could negatively affect short- and long-run student outcomes.[36] However, Texas' K-12 finance system provides insurance to local fluctuations in property tax revenues through its "Robin Hood" formula, which redistributes excess property tax revenues to equalize per-pupil spending across districts. Columns (4) and (5) of Table 4 show that despite district revenues from local property taxes falling by approximately $2,800 per-pupil in more-exposed counties, school spending was unaffected.[37] Appendix Table C2 supports that the "insurance" provided by the Robin Hood formula is unique relative to the rest of the country: while school spending in Texas fell by 0 cents per dollar lost in district property tax revenues due to local shocks, in the rest of the country, school spending fell by 84 cents per dollar lost in local revenues. Still, exposed students on average experienced declines in family income, as evident by an increase in eligibility for free-or-reduced-price lunch shown in Column (6).

All together, these results support that, in Texas, marginal students experienced increased incentives to acquire more education. Although they did not see reductions in the "external" public investments that support such attainment (i.e., K-12 funding), they may have experienced declines in "external" private investments (i.e., family expenditures). Given these opposing channels, the direction of the effect of the labor demand shock on overall human capital accumulation is theoretically ambiguous.

[36] For a review of recent literature on the causal effects of K-12 school spending, see Jackson (2018).

[37] Appendix Figure A6 presents the corresponding event studies.

5 Main Results

My primary analyses examine how students adjusted their educational decisions in response to the labor demand shock and whether these adjustments shielded students from the shock's negative effects on earnings.

5.1 Human Capital Adjustments in High School

The first-stage results discussed in Section 4.2 are consistent with the China shock incentivizing students on the margin of dropping out of high school to instead graduate by reducing the opportunity cost of schooling and increasing the returns to higher levels of education. However, existing estimates of the effects of the China shock on high school graduation rates with school-level data give mixed evidence on whether such responses occurred. (Greenland and Lopresti, 2016; Burga and Turner, 2022).[38] Figure 4a and Table 5 present de-trended event study and difference-in-differences estimates of the effects of local shock exposure on high school graduation. I find no statistically distinguishable effects across all students and can rule out effects larger than a 2.0 percentage-point increase in the probability of high school graduation. The null result is consistent with the findings of Burga and Turner (2022), and my intent-to-treat specification is not subject to the potential bias from student migration that they caution threatens identification of the China shock's effects on high school graduation.[39]

Null effects on high school graduation do not rule out important *intensive-margin* adjustments students could make in response to the shock. Vocational courses can give students

[38]Greenland and Lopresti (2016) and Burga and Turner (2022) both examine the effects of the China shock on high school graduation rates using aggregated graduation counts for nationwide samples but come to different conclusions. Greenland and Lopresti (2016) find that graduation rates increase in local labor markets exposed to the China shock by 3.6 percentage-points; however, Burga and Turner (2022) provide evidence that this result is mostly explained by outmigration and weak instrument bias.

[39]Identification with my ITT specification still would be threatened by exposure to the China shock increasing the likelihood that families migrated out of Texas, altogether. However, in Section 7, I test for differential attrition from Texas and find no statistically significant evidence of such behavior.

Notably, the coefficients in Figure 4a representing the effects of exposure to the labor demand shock on high school graduation rates among cohorts entering high school between 2004 and 2008 are all marginally significant and fall between 1.0 and 1.9 percentage point increases. The 95% confidence interval for each coefficient still rule out the 3.6 percentage-point increase found by Greenland and Lopresti (2016), potentially due to the use of different estimators, differences in responses in Texas and the rest of the country, or selective outmigration reflected in their aggregated graduation counts.

specialized human capital that prepares them to find employment in a particular industry, and research links participation in such courses with higher earnings (Bishop and Mane, 2004). However, the loss of manufacturing jobs in counties exposed to import competition likely reduced the labor market return to completing vocational courses in manufacturing, in particular. Thus, I examine whether exposure to the labor demand shock affected overall and manufacturing-specific vocational course-taking in high school using Texas Education Agency field classifications.[40] Table 5 shows no evidence of changes in overall vocational course-taking, but that consistent with salient reductions in local demand for manufacturing workers, students enrolled in 22% fewer "industrial" electives ($p < 0.01$). Instead, students completed 4% more business electives. I also use a more data-driven approach by categorizing courses based on their strength in predicting employment in industries directly hurt by PNTR among pre-period cohorts. Estimates in Appendix Table A15 show that local shocks caused students to complete fewer courses that were historically taken by individuals that found employment in industries later exposed to the policy change.

Students may also actively prepare for pursuing a postsecondary education while in high school by taking courses eligible for college credit. I test if students responded to salient increases in the college earnings premium by doing so and present results in in Figure 4 and Column (5) of Table 5. I find that students exposed to larger local shocks completed 0.5 more courses through dual-enrollment with local colleges.[41] These estimates suggest the reduction in labor market opportunities for workers without college experience increased desire to pursue a college education. Moreover, Jackson (2010) provides evidence that taking courses eligible for college credit increases the likelihood of college matriculation, such that the estimated increases in dual-credit completions also represent mechanisms that may have aided students in reaching college as a manner of adjustment to the labor demand shock.[42]

[40] Appendix B.3 details each elective category.

[41] The magnitude of the estimated effect on dual-credit completions corresponds to a 252% increase relative to the pre-period mean, but this should be interpreted with caution when compared because overall take-up of dual-credit increased four-fold during the sample period across both high-exposure and low-exposure counties.

[42] On the other hand, (Hemelt et al., 2019) find no discernible effects of dual-credit courses on college enrollment from an experiment in Tennessee.

5.2 Postsecondary Human Capital Adjustments

College plausibly offered a critical avenue for students to shield themselves from the labor demand shock. First-stage estimates from Section 4.2 suggest that earnings for workers with a college degree increased relative to those without college experience as a result of the shock, and existing work provides evidence of considerable returns to both two-year and four-year degrees even for marginal students (e.g., Card, 2001; Smith et al., 2020). However, the substantial monetary costs of higher education in combination with the shock's negative effects on household income may have prevented the above increases in dual enrollment from translating into college matriculation by marginal students.

Figure 5 presents de-trended event study estimates of college enrollment within two years of expected high school completion that suggest students were able to adjust to the shock by pursuing a higher education at two- and four-year institutions. De-trended difference-in-differences estimates in Columns (1) through (3) of Table 6 indicate that students exposed to local shocks were 1.8 percentage-points (4%) more likely to enroll in any Texas public college, 1.2 percentage-points (4%) more likely to enroll in a two-year college, and 1.6 percentage-points (10%) more likely to enroll at a four-year university.[43] Each estimate is statistically significant ($p < 0.01$), and the magnitudes are comparable to existing estimates of the effects of placement in smaller elementary school classrooms (Chetty et al., 2011) or receipt of $1,000 in additional financial aid (Castleman and Long, 2016).[44] Because I only observe whether students enroll in in-state colleges and universities, a notable concern is that the estimated increases in enrollment may in part reflect substitution from more expensive out-of-state to cheaper in-state institutions in response to the shock's negative effect on family income. However, in Section 7, I find no evidence of substitution in enrollment from private to public in-state universities or from higher- to lower-priced in-state institutions.

[43] Because I define enrollment outcomes based on attending a college at any point within two years of expected high school graduation, variables for enrollment at two-year and four-year institutions are not mutually exclusive measures.

[44] Chetty et al. (2011) find that assignment to small classes (averaging 15 students, rather than 22) for grades K-3 through the Tennessee STAR experiment caused a 1.8 percentage-point increase in college enrollment. Castleman and Long (2016) find that a $1,995 (2020$) increase in need-based aid in Florida caused a 3.2 percentage-point increase in enrollment at public four-year universities. Assuming linearity, my estimated effect on four-year enrollment of 1.6 percentage-points scaled by their coefficient corresponds to a $998 increase in aid.

The above increase in college enrollment may not have improved labor market outcomes for students if they did not persist toward degree receipt. Thus, I use two exercises to assess whether exposure to local shocks caused meaningful increases in postsecondary educational attainment beyond initial college enrollment. First, I separately estimate the effects of exposure to local shocks on college enrollment – including dual enrollment while in high school – at each age from 16 through 30. Figure 6 presents coefficients from de-trended difference-in-difference specifications estimated separately for each age. Each coefficient from ages 16 through 22 is positive, and the nearly 2 percentage-point increases at each age from 18 through 20 are statistically distinguishable from zero at 95% confidence. These estimates suggest that the shock led to sustained increases in college enrollment. Moreover, I find only small and statistically insignificant negative coefficients at older ages, suggesting that students did not merely adjust the timing of college enrollment.

Second, I estimate the effects of exposure to the labor demand shock on cumulative college attainment measures and degree receipt in Columns (4) through (7) of Table 6. Estimates indicate that the shock significantly increased cumulative semesters of college enrollment by age 25 by 5% percent. Affected students were 1.1 percentage-points (8%) more likely to earn a bachelor's degree by age 25, but effects on associate's degree or certificate receipt are small and statistically insignificant. The estimated increase in bachelor's degree receipt may reflect receipt by both marginal enrollees and by students that would have enrolled but left college without a degree in the absence of the shock. Existing research on the persistence toward degree receipt of marginal four-year enrollees suggests that the former group may make a sizable contribution to the estimate (Goodman et al., 2017).

The labor demand shock's incidence on manufacturing may have incentivized students to adjust major choices in similar manners to how results in Section 5.1 suggest they shifted course selections while in high school. I estimate the effects of exposure to local shocks on enrollment across fields of study at community and technical colleges, adopting categories of majors defined by Foote and Grosz (2020).[45] Consistent with students observing changes to earnings premia across majors, results in Table 7 show that enrollment in manufacturing-

[45] Appendix Table B4 presents the two-digit Classification of Instructional Programs codes comprising each broader category.

based programs fell by 37% ($p < .01$), while significantly increasing in information technology, health, and business programs. The magnitude of the decline in manufacturing-aligned enrollment implies that students perceived an approximately 20% relative decline in potential earnings along this career path if scaled by the results of an informational experiment (Baker et al., 2018).[46] I supplement this exercise with a data-driven approach to defining field categories in Appendix A.1 and find that community college enrollment increased in fields with the smallest shares of graduates working in industries directly threatened by import competition prior to the shock.

5.3 Human Capital Adjustments Protected Against Earnings Losses

The above results indicate that in Texas, exposure to the China shock caused students to acquire both more and in-demand human capital in high school and college. Although these responses conform with changes in the returns to education caused by the shock, the results of Choi (2024) and the persistence of depressions in local labor demand shown in Section 4.2 imply the ambiguity of whether labor market benefits of the adjustments were realized. To assess whether adjustments to educational decisions protected against potential earnings losses, I first estimate the effects of the shock on earnings of recent labor market entrants (i.e., "non-adjusting" cohorts) in high-exposure counties. Then, I estimate the relative earnings improvements of individuals young enough to make key educational decisions before the onset of the shock (i.e., "adjusting" cohorts). Comparing the estimated improvements relative to the benchmark estimate of earnings losses characterizes the degree to which human capital adjustments protected against the labor demand shock.

Individuals that made key educational decisions before the onset of the China shock may have entered the labor market with skills that were no longer demanded by employers and, thus, have experienced lasting declines in earnings (Liu et al., 2016). I construct a sample of individuals that entered the labor market as high-school dropouts, high-school graduates, or graduates of two- or four-year colleges before the establishment of PNTR and estimate the

[46] In a randomized control trial, Baker et al. (2018) find that revealing a 10% increase in salaries for graduates of a major caused a 14-17% increase in the likelihood California community college students selected the major.

following individual fixed-effects equation:

$$y_{icpd} = \Pi^{LOSS} HighExposure_c * \mathbf{1}\{p \geq 2000\} + \alpha_i + \alpha_{dp} + \Gamma_\tau \mathbf{1}\{p \geq 2000\} \times X_c + \Theta Z_{cp} + \epsilon_{icp} \tag{5}$$

Annual earnings (winsorized at the 99th percentile and in 2020$) and employment outcomes for individual i in year p from county c and holding terminal degree d are regressed on an indicator for entering the labor market in a high-exposure county interacted with the outcome year (p) occurring after the establishment of PNTR. I control for individual (α_i) and year-by-degree-type (α_{dp}) fixed effects, along with the same set of county-level controls from equation (4), to identify Π^{LOSS} from within-person changes in earnings of young workers exposed to larger vs. smaller local shocks.[47]

Table 8 presents estimates of the PNTR's harmful effects on earnings and employment outcomes of recent labor market entrants in more-exposed counties. Exposure to larger local shocks caused a $1,248 average loss in annual earnings for young workers over the next 10 years after the onset of the shock, a 9% percent decline relative to their pre-shock earnings. Deterioration of both extensive-margin employment by 1.3 percentage-points and intensive-margin earnings conditional on full-year employment by $1,641 contributed to this persistent harm.

The acquisition of more and in-demand human capital by younger cohorts from counties that experienced large labor demand shocks may have buffered against similar earnings losses to those experienced by non-adjusting cohorts. I estimate the earnings gains from these human capital adjustments using de-trended difference-in-differences specifications (equation 4) with later-life earnings and employment measures as outcome variables.[48] Each estimate

[47] I control for year-by-degree-type (α_{dp}) to account for any statewide time-varying shocks that may differentially affect students across different education levels.

[48] Examining effects of human capital adjustments on earnings is complicated by the perfect colinearity between ninth-grade cohort, age at which earnings are observed, and year of observed earnings. I estimate de-trended event study and difference-in-differences specifications that hold constant the *age* at which earnings are observed. This is consistent with the specifications in Sections 5.1 and 5.2, which which hold age at which a particular educational achievement has been achieved constant. More-exposed students' increased attachment to the education system in their early twenties may negatively bias earnings estimates from specifications that instead hold calendar *year* constant, which would measure earnings at younger ages for cohorts entering high school in the post-period than for those entering high school in the pre-period. On the other hand, estimates from specifications that hold age constant would be positively biased if the effects of

can be interpreted as the improvement (or decline) in earnings for individuals exposed to local shocks before entering high school relative to those that made key educational decisions prior to PNTR.

Figure 7 presents de-trended difference-in-differences estimates of the effects of exposure to local shocks during youth on earnings at each age between 20 and 30. The results suggest short-term concessions during schooling-aged years yielded sustained protection against the labor demand shock. During their early 20s, individuals exposed to the shock during youth earned even less than their directly harmed older peers, plausibly due to the former group's increased attachment to the education system. However, exposed individuals young enough to adjust their educational decisions experienced sustained earnings benefits later in adulthood. The statistically significant increases in earnings from 26 through 30 averaged \$1,158 annually, equivalent to protection against 93% of the \$1,248 annual earnings losses estimated for individuals that had already made key educational decisions. Notably, these earnings gains among adjusting cohorts occurred despite the continued persistence of overall earnings declines in exposed counties shown in Figure 2.

Focusing on age 30, I examine how extensive-margin effects on employment and intensive-margin effects on income contributed to the overall earnings gains discussed above. De-trended difference-in-differences estimates in Table 9 indicate that exposed students who were young enough to change key educational decisions experienced statistically significant earnings gains of \$1,579 in adulthood. This magnitude corresponds to more than a full erasure (127%) of the average annual earnings losses for workers that made such decisions before the onset of the shock. Improvements in the share of quarters employed and earnings conditional on employment of 115% and 110% the magnitudes of the respective declines among non-adjusting cohorts drove the overall raw earnings gains.

PNTR on exposed local labor markets dissipated as time passed. Estimates in Section 4.2 – along with the results of Autor et al. (2021) – indicate that there was no such recovery. If anything, Figure 2 suggests that labor demand continued to worsen in more-exposed relative to less-exposed counties as time passed from the initial shock. This would bias against finding positive effects of human capital adjustments on earnings.

5.4 Adjustments by Vulnerable Subgroups

The results in sections 5.1 through 5.3 indicate that the average student exposed to the China shock in Texas adjusted their human capital investments along extensive and intensive margins and experienced subsequent relative earnings gains. However, not all students may have been able to make these critical adjustments. Therefore, I explicitly examine effects of the shock on particularly vulnerable subgroups of students: male students, students from low-income households, and racial and ethnic minorities.

Male dominance of the manufacturing workforce made men especially susceptible to job displacement from the China shock (Autor et al., 2019).[49] Over one-fifth of employed prime-aged males in Texas worked in manufacturing in 1995, and the salience of manufacturing as a viable career path for males in particular prior to the establishment of PNTR was reflected in the educational choices of male students from areas of Texas that were later exposed to larger shocks. Male students in high-exposure counties were 4 times more likely to complete manufacturing-based electives in high school, 11 times more likely to enroll in manufacturing-based programs at community colleges, and 9 percentage-points less likely to pursue a college education than female students prior to the shock. Thus, human capital adjustments may have been particularly important for male students to buffer against the labor demand shock.

I estimate effects of exposure to the shock on educational attainment and later-life earnings for males and present results in Panel A of Table 10. The results suggest that exposure to local shocks caused a significant increase in dual-credit course-taking, initial and sustained college enrollment, and bachelor's degree receipt. Moreover, male students made dramatic shifts in their fields of study away from those directly affected by the shock, as evident by a 26% decline in manufacturing-based course-taking in high school and a 44% decline in enrollment in manufacturing or construction programs at two-year colleges. Relative to males that made these decisions before the onset of the shock, more-exposed male students earned 9% more at age 30.

Credit constraints may have prevented students from lower-income households from ad-

[49] Autor et al. (2019) find that the China shock caused larger declines in earnings and employment of men in exposed local labor markets of those of women.

justing to the labor demand shock by enrolling in college.[50] However, such constraints should not have hindered students' ability to make adjustments that do not impose direct monetary costs. I proxy for the presence of binding credit constraints with a student's *pre-shock* eligibility for free-or-reduced price lunch (FRPL) and present estimated effects of exposure to the shock on educational attainment and earnings for plausibly constrained students in Panel B.[51] Consistent with the importance of credit constraints for postsecondary investments in human capital, estimates show that FRPL-eligible students did not respond to local shocks by attending college. However, these students took fewer manufacturing-aligned electives and more dual-credit courses in high school – a method of acquiring college-level human capital that did not impose additional monetary costs. The estimated significant \$1,643 increase in earnings in Column (9) suggests the importance of these adjustments.

Negative labor demand shocks typically cause greater employment losses for Black and Hispanic individuals (Hoynes et al., 2012), and the myriad of factors contributing to historical Black-white and Hispanic-white gaps in educational attainment may have hindered human capital adjustments by minority students. I present estimates of the effects of exposure to larger local shocks on students identifying as racial or ethnic minorities in Panel C. The results suggest these students made similar extensive- and intensive-margins as did the student population at large and that these adjustments yielded substantial labor market benefits.

6 Mechanisms

In this section, I provide additional context to the main results by examining mechanisms underlying student adjustments.

[50]Economists debate whether incomplete lending markets for financing the direct monetary costs (i.e., tuition and fees) of attending college result in "credit constraints" that prevent low-income students from obtaining a college education (e.g., Cameron and Taber, 2004; Lochner and Monge-Naranjo, 2012).

[51]Students from households with income below 130% of the poverty line are eligible for free school lunches, and those from households with income between 130% and 185% of the poverty line are eligible for reduced-price lunches. Students from households receiving benefits from means-tested federal programs such as Supplemental Nutritional Assistance Program or Temporary Assistance for Needy Families are automatically eligible for free lunch.

6.1 Did Increased Educational Attainment Reflect Student Learning?

Increases in human capital investment in response to the shock and corresponding earnings gains could represent returns to additional human capital, signaling, or a combination of the two (Spence, 1973). Thus, I examine the effects of local shocks on student learning using data on standardized tests in math scores and reading administered in 8th grade. Estimates in Table 11 suggest that students exposed to local shocks performed better on these tests, although only the estimated 0.096 standard deviation improvement in math is statistically significant ($p < 0.10$). One interpretation of these estimates is that forward-looking students responded to increases in the labor-market *benefits* of higher education by increasing their K-12 academic effort. This behavior is symmetric to existing evidence of the effects of decreases in the *costs* of higher education on academic outcomes in high school (Laajaj et al., 2022; Londoño-Vélez et al., 2020; Bartik and Lachowska, 2014). I caution, though, that evidence of increased student effort does not rule out that the signaling value of changes in educational attainment contributed to earnings gains.

6.2 Did K-12 and College Supply-Side Responses Help or Hinder Adjustments?

Intensive-margin adjustments across fields of study in both high school and college can be aided or constrained by the responsiveness of programmatic offerings to changes in demand, and students may not be able to access high-demand courses and majors if high schools and colleges have capacity constraints (Grosz, 2022; Grosz et al., 2022). I examine the number of courses and unique programs across fields offered in high school and college, respectively.[52]

I use TEA data to define the average number of course offerings by vocational field across high schools in each county and use IPEDS data to define the number of unique programs (defined by 6-digit CIP codes) within broader major categories (defined by 2-digit CIP codes) across a county's community and technical colleges. Tables A4 and A5 present estimates of

[52] An ideal test would estimate effects on the number of *seats* offered in a given course or field of study, but I cannot observe course capacity in my datasets.

the effects of local shocks on course and major offerings across fields in local high schools and two-year colleges, respectively. I find no statistically significant evidence of "supply-side" adjustments of course or program offerings, although I cannot rule out economically significant effects in either direction.

6.3 Did Students Move Across Geographies or Industries?

In addition to adjusting their human capital investments – and in many cases, complementing such adjustments, individuals exposed to local shocks during youth may have moved across geographies, industries, or occupations as adults as manners of adjustment. I first examine whether students exposed to large local shocks migrated to less-exposed counties after high school. Using the location of a student's college or university as a proxy for their adult residence, I estimate effects of greater exposure to the shock on attending college in any high-exposure (low-exposure) county and on attending college in a high-exposure (low-exposure) county other than where a student attended K-12.[53] Estimates in Table 12 suggest that students exposed to larger shocks did not adjust to the shock by moving to less-exposed counties. Greater exposure to the shock significantly increased the likelihood students enrolled in colleges in high-exposure counties without affecting the likelihood they did so in low-exposure counties. Although students exposed to larger shocks were significantly more likely to enroll in colleges outside of their home county, they did so in other high-exposure counties.

I next estimate the effects of exposure to the local shocks on later-life employment across groupings of two-digit NAICS industry codes (Table 13). Exposure to shocks during K-12 significantly increased the likelihood that students worked in the manufacturing; construction and transportation; oil and gas; finance, insurance and real estate services; and information services; and health services sectors at age 30.

Due to data limitations, assessing whether exposure to local shocks during youth and the ensuing human capital adjustments affected occupation choice is beyond the scope of this

[53] Because the UHERC workforce data does not include information on county of residence or employment, I cannot construct employment-based migration measures.

paper.[54]

7 Robustness

I test the robustness of my main results to alternative specifications and variable definitions. My primary specification controls for student demographics, 1990 county characteristics interacted with cohort dummies, shale and natural gas presence interacted with cohort dummies, and exposure to other changes to U.S. trade policy.[55] I show that my main results are robust to excluding these covariates and to sequentially adding back each set of control variables (Table A8). Column (1) corresponds to the preferred specification utilized throughout the paper. Across five specifications, estimates of the increase in college enrollment in Panel A range from 2.9% to 4.2%, and all are statistically significant ($p < 0.10$). Estimates of the increase in earnings at age 30 vary from \$1,401 to \$1,593 and are all significant ($p < 0.01$).

The China shock was not the only labor demand shock to occur during my sample period, and correlations in the incidence of shocks across counties could confound my estimations and violate the required assumption that differences in outcomes across high-exposure and low-exposure counties would continue to evolve along existing linear trends if both experienced low treatment dosages. In Columns (6) through (9) of Table A8, I sequentially add controls for county-level exposure to the 2000s housing boom and bust, the 2007-2008 financial crisis, the 2000 dot-com bubble crash, and the passage of the North American Free Trade Agreement – four labor demand shocks which prior research has shown affected educational attainment (Charles et al., 2018; Weinstein, 2022; Lee, 2021).[56] Across the three additional specifications, the estimated increase in college enrollment ranges from 3.0% to 5.1%, and the estimated

[54] The UHERC dataset includes no information on worker occupation.

[55] These covariates all follow Pierce and Schott (2020), with the exception of controlling for the Texas-specific confounder of fracking boom.

[56] I follow Charles et al. (2018) and specify the size of a county's housing bubble as the magnitude of the largest structural break from trend in housing prices occurring between 2000 and 2006, using county-level housing price indices from the Federal Housing Finance Agency. I specify differential exposure to the financial crisis as a county's pre-period debt-to-income ratio (Mian et al., 2013) and specify a county's exposure to the dot-com crash as their employment share in "high-technology" industries (Hecker, 2005; Weinstein, 2022). Finally, I use the county-level NAFTA vulnerability measure from Choi et al. (2024). I interact each of these cross-sectional exposure measure with cohort dummies to allow them to flexibly affect outcomes across cohorts.

increase in earnings ranges from \$1,604 to \$1,724. All six estimates are statistically significant ($p < 0.05$).

In addition to including covariates, my primary specification imposes a linear trend in the differences in outcomes across cohorts of students reaching ninth grade prior to the onset of local shocks to account for existing differential secular trends in economic outcomes across high-exposure and low-exposure counties. Causal identification with this specification requires the assumption that differences in outcomes across treatment and control counties would continue to evolve along existing linear trends if both groups of counties experienced only small labor demand shocks. I test for robustness of my main results to relaxing this assumption in two exercises, both following Rambachan and Roth (2023).

First, I construct confidence sets under the assumption of parallel trends in the post-period, despite the evidence of existing pre-trends. Second, I test for robustness to relaxing the assumption that differences in outcomes between high-exposure and low-exposure counties continued *exactly* linearly after 2000 by allowing for smooth deviations from the continuation of linear trends. Table A9 presents 90% confidence sets of the estimated effects of exposure to the labor demand shock on college enrollment (Panel A) and earnings (Panel B) derived from both of these exercises (Rambachan and Roth, 2023). The first row of each panel presents confidence sets of effects under a standard parallel trends assumption, and subsequent rows present confidence sets when allowing for changes in slope of the extrapolated pre-trend by an additional m each period. Although the center of confidence sets for enrollment and earnings effects fall under a parallel trends assumption relative to the preferred assumption of continuing linear trends, both confidence sets exclude 0. Moreover, Panel A shows that one can still conclude that shock exposure increased college enrollment even when allowing additional slope deviations of up to 0.1 percentage points each period, corresponding to a maximum deviation of 0.9 percentage points by the end of the sample (50% of the treatment effect). Panel B shows that the estimated effect on earnings is robust to a \$75 additional deviation in slope each period, corresponding to a maximum deviation of \$225 by the end of the sample (14% of the treatment effect).

My preferred specification classifies counties as facing "high exposure" and "low exposure" to the China shock using the population-weighted median tariff gap as a treatment threshold. Estimates in Table A11 show that my main results are robust to defining treatment groups based on alternate percentile thresholds. Moreover, I present estimates demonstrating robustness to dropping observations from counties with tariff gaps close to the median threshold in "donut" specifications in Table A12. My preferred specification also assigns a student's treatment status based on the county where they attended school during their first appearance in the UHERC dataset. In Table A13, I present estimates showing my main results are robust to instead assigning treatment based on the county where a student attended school in ninth grade.

I find evidence that students exposed to the China shock were more likely to enroll in public colleges in Texas. This result may not reflect an increase in overall attendance if local shocks caused students to substitute from more-expensive out-of-state or private institutions to cheaper in-state public colleges and universities. I assess this threat with two exercises. Although private college enrollment is not available in the UHERC data for my entire analysis period, I test for effects of local shocks on enrolling at not-for-profit private colleges using a subsample of ninth-grade cohorts for whom I can observe private enrollment.[57] I also note that if local shocks caused students to substitute enrollment in more-expensive colleges to less-expensive colleges, we would expect such an enrollment shift to manifest across the cost distribution *within* Texas, too. I group Texas public universities by their stated in-state tuition and fees using data from the NCES Integrated Postsecondary Education Data System and estimate the effects of exposure to local shocks on enrollment across gross tuition quartiles. Table A14 presents results from both of these exercises. Contrary to a story of substitution, I find that exposed students were more likely to attend colleges in all but the third tuition quartile. Moreover, although the estimated effect on private enrollment is negative, the coefficient is not statistically distinguishable from zero and is an order of magnitude smaller than the estimated effect on public university enrollment found in Table 6.

Finally, I provide evidence that no single county is driving my results. Figure A4 presents

[57] Private school enrollment is only available starting in Fall 2002, corresponding to the on-time freshman year for ninth-graders in 1998. I follow Mountjoy (2022) in taking advantage of high persistence rates at private colleges and backward inducting the enrollment timeline for upperclassmen at private colleges in Fall 2002 in previous semesters, assuming on-time persistence.

histograms of the estimated effects of exposure to the labor demand shock on college enrollment and earnings when iteratively dropping each county from the sample. All estimated effects on college enrollment surpass 1 percentage point, and all estimated effects on earnings at age 30 exceed $1,000.

8 Conclusion

This paper examines how students adjust their human capital investments in response to structural declines in local labor demand and whether these adjustments successfully buffer against harmful effects on labor market outcomes. I use linked student-level administrative data from Texas and leverage variation in local labor demand generated by exposure to Chinese import competition in a de-trended difference-in-differences design. Consistent with reductions in the opportunity cost of schooling, I find that students exposed to larger shocks were 4% more likely to enroll at a two- or four-year college and 8% more likely to obtain a bachelor's degree. Moreover, exposed students completed more courses offered jointly with local colleges while in high school and shifted away from studying fields closely linked to industries that were more-exposed to import competition in favor of those associated with unaffected sectors in both high school and college. These responses suggest that students internalized salient changes to lifetime earnings premia across attainment levels and fields of study, and I provide evidence that the adjustments provided substantial protection against the sustained decline in local labor demand.

Existing research on structural declines in labor demand – including the China shock – paints a gloomy picture for exposed local labor markets, which still exhibit elevated nonemployment and reduced income more than a decade after the onset of local shocks (Autor et al., 2021; Choi, 2024). Such labor demand shocks cause lasting declines in the earnings of affected prime-aged workers, whom have already made critical educational decisions (Pierce et al., 2022; Autor et al., 2014). My findings provide evidence that when individuals coming of age are able to make considerable adjustments to their human capital investments, they can avoid suffering the same fate.

Programs such as Trade Adjustment Assistance help participating prime-aged workers adjust to technological or structural changes in labor demand (Hyman, 2018; Hyman et al., 2024), but there are no analogous programs designed explicitly to help *youth* adjust to structural changes to labor demand. Still, my findings illustrate that forward-looking students can make substantial human capital adjustments in response to changes in local labor demand and suggest considerable labor market returns to such adjustments. My results also suggest the type of policies that enable students to make human capital adjustments. By preventing declines in local property values from translating into meaningful reductions in local school spending, K-12 finance equalization systems such as Texas' Robin Hood system likely help to protect students from consequences of local shocks. Future research should explicitly examine the roles of state K-12 finance formulas and other policies – such as need-based financial aid programs – in guarding students against adverse economic shocks. Such research may help guide policymakers in designing methods to help students adjust to current and future manifestations of skill-biased technological change, such as automation, the transition away from fossil fuels, and artificial intelligence.

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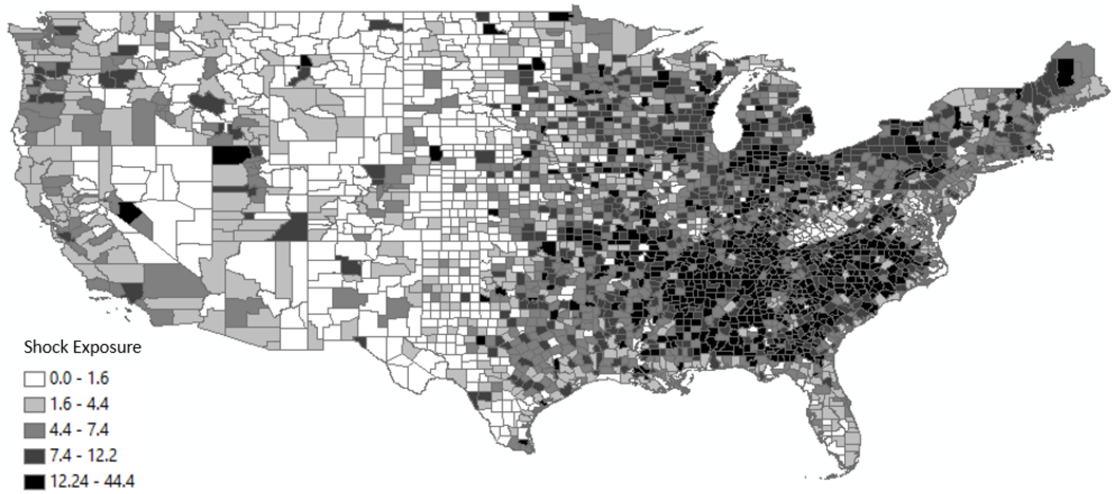
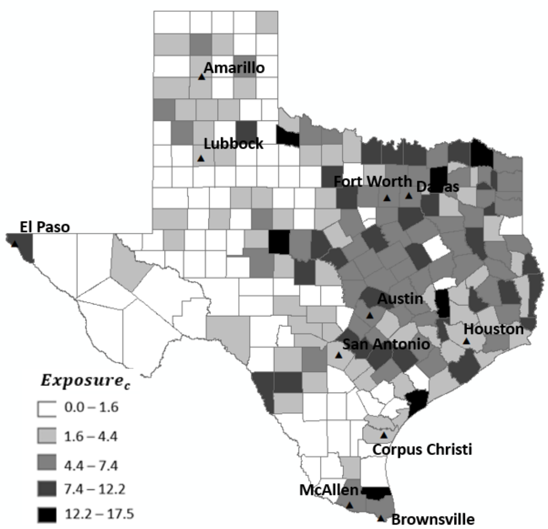
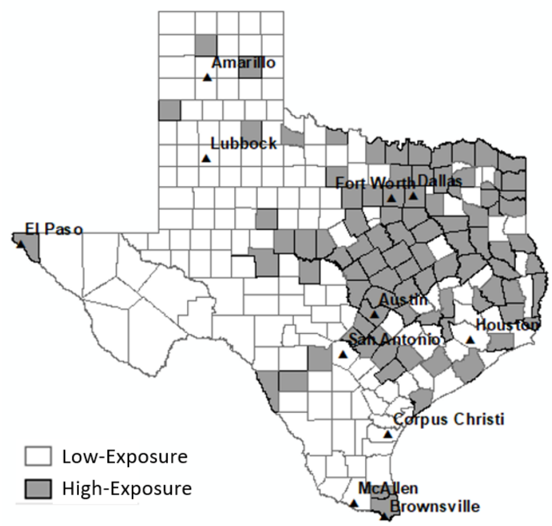


(a) Shock Exposure, U.S.



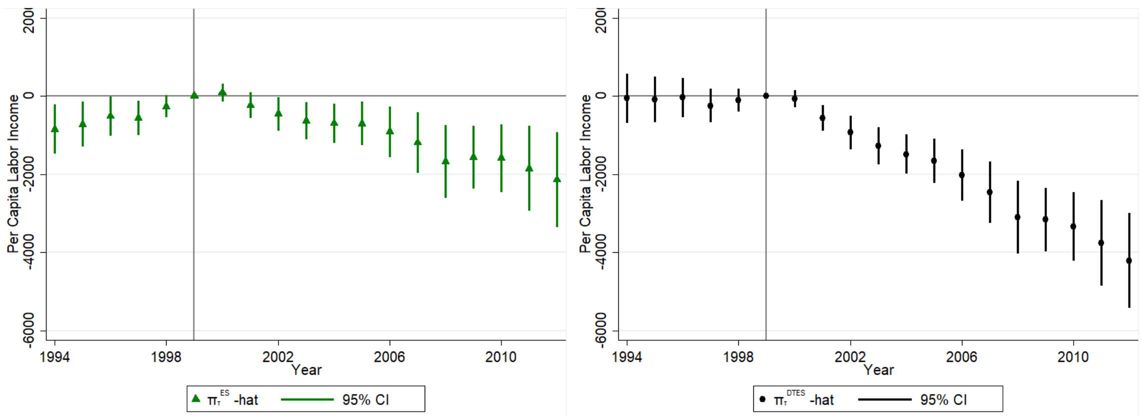
(b) Shock Exposure, TX



(c) Binary Treatment Groups

Figure 1: County-Level Exposure to Local Shocks in the U.S. & Texas

This map county-level variation in exposure to adverse local shocks caused by Chinese import competition, as measured by a county's employment-share weighted "tariff gap" across the industries (i.e., product specialties) present in the county. Each industry's tariff gap is defined as the difference between preferred tariff rates locked in by the establishment of Permanent Normal Trade Relations with China in 2000 and punitive import tariff rates set by the Smoot-Hawley Tariff Act of 1930. Following Pierce and Schott (2020), I define employment shares based on 1990 Census County Business Patterns data and industry-level tariff gaps are measured in 1999. Panel (a) divides counties into quintiles of exposure for the entire U.S. and panel (b) zooms in to Texas. Panel (c) groups counties into binary high-exposure and low-exposure groups based on the population-weighted median.

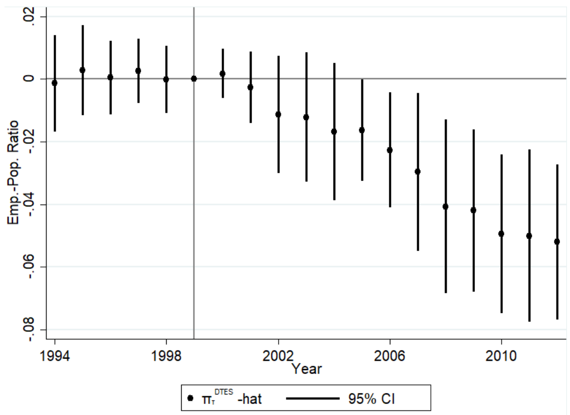


(a) Per-Capita Labor Income

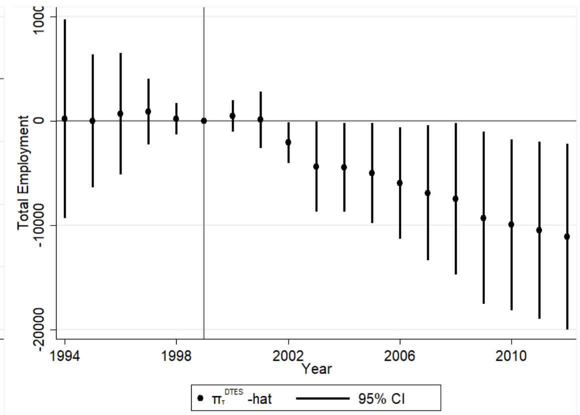
(b) Adjusted for Linear Pre-trends

Figure 2: Import Competition Caused Declines in Local Labor Demand

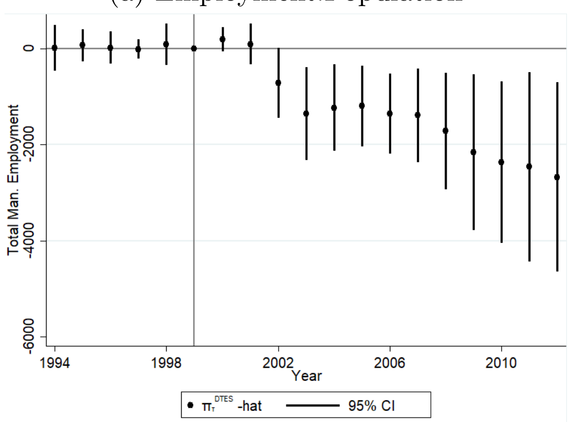
Notes: These figures present estimates of the effect of exposure to Chinese import competition on per-capita labor income using personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset and population counts by age group from the Survey of Epedemiology and End Results. Estimates in panel (a) reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between counties with above- and below-median exposure to local labor market shocks caused by import competition over time relative to this relationship in 1999, one year prior to the start of treatment. Estimates in panel (b) reflect coefficients from a the two-step de-trended event study (equation (3)) that partials out a linear pre-trend in the first step. Both specifications control for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county. Standard errors for the two-step procedure reflected in panel (b) account for parameters estimated in the first step via a degrees-of-freedom adjustment.



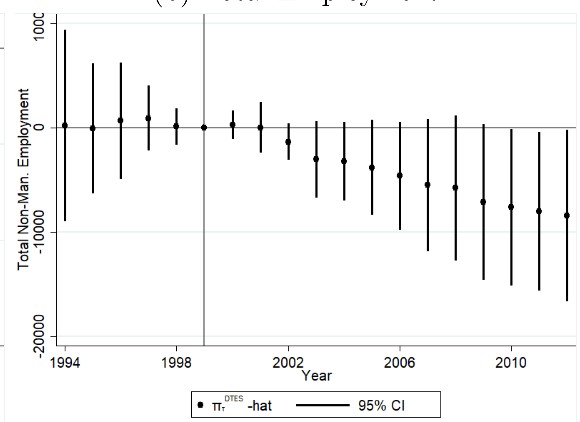
(a) Employment:Population



(b) Total Employment



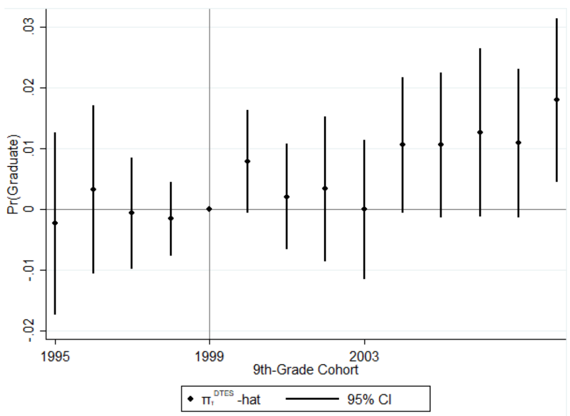
(c) Manufacturing Employment



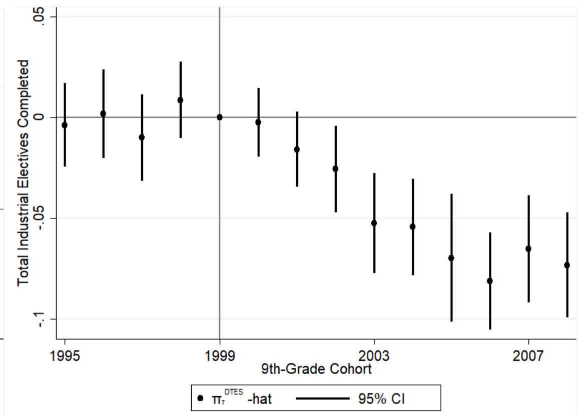
(d) Non-Manufacturing Employment

Figure 3: Import Competition Caused Declines in Manufacturing and Overall Employment

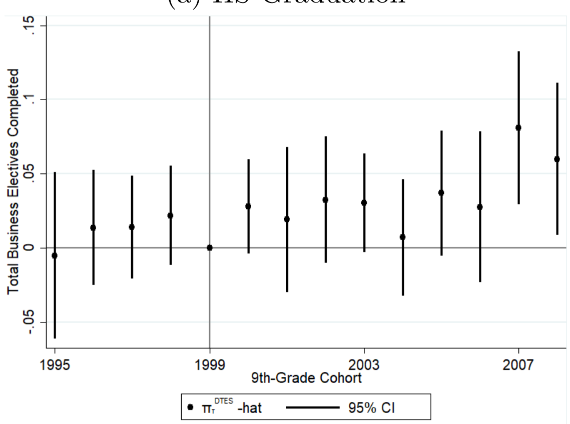
Notes: These figures present estimates of the effect of exposure to Chinese import competition on employment using employment counts from the County Business Patterns Database, population counts by age group from the Survey of Epedemiology and End Results, and personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset. Estimates reflect coefficients from two-step event study regressions that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to the continuation of a linear pre-trend (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable.



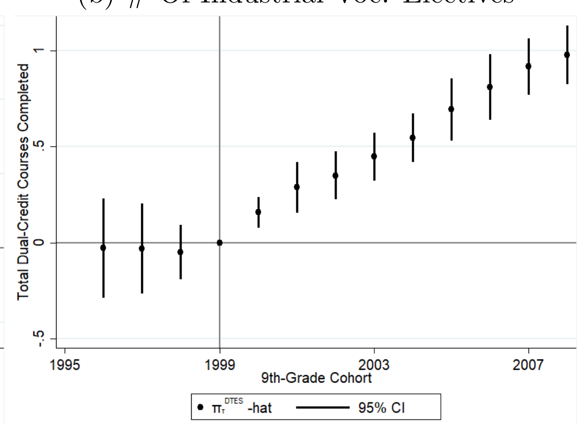
(a) HS Graduation



(b) # Of Industrial Voc. Electives



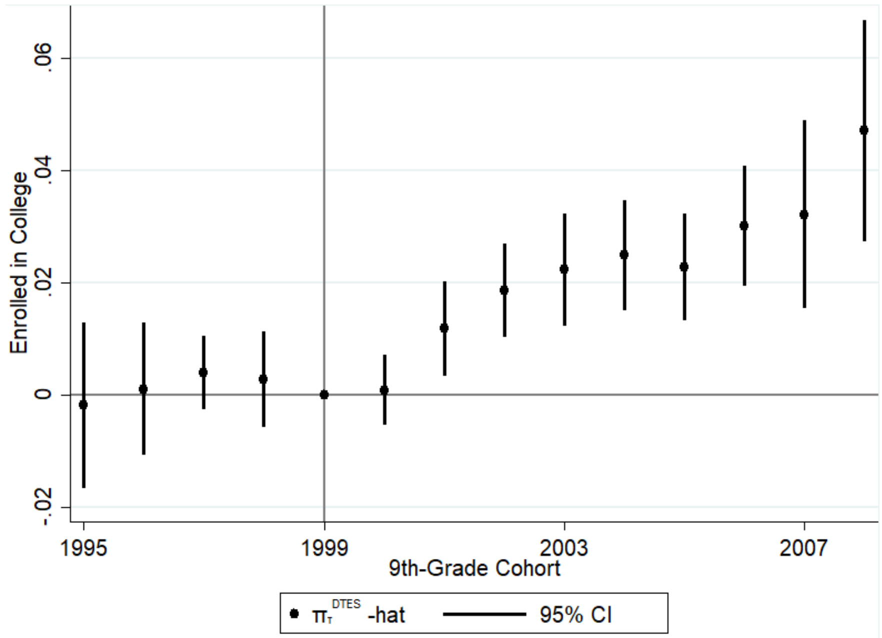
(c) # of Business Electives



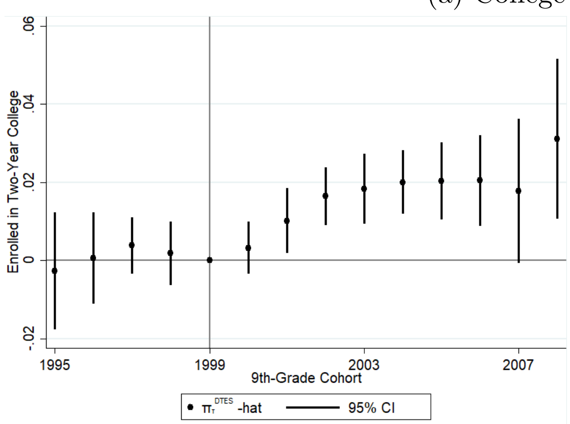
(d) Dual Enrollment

Figure 4: Effects of the Labor Demand Shock on Human Capital Accumulation in HS

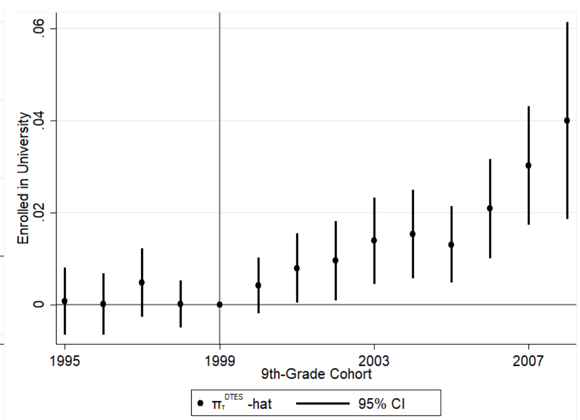
Notes: These figures present estimates of the effect of exposure to negative labor demand shocks during youth and adolescence on high school graduation and course selection. Estimates reflect coefficients from two-step event study regressions that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to the continuation of a linear pre-trend (Goodman-Bacon, 2021). The outcomes are (a) an indicator for graduating high school, counts of the number of (b) manufacturing-aligned electives and (c) business electives completed, and (d) the number of courses completed through dual-enrollment at a local college. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable.



(a) College Enrollment



(b) CTC Enrollment



(c) University Enrollment

Figure 5: Effects of the Labor Demand Shock on College Enrollment

Notes: These figures present estimates of the effect of exposure to negative labor demand shocks during youth and adolescence on college enrollment. Estimates reflect coefficients from two-step event study regressions that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to the continuation of a linear pre-trend (Goodman-Bacon, 2021). The outcomes are (a) an indicator for enrolling at any public two- or four-year college or university in Texas within two years of expected high school graduation and separate indicators for enrolling at (b) a public two-year community or technical college (CTC) and (c) a public four-year university. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable.

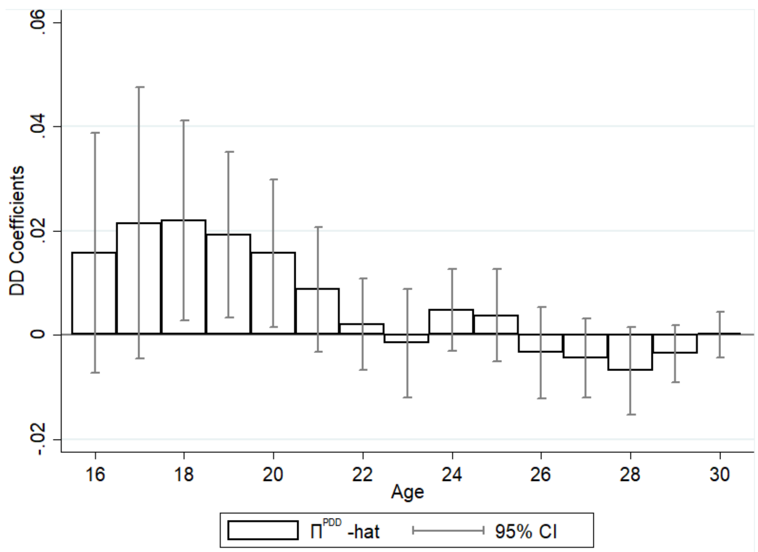


Figure 6: Effects of the Labor Demand Shock on College Enrollment by Age

Notes: This figure presents estimates of the effect of exposure to negative labor demand shocks during youth and adolescence on college enrollment (including dual enrollment while in high school) from ages 16-30. Each coefficient represents the estimate from a separate de-trended difference-in-differences regression that compares changes in the differences between outcomes measured at that specific age of students from more- and less-exposed counties among cohorts that reach ninth grade after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021) . The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, county exposure to the fracking boom, and other changes to U.S. trade policy. The outcome in each regression is an indicator variable for being enrolled in a public two- or four-year college in Texas (including dual enrollment while in high school) during that specific age. Students are assigned to the county where they first appeared in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable.

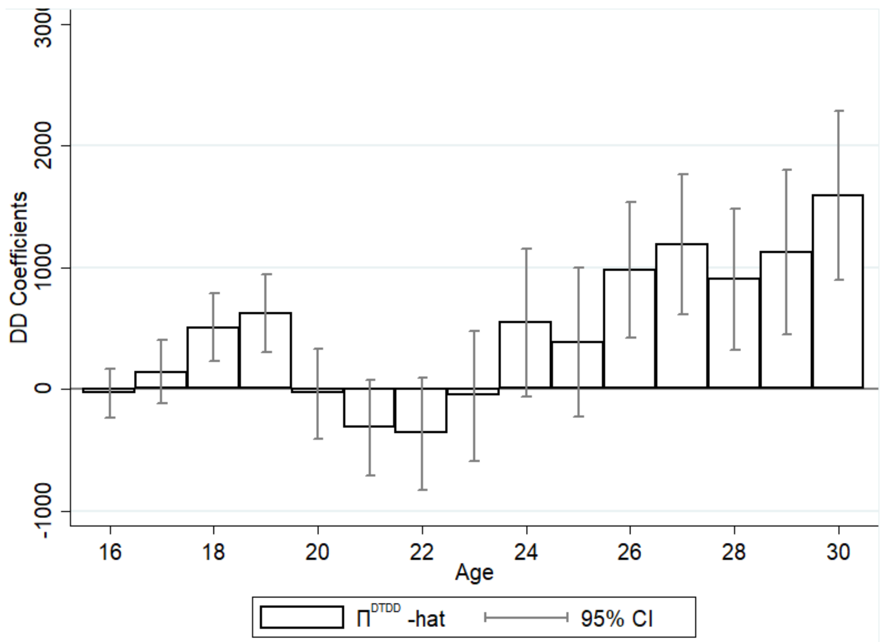


Figure 7: Effects of the Labor Demand Shock on Earnings by Age

Notes: This figure presents estimates of the effect of exposure to negative labor demand shocks during youth and adolescence on earnings from ages 16-30. Each coefficient represents the estimate from a separate de-trended difference-in-differences regression that compares changes in the differences between outcomes measured at that specific age of students from more- and less-exposed counties among cohorts that reach ninth grade after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, county exposure to the fracking boom, and other changes to U.S. trade policy. The outcome in each regression is annual earnings from employment in occupations covered by unemployment insurance in Texas. Students are assigned to the county where they first appeared in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable.

Table 1: Pre-Period Descriptive Statistics: Student Demographics & Outcomes

	(1) Below-Median Shock	(2) Above-Median Shock	(3) Diff
FRPL	0.470	0.416	0.054
	(0.499)	(0.493)	(0.046)
White	0.415	0.536	-0.121*
	(0.493)	(0.499)	(0.623)
Hispanic	0.424	0.289	0.135
	(0.494)	(0.453)	(0.079)
Black	0.132	0.149	-0.017
	(0.339)	(0.356)	(0.044)
Total AP/IB Courses	0.149	0.132	0.017
	(0.570)	(0.548)	(0.016)
Total Dual-Credit Courses	0.029	0.011	0.018
	(0.386)	(0.188)	(0.015)
Total Vocational Electvives	1.262	1.333	-0.071
	(1.451)	(1.461)	(0.090)
Total Industrial Electives	0.200	0.179	0.021
	(0.646)	(0.596)	(0.015)
Graduated HS	0.645	0.649	0.004
	(0.479)	(0.477)	(0.018)
Enrolled at Postsecondary Institution	0.392	0.387	0.005
	(0.488)	(0.487)	(0.014)
Enrolled at CTC	0.294	0.305	-0.011
	(0.456)	(0.460)	(0.013)
Enrolled at University	0.173	0.155	0.018*
	(0.378)	(0.362)	(0.010)
Certificate by 25	0.018	0.017	0.001
	(0.132)	(0.129)	(0.003)
Associate's by 25	0.037	0.039	-0.003
	(0.188)	(0.194)	(0.004)
Bachelor's by 25	0.121	0.117	-0.004
	(0.327)	(0.322)	(0.008)
Share of Qtrs Employed in TX at 30	0.519	0.505	0.014
	(0.471)	(0.472)	(0.016)
Unconditional Earnings at 30	22,602	21,375	1,227
	(34,371)	(35,913)	(921)
Employed All Qtrs in TX at 30	0.449	0.437	0.012
	(0.497)	(0.496)	(0.014)
Conditional Earnings at 30	47,064	45,661	1,403
	(37,655)	(37,843)	(1,831)
Observations	745,128	717,843	1,462,971

Notes: This table presents descriptive statistics for students from the 1995 to 1999 cohorts of ninth graders attending public schools in Texas. Students are divided by whether their county's $Exposure_c$ falls above or below the population-weighted median. College enrollment outcomes reflect enrollment in a public two- or four-year college or university in Texas within two years of expected high school graduation. Enrollment indicators for two- and four-year colleges are not mutually exclusive. Employment variables reflect employment in a position covered by unemployment insurance in Texas. College degree receipt is measured at age 25 and employment is measured at age 30.

Table 2: Specification Tests: Local Shocks Did Not "Affect" Pre-determined Characteristics

	(1) White	(2) Hispanic	(3) Black	(4) Male	(5) ELL	(6) FRPL Eligiblity	(7) Predicted Earnings
De-trended DD	0.003	-0.000	-0.006	0.003	0.008	0.022	-62
	(0.007)	(0.007)	(0.008)	(0.005)	(0.011)	(0.023)	(76)
Percent Change	0.6	-0.1	-4.0	0.6	7.3	5.1	-0.1
Pre-period Mean	0.516	0.325	0.137	0.514	0.109	0.435	48,265
N	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates from falsification exercises of the "effects" of exposure to local shocks on pre-determined student demographics, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021) The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset and assigned demographic measures from their first appearence in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome variables across columns are as follows: (1) an indicator for reporting white as race and non-Hispanic as ethnicity, (2) an indicator for reporting Black as race and non-Hispanic as ethnicity, (3) an indicator for reporting Hispanic as ethnicity, (4) an indicator for reporting male as sex, (5) an indicator for classification as an English-Language Learner, and (6) an indicator for Free-or-Reduced-Price-Lunch eligibility. The outcome in column (7) is a predicted index of earnings at age 30 constructed by estimating the relationship between earnings and student demographics with a sample of pre-period cohorts and then predicting later-life earnings for all cohorts based on these relationships. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 3: Effects of Import Competition on Local Employment & Income in Texas

	(1) Earnings Per Capita	(2) Man. Emp.	(3) Total Emp.	(4) Emp:Pop
De-trended DD	-2,194***	-1,443***	-5,370*	-0.030***
	(258)	(462)	(3,158)	(0.008)
Percent Change	-17.6	-22.3	-13.2	-6.8
Pre-period Mean	12,432	6,475	40,702	0.436
N	4,810	4,810	4,810	4,810

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effects of exposure to Chinese import competition on employment-to-population ratios and per capita income, using employment counts from the County Business Patterns Database, population counts by age group from the Survey of Epedemiology and End Results, personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset, and employment and wage data broken down by age and education levels from the Quarterly Workforce Indicators dataset. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator. The outcome variables across columns are as follows: (1) per-capita wage and salary income, manufacturing (2) and overall employment (3) in the county, and (4) employment-to-population ratio. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 4: Relevance of the Labor Demand Shock to Human Capital Decisions

	(1) Earnings, 14-24	(2) Earnings, No College	(3) College Earnings Premia	(4) Per-pupil Property Tax Revenue	(5) Per-pupil K-12 Spending	(6) FRPL Eligibility
De-trended DD	-3,620***	-2,839***	0.257***	-2,786***	39	0.030***
	(365)	(477)	(0.025)	(421)	(98)	(0.010)
Percent Change	-18.0	-8.2	15.0	-75.7	0.43	8.0
Pre-period Mean	20,166	34,621	1.709	3,681	9,094	0.370
N	4,286	4,298	4,302	4,810	4,810	3,678,707

-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effects of exposure to Chinese import competition on earnings by age and educational attainment, K-12 spending, and eligibility for Free-or-Reduced-Price Lunch using wage data broken down by age and education levels from the Quarterly Workforce Indicators dataset, K-12 finance data from the National Center for Education Statistics Common Core of Data, and student-level data from the UHERC. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level (and student-level) outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Column (6) further controls for student demographics, including pre-shock FRPL-eligibility. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator. The outcome variables in columns (1) and (2) are average earnings for school-aged workers and workers without any college attainment, respectively. The outcome in column (3) is the ratio of earnings for workers with a college degree to those without any college attainment. The outcomes in columns (4) and (5) are per-pupil school district revenue from local property taxes and per-pupil current expenditure, respectively. The outcome in column (6) is an indicator variable for eligibility for the Free-or-Reduced-Price Lunch program. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 5: Effects of the Labor Demand Shock on HS Graduation and Course Selection

	(1) HS Grad	(2) Total Voc. Electives	(3) Industrial Electives	(4) Business Electives	(5) Dual-Enrollment Courses
De-trended DD	0.008	-0.059	-0.156***	0.083*	0.507***
	(0.006)	(0.096)	(0.032)	(0.045)	(0.106)
Percent Change	1.2	-1.3	-22.0	4.0	251.5
Pre-period Mean	0.706	4.615	0.709	2.066	0.202
N	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on high school educational attainment and course selection using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome in column (1) is an indicator for graduating high school. The outcomes in columns (2) - (4) are counts of the total vocational elective courses, industrial elective courses, and business electives completed completed in high school. Column (4) reflects a smaller sample size, because dual-credit courses could only be observed starting with the 1996 ninth-grade cohort. The outcome column (5) is the total number of courses completed through dual-enrollment at local colleges while in high school. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for treated counties.

Table 6: Effects of the Labor Demand Shock on College Enrollment and Degree Receipt

	(1) Enrolled	(2) Enrolled at CTC	(3) Enrolled at Uni	(4) Total Semesters	(5) Certificate	(6) AA/AS	(7) BA/BS
De-trended DD	0.018***	0.012***	0.016***	0.203***	-0.001	-0.001	0.011***
	(0.005)	(0.005)	(0.004)	(0.067)	(0.001)	(0.002)	(0.002)
Percent Change	4.2	3.6	9.5	4.7	-3.1	-3.2	8.3
Pre-period Mean	0.423	0.328	0.173	4.334	0.018	0.043	0.127
N	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on college enrollment and degree receipt using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome in column (1) is an indicator for enrolling in a public two- or four-year college or university within two years of expected HS graduation. The outcomes in columns (2) and (3) are indicators for enrollment at a two-year community or technical college and a four-year university within two years of expected HS graduation, respectively; these variables are not defined to be mutually exclusive (i.e., for a student that enrolls at both a two-year and a four-year institution within two years of expected HS graduation, both indicators will populate as 1). The outcome in column (4) is the total number of semesters a student enrolled in by age 25. The outcomes in columns (5) through (7) are indicator variables for receiving a certificate, associate's degree, and bachelor's degree by age 25, respectively. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 7: Intensive-Margin Adjustments: Two-Year College Enrollment By Field

	(1) Manufacturing/ Construction	(2) IT	(3) Health	(4) Business	(5) Education
Parametric DD	-0.003***	0.002*	0.009***	0.002*	0.000
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Percent Change	-37.4	25.5	56.5	16.0	0.6
Pre-period mean	0.009	0.006	0.015	0.015	0.007
N	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on two-year college enrollment by field of study using data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. Outcomes are indicators for enrolling in a public two-year college in Texas and majoring in a field of study belonging to particular categories defined by Foote and Grosz (2020) based on Classification of Instructional Programs codes. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 8: Direct Effects of the Labor Demand Shock on Earnings of Young Workers

	(1) Share of Qtrs Employed	(2) Earnings (2020$)	(3) Employed Every Qtr	(4) Conditional Earnings
Shock Exposure	-0.013*	-1,248***	-0.019***	-1,641**
	(0.007)	(388)	(0.007)	(696)
Percent Change	-2.3	-8.8	-4.4	-5.6
Pre-period Mean	0.567	14,253	0.429	29,517
N	655,560	655,560	655,560	288,407

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on earnings of recent labor market entrants. Estimates reflect coefficients from an individual fixed effects model that compares within-person changes in earnings of young workers from counties that were more vs. less exposed to the China shock. The specification controls for county and degree-type-by-year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county. The outcome in column (1) is the annual share of quarters an individual was employed in Texas. The outcome in column (2) is annual earnings (2020$). The outcome in column (3) is an indicator for being employed in Texas in every quarter of the year. The outcome in column (4) is earnings (2020$), conditional on being employed in Texas in each quarter that year. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 9: Effects of Exposure to the Labor Demand Shock During K-12 on Earnings at 30

	(1) Share of Qtrs Employed	(2) Earnings (2020$)	(3) Employed Every Qtr	(4) Conditional Earnings
De-trended DD	0.015**	1,579***	0.016**	1,802***
	(0.008)	(410)	(0.007)	(267)
Percent Change	2.8	7.3	3.7	4.0
Pre-period Mean	0.513	21,705	0.445	45,561
N	2,135,226	2,135,226	2,135,226	1,074,826

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks during youth and adolescence on employment and earnings outcomes at age 30 using individual-level linked data from the University of Houston Education Research Center. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome in column (1) is the share of quarters at age 30 an individual was employed in Texas. The outcome in column (2) is annual earnings (2020$) at age 30. The outcome in column (3) is an indicator for being employed in every quarter at age 30 in Texas. The outcome in column (4) is earnings (2020$) at age 30, conditional on being employed in Texas in each quarter that year. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 10: Heterogeneous Effects of the Shock on Vulnerable Populations

	(1) HS Grad	(2) Total Ind. Courses	(3) Total Dual-Credit Courses	(4) Enrolled	(5) Total Semesters	(6) BA/BS	(7) Enrolled in Man./Con.	(8) Earnings
A. Male Students								
De-trended DD	0.001	-0.080***	0.490***	0.020***	0.253***	0.009***	-0.007***	2,133***
	(0.007)	(0.018)	(0.088)	(0.005)	(0.054)	(0.002)	(0.001)	(525)
Percent Change	0.2	-25.9	291.2	5.4	6.9	9.0	-43.6	8.8
Pre-period Mean	0.673	0.308	0.168	0.380	3.651	0.101	0.016	24,224
B. FRPL-Eligible Students								
De-trended DD	-0.012	-0.073***	0.468***	0.000	0.047	0.000	-0.005***	1,643***
	(0.009)	(0.016)	(0.066)	(0.005)	(0.050)	(0.001)	(0.001)	(402)
Percent Change	-2.1	-34.3	388.0	0.1	1.8	1.0	-64.7	9.9
Pre-period Mean	0.606	0.213	0.121	0.277	2.617	0.049	0.008	16,624
C. Racial and Ethnic Minorities								
De-trended DD	0.002	-0.075***	0.687***	0.012**	0.201***	0.007***	-0.005***	1,750***
	(0.009)	(0.016)	(0.098)	(0.006)	(0.075)	(0.002)	(0.001)	(401)
Percent Change	0.4	-36.0	466.4	3.6	5.8	9.4	-64.6	9.9
Pre-period Mean	0.650	0.208	0.147	0.340	3.443	0.077	0.008	17,601

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the heterogeneous effects of exposure to negative local labor demand shocks on human capital accumulation for subgroups of students that were particularly vulnerable to the shock: male students (A), students eligible for free-or-reduced-price lunch (B), and racial and ethnic minorities (C). Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome in column (1) is an indicator for graduating from a public TX high school. The outcomes in columns (2) and (3) are total industrial and dual-credit courses completed, respectively. The outcome in column (4) is an indicator for enrolling in a public two- or four-year college or university within two years of expected HS graduation. The outcomes in columns (5) and (6) are the total number of semesters a student enrolled in by age 25 and an indicator for earning a bachelor's degree by age 25. The outcome in enrollment at a two-year community or technical college and a four-year university within two years of expected HS graduation, respectively; these variables are not defined to be mutually exclusive (i.e., for a student that enrolls at both a two-year and a four-year institution within two years of expected HS graduation, both indicators will populate as 1). The outcome in column (7) is an indicator for enrolling in a manufacturing or construction program at a community or technical college. The outcome in column (8) is earnings in positions covered by unemployment insurance in Texas at age 30. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 11: Effects of the Labor Demand Shock on Standardized Test Scores

	(1) Combined	(2) Math	(3) Reading
De-trended DD	0.144	0.096^{*}	0.048
	(0.112)	(0.054)	(0.060)
N	3,678,707	3,678,707	3,678,707

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the effects of exposure to local shocks on 8th-grade test scores, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021) The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome variables are combined, math, and reading test stores on exams administered in 8th grade. All test scores are standardized to have a mean of 0 and standard deviation of 1.

Table 12: Students Did not Migrate to Less-Exposed Counties for College

	(1) Enrolled in High-Exp. County	(2) Enrolled in Low-Exp. County	(3) Moved for College	(4) Moved to High-Exp. for College	(5) Moved to Low-Exp. for College
De-trended DD	0.017**	-0.001	0.014*	0.013	0.001
	(0.008)	(0.008)	(0.007)	(0.008)	(0.004)
Percent Change	3.8	-1.1	4.5	6.4	1.0
Pre-period Mean	0.439	0.134	0.305	0.198	0.107
N	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on migration patterns, proxying for a student's adult residence with the location of the college or university they attended. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome in column (1) and (2) are indicators for enrolling in a public two- or four-year college or university within two years of expected HS graduation in a county with above-median and below-median exposure to PNTR, respectively. The outcome in column (3) is an indicator for enrolling in a college or university outside of the county where a student attended K-12. The outcomes in columns (4) and (5) are indicator variables for enrolling in a college or university in a high-exposure (low-exposure) county other than where a student attended K-12. In all outcomes, students are assigned the location of the modal postsecondary institution they attended. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 13: Effects of Exposure to the Labor Demand Shock on Later-Life Employment by Industry

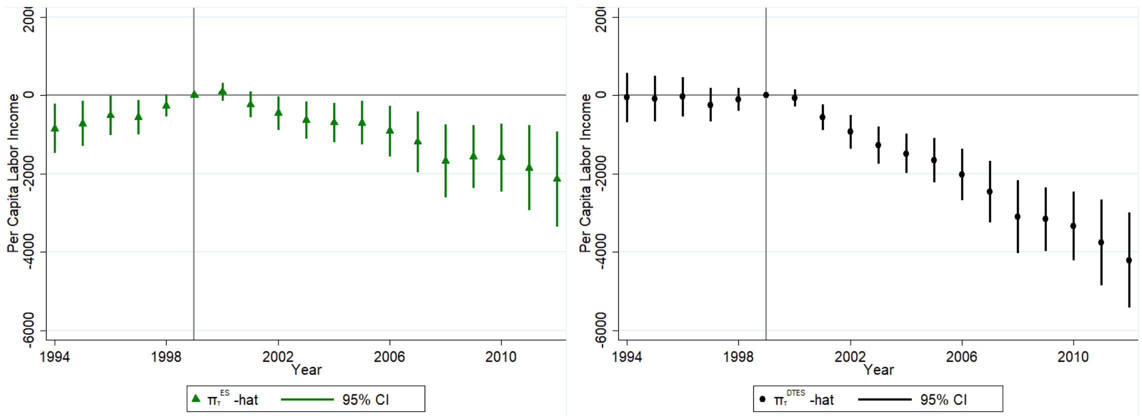
	(1) Man.	(2) Cost./Trans.	(3) Oil and Gas	(4) Retail	(5) Food and Accomm.
De-trended DD	0.004***	0.002**	0.002***	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Percent Change	11.7	5.1	20.8	-0.3	-0.0
Pre-period Mean	0.033	0.042	0.012	0.059	0.036
N	2,135,226	2,135,226	2,135,226	2,135,226	2,135,226
	(6) FIRE	(7) Prof. Services	(8) Information	(9) Admin. Services	(10) Health
De-trended DD	0.004***	0.001	0.002***	0.000	0.003*
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Percent Change	10.3	2.7	15.7	0.9	3.5
Pre-period Mean	0.040	0.033	0.010	0.048	0.075
N	2,135,226	2,135,226	2,135,226	2,135,226	2,135,226

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks during youth and adolescence on employment by industry at age 30 using individual-level linked data from the University of Houston Education Research Center. Industries are categorized by two-digit NAICS codes. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

A Additional Results



(a) Per-Capita Labor Income (Nationwide)

(b) Adjusted for Linear Pre-trends (Nationwide)

Figure A1: Pre-Trends in Labor Demand Present in Texas Were Exhibited Nationwide

Notes: These figures present estimates of the effect of exposure to Chinese import competition on per-capita labor income across all U.S. counties using personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset and population counts by age group from the Survey of Epedemiology and End Results. Estimates in panel (a) reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between counties with above- and below-median exposure to local labor market shocks caused by import competition over time relative to this relationship in 1999, one year prior to the start of treatment. Estimates in panel (b) reflect coefficients from a the two-step de-trended event study (equation (3)) that partials out a linear pre-trend in the first step. Both specifications control for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county. Standard errors for the two-step procedure reflected in panel (b) account for parameters estimated in the first step via a degrees-of-freedom adjustment.

Table A1: "First-Stage" Effects: Robustness to Alternative Panel Lengths

	(1) Earnings Per Capita	(2) Man. Emp	(3) Total Emp	(4) Emp:Pop
Panel, 1994-2007				
De-trended DD	-1,210***	-894***	-3,392	-0.014**
	(209)	(259)	(2,533)	(0.007)
Panel, 1994-2012				
De-trended DD	-2,194***	-1,443***	-5,370*	-0.030***
	(258)	(462)	(3,158)	(0.008)
Panel, 1994-2016				
De-trended DD	-3,144***	-1,710***	-6,174	-0.039***
	(322)	(570)	(3,960)	(0.008)
Percent Change, 2007	-9.7	-13.8	-8.3	-3.3
Percent Change, 2012		-17.6	-22.3	-13.2
-6.8				
Percent Change, 2016	-25.3	-26.4	-15.2	-8.9
Pre-period Mean	12,432	6,475	40,703	0.436
Number of Counties	254	254	254	254

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to Chinese import competition on employment-to-population ratios and per capita income, using employment counts from the County Business Patterns Database, population counts by age group from the Survey of Epedemiology and End Results, personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset, and employment and wage data broken down by age and education levels from the Quarterly Workforce Indicators dataset. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). Each row corresponds to an analysis sample with a different panel end-period. The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator. The outcome variables in columns (1) and (2) are manufacturing and overall employment in the county. Columns (3) and (4) scale employment and earnings relative to the working-age population. The outcomes and column (5) and (6) are average annual earnings for workers aged 14-24 and for workers without a college degree. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A2: Effects of Import Competition on Earnings by Age Group

	(1) 14-24	(2) 25-33	(3) 35-54	(4) 55+
De-trended DD	-1,207***	-1,261***	-429*	-1,036***
	(122)	(191)	(223)	(224)
Percent Change	-18.0	-10.3	-2.8	-7.5
Pre-period Mean	6,722	12,250	15,589	13,843
N	4,286	4,299	4,302	4,299

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to Chinese import competition on per-capita income by age range, using data on earnings for stable workers broken down by age and education levels from the Quarterly Workforce Indicators dataset and population counts by age group from the Survey of Epedemiology and End Results. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A3: Effects of Import Competition on Earnings by Education Level

	(1) Less than HS	(2) HS Only	(3) Some College	(4) Bachelor's or More
De-trended DD	-1,261***	-799***	-835***	1,118***
	(153)	(168)	(188)	(378)
perc_change12	-12.5	-6.5	-5.9	5.4
Pre-period Mean	10,101	12,242	14,265	20,819
N	4,298	4,302	4,302	4,302

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to Chinese import competition on per-worker income by education levels, using data on employment and earnings for stable workers broken down by education levels from the Census Quarterly Workforce Indicators database. The outcome variables are county-level average annual earnings among workers with stable employment among each educational attainment group. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A4: Did Local High Schools Adjust Course Offerings?

	(1) AP/IB	(2) All Vocational	(3) Industrial	(4) Technical	(5) Agricultural	(6) Health	(7) Business
Parametric DD	0.7206	0.1275	-0.9955	0.1092	0.4573	0.1118	0.4048
	(0.5795)	(1.9064)	(1.0153)	(0.0853)	(0.5444)	(0.1312)	(0.6887)
Percent Change	15.5	0.5	-24.8	109.9	5.0	17.3	5.7
Pre-period mean	4.6	26.4	4.0	0.1	8.8	0.7	7.1
	3744	3744	3744	3744	3744	3744	3744

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to Chinese import competition on course offerings at local high schools. I define average categorical counts of courses across all public high schools in each county. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county.

Table A5: Did Local Two-Year Colleges Adjust Program Offerings?

	(1) Q1	(2) Q2	(3) Q3	(4) Q4
De-trended DD	-0.53	-0.94	-0.79	1.47
	(1.39)	(0.69)	(0.83)	(0.91)
Percent Change	-6.1	-26.3	-7.7	28.3
Pre-period Mean	8.6	3.6	10.3	5.2
N	1,313	1,285	1,349	1,340

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to Chinese import competition on programmatic offerings (six-digit CIP codes) at two-year public colleges by the major's exposure to the shock. I define shock exposure for each major (two-digit CIP code) as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator. The outcome variables in each column are the number of unique six-digit CIP code programs offered in a county within that quartile of exposure. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A6: Heterogeneous Effects on College Enrollment by Student Demographics

	(1) White	(2) Black	(3) Hispanic	(4) Male	(5) Female	(6) ELL	(7) Non-ELL
A. Enrollment							
De-trended DD	0.010*	0.011	0.032***	0.027***	0.021***	-0.017**	0.020***
	(0.005)	(0.008)	(0.008)	(0.005)	(0.006)	(0.008)	(0.006)
Percent Change	2.0	3.3	9.8	7.1	4.5	-9.1	5.6
Pre-period Mean	0.500	0.336	0.324	0.380	0.467	0.187	0.443

Standard errors clustered by county in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the heterogeneous effects of exposure to negative local labor demand shocks on college enrollment by student demographics, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome is an indicator variable for enrolling in a public two- or four-year college in Texas within two years of expected HS graduation. Columns denote the subgroup composing estimating samples: (1) white students, (2) Black students, (3) Hispanic students, (4) male students, (5) female students, (6) English-Language Learners, (7) non-English-Language-Learners. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A7: Heterogeneous Effects on Earnings by Student Demographics

	(1) White	(2) Black	(3) Hispanic	(4) Male	(5) Female	(6) ELL	(7) Non-ELL
De-trended DD	1,182**	1,964***	1,883***	2,198***	1,080***	347	1,594***
	(473)	(510)	(398)	(538)	(339)	(545)	(452)
Percent Change	4.6	12.4	10.6	9.1	5.7	2.9	5.7
Pre-period Mean	25,727	15,827	17,784	24,224	19,045	11,875	22,609
N	522,097	138,647	387,744	550,684	524,142	132,000	942,826

Standard errors clustered by county in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the heterogeneous effects of exposure to negative local labor demand shocks on earnings at age 30 by student demographics, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a parametric difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. The outcome is earnings (2020$) at age 30. Columns denote the subgroup composing estimating samples: (1) white students, (2) Black students, (3) Hispanic students, (4) male students, (5) female students, (6) English-Language Learners, (7) non-English-Language-Learners. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A8: Robustness of Main Results to Sequentially Adding Covariates

	(1) Preferred (Col. (5) + Fracking)	(2) No Controls	(3) + Demos	(4) + County Covariates	(5) + Trade Policies	(6) + Housing Bubble	(7) + Financial Crisis	(8) + Dot-Com Crash
A. College Enrollment								
De-trended DD	0.018***	0.012*	0.012*	0.014***	0.018***	0.021***	0.019***	0.013**
	(0.005)	(0.007)	(0.007)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)
Percent Change	4.2	2.9	2.9	3.3	4.2	5.1	4.6	3.0
Pre-period Mean	0.423	0.423	0.423	0.423	0.423	0.423	0.423	0.423
N	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707	3,678,707
B. Earnings								
De-trended DD	1,579***	1,431***	1,450***	1,401***	1,593***	1,724***	1,810***	1,604***
	(410)	(352)	(354)	(427)	(411)	(403)	(403)	(360)
Percent Change	7.3	6.6	6.7	6.5	7.3	7.9	8.3	7.4
Pre-period Mean	21,705	21,705	21,705	21,705	21,705	21,705	21,705	21,705
N	1,074,826	1,074,826	1,074,826	1,074,826	1,074,826	1,074,826	1,074,826	1,074,826

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on college enrollment and later-life earnings, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The outcome variable in Panel A is an indicator variable for enrolling in a public two- or four-year college in Texas within two years of expected HS graduation. The outcome variable in Panel B is earnings at age 30, conditional on being employed in all four quarters in Texas. Column (1) controls for student-level demographics, 1990 county characteristics interacted with a post dummy, exposure to changes in other trade policies, and exposure to the fracking boom and corresponds to the preferred specification throughout the paper. Column (2) includes no additional controls and columns to the right sequentially add control variables. Column (3) controls for student demographics. Column (4) controls for 1990 county characteristics interacted with a post dummy. Column (5) controls for changes to other trade policies. Column (6) controls for the housing boom. Column (7) controls for exposure to the financial crash. Column (8) controls for exposure to the dot-com bubble. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A9: Robustness to Smooth Deviations from Linear Trends (Rambachan and Roth, 2023)

	(1) Deviation Size (m)	(2) 90% Confidence Set Lower Bound	(3) 90% Confidence Set Upper Bound
A. College Enrollment			
	Parallel	0.001	0.015
	0 (Linear)	0.011	0.041
	0.0002	0.011	0.045
	0.0004	0.009	0.050
	0.0006	0.006	0.056
	0.0008	0.003	0.061
	0.0010	0.000	0.067
B. Earnings at 30			
	Parallel	422	1,959
	0 (Linear)	177	3582
	25	123	3,633
	50	67	3,684
	75	10	3,755
	100	-48	3,807
	125	-107	3,879

Notes: This table presents results from tests for the robustness of main estimates to allowing smooth deviations from the imposed continuation of linear trends (Rambachan and Roth, 2023). Estimated 90% confidence sets in row 1 of each panel reflect pooled event study coefficients under the assumption of parallel trends. Estimates in row 2 of each panel reflect pooled parametric event study coefficients under the assumption of continued linear pre-trends. All other estimates reflect robust 90% confidence sets that account for both statistical uncertainty and allow for the slope of the difference in trends between high-exposure and low-exposure students to deviate by an additional m in either direction each period. Inference is based on fixed length confidence intervals (Rambachan and Roth, 2023). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset.

Table A10: Robustness to Alternate Linear Trend Specifications

	(1) DTDD	(2) Parametric Event Study	(3) Linear Treatment
A. Pr(Enrolled)			
	0.018***	0.017*	0.034***
	(0.005)	(0.009)	(0.009)
Percent Change	4.2	3.9	8.1
Pre-period Mean	0.423	0.423	0.423
N	3,678,707	3,678,707	3,678,707
B. Earnings			
	1,579***	1,581**	6,032***
	(410)	(530)	(1,563)
Percent Change	7.3	7.3	27.8
Pre-period Mean	21,705	21,705	21,705
N	2,135,226	2,135,226	2,135,226

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks during youth and adolescence on college enrollment (Panel A) and earnings at age 30 (Panel B) using individual-level linked data from the University of Houston Education Research Center. Estimates in Column (1) correspond to our preferred de-trended difference-in-differences specification. Column (2) presents estimates of the average post-period coefficient from a "parametric event study" that includes a linear pre-trend and allows flexible effects relative to this trend in the post-period (Dobkin et al., 2018). Column (3) presents estimates of an augmented de-trended difference-in-difference specification where the $Post_c$ indicator is replaced by the share of years between kindergarten and high school that occurred after the establishment of PNTR. Each specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated treatment effect divided by the pre-period mean for high-exposure counties.

Table A11: Robustness of Main Results to Alternate Treatment Thresholds

	(1) Preferred (Median)	(2) 33rd Pct	(3) 40th Pct	(4) 45th Pct	(5) 55th Pct	(6) 60th Pct	(7) 67th Pct
A. College Enrollment							
De-trended DD	0.018***	0.016***	0.015***	0.016***	0.012**	0.020***	0.026***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)
Percent Change	4.2	3.8	3.6	3.8	2.8	4.6	6.0
Pre-period Mean	0.423	0.426	0.426	0.426	0.424	0.435	0.440
B. Earnings							
De-trended DD	1,579***	1,337***	1,335***	1,265***	1,436***	536*	591**
	(410)	(373)	(378)	(381)	(363)	(307)	(272)
Percent Change	7.3	5.6	5.6	5.3	6.0	2.2	2.4
Pre-period Mean	21,705	24,038	24,032	23,941	23,917	24,108	24,183

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on college enrollment and later-life earnings, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The outcome variable in Panel A is an indicator variable for enrolling in a public two- or four-year college in Texas within two years of expected HS graduation. The outcome variable in Panel B is earnings at age 30, conditional on being employed in all four quarters in Texas. Results in each column reflect estimates that define high-exposure counties based on whether their tariff gap falls above the labeled percentile as a "treatment" threshold. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

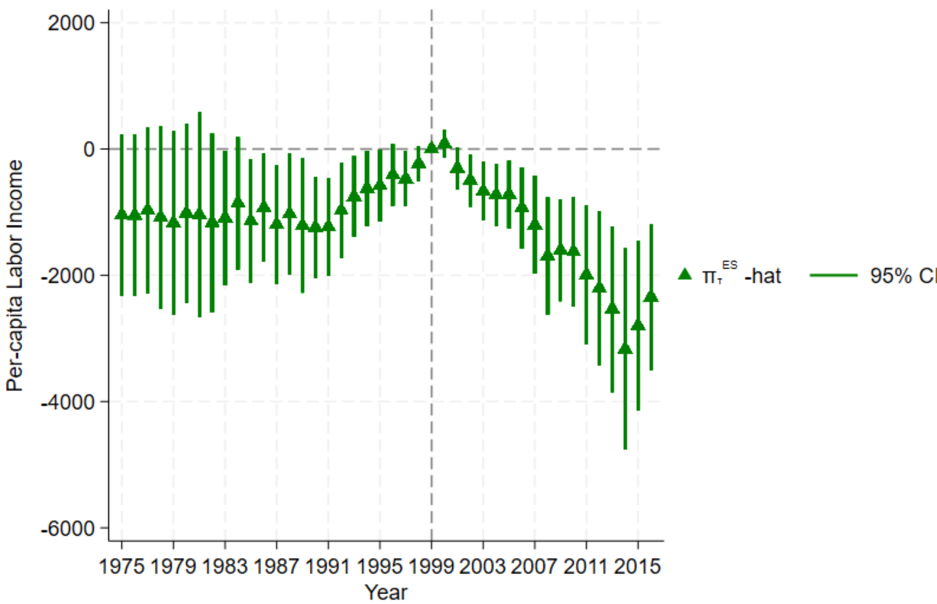
Table A12: Robustness of Main Results to "Donut" Treatment Thresholds

	(1) Preferred (Median)	(2) 45th-55th Donut	(3) 40th-60th Donut	(4) 33rd-67th Donut
A. College Enrollment				
De-trended DD	0.018***	0.015***	0.027***	0.037***
	(0.005)	(0.005)	(0.006)	(0.006)
Percent Change	4.2	3.6	6.1	8.4
Pre-period Mean	0.423	0.424	0.435	0.440
B. Earnings				
De-trended DD	1,579***	1505***	1598***	1317***
	(410)	(390)	(352)	(347)
Percent Change	7.3	6.3	6.6	5.5
Pre-period Mean	21,705	23,917	24,183	24,108

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on college enrollment and later-life earnings, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The outcome variable in Panel A is an indicator variable for enrolling in a public two- or four-year college in Texas within two years of expected HS graduation. The outcome variable in Panel B is earnings at age 30, conditional on being employed in all four quarters in Texas. Results in Column (1) reflect the preferred specification. Results in each additional column reflect estimates from "donut" specifications that drop individuals from counties with tariff gap falling between the specified middle range around the median, the "treatment" threshold. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.



(a) Per-Capita Labor Income

Figure A2: Existing Differential Trends in Labor Demand Started in 1991

Notes: This figure presents estimates of the effect of exposure to Chinese import competition on per-capita labor income using personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset and population counts by age group from the Survey of Epedemiology and End Results. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between counties with above- and below-median exposure to local labor market shocks caused by import competition over time relative to this relationship in 1999, one year prior to the start of treatment. The specification controls for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county.

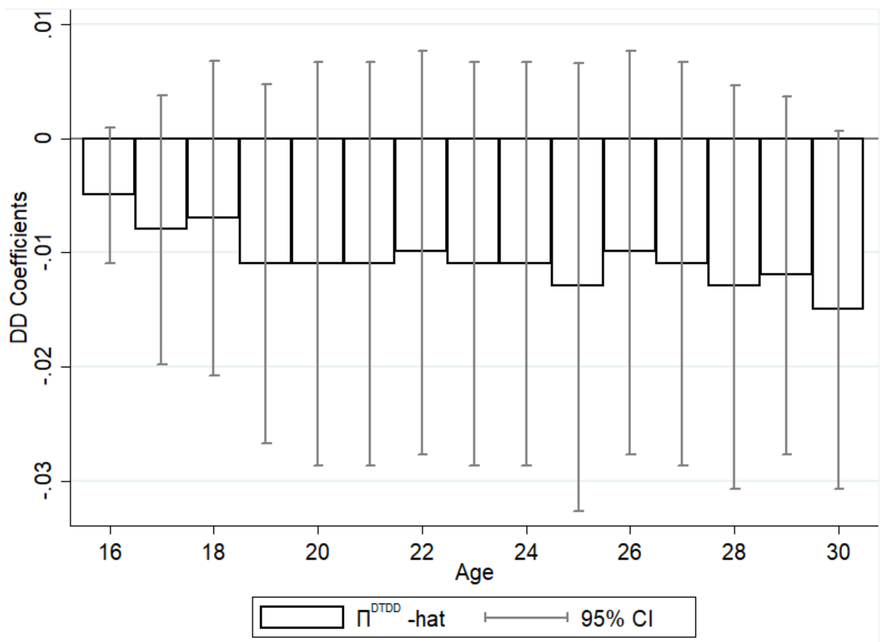
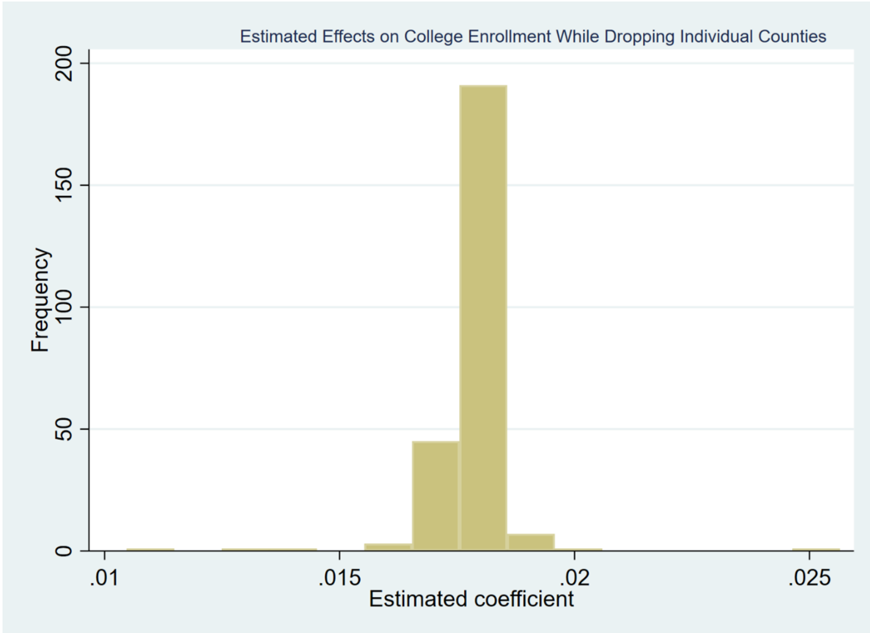
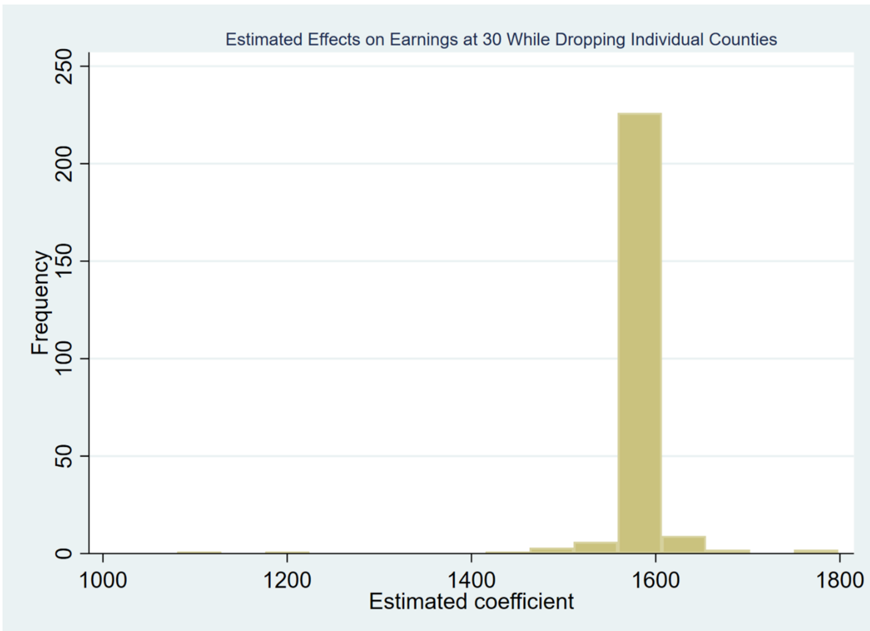


Figure A3: No Statistically Significant Evidence of Differential Attrition from the Dataset

Notes: This figure present estimates of the effect of exposure to Chinese import competition on never being observed again in the UHERC dataset from that age through age 30. Each coefficient reflects the estimate of a de-trended difference-in-differences specification that compares changes in outcomes measured at a specific age of students from counties with above- and below-median exposure to local labor market shocks caused by import competition relative to existing differential linear trends (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, and other changes to US trade policy. Standard errors are clustered by county and adjusted to account for using a regression-adjusted estimator.



(a) Pr(Enrolled)



(b) Earnings

Figure A4: Robustness to Dropping Each County from the Sample

Notes: These figures present histograms of the estimated effects of exposure to the labor demand shock on (a) college enrollment and (b) earnings at age 30 when iteratively dropping each county from the sample.

Table A13: Robustness of Main Results to Assigning Treatment Based on 9th Grade County

	(1) Enrolled	(2) Earnings
A. College Enrollment		
De-trended DD	0.019***	1,606***
	(0.005)	(415)
Percent Change	4.4	7.4
Pre-period Mean	0.423	21,705
N	3,678,707	1,074,826

DoF-adjusted standard errors in parentheses are clustered at the county level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on college enrollment and later-life earnings, using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students that entered 9th grade in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The outcome variable in Column (1) is an indicator variable for enrolling in a public two- or four-year college in Texas within two years of expected HS graduation. The outcome variable in Column (2) is earnings at age 30, conditional on being employed in all four quarters in Texas. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table A14: Four-Year Enrollment Responses Across Tuition Quartiles

	(1) Q1 Tuition	(2) Q2 Tuition	(3) Q3 Tuition	(4) Q4 Tuition	(5) Private
De-trended DD	0.008***	0.003*	-0.005**	0.008***	-0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)
Percent Change	57.3	8.3	-18.9	9.0	-7.1
Pre-period Mean	0.014	0.038	0.028	0.094	0.028
N	3,678,707	3,678,707	3,678,707	3,678,707	3,154,442

Standard errors clustered by county in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on enrollment at public four-year college enrollment across the distribution of in-state tuition and fees and at private four-year colleges. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. Standard errors are clustered by county. Outcomes in columns (1) through (4) are indicators for enrolling within two years of expected HS graduation at public four-year colleges grouped by quartile of 1999 in-state tuition and fees. The outcome in column (5) is an indicator for enrolling within two years of expected HS graduation at a private college Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

A.1 Data-Driven Definitions of Course and Major Adjustments

In Section 5.1, I show that exposure to the China shock caused students to take fewer manufacturing-based vocational electives and more business electives in high school. I argue that this adjustment reflects students observing salient declines in the returns to manufacturing-based human capital. To further support this interpretation, I examine whether students took more or fewer courses that predict employment in manufacturing subsectors affected by PNTR. I define field-level exposure to the China shock by regressing employment in an industry with a high tariff gap on counts of course completions across two-digit TEA field classifications for cohorts that graduated high school and entered the labor force before the onset of the shock.

Table A15 shows that students exposed to larger local shocks took 5% and 4% fewer courses in the two highest quartiles of strength in predicting employment in industries exposed to the China shock, while taking 6% more courses in the second quartile. I find no statistically distinguishable effects on the number of courses completed in the lowest quartile.

I also find that exposure to the China shock made students less likely to enroll in manufacturing-based programs at two-year colleges in Section 5.2. I adopt a similar data-driven exercise as above to complement this result and support the interpretation that this reflects students internalization of the labor demand shock's differential incidence across sectors. Using linked college and workforce records for pre-shock graduates, I define major-level exposure to the labor demand shock as the employment-weighted average tariff gap of industries employing graduates from a given field and credential level (i.e., AA/AS vs. BA/BS) in Texas. Table A16 presents estimates of the effects of shock exposure on enrollment across fields grouped into quartiles by exposure to the labor demand shock.[58] Consistent with students observing changes to earnings premia across majors, I find statistically significant estimates of enrollment increases in less-exposed majors in Panel A of Table A16. Students from more-exposed counties were 55% and 54% more likely to enroll in community college and choose a major in the least-exposed and second-least-exposed quartiles, respectively. I find imprecisely estimated enrollment declines of 5% and 18% for majors the second-most-

[58] Appendix B.3 shows the two-digit Classification of Instructional Programs codes in each exposure quartile.

exposed and most-exposed quartiles, further (suggestively) supporting that community college students substituted from more- to less-exposed majors.

Panel B of Table A16 presents the corresponding estimates of effects of local shocks on enrollment across more- and less-exposed majors at four-year universities. Estimates indicate that enrollment increases at universities occurred fairly evenly across majors. The lack of substitution in major choices by university students suggests that the local shocks did not differentially affect the earnings premia for bachelor's degrees across fields of study, consistent with both the more direct link between community college field of study and employment opportunities and evidence that the China shock disproportionately affected earnings for less-educated workers in Section 4.2 and other literature (Autor et al., 2014).

Table A15: Effects on Courses Completed Across Course-Exposure Quartiles

	(1) Total Q1 Courses	(2) Total Q2 Courses	(3) Total Q3 Courses	(4) Total Q4 Courses
De-trended DD	-0.095	0.367***	-0.753***	-0.113**
	(0.087)	(0.088)	(0.175)	(0.046)
Percent Change	-1.4	6.1	-5.1	-4.0
Pre-period Mean	6.672	5.971	14.866	2.831
N	3,678,707	3,678,707	3,678,707	3,678,707

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect to Chinese import competition on high school course-taking across quartlies of how strongly a course's field predicts employment in industries exposed to the China shock, using individual-level data from the University of Houston Education Research Data. I define field-level exposure by regressing employment in an industry with a high tariff gap on counts of course completions across two-digit TEA field classifications for cohorts that graduated high school and entered the labor force before the onset of the shock. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. The outcome in each column is the count of total courses completed in high school that fall within each quartile of strength in predicting employment in a high-NTR industry.

Table A16: Intensive-Margin Adjustments: Field of Study by Exposure to the China Shock

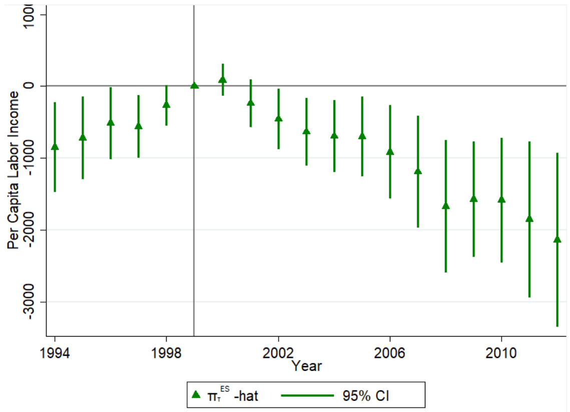
	(1) Q1	(2) Q2	(3) Q3	(4) Q4
A. Two-Year Colleges				
De-trended DD	0.017***	0.004***	-0.003	-0.005
	(0.003)	(0.001)	(0.010)	(0.003)
Percent Change	55.0	53.9	-4.7	-18.4
Pre-period Mean	0.031	0.007	0.071	0.024
N	3,678,707	3,678,707	3,678,707	3,678,707
B. Four-Year Universities				
De-trended DD	0.005***	0.002***	0.008***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Percent Change	16.9	10.4	17.1	4.6
Pre-period Mean	0.028	0.016	0.044	0.040
N	3,678,707	3,678,707	3,678,707	3,678,707

Standard errors clustered by county in parentheses

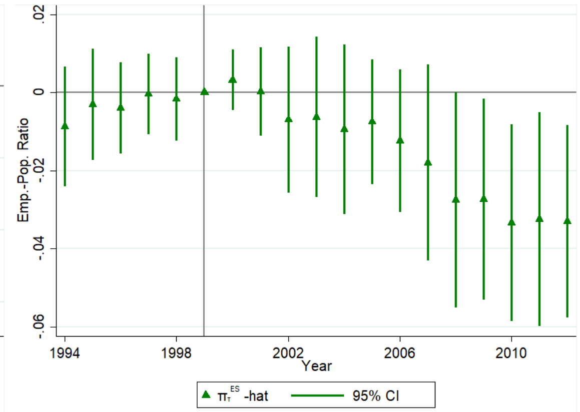
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on college enrollment by field of study where majors are grouped according to field-specific shock exposure. I define shock exposure for each major (two-digit CIP code) as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period. I define these field-specific exposure measures separately for two-year and four-year graduates. Estimates reflect coefficients from a de-trended difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences (Goodman-Bacon, 2021). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. Panel A presents estimates for two-year enrollment outcomes and Panel B presents estimates for four-year enrollment outcomes. Outcomes are indicators for enrollment in fields grouped by field-specific shock quartile. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

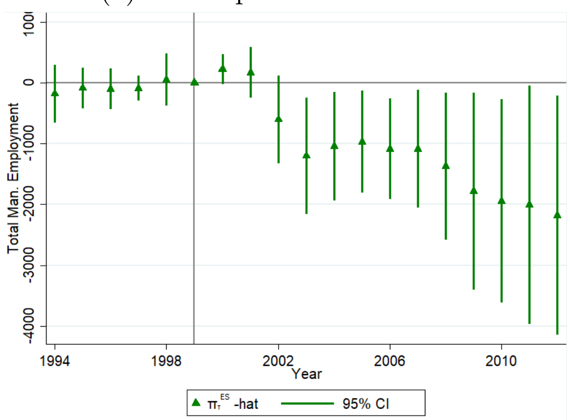
A.2 Unadjusted Event Studies



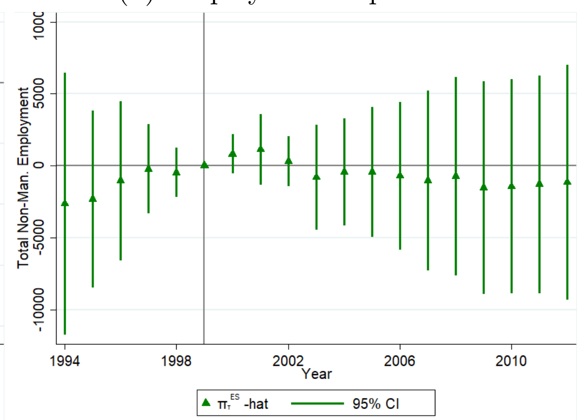
(a) Per-Capita Labor Income



(b) Employment:Population



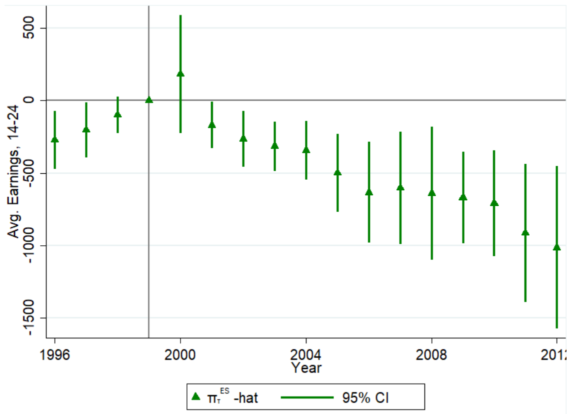
(c) Manufacturing Employment



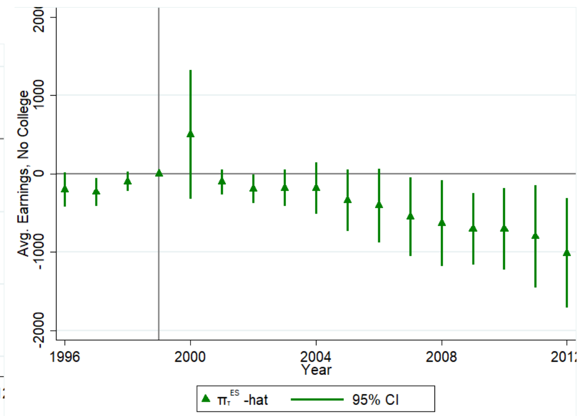
(d) Non-Manufacturing Employment

Figure A5: "First-Stage" Effects of Import Competition on Local Labor Markets

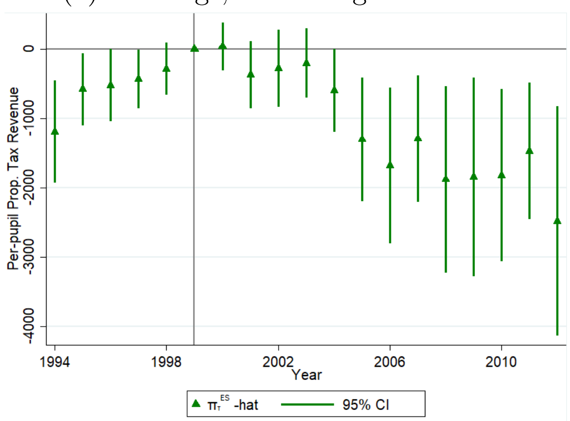
Notes: These figures present estimates of the effect of exposure to Chinese import competition on employment and labor income relative to population using employment counts from the County Business Patterns Database, population counts by age group from the Survey of Epedemiology and End Results, and personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between counties with above- and below-median exposure to local labor market shocks caused by import competition over time relative to this relationship in 1999, one year prior to the start of treatment. The specification controls for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county.



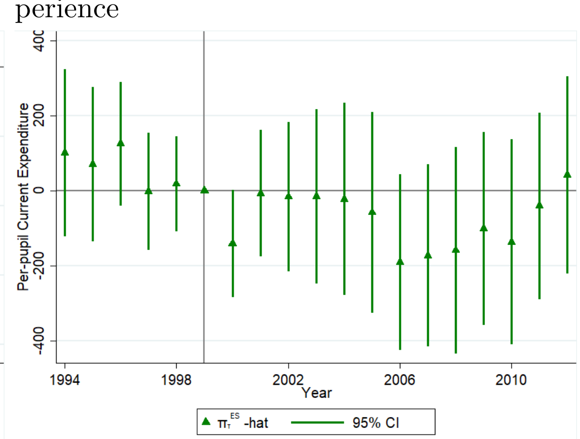
(a) Earnings, School-Aged Workers



(b) Earnings, Workers Without College Experience



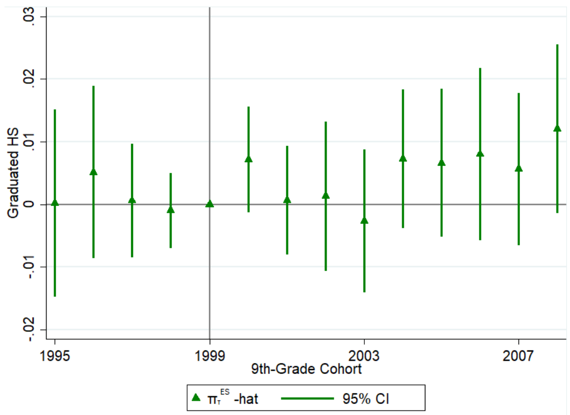
(c) Per-Pupil Property Tax Revenue



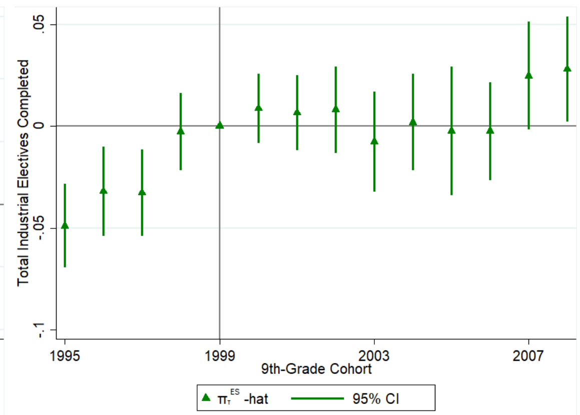
(d) Per-Pupil K-12 Expenditure

Figure A6: The Labor Demand Shock Reduced Opportunity Costs But Did Not Affect K-12 Spending

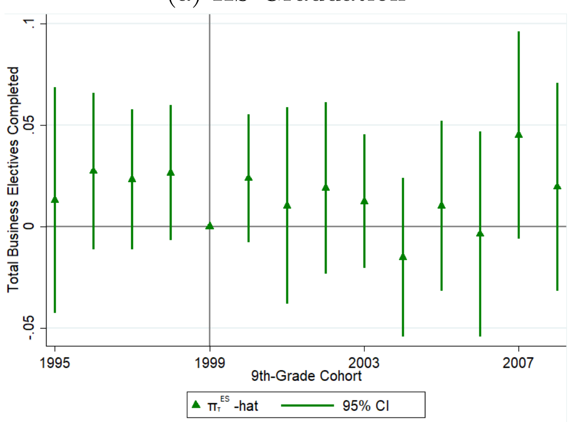
Notes: These figures present estimates of the effect of exposure to Chinese import competitionon opportunity costs and school spending using wage data broken down by age and education levels from the Quarterly Workforce Indicators dataset and K-12 finance data from the National Center for Education Statistics Common Core of Data. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between counties with above- and below-median exposure to local labor market shocks caused by import competition over time relative to this relationship in 1999, one year prior to the start of treatment. The specification controls for county and year fixed effects, 1990 county characteristics flexibly interacted with year dummies, and other changes to US trade policy. Standard errors are clustered by county.



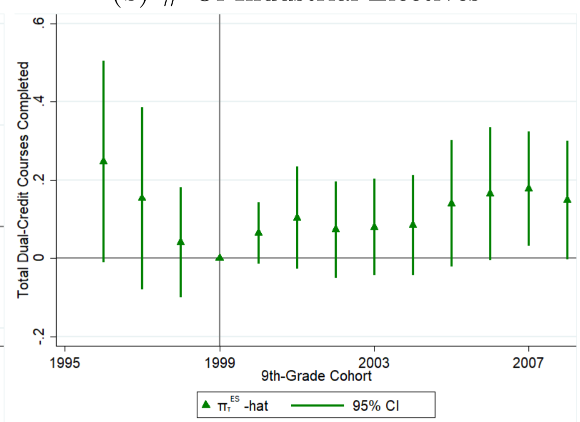
(a) HS Graduation



(b) # Of Industrial Electives



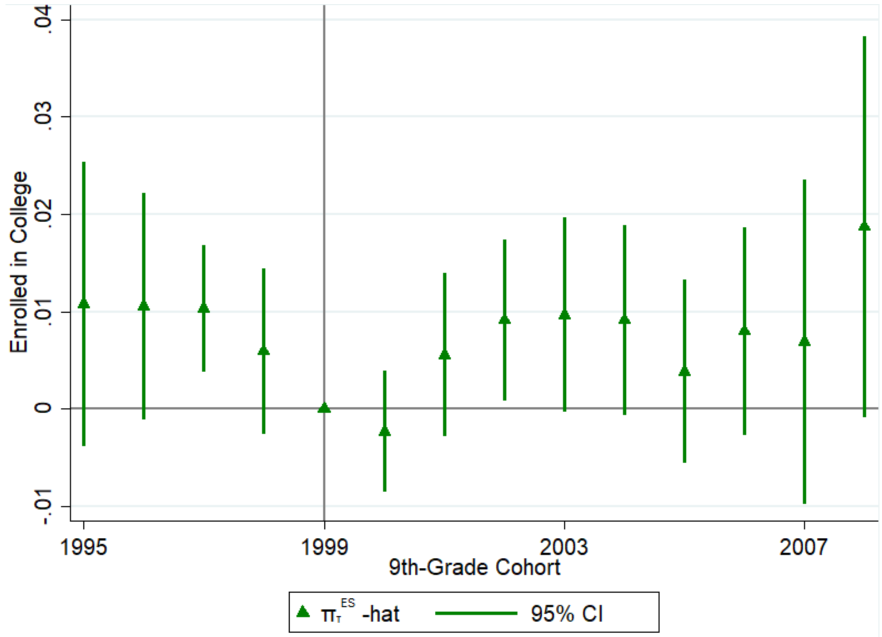
(c) # Business Electives



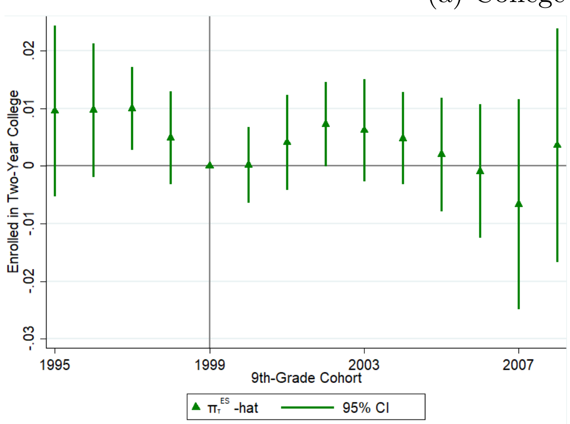
(d) Dual Enrollment

Figure A7: Effects of the Labor Demand Shock on Human Capital Accumulation in HS

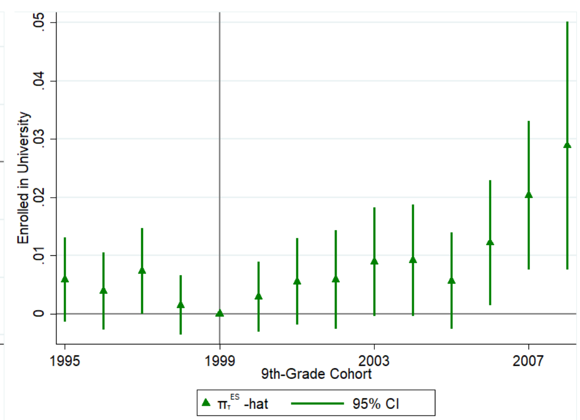
Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on high school graduation and course selection. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to this relationship for the cohort entering ninth grade in 1999, one year prior to the onset of local shocks. The outcomes are (a) an indicator for graduating high school, counts of the number of (b) manufacturing-aligned electives and (c) business electives completed, and (d) an indicator variable for enrolling in a dual-credit course at a local college. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom and other changes to US trade policy. Standard errors are clustered by county.



(a) College Enrollment



(b) CTC Enrollment

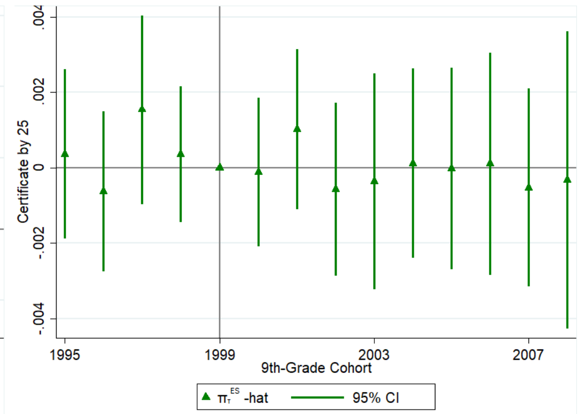


(c) University Enrollment

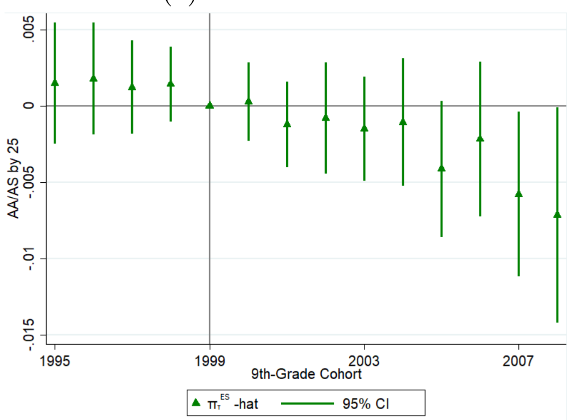
Figure A8: Effects of the Labor Demand Shock on College Enrollment

Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on college enrollment. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to this relationship for the cohort entering ninth grade in 1999, one year prior to the onset of local shocks. The outcomes are (a) an indicator for enrolling at any public two- or four-year college or university in Texas within two years of expected high school graduation and separate indicators for enrolling at (b) a public two-year community or technical college (CTC) and (c) a public four-year university. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to the fracking boom, and other changes to US trade policy. Standard errors are clustered by county.

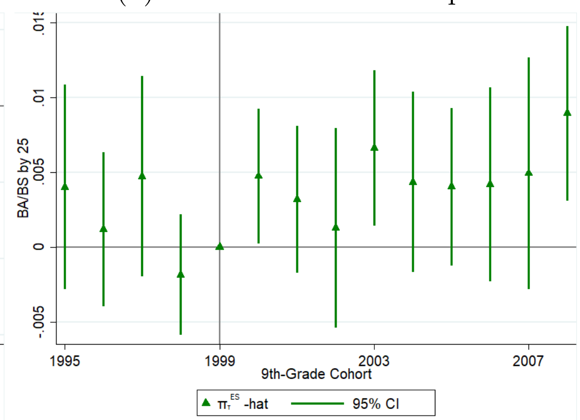
(a) Total Semesters



(b) CTC Certificate Receipt



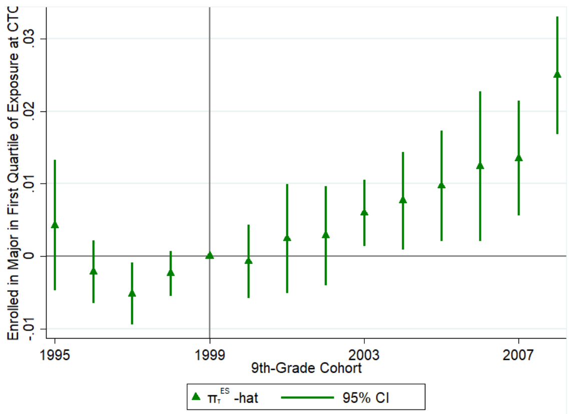
(c) Associate's Degree Receipt



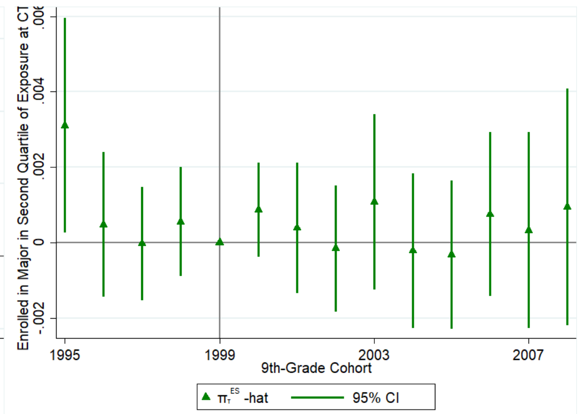
(d) Bachelor's Degree Receipt

Figure A9: Effects of the Labor Demand Shock on College Attainment

Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on human capital accumulation. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to this relationship for the cohort entering ninth grade in 1999, one year prior to the onset of local shocks. The outcomes are total semesters enrolled in public colleges and universities in Texas by age 25 (a), receipt of a technical certificate from a two-year college by age 25 (b), associate's degree receipt by 25 (c), and bachelor's degree receipt by 25 (d). Students are assigned to the county where they first appeared attending school prior to the start of the shock. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to fracking, and other changes to US trade policy. Standard errors are clustered by county.

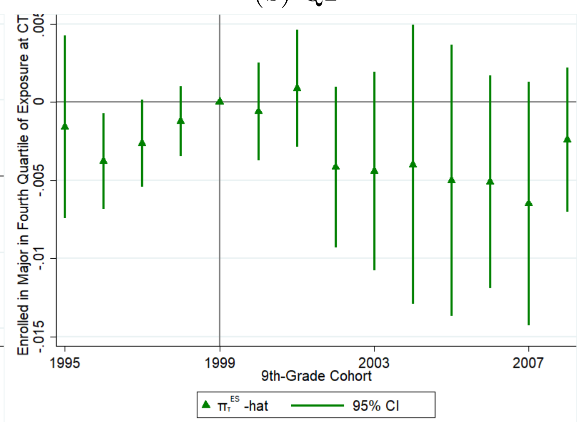


(a) Q1



(b) Q2

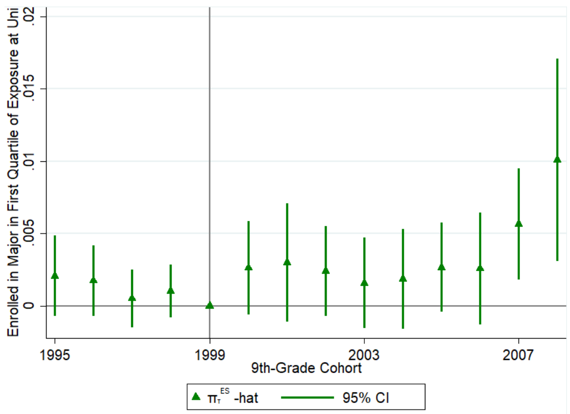
(c) Q3



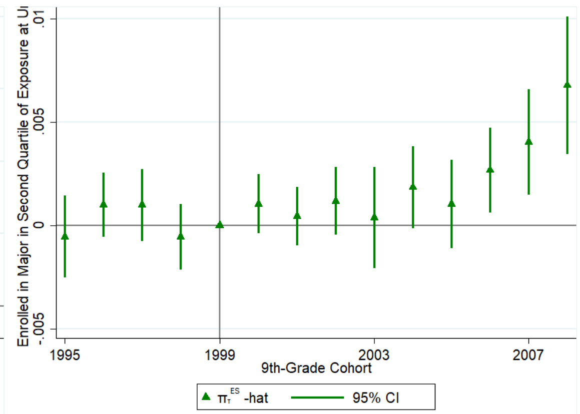
(d) Q4

Figure A10: Effects on Enrollment by Field Exposure Quartile at Two-Year Colleges

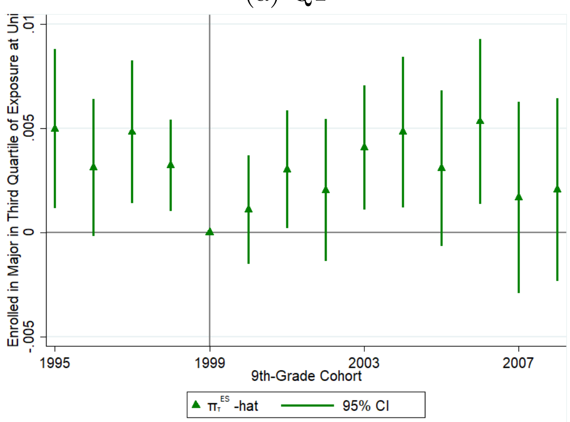
Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on enrollment by major at public two-year colleges. I define shock exposure for each major (two-digit CIP code) as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period and bin majors into quartiles. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to this relationship for the cohort entering ninth grade in 1999, one year prior to the onset of local shock. Outcomes are indicator variables for enrollment in a two-year college and selection of a major within that quartile of exposure. Students are assigned to the county where they first appeared attending school prior to the start of the shock. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to fracking, and other changes to US trade policy. Standard errors are clustered by county.



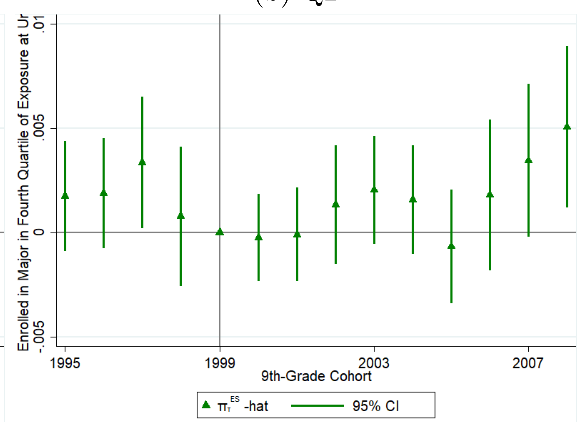
(a) Q1



(b) Q2



(c) Q3



(d) Q4

Figure A11: Effects on Enrollment by Field Exposure Quartile at Four-Year Colleges

Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on enrollment by major at public four-year universities. I define shock exposure for each major (two-digit CIP code) as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period and bin majors into quartiles. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from counties that were more or less exposed to negative labor demand shocks across cohorts relative to this relationship for the cohort entering ninth grade in 1999, one year prior to the onset of local shock. Outcomes are indicator variables for enrollment in a four-year university and selection of a major within that quartile of exposure Students are assigned to the county where they first appeared attending school prior to the start of the shock. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics flexibly interacted with year dummies, county exposure to fracking, and other changes to US trade policy. Standard errors are clustered by county.

B Data

B.1 Sample Selection

My primary analysis sample consists of 3,678,707 students that attended ninth grade at a public high school in Texas between fall 1995 and 2008. I assign cohort based on a student's first attempt at ninth grade. Because I am interested in adjustment mechanisms that occur during high school, I limit the sample to students that I ever observe starting ninth grade.[59] In order to an adopt an intent-to-treat framework that accounts for endogenous migration, I also limit the sample to students that I observe in the data *before* the onset of the labor demand shock in 2000. This rule produces the 2008 cohort endpoint: on-time ninth-graders in 2008 attended kindergarten in 1999, making them the youngest students I can assign to a county prior to the onset of the shock. Although my dataset starts in 1994, I do not begin my analysis sample until the 1995 ninth-grade cohort, so that I can observe eighth-grade standardized test scores. I drop students participating in special education, because of their unique labor market opportunity set relative to the rest of the sample and the labor demand shock.

Not all students that attend a public high school in Texas remain in the state after exiting K-12. If outmigration from Texas – or, complete detachment from the labor force – are correlated with treatment, then selective attrition from my dataset could introduce non-classicial measurement error that biases estimations of the effects on exposure to local shocks on college enrollment and labor market outcomes in a direction determined by the selection. However, if attrition is uncorrelated with treatment, then it would only represent classical measurement error and bias my results toward a null result. Figure A3 presents estimates of de-trended difference-in-difference specifications representing changes in the difference in attrition rates between students from more- and less-exposed counties relative to existing trends. Each coefficient represents an estimate from a separate specification with the outcome variable as never being observed across K-12, postsecondary, or workforce records in Texas from that age through age 30. No coefficient is statistically distinguishable from

[59] I cannot observe comprehensive course completions for students that do not appear in the dataset until tenth grade or later.

zero, suggesting attrition only causes classical measurement error. The increase in magnitude of coefficients as the age of definition approaches 30 largely because of a mechanical effect. Because I observe no workers past age 30, "attrition" at age 29 only reflects non-participation in the labor force or education system in Texas for 2 years, as opposed to non-participation for 10 years for attrition defined at age 20. To the extent that human capital adjustments increased labor force opportunities, increases in employment for a given year appear as reduced attrition for later ages, while more weakly relating to earlier defined measures. I drop all students from the analysis sample that attrit at age 17 or earlier and measure employment and earnings outcomes for stayers that exhibit stretches of detachment from the dataset as zeroes when not observed.

B.2 Other Data Sources

B.2.1 Exposure to PNTR

I define exposure to Chinese import competition following the establishment of Permanent Normal Trade Relations using industry-level and county-level tariff gaps from Pierce and Schott (2016a) and Pierce and Schott (2020). Industry-level tariff gaps are defined as the differences between Normal Trade Relations and non-Normal Trade Relations *ad valorem* tariff rates in 1999 for all four-digit Standard Industrial Classification codes (Feenstra et al., 2002). Pierce and Schott (2020) define county-level tariff gaps as the employment-weighted industry-level tariff gaps across all industries present in the county using 1990 industry-level employment counts from the US County Business Patterns database.

B.2.2 Control Variables

Baseline County Characteristics: My preferred de-trended difference-in-differences specification includes 1990 county characteristics interacted with individual year dummies to flexibly control for confounders related to county economic profiles. I follow Pierce and Schott (2020) in controlling for the percent of the population without any college education, median household income, the foreign-born population share, and the share of employment in manufacturing.[60] The first three measures come from the Census Bureau's 1990 Decennial Census, while the fourth comes from the County Business Patterns database.

Trade Policy Changes: I follow Pierce and Schott (2020) in controlling for four time-varying measures of a county's exposure to changes in trade policy aside from PNTR. First, I include a county's annual labor-share-weighted import tariff rate (i.e., the average tariff rate among goods produced in that county) under Normal Trade Relations, which ensures that the identifying variation does not reflect changes to preferred tariff rates for all countries subject to Normal Trade Relations prior to the establishment of PNTR with China. I also control for a county's exposure to the phasing out of quotas on textile and clothing imports under the Multifiber Agreement during the 1990s and 2000s. The relaxation of quotas occurred over four phases (January 1, 1995, 1998, 2002, and 2005), and quotas on Chinese imports were not affected until after it joined the WTO in 2001. The county-year-level measure of exposure to the quota eliminations weights the quota fill rate (i.e., how binding the quota was during a particular phase) for each industry in a county by the employment share in that particular industry. Finally, upon joining the WTO in 2001, China reduced its import tariff rates and reduced production subsidies, potentially increasing demand for exports of U.S. manufactured goods. I control for individual year dummies interacted with (1) a labor-share-weighted-average of the change in Chinese import tariffs across industries for each county and (2) a labor-share-weighted average of the change in production subsities across industries for each county. Pierce and Schott (2020) compute these measures using product-level data on Chinese import tariffs from Brandt et al. (2017) and subsidies reported in the Chinese National Bureau of Statistics' Annual Report of Industrial Enterprise Statistics.

Fracking: Innovation in oil and gas extraction technology in the early 2000s made extraction of previously written-off oil and gas deposits contained in shale formations suddenly economically and technically feasible. The ensuing "fracking boom" across shale-rich regions of Texas increased labor market opportunities for workers with little education, negatively affecting educational attainment (Kovalenko, 2023). I control for exposure to the fracking boom as the per-capita energy potential (in millions of British Thermal Units) of the shale

[60] Unlike (Pierce and Schott, 2020), I do not control for the Veteran population share, a potential confounder specific to their outcome of interest of deaths of despair.

reserves beneath a county's borders (Kovalenko, 2023; Cascio and Narayan, 2022). I use shapefiles of shale plays and maxium estimates of shale oil and gas reserve volumes from the Energy Information Administration (EIA).

Other Labor Demand Shocks: In robustness exercises, I control for a county's exposure to three additional labor demand shocks during my sample period: the 2000s housing boom and bust, the 2007-2008 financial crisis, and the 2000 dot-com bubble crash – three labor demand shocks which prior research have shown affected educational attainment (Charles et al., 2018; Weinstein, 2022). I follow Charles et al. (2018) and specify the size of a county's housing bubble as the magnitude of the largest structural break from trend in housing prices occurring between 2000 and 2006, using county-level housing price indices from the Federal Housing Finance Agency. I specify differential exposure to the financial crisis as a county's pre-period debt-to-income ratio (Mian et al., 2013) using data from the Federal Reserve Enhanced Financial Accounts database. Finally, I specify a county's exposure to the dot-com crash as their employment share in "high-technology" industries (Hecker, 2005; Weinstein, 2022).[61] I interact each of these cross-sectional exposure measure with year dummies to allow them to flexibly affect outcomes across cohorts.

B.2.3 County-Level Outcomes

Employment and Earnings: I use county-level measures of employment and earnings outcomes from three data sources. County-level employment counts (overall and by industry) fome from the County Business Patterns Database (Eckert et al., 2020), which imputes missing industry-county-year cells from the Census Bureau County Business Patterns dataset that are suppressed for confidentiality and standardizes consistent industry codes across years. County-level aggregate personal income – broken down by wage and salary income and government transfer income – come from the Bureau of Economic Analysis Regional Economic Accounts dataset. I use samples of both of these datasets starting in 1994, the first year of

[61] The level-I high-technology industries defined by Hecker (2005) are as follows: pharmaceutical and medicine manufacturing; computer and peripheral equipment manufacturing, communications equipment manufacturing; semiconductor and other electronic component manufacturing; navigational, measuring, electromedical, and control instruments manufacturing; aerospace product and parts manufacturing; software publishers; internet service providers and web search portals; data processing, hosting, and related services; architectural, engineering, and related services; computer systems design and related services; scientific research-and-development services.

my student-level panel. I supplement these datasets – which reflect entire county populations – with data on earnings and employment broken down by subgroups of interest (education levels, age groups, race, and gender) from the Census Quarterly Workforce Indicators (QWI) dataset. The QWI reflects the combination of state Unemployment Insurance earnings data and the Quarterly Census of Employment and Wages data with administrative Census Bureau data through the Longitudinal Employer-Household Dynamics program and provides statistics on job flows, employment, and earnings for uniquely detailed geographies, firm, and worker characteristics. Although my UHERC administrative dataset already includes earnings and employment records for all workers in positions covered by Unemployment Insurance in Texas, I can only observe demographics and education levels for workers that attended K-12 or college in Texas during my sample period, making the QWI preferable for first-stage estimations of the effects of the labor demand shock on earnings by age groups and education levels. However, the QWI does not start until 1996, so my analysis period when estimating specifications using QWI-based outcomes differs slightly from that of my primary estimations.

Population: I use annual county-level population counts by age group from the Surveillance, Epeidemiology, and End Results dataset to construct employment rates and per-capita measures of outcomes from the above datasets. I adopt the standard definition of the working-age population as individuals from 15 to 64 and prime-age workers as 25 - 54.

K-12 District Spending: To construct per-pupil measures of categorized K-12 school district revenues and expenditures, I use data from the National Center for Education Statistics Local Education Agency Finance Survey (F-33) and Local Education Agency Universe Survey.

Converting Nominal to Real Measures: I use the Bureau of Labor Statistics Consumer Price Index to convert all nominal measures to 2020 dollars.

B.3 HS Elective & College Major Categories

Table B1: High School Vocational Course Subject Groups

Group	TEA Subject Areas	Example Courses
Agriculture	Agricultural Science (63)	Agricultural Mechanics
Business	Business Education (70); Marketing (65); Office Education (67)	Business Management; Principles of Marketing; Introduction to Computers
Health	Health (81)	Sports Medicine
Industrial	Industrial/Tech Electronics (59); Industrial Arts (60); Trade and Industrial (62)	Digital Electronics; Manufacturing Systems; Intro. to Preceision Metal Manufacturing
Technology	Technology Education (69)	Electricity/Electronics Technology

Notes: This table presents definitions and example courses from categories of high school vocational electives. I group courses belonging to similar Texas Educational Agency Subject Areas to define broad vocational categories.

Table B2: Quartiles of Major-Level Shock Exposure at Two-Year Colleges

Quartile	CIP Codes	CIP Descriptions
Q1:	02, 13, 19, 22, 25, 31, 32, 34, 36, 43, 44, 45, 49, 51, 54	Agriculture; Education; Home Economics; Legal Studies; Library Science; Recreation Studies; Basic Skills; Health-Related Skills; Leisure; Protective Services; Public Administration; Social Sciences; Transportation; Health Professions; History
Q2:	01, 08, 09, 10, 12, 26, 42	Agricultural Bus. & Prod.; Marketing; Communications; Communications Tech.; Personal Services; Biological Sciences; Psychology
Q3:	16, 24, 52	Foreign Languages; General Studies; Business
Q4:	03, 04, 05, 11, 14, 15, 20, 23, 27, 30, 38, 39, 40, 41, 46, 47, 48, 50	Natural Resources; Architecture; Cultural Studies; Information Sciences; Engineering; Engineering Tech.; Vocational Home Ec.; English; Mathematics; Interdisciplinary; Philosophy; Religious Vocations; Physical Sciences; Science Tech.; Construction Trades; Mechanics and Repairers; Precision Production Trades; Visual & Performing Arts

Notes: This table presents fields of study at two-year colleges grouped into quartiles of major-level exposure. Majors are defined as two-digit Classification of Instructional Programs codes, using NCES crosswalks from 1990 to 2000 and 2010 to make consistent codes. I define major-level shock exposure as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period. Estimations of the effects of exposure to local shocks on enrollment across major quartiles are presented in Table A16.

Table B3: Quartiles of Major-Level Shock Exposure at Four-Year Universities

Quartile	CIP Codes	CIP Descriptions
Q1:	12, 13, 19, 22, 25, 30, 31, 32, 36, 43, 44, 46, 47, 49, 51, 54	Personal Services; Education; Home Economics; Legal Studies; Library Science; Interdisciplinary Studies; Recreation Studies; Basic Skills; Leisure; Protective Services; Public Administration; Construction Trades; Mechanic & Repair Technologies; Transportation; Health Professions; History
Q2:	02, 16, 26, 42	Agricultural Bus. & Prod.; Marketing; Foreign Languages; Biological Sciences; Psychology
Q3:	01, 03, 04, 05, 09, 23, 24, 27, 38, 45, 50	Agriculture; Natural Resources; Architecture; Cultural Studies; Communications; English; General Studies; Mathematics; Social Sciences; Visual & Performing Arts
Q4:	08, 10, 11, 14, 15, 20, 40, 48, 52	Marketing; Communications Tech.; Engineering; Engineering Tech.; Vocational Home Ec.; Physical Sciences; Preceision Production; Business

Notes: This table presents fields of study at four-year universities grouped into quartiles of major-level exposure. Majors are defined as two-digit Classification of Instructional Programs codes, using NCES crosswalks from 1990 to 2000 and 2010 to make consistent codes. I define major-level shock exposure as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period. Estimations of the effects of exposure to local shocks on enrollment across major quartiles are presented in Table A16.

Table B4: Foote and Grosz (2020) Community College Major Categories

Category	CIP Code		# of Codes
Information Tech.	10	Communications Technologies/Technicians and Support Services	15
Construction	15	Engineering Technologies/Technicians	54
Manufacturing	46	Construction Trades	22
	47	Mechanic and Repair Technologies/Technicians	34
	48	Precision Production	16
	49	Transportation and Materials Moving	16
Public Services	43	Security and Protective Services	16
	44	Public Administration and Social Service Professions	5
Health	51	Health Professions and Related Clinical Services	196
Business	52	Business, Management, Marketing, and Related Support Services	84
Family/Personal	19	Family and Consumer Sciences/Human Sciences	32
	12	Personal and Culinary Services	25
Education	13	Education	89

Notes: This table is adopted from Foote and Grosz (2020) and presents broad major categories at two-year colleges as defined by groups of two-digit NCES CIP codes. The final column displays the number of individual six-digit CIP code major classifications within the broader two-digit classification. Estimations of the effects of exposure to local shocks on two-year enrollment by major group are presented in Table 7.

C Policy and Setting Specifics

C.1 Texas K-12 Finance Policies

Since 1993, Texas has employed a school finance system known as "the Robin Hood Plan." As is often the case with school finance reforms, implementation of this system was the result of a lengthy legal battle. In 1984, Edgewood Independent School District (ISD) and 67 other Texas school districts sued the State over the disparity in resources across the state's school districts. The Texas Supreme Court ruled in favor with the plaintiff districts in *Edgewood v. Kirby* and insisted that conditional on similar levels of local tax effort, districts should receive similar funding levels. Attempts by the state legislature to reform the existing finance system in 1989 and 1991 were struck down for insufficiently addressing disparities and creating an unconstitutional *de facto* state property tax, respectively, prior to the ultimate passage of the Robin Hood plan in 1993. Formally known as the Foundation School Program, the Robin Hood school finance system consists of three tiers of funding, along with a controversial recapture provision.

Tier 1 (Basic Allotment) funding guarantees a minimum level of per-pupil funding to each K-12 district ($2,300 in 1993), conditional on districts meeting a minimum property tax rate threshold ($0.86 per $1,000 in 1993). Districts with insufficient property wealth to meet the minimum funding level at the specified tax rate receive additional state assistance to make up the gap. District basic allotments are distributed per weighted average daily attendance (WADA), a student count measure that adjusts for district and student characteristics representing additional costs.[62]

Tier 2 (Guaranteed Yield) guarantees districts a specified per-WADA funding level ($20.55 in 1993) per penny in property tax effort between the minimum tax rate specified in Tier 1 and a maximum rate ($1.50 in 1993).[63] Property-poor districts that do not meet the

[62] District characteristics weighted as additional costs are the average starting salary of teachers in neighboring school districts, the economically disadvantaged student population share, district average daily attendance, location in a rural county, sparsity, and classification as an independent town or small or mid-sized district. Student characteristics weighted as additional costs are participation in special education, compensatory education, career and technology courses, English-Language Learners programs, and gifted and talented programs.

[63] The parameters specifically apply to maintenance and operations tax rates, and from 1999 onward, districts could not use Tier 2 funds to service debt or make facility investments.

guaranteed yield from their own collections receive supplemental state funds to close the gap.

Tier 3 (Facilities) consists of guaranteed-yield funding for facility investments, but unlike Tier 2, operates as a "sum certain" competitive grant program and is not guaranteed to all districts.

Finally, the recapture provision earns the Robin Hood system its name. The state stipulates that districts with per-pupil property wealth that exceeds a specified threshold ($280,000 in 1993) cannot fund their schools with more than the amount equal to their tax rate scaled by the property wealth threshold. The excess property tax revenue is recaptured by the state and redistributed to property-poor districts via Tier 1 and 2.

Hoxby and Kuziemko (2004) discusses efficiency concerns with the Robin Hood system's design relative to a state property tax that could deliver the same degree of redistribution in K-12 funding. The authors note that the system's reliance on *marginal* gains in property values relative to the per-pupil wealth cutoff exacerbate negative capitalization of the tax in property-rich districts and contend that this decline in property values is unlikely to be fully offset by increases in property-poor districts if these districts were already operating at the efficient level of local K-12 funding. Declining property values in districts above the wealth cutoff would force the state to lower the cutoff in order to raise additional revenue, triggering another round of capitalization. Empirical analyses support these claims: the authors find that although Robin Hood decreased the per-pupil spending gap between property-poor and property-rich districts by $500, it also caused the loss of $27,000 per pupil in property wealth. Efficient confiscation and investment of this amount of wealth by the state could have instead funded all Texas schools at the realized spending level of the top 5% of districts.

C.2 Generalizability

Table C1 presents descriptive statistics on the composition, funding levels, and graduation rates of public K-12 and postsecondary institutions in Texas and the United States as a whole. Public K-12 schools in Texas differed from those of the U.S. as a whole in the years before the trade shock in terms of student demographics – 40% of students in public K-12 schools in Texas were Hispanic, as opposed to only 16% across the country overall – but received similar per-pupil funding levels and produced similar high school graduation rates. Students at both two- and four-year colleges in Texas also exhibited similar differences in demographics from U.S. college students as a whole. Although graduation rates trailed at Texas colleges, funding levels mirrored nationwide averages.

Relative to the rest of the country, Texas employs a fairly unique school finance system, known as the "Robin Hood" plan. Under the Robin Hood plan, excess property tax revenues from wealthy school districts are collected by the state government and redistributed to poor districts. In addition to promoting equitable funding *levels* across rich and poor districts, the system mechanically makes local school funding levels essentially orthogonal to *changes* to local property values (in relative terms). I use data from the National Center for Education Statistics (NCES) District Finance Survey to estimate the effects of local shocks on district revenues and expenditures in Table C2. I find that despite district revenues from local property taxes falling by nearly $2,000 per-pupil in more-exposed counties, school spending was unaffected.[64] Moreover, estimates in Panel B of the effects of the China shock on property tax revenues and school spending in districts across the rest of the country support the uniqueness of this "insurance value" provided by the Robin Hood formula: while school spending in Texas fell by just 2 cents per dollar lost in district property tax revenues due to local shocks, school spending fell by 80 cents per dollar lost in the rest of the country.

[64] Appendix Figure A6 presents the corresponding event studies.

Table C1: K-12 And College 1999 Characteristics: Texas vs. U.S.

	(1) Texas	(2) United States
K-12		
% White	0.431	0.620
% Hispanic	0.396	0.156
% Black	0.144	0.172
Per-pupil Expenditure	$8,165	$7,822
Four-Year Graduation Rate	71%	69%
Two-Year Colleges		
% White	0.522	0.658
% Hispanic	0.291	0.077
% Black	0.121	0.108
Appropriations Per FTE	4,437	4,526
Three-Year Graduation Rate	18%	33%
Four-Year Colleges		
% White	0.620	0.696
% Hispanic	0.195	0.063
% Black	0.089	0.101
Appropriations Per FTE	5,183	5,145
Six-Year Graduation Rate	53%	71%

Notes: This table presents average characteristics for K-12 and college students and schools in Texas and the United States using data from the National Center for Education Statistics Common Core of Data and Integrated Postsecondary Education Data System. All data are from the 1998-1999 school year.

Table C2: The Robin Hood Formula Uniquely Shielded TX Students from Local Shocks

	(1) Per-Pupil Property Tax Rev.	(2) Per-pupil Current Exp.	(3) Pass-Through
Texas			
Shock Exposure	-2,786***	39	-0.01
	(421)	(387)	
Percent Change	-75.7	0.43	
Pre-period Mean	3,681	9,094	
N	4,810	4,810	
Rest of U.S.			
Shock Exposure	-322***	-271***	0.84
	(56)	(80)	
Percent Change	-10.2	-2.9	
Pre-period Mean	3,152	9,465	
N	66,045	66,045	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of exposure to negative local labor demand shocks on per-pupil property tax revenues and school spending in TX and the rest of the U.S., using data from the NCES Common Core of Data. Estimates reflect coefficients from a parametric difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after the start of the shock (2000) relative to existing trends in these differences. The specification controls for county and year fixed effects, 1990 county characteristics interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Pass-through estimates and column three represent the average decline in school spending per dollar in property tax revenue. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.