Human Capital Adjustments and Labor Market Resilience: Evidence from Linked Education and Earnings Data

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Abstract

Negative labor demand shocks can have lasting consequences on the labor market outcomes of both prime-aged workers and new labor market entrants, known as "scarring effects." However, individuals coming of age may not suffer the same fate if they internalize salient changes to the returns to education and adjust their human capital investments. This paper studies the effects of exposure to negative 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 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 90% of the shock's scarring 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, the costs of such shocks can largely be confined to a single generation.

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1 Introduction

Negative labor demand shocks can cause remarkably persistent harmful effects on exposed individuals. Both established workers exposed to a labor demand shock and individuals first entering the labor market during periods of depressed labor demand experience sustained declines in earnings, known as "scarring effects" (e.g., Ruhm, 1991; Kahn, 2010). Having already made key educational decisions, these individuals may adjust to such a shock by moving across geographic locations, industries, or occupations – often imposing substantial moving costs or requiring considerable retraining. On the other hand, individuals coming of age may have greater leeway to make extensive-margin (i.e., attending college) and intensive-margin (i.e., field of study) adjustments to their human capital investments, since they are still attached to the education system. The degree to which students can make such adjustments likely determines whether they are also "scarred" by sustained depressions in local labor demand. As automation, decarbonization, and other looming changes to the U.S. economy threaten to bear uneven benefits and costs across workers and local labor markets, understanding this phenomenon is particularly important.

In this paper, I study the effects of exposure during youth and adolescence to negative local labor demand shocks 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 90% of the decline in earnings experienced by young adults 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 negative labor demand shocks (e.g., Stuart, 2022): if individuals coming of age sufficiently adjust their human capital investments, the costs of such shocks can largely be confined to a single generation.

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 competition from Chinese exporters (Pierce and Schott, 2016). 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, 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 (2016, 2020), I use this variation in a two-step "de-trended" 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. To account for bias from endogenous migration in response to the shock, I estimate intent-to-treat models that assign treatment to students based on the county where they attended school prior to the policy change. 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 experience large and small shocks would continue to evolve along existing differential linear trends if both groups were exposed to small labor demand shocks (Callaway et al., 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 that my main results are generally robust to relaxing this assumption by allowing trends in potential outcomes to revert to parallel after the onset of the shock and by

[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 parametric specification. An alternative interpretation of the exercise is that it tests for selection on observables into treatment.

allowing smooth deviations in potential outcomes from existing linear trends (Rambachan and Roth, 2023).

To inform interpretation of my main estimations, 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 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 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

[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 in exposed manufacturing subsectors (Autor et al., 2013, 2015; Pierce and Schott, 2016; Greenland and Lopresti, 2016; Autor et al., 2021).

[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, Herhbein and Stuart (2023) 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 in local labor markets exposed to the China shock; however, (Burga and Turner, 2022) provide evidence that this result is mostly explained by outmigration and weak instrument bias.

vocational elective courses and took more Advanced Placement, International Baccalaureate, and college-credit courses. 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 in Texas.[6] This magnitude is comparable to previously estimated effect sizes of smaller elementary school classrooms (Chetty et al., 2011) or 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 directly exposed to import competition. Using the distribution of recent graduates of specific majors across industries of employment prior to the onset of PNTR to define *major-level* exposure to the policy change, I find that community college enrollment significantly increased in the least-exposed fields but fell in those most exposed to the shock.

Evidence of the persistence of the China shock's negative effects on earnings of prime-aged workers (Autor et al., 2021) and of the "scarring" effects of entering the labor market during economic downturns (e.g., Kahn, 2010) raise the question of whether the above human capital adjustments translated into improved labor market outcomes for young adults exposed to the shock.[8] Consistent with scarring effects, individuals from high-exposure counties that

[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.

[8] Kahn (2010) finds that a one percentage-point increase in the state unemployment rate at the time of labor market entry decreases the wages of college graduates by 9%, an effect that persists at least 15 years after college graduation. Oreopoulos et al. (2012) and Altonji et al. (2016) similarly find persistent declines in wages for college graduates entering the labor markets during a recession, and Hershbein (2012) shows smaller (1-2%) and less persistent wage declines for high school graduates. Schwandt and von Wachter

were old enough to make key human capital investments before 2000 earned 8% less than those from low-exposure counties after the onset of the shock. However, exposed students young enough to adjust their educational decisions experienced statistically significant relative earnings gains large enough to erase 90% of this gap. This suggests that human capital adjustments nearly fully buffered against scarring effects of the shock, despite the persistence of the shock's negative effects on per-capita earnings and employment rates in exposed local labor markets.

Finally, I examine potential mechanisms that aided or inhibited these adjustments. I show that Texas' "Robin Hood" school finance equalization system shielded students from declines in local property values leading to decreases in school funding, a key channel through which local economic downturns can harm the next generation (Burga and Turner, 2022). I also provide evidence suggesting borrowing constraints prevented students from low-income households from responding to local shocks by obtaining a college education. Notably, these students still made "costless" adjustments in course selections in high school.

Previous work finds mixed evidence on the effects of exposure to negative labor demand shocks during youth and adolescence on human capital accumulation.[9] My rich administrative dataset allows me to make two contributions to this literature, both of which inform the interpretation of previous findings. First, I find evidence of key adjustments along novel dimensions such as high-school course-taking that suggest forward-looking students observed and responded to 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 human capital accumulation (Stuart, 2022; Burga and Turner, 2022; Ferriere et al., 2018). Thus, "null" results in these settings may still reflect meaningful changes in human capital accumulation. Moreover, changes in expectations and forward-looking adjustments in high school plausibly aided complementary shifts in fields of study in college, potentially explaining why I find "larger"

(2019) finds that entering the labor market when your birth state exhibits a high unemployment rate has substantial negative effects on earnings that persist for 10 years and are strongest for non-white workers and high school dropouts.

[9] Work in this area analyzes local shocks caused by recessions (Stuart, 2022; Weinstein, 2022), mass layoffs (Foote and Grosz, 2020; Acton, 2021; Salvanes et al., 2021), automation (Di Giacomo and Lerch, 2021), housing busts (Charles et al., 2018), energy busts(Black et al., 2005), deindustrialization (Choi, 2023), and trade (Greenland and Lopresti, 2016; Ferriere et al., 2018; Lee, 2021; Burga and Turner, 2022).

shifts of fields than prior work examining effects of salient labor demand shocks occurring closer to college entrance (Acton, 2021).[10]

Second, I provide evidence that human capital adjustments in my setting translated into labor market benefits. In doing so, I bridge the above literature with that on the scarring effects of negative labor demand shocks on earnings. Stuart (2022) finds that exposure to the 1980-1982 recession during childhood or adolescence caused decreases in college degree receipt and long-term earnings and attributes these effects to decreases in "childhood investments" in human capital. In contrast, I find that students exposed to the China shock before entering high school – and as early as in kindergarten – adjusted their educational decisions in manners that protected them against similar scarring effects on earnings. I show that Texas' "Robin Hood" K-12 finance system prevented declines in local property values from manifesting in declines in school spending – a key component of "childhood" human capital investments, potentially contributing to the differences in our results.[11]

The literature on scarring effects also finds that individuals that enter a labor market during a period of depressed labor demand experience lasting declines in earnings (Kahn, 2010; Hershbein, 2012; Oreopoulos et al., 2012; Schwandt and von Wachter, 2019). One proposed explanation for these scarring effects is that individuals entering the labor market during downturns disproportionately possess skills that do not match those demanded by employers (Liu et al., 2016). Consistent with this hypothesis, I find evidence that adjustments of human capital investments to align with changes in local labor demand across sectors dramatically reduce scarring effects on average, despite the persistence of overall declines in local labor demand and earnings losses for prime-aged workers.

Finally, this paper contributes to existing research on the China shock. In contrast to classical trade models, this literature finds substantial and persistent consequences of expo-

[10] Acton (2021) finds that high school graduates responded to local plant closures in their senior year of high school by shifting community college program choices from those related to jobs lost in the closure and toward occupations that required similar skillsets. Examining a longer time horizon and a more "permanent" shock, I find that students exposed to the China shock before entering high school were less likely to enroll in manufacturing-aligned community college programs and more likely to enroll in health, IT, or business programs.

[11] 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 *ex ante* permanence of the China shock, among others.

sure to Chinese import competition on exposed local labor markets and workers (e.g., Autor et al., 2014, 2021). I provide evidence that, at least in Texas, the costs of trade liberalization may have largely been confined to a single generation, and my results suggest that the extent to which the costs of other skill-biased technological changes (e.g., automation) transmit across generations may also depend on the ability of individuals coming of age to adjust their human capital investments and the presence of policies to support these investments.[12]

2 Background

My research design leverages variation in decreases to local labor demand resulting from a change in U.S. trade policy toward China in 2000. The U.S. subjects 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 manufactured 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 with 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, 2016).[14]

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

[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

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, 2016). Intuitively, 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, 2016), 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 good provision (Feler and Senses, 2017) and increases in fatal drug overdoses (Pierce and Schott, 2020;

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)

[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.

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 could would 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 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.

Finally, the negative shock to local labor demand may tighten constraints on "external" investments in human capital by a student's and school.[16] The specifics of my setting allow me to examine the effects of a negative shock to local labor demand while holding constant 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

[16]Both theoretical and empirical literatures provide 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

is largely funded through property taxes, district funding 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, which often requires imposes a substantial monetary cost.[19]

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.[20]

3 Data

My primary dataset consists of administrative student-level data from the University of Houston Education Research Center (UHERC), featuring individual-level linked 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 ma-

system.

[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 (2019) 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.

jors, 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 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. I note two factors related to the nature of the administrative data that affect further selection. First, I only observe students attending public schools, colleges, and universities in Texas; hence, students who permanently leave Texas attrit from the sample. I discuss the implications of this for idenficiation 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. 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 char-

[21]The records report employer industry codes (6-digit NAICS) in all years and also report county of residency and county of employment starting in 2000. However, both county of residency and county of employment are not reported for over 45% of person-quarter observations from 2000 to 2018.

[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 detail my sample selection in Appendix B

acterize the "first-stage" effects of PNTR on local labor markets that may have affected students' educational choices.

4.1 Identification

PNTR's differential impact on local labor demand across counties motivates an identification strategy that compares 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 the employment-weighted average tariff gap using 1990 employment counts from the harmonized County Business Patterns Database (Eckert et al., 2021). 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]

$$y_{ict} = \sum_{\substack{\tau=1995 \\ \tau\neq 1999}}^{2008} \pi_{\tau}^{ES} \mathbf{1}\{t=\tau\} * HighExposure_c + \alpha_c + \alpha_t + \Phi S_i + \sum_{\substack{\tau=1995 \\ \tau\neq 1999}}^{2008} \Gamma_{\tau} \mathbf{1}\{t=\tau\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \tag{1}$$

[23] Existing literature studying the local labor market effects of the China shock utilize 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.

[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.

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 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 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

[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.

students high-exposure counties would on average evolve in parallel to those of students from low-exposure counties if all counties had instead experienced a small shock.[27] In other words, we 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 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 employment and earnings grew faster leading up to the policy change in counties that were more exposed to PNTR-induced labor demand shocks than in less-exposed counties. These patterns suggest that student outcomes would not have trended in parallel across more- and less-exposed counties if both groups were exposed to the same treatment dosage.[28] Therefore, I explicitly control for differences in existing trends in outcomes using the following two-step procedure proposed by Goodman-Bacon (2021):[29]

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

[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]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.

[29]Goodman-Bacon (2021) outlines this two-step procedure in the paper's Online Appendix D, Section C. This 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 (Autor et al., 2013).

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

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. 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. 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:

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

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

To further improve the plausibility of required identifying assumptions, I assign treatment status and county-level covariates to students based on their county of residence *prior* to the onset of the shock. Thus, my estimates represent 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.[30] Because I cannot observe students that leave Texas, I also test for differential attrition rates (Figure A1) and find no statistically significant evidence of differential attrition by treatment status at any age from 16 through 30.[31]

The assumption that differences in student outcomes across more- and less-exposed counties would continue along existing linear trends if both groups experienced small labor demand shocks is fundamentally untestable; however, a testable implication of this assumption is that differences in *time-invariant* characteristics of students in ninth grade should not deviate from such trends in years following the policy change. I estimate "effects" on baseline (pre-shock) characteristics using equation (4). Estimates in Table 2 show no evidence of local shocks "affecting" previously measured student race, ethnicity, gender, English-Language Learner status, or free-or-reduced-price-lunch eligibility. The null point estimates may still reflect meaningful selection on potential earnings. I predict later-life earnings for all cohorts based on the relationship between these baseline student characteristics and earnings at 30 for pre-shock cohorts and estimate the "effects" of local shocks on the predicted earnings

[30] Using an ITT approach is important as Greenland et al. (2019) find that exposure to the China shock increased out-migration, 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.

[31] Figure A1 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 A1 as the age of definition approaches 30 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.

index. Treatment correlates with a statistically insignificant \$62 (2020\$) decrease in earnings predicted on fixed student characteristics, and the 95% confidence interval rules out decreases in predicted earnings larger than \$214 and increases larger than \$90 in absolute value.

In Section 7, I show that my main results are generally robust to two relaxations of my identifying assumption. First, I allow potential outcomes of students from high-exposure and low-exposure counties to revert to trending in parallel after continuing along existing differential linear trends for only p periods, rather than the entire sample period. In practice, this exercise makes comparisons of differences in outcomes from high-exposure and low-exposure counties relative to a pre-trend that flattens off after p periods, instead of relative to the continuation of $\hat{\lambda}$ throughout the post-period. Second, I allow for smooth deviations in parallel outcomes from the existing linear pre-trends, rather than assuming exact linearity (Rambachan and Roth, 2023).

4.2 Effects of PNTR on Local Labor Markets

Using data from the County Business Patterns Database (Eckert et al., 2021), 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 the relevance of the specified exposure measure and guiding interpretation of the channels through which the China 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.1.

I first examine how exposure to PNTR affected local 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 wage and salary 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 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 exposed local labor markets.[32] Moreover, the point estimate in column (3) suggests that the negative employment effects of import competition spilled over outside of manufacturing.[33]

[32] Autor et al. (2021) finds a 1.9 percentage-point decline in employment relative to population in exposed local labor markets. Herhbein and Stuart (2023) 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 persist 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.

[33] 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

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 also represents a decline in lifetime earnings if they were to enter the labor market before or directly after high school completion. The estimate in Column (3) indicate 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 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 by Feler and Senses (2017), accompanying reductions in school spending could negatively affect short- and long-run student outcomes.[34] 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.[35] 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. 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

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.

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

[35]Appendix Figure A3 presents the corresponding event studies.

"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). Thus, the degree to which they could make human capital adjustments is theoretically ambiguous.

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 nationwide samples give mixed evidence on whether such responses occurred. (Greenland and Lopresti, 2016; Burga and Turner, 2022).[36] 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.[37]

[36](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; however, (Burga and Turner, 2022) provide evidence that this result is mostly explained by outmigration and weak instrument bias.

[37]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

Null effects of 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 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.[38] 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 24% fewer industrial electives ($p < 0.01$). Appendix Table A4 presents estimates of the effects of shock exposure on course-taking across other individual categories of vocational electives, none of which are statistically distinguishable from zero.

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 columns (4) and (5) of Table 5. I find that students exposed to larger local shocks completed 40% more Advanced Placement or International Baccalaureate courses and 0.5 (299%) more courses through dual-enrollment with local colleges.[39] These estimates suggest the reduction in labor market opportunities for workers without college experience increased desire to purse a college education. Moreover, Jackson (2010) provides evidence that AP course-taking increases the likelihood of college matriculation, such that the estimated increases in AP course-taking and dual enrollment also represent mechanisms that may have aided students in reaching college as a manner of adjustment.

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.

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

[39]The magnitude of the estimated effect on dual-credit completions should be interpreted with caution when compared to the pre-period mean, because overall take-up of dual-credit increased four-fold during the sample period.

5.2 Postsecondary Human Capital Adjustments

College plausibly offers 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 with out 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 AP and IB course-taking and dual enrollment from translating into college matriculation by marginal students.

Figure 5 presents de-trended event study estimates of trends in 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.[40] Each estimate is statistically significant ($p < 0.01$). 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.

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

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

school – at each age from 16 through 30. Figure 6 presents coefficients from parametric 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. 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). Table 7 presents estimates of the effects of shock exposure on enrollment across fields grouped into quartiles by exposure to the labor demand shock.[41] 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 7. 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 quar-

[41] Appendix B.3 shows the two-digit Classification of Instructional Programs codes in each exposure quartile and Appendix Table A5 presents results using major-groupings according to broad categories defined by Foote and Grosz (2020).

tiles, respectively. I find imprecisely estimated enrollment declines of 5% and 18% for majors the second-most-exposed and most-exposed quartiles, further (suggestively) supporting that community college students substituted from more- to less-exposed majors.

Panel B of Table 7 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 the exposure distribution. 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).

5.3 Human Capital Adjustments Protect Against Scarring Effects

In Texas, that exposure to the China shock caused students to acquire both more and better-fitting human capital in high school and college. I next assess whether these adjustments to human capital investments helped protect against lasting declines in earnings due to scarring effects (Schwandt and von Wachter, 2019; Oreopoulos et al., 2012; Kahn, 2010). Examining effects on earnings is complicated by the perfect collinearity between ninth-grade cohort, age at which earnings are observed, and year of observed earnings. I estimate detrended event study and difference-in-differences specifications that hold constant the *age* at which earnings are observed. More-exposed students' increased attachment to the education system in their early twenties may negatively bias earnings estimates from specification 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 biased if the effects of 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.[42]

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). Consistent with this notion, individuals from more-exposed counties that entered high school prior to the shock earned $1,761 (8%) less than their counterparts from less-exposed counties at age 30. However, de-trended difference-in-differences estimates in Table 8 show that exposed students who were young enough to adjust their human capital investments experienced statistically significant earnings gains large enough to erase 90% of this gap, despite the persistence of the decline in local labor demand in more-exposed counties. This suggests that human capital adjustments nearly fully buffered against the "scarring effects" of the shock on earnings that were experienced by older cohorts whom had already made critical educational decisions prior to the onset of the shock. Table 8 shows that both extensive-margin Columns 2 and 3) increases in employment and intensive-margin increases in earnings conditional on employment (Column 4) contributed to these earnings gains.

The scarring effects of negative labor demand shocks are noteworthy for their persistence (Kahn, 2010). I examine whether human capital adjustments provided lasting protection against scarring effects by estimating the effects of exposure to local shocks during youth and adolescence on earnings at each age from 16 through 30 and present results in Figure 7. The estimates suggest that adjustments caused sustained earnings increases in their late 20s averaging over $1,158 each year – despite the continued persistence of overall earnings declines in exposed counties shown in Figure 2. Each coefficient from ages 26 to 30 are all statistically distinguishable from zero with at least 95% confidence, and their magnitudes all correspond to substantial decreases in the earnings losses due to the shock found in older cohorts.

[42] 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.

6 Mechanisms & Heterogeneity

In this section, I provide additional context to the main results by examining mechanisms underlying student adjustments and estimating heterogeneous treatment effects across subgroups of interest.

6.1 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 9 suggest that students exposed to local shocks performed better on these tests, although only the estimated 0.096 standard deviation improvement 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).

6.2 Credit Constraints

Reductions in opportunity costs and increases in the labor market returns to college should increase college enrollment as long as students do not face credit constraints. In contrast, credit constraints may prevent some students from making such otherwise-optimal adjustments to their human capital investments in response to negative labor demand shocks.[43]

To indirectly test for the importance of credit constraints in young adults' responses to the shock, I rely on the fact that adjustments made while in high school do not impose a direct monetary cost. Further, I proxy for the presence of binding credit constraints with a student's *pre-shock* eligibility for free-or-reduced-price lunch (FRPL) through the National

[43] 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).

School Lunch Program and estimate the effects of exposure to local shocks on "costly" and "costless" forms of human capital accumulation separately for (plausibly) constrained and unconstrained students.[44] Consistent with the importance of credit constraints for postsecondary investments in human capital investments, estimates in Panel A of Table 10 show that FRPL-eligible students did not respond to local shocks by attending college. However, these students took fewer manufacturing-aligned electives and more AP or IB and dual-credit courses in high school – two methods of acquiring college-level human capital that do not impose additional monetary costs. Estimates from Panel B provide evidence that non-FRPL students responded to local shocks through both costless human capital adjustments and enrolling in college.

6.3 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). While I am not able to test for the effects on the number of seats offered in a given course or field of study, I can examine the number of courses and unique programs offered in high school and college, respectively.

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 A6 and A7 present estimates of the effects of local shocks on course and major offerings across fields in local high schools and two-year colleges, respectively. Although estimates are not statistically significant at

[44] 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.

conventional inference levels, they suggest that the typical high school in an exposed county reduced offerings in industrial electives and increased offerings in AP or IB courses, which would have aided the adjustments shown in 5.1. In contrast, imprecise results in table A7 suggests that local community and technical colleges increased programmatic offerings within the most-exposed fields and decreased those in less-exposed fields, which would have hindered the adjustments shown in 5.2.

6.4 Marginal Responses Across the Ability Distribution

If the returns to education are positively correlated with student ability, then students of lower ability acquire less formal education in equilibrium and respond along different extensive margins to a broad decrease in opportunity costs. Students with sufficiently high ability would graduate high school with or without the labor demand shock (i.e., they are "always-takers" at this margin, and students with sufficiently low ability would not obtain a bachelor's degree with or without the shock (i.e., they are "never-takers at this margin). To examine differential responses to the shock by student ability, I estimate effects separately for students with above-median and below-median scores on standardized tests taken prior to the onset of local shocks. Results in Table 11 are consistent with "compliance" along both shared and separate margins. Low-ability students exposed to the shock were more likely to graduate high school, while exposed high-ability students were significantly more likely to obtain a bachelor's degree. More-exposed students among both groups were significantly more likely to enroll in college.

6.5 Additional Heterogeneity

I present estimates of treatment effects on college enrollment and earnings for subsamples by student race and ethnicity, gender, and English-Lanugage Learner (ELL) status in Tables A8 and A9. Point estimates of the effect of the labor demand shock on college enrollment are positive for all subgroups except for ELL students, for whom language barriers may be binding constraints. Earnings significantly increased for all subgroups besides ELL students, consistent with human capital acquisition in college positively affecting earnings for these students.

6.6 Other Methods of Adjustment

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 industries, occupations, or geographies as adults as manners of adjustment. I estimate the effects of exposure to the local shocks on later-life employment across groupings of two-digit NAICS industry codes (Table 12). 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 employment across occupations or shifted migration patterns is beyond the scope of this paper.[45]

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.[46] I show that my main results are robust to excluding these controls and to sequentially adding back control variables (Table A10). 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,

[45] The UHERC dataset includes no information on worker occupation. Moreover, I cannot observe either the county of residence or employment for 45% of person-quarter observations from 2000 to 2018.

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

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 (8) of Table A10, I sequentially add controls for county-level exposure to 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 has shown affected educational attainment (Charles et al., 2018; Weinstein, 2022).[47] Across the three additional specifications, the estimated increase in college enrollment ranges from 3.0% to 5.1%, and the estimated 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.

First, I test for sensitivity to relaxing the assumption that differential linear trends between high-exposure and low-exposure counties would have continued for the *entire* sample period if they were not exposed to large vs. small shocks. It may instead be reasonable to believe that such differential trends would have only continued for a set period of time. Thus, I estimate specifications that only impose the continuation of linear pre-trends for p periods after the onset of the shock before reverting to parallel trends. A standard assumption of parallel trends corresponds to the case of $p = 0$, while the main results correspond

[47] 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). I interact each of these cross-sectional exposure measure with year dummies to allow them to flexibly affect outcomes across cohorts.

to the case of $p = 8$.[48] Panel A of Table A11 shows that the estimated increase in college enrollment is still statistically significant when allowing reversion to parallel trends after as few as two post periods. Panel B shows that the estimated increase in later-life earnings is even robust to assuming parallel trends in the post period.

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. Following Rambachan and Roth (2023), I allow for smooth deviations from the continuation of linear trends. Table A12 presents 95% confidence sets of the estimated effects of exposure to the labor demand shock on college enrollment and earnings when allowing for deviations from the extrapolated linear pre-trend by an additional m each period. Panel A. shows that we can still conclude that shock exposure increased college enrollment if we allow additional deviations of up to 0.06 percentage points each period (approximately 3% of the estimated effect size). Panel B shows that the estimated effect on earnings is not robust to even a $10 additional deviation each period.

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.[49] Moreover, 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

[48] Because earnings at age 30 are only observed through the 2002 ninth-grade cohort, the main earnings results correspond to the case of $p = 3$.

[49] 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.

quartiles. Table A13 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.

8 Conclusion

This paper examines how students adjust their human capital investments in response to negative shocks to 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 eligible for college credits 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 feeding into unaffected sectors in both high school and college, suggesting they internalized salient changes to lifetime earnings premia across attainment levels and fields of study. Following students into the labor market, I provide evidence that these adjustments provided substantial protection against the sustained decline in local labor demand.

Existing research on adverse labor demand shocks – 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; Herhbein and Stuart, 2023). Negative labor demand shocks scar prime-aged workers and new labor market entrants, whom have already made critical educational decisions (Ruhm, 1991; Kahn, 2010; 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.

Concerns over adjustment to adverse labor demand shocks are particularly prevalent when shocks represent "permanent" technological or structural changes, as with the China shock. Programs such as Trade Adjustment Assistance help participating prime-aged workers adjust to such shocks (Hyman, 2022). Although there are no analogous programs designed explicitly to help *youth* adjust to structural changes to labor demand, 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. In contrast, nationwide studies of similar shocks find that declines in family income and school funding can counteract and even dominate increased incentives for educational attainment (Burga and Turner, 2022; Stuart, 2022). Comparisons of my results with the literature 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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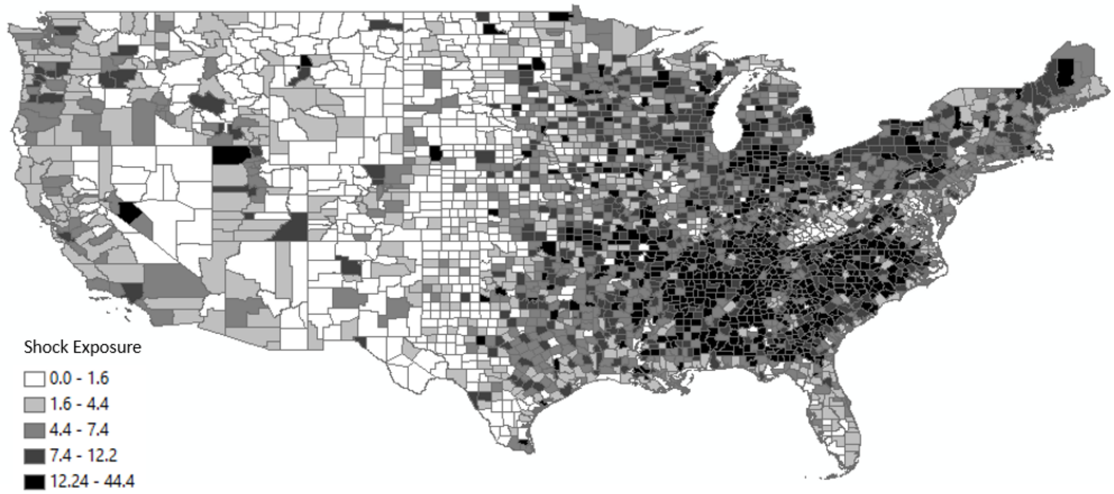
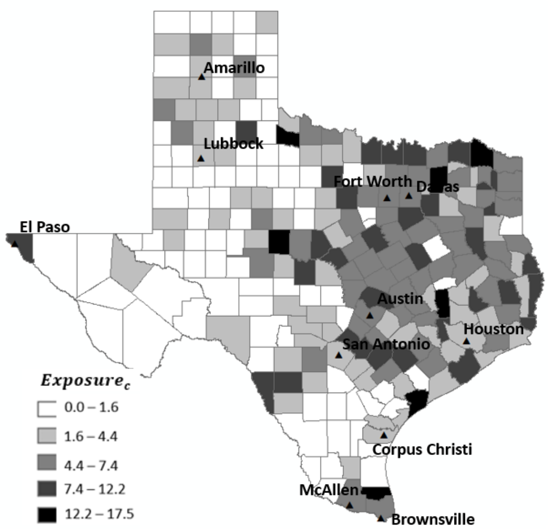
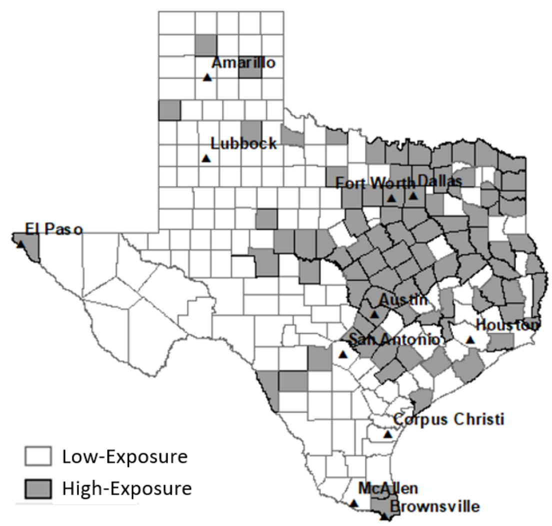


(a) Shock Exposure, U.S.



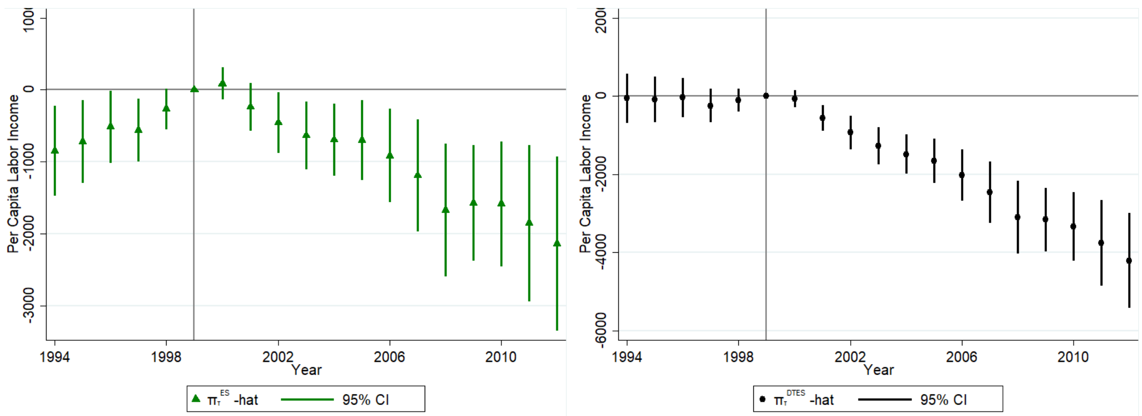
(b) Shock Exposure, TX



(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 and Panel 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.

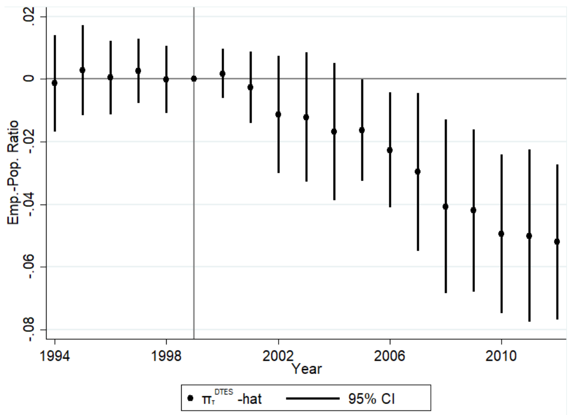


(a) Per-Capita Labor Income

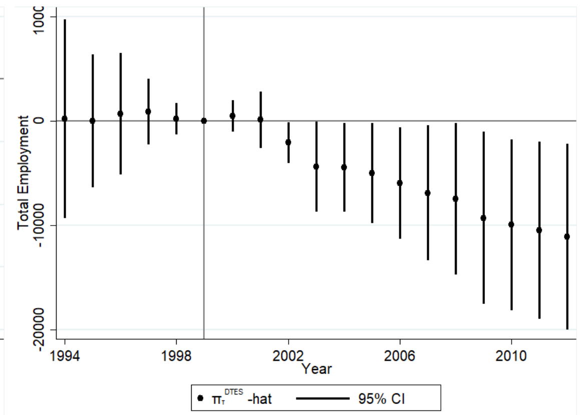
(b) Adjusted for Linear Pre-trends

Figure 2: Import Competition Caused Declines in Local Labor Demand

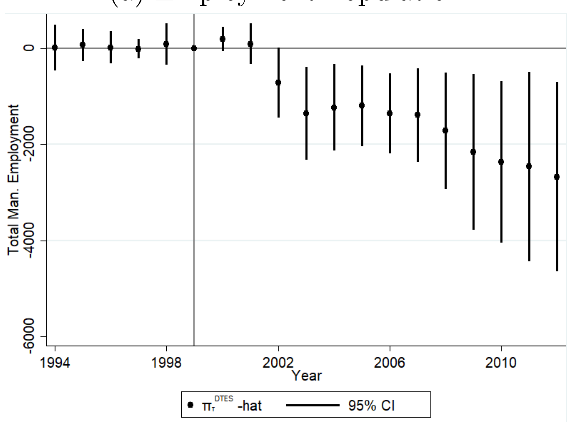
Notes: These figures present estimates of the effect of exposure to Chinese import competition on per-capita labor income using personal income data from the Bureau of Economic Analysis Regional Economic Accounts dataset and population counts by age group from the Survey of Epedemiology and End Results. 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 and 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 pre-trend-adjusted event study (equation (3)) that partials out a linear pre-trend in the first step. Both specifications 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. 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.



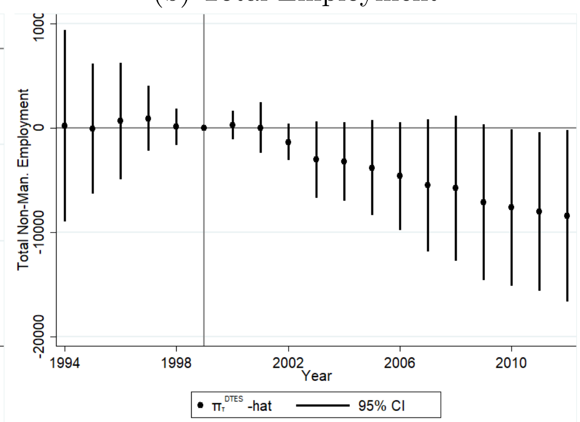
(a) Employment:Population



(b) Total Employment



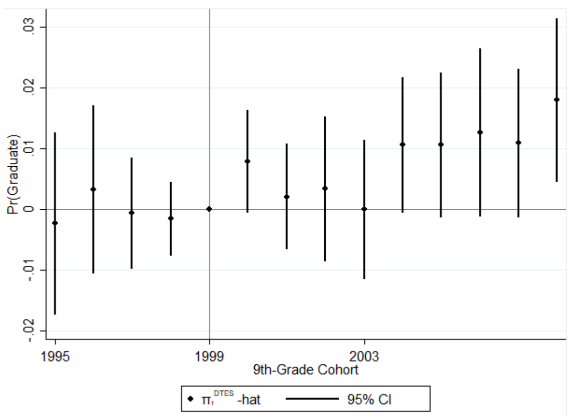
(c) Manufacturing Employment



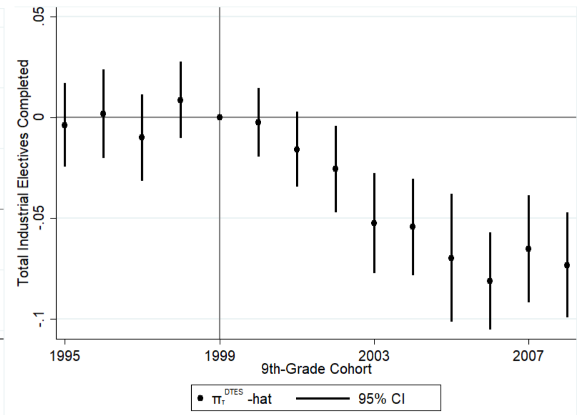
(d) Non-Manufacturing Employment

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

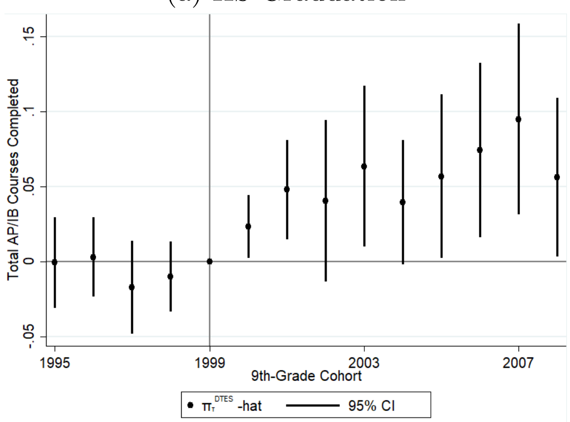
Notes: These figures present estimates of the effect of exposure to Chinese import competition on employment using employment counts from the County Business Patterns Database and population counts by age group from the Survey of Epedemiology and End Results, and personal income data from the Bureau 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.



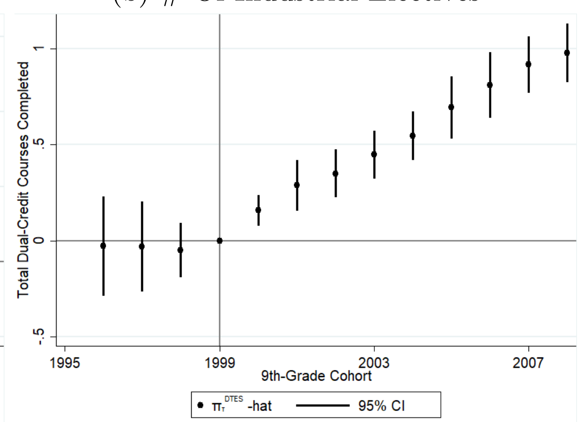
(a) HS Graduation



(b) # Of Industrial Electives



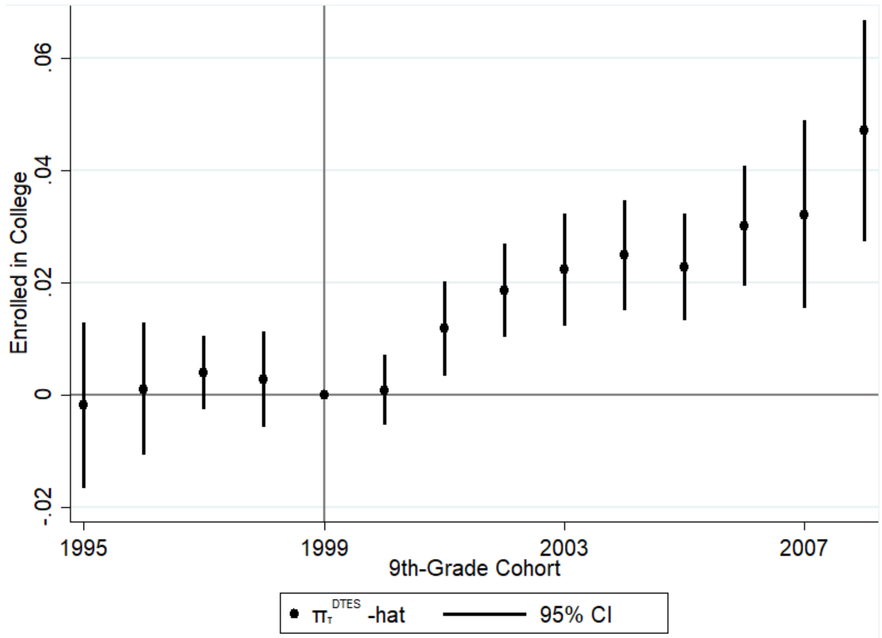
(c) # of AP/IB Courses



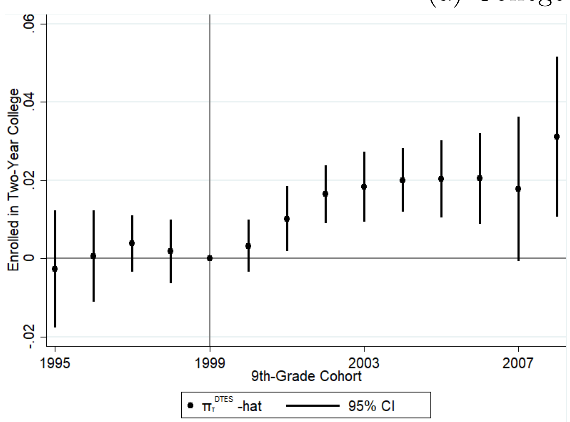
(d) Dual Enrollment

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

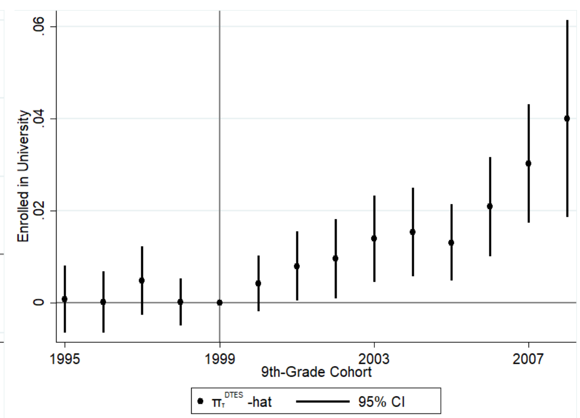
Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on high school graduation and course selection. Estimates reflect coefficients from two-step event study regressions that compare the difference in outcomes between students from counties 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) AP/IB courses 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 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 adverse local 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.

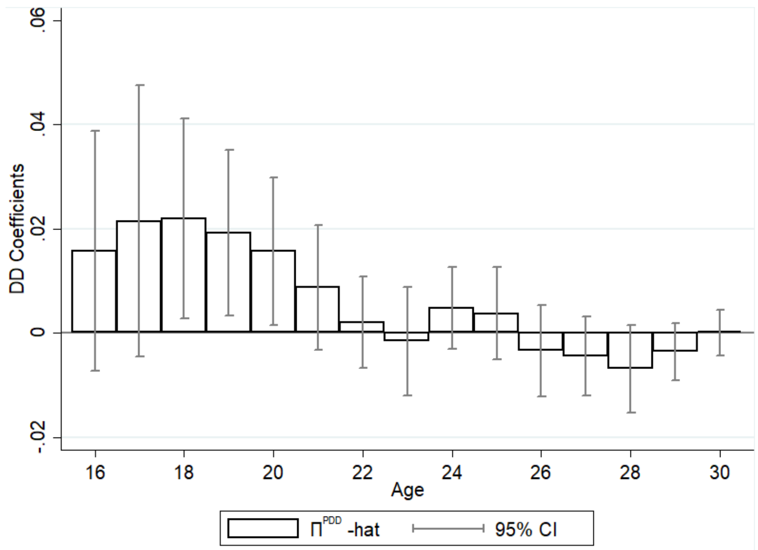


Figure 6: Effects of the Labor Demand Shock on College Enrollment by Age

Notes: This figure presents estimates of the effect of exposure to local shocks during youth and adolescence on college enrollment (including dual enrollment while in high school) from ages 16-30 using data from the University of Houston Education Research Center. 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 counties 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 attended ninth grade. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable.

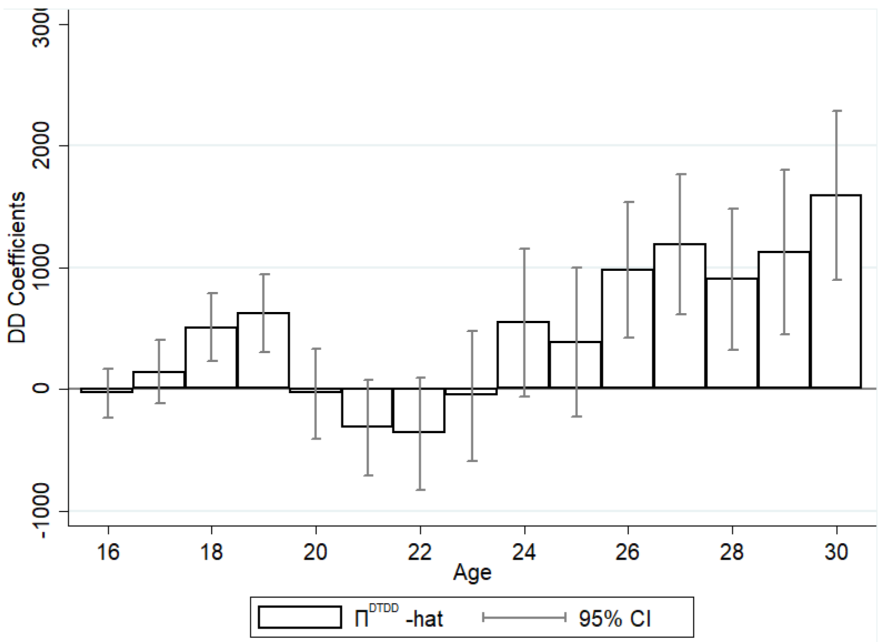


Figure 7: Effects of the Labor Demand Shock on Earnings by Age

Notes: This figure presents estimates of the effect of exposure to local shocks during youth and adolescence on earnings from ages 16-30 using data from the University of Houston Education Research Center. 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 counties 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 attended ninth grade. 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 $Exposure_c$ of the county in which they attended school 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) am 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 earnings for the pre-period cohorts on student demographics in the pre-period and 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

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). 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) Earnings, College Graduate	(4) Per-pupil Property Tax Revenue	(5) Per-pupil K-12 Spending	(6) FRPL Eligibility
De-trended DD	-3,620***	-2,839***	3,353***	-2,786***	39	0.030***
	(365)	(477)	(1,135)	(421)	(98)	(0.010)
Percent Change	-18.0	-8.2	5.4	-75.7	0.43	8.0
Pre-period Mean	20,166	34,621	62,457	3,681	9,094	0.370
N	4,286	4,298	4,302	4,810	4,810	3,678,707

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 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) through (3) are average earnings for school-aged workers, workers without any college attainment, and workers with a college degree, respectively. The outcomes in columns (3) 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) Dual-Enrollment Courses	(5) AP/IB Courses
De-trended DD	0.008	-0.039	-0.047***	0.581***	0.059**
	(0.006)	(0.038)	(0.012)	(0.095)	(0.023)
Percent Change	1.2%	-2.8	-24.3	299.3	40.3
Pre-period Mean	0.706	1.398	0.191	0.194	0.147
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 effect 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 dual-credit courses 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 AP or IB courses completed 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

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 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., 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: 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 effect 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.

Table 8: 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 effect 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 9: 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 clustered by county in parentheses

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

Notes: This table presents estimates from falsification exercises 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 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 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 10: Credit-Constrained Students Made "Costless" Adjustments

	(1) Enrolled	(2) BA/BS	(3) Dual-Credit	(4) Industrial	(5) AP/IB
A. FRPL-Eligible					
De-trended DD	0.000	0.000	0.307***	-0.073***	0.070***
	(0.005)	(0.001)	(0.058)	(0.016)	(0.019)
Percent Change	0.1	1.0	306.3	-34.3	63.6
Pre-period Mean	0.277	0.049	0.100	0.213	0.109
N	1,836,467	1,836,467	1,836,467	1,836,467	1,836,467
B. Non-FRPL					
De-trended DD	0.020***	0.010***	0.428***	-0.036***	0.054
	(0.006)	(0.004)	(0.104)	(0.009)	(0.042)
Percent Change	3.8	5.3	213.0	-20.6	31.5
Pre-period Mean	0.535	0.188	0.201	0.175	0.176
N	1,842,240	1,842,240	1,842,240	1,842,240	1,842,240

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 heterogeneous effects of exposure to negative local labor demand shocks on human capital accumulation by a proxy for borrowing constraints (Free-or-Reduced-Price Lunch Eligibility), 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 and similarly assigned FRPL eligibility based on their first appearance in the dataset. Standard errors are clustered by county and adjusted to account for using a regression-adjusted outcome variable. Panels A and B limit the analysis sample to students that were and were not FRPL-eligible, respectively. 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) is an inidcator variables for receiving a bachelor's degree by age 25. The outcomes in columns (3) through (5) are counts of dual-enrollment courses, industrial elective courses, and AP or IB courses completed in high school. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean for high-exposure counties.

Table 11: The Shock Increased Human Capital Accumulation Across the Ability Distribution

	(1) HS Grad	(2) Enrolled	(3) Enrolled at CTC	(4) Enrolled at Uni	(5) Total Semesters	(6) Certificate	(7) AA/AS	(8) BA/BS
A. Below-Median Test Scores								
De-trended DD	0.013	0.013***	0.009**	0.007*	0.167***	-0.001	0.002	0.004
	(0.008)	(0.005)	(0.005)	(0.004)	(0.0069)	(0.001)	(0.001)	(0.003)
Percent Change	2.0	3.6	3.1	7.6	5.2	-4.7	4.9	6.7
Pre-period Mean	0.662	0.356	0.306	0.095	3.243	0.021	0.037	0.065
N	1,163,679	1,163,679	1,163,679	1,163,679	1,163,679	1,163,679	1,163,679	1,163,679
B. Above-Median Test Scores								
De-trended DD	0.002	0.021***	0.024***	0.009*	0.237***	-0.001	0.001	0.014***
	(0.003)	(0.007)	(0.006)	(0.005)	(0.075)	(0.001)	(0.002)	(0.003)
Percent Change	0.2	3.5	5.6	2.9	3.5	-5.0	1.0	5.8
Pre-period Mean	0.846	0.604	0.431	0.313	6.696	0.019	0.061	0.236
N	1,187,742	1,187,742	1,187,742	1,187,742	1,187,742	1,187,742	1,187,742	1,187,742

DoF-adjusted 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 human capital accumulation by a proxy for student ability (baseline standardized 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, 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 and are assigned standardized test scores from their earliest appearance within grades 3 through 8 in the dataset. Test scores are standardized to have mean 0 and standard deviation of 1 and residualized by grade-by-year dummy variables to allow for comparisons across grades. 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 outcome in column (2) 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 (3) and (4) 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., 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 (5) is the total number of semesters a student enrolled in by age 25. The outcomes in columns (6) through (8) are inidcator 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 12: 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. EEstimates 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

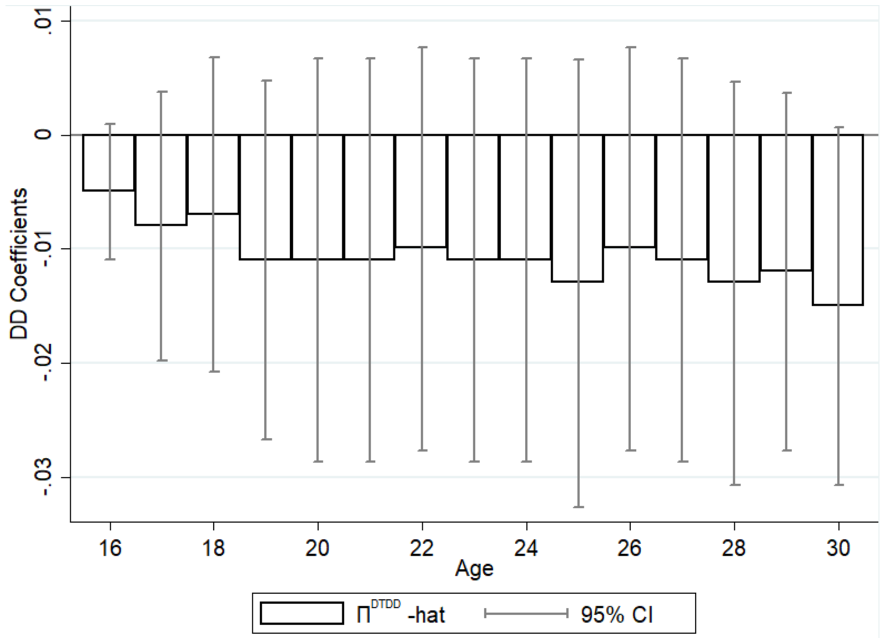


Figure A1: No Statistically Significant Evidence of Differential Attrition from the Dataset

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.

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)	(2)	(3)	(4)
	14-24	25-33	35-54	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: Effects on Vocational Elective Selections

	(1) All Vocational	(2) Industrial	(3) Technical	(4) Agricultural	(5) Health	(6) Business
De-trended DD	-0.039	-0.047***	-0.023***	0.012*	0.000	0.026
	(0.038)	(0.012)	(0.006)	(0.007)	(0.006)	(0.017)
Percent Change	-2.8	-24.3	-66.2	6.3	0.5	4.0
Pre-period Mean	1.398	0.191	0.035	0.190	0.048	0.631
N	3,678,707	3,678,707	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 of exposure to Chinese import competition on vocational elective course completions in high school 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 a count of total vocational electives completed in high school and those of the remaining columns correspond to completions of courses in a particular Texas Education Agency category of vocational electives.

Table A5: 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

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 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 A6: 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 A7: 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 A8: 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 A9: 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 A10: 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 A11: Robustness to Reversions to Parallel Trends

	(1) Parallel in Post	(2) Linear through 1	(3) Linear through 2	(4) Linear through 3	(5) Linear through 4	(6) Linear through 5	(7) Linear through 6	(8) Linear through 7	(9) Linear through 8
A. College Enrollment									
De-trended DD	0.005	0.008	0.010*	0.012**	0.014***	0.015***	0.016***	0.017***	0.017***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Percent Change	1.2	1.8	2.4	2.8	3.2	3.6	3.8	4.0	4.1
Pre-period Mean	0.423	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	3,678,707
B. Earnings									
De-trended DD	1,113***	1,347***	1,502***	1,579***					
	(410)	(410)	(410)	(410)					
Percent Change	5.1	6.2	6.9	7.3					
Pre-period Mean	21,705	21,705	21,705	21,705					
N	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 modifications of 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 a continuation of existing trends that reverts to parallel trends after p periods. 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) presents estimates where the first-step partials out only the linear pre-trend and assumes that outcomes revert to trending in parallel in the post period. Columns (2) through (9) partial out present estimates where the first-step partials out a linear pre-trend that extends p periods into the post period before reverting to parallel. Column (9) corresponds to the preferred specification used throughout the main analyses in Panel A. Earnings at age 30 is only observed through the 2002 ninth-grade cohort, so Column (4) correspondes to the preferred specification used throughout the main analyses in Panel (B)

Table A12: Robustness to Smooth Deviations from Linear Trends (Rambachan and Roth, 2023)

	(1) Deviation Size (m)	(2) 95% Confidence Set Lower Bound	(3) 95% Confidence Set Upper Bound
A. College Enrollment			
	0.0002	0.008	0.048
	0.0004	0.005	0.053
	0.0006	0.002	0.058
	0.0008	-0.002	0.064
	0.0010	-0.005	0.069
A. Earnings at 30			
	10	-178	3,932
	20	-198	3,965
	30	-217	3,980
	40	-237	4,013
	50	-257	4,027

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). Estimates reflect robust 95% 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). original 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.

Table A13: Four-Year Enrollment Responses Across Tuition Quartiles

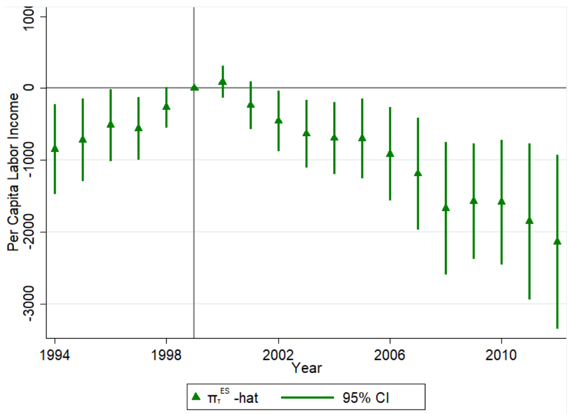
	(1) Q1 Tuition	(2) Q2 Tuition	(3) Q3 Tuition	(4) Q4 Tuition	(5) Private
De-trended DD	0.008***	0.003*	-0.005**	0.008***	-0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)
Percent Change	57.3	8.3	-18.9	9.0	-7.1
Pre-period Mean	0.014	0.038	0.028	0.094	0.028
N	3,678,707	3,678,707	3,678,707	3,678,707	3,154,442

Standard errors clustered by county in parentheses

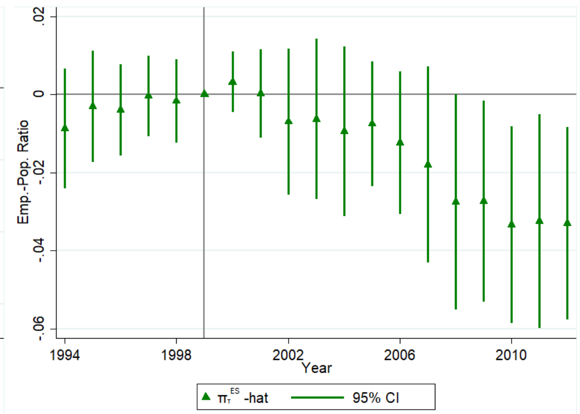
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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.

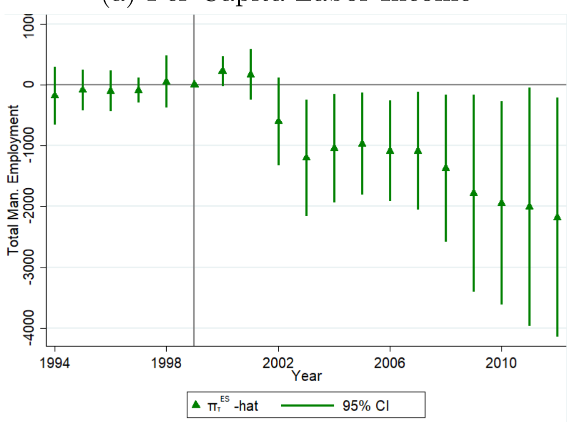
A.1 Unadjusted Event Studies



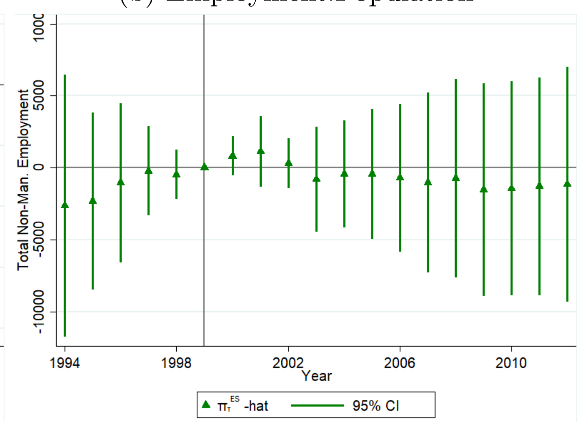
(a) Per-Capita Labor Income



(b) Employment:Population



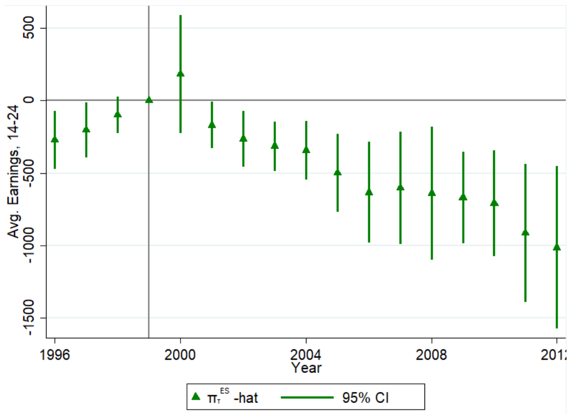
(c) Manufacturing Employment



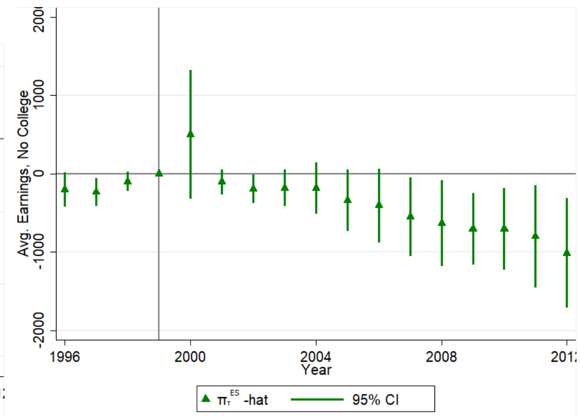
(d) Non-Manufacturing Employment

Figure A2: "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 and 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.

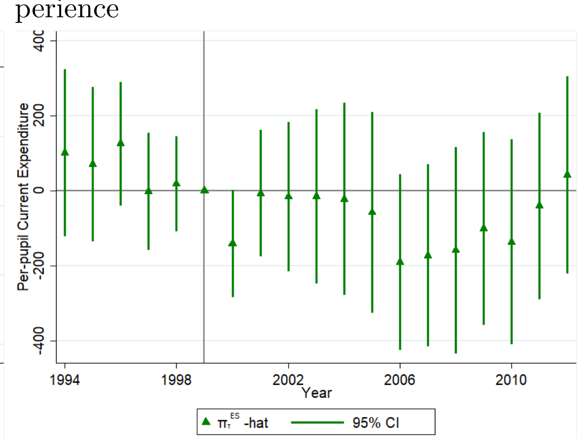


(a) Earnings, School-Aged Workers



(b) Earnings, Workers Without College Experience

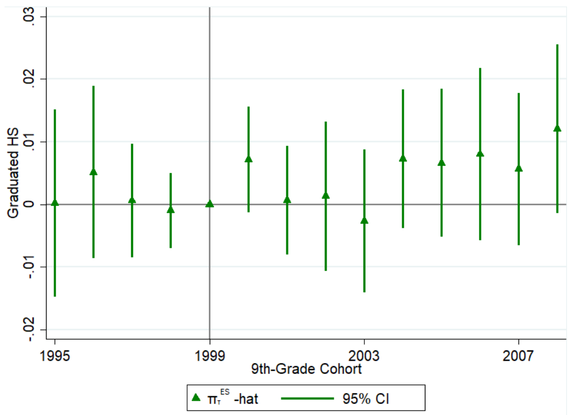
(c) Per-Pupil Property Tax Revenue



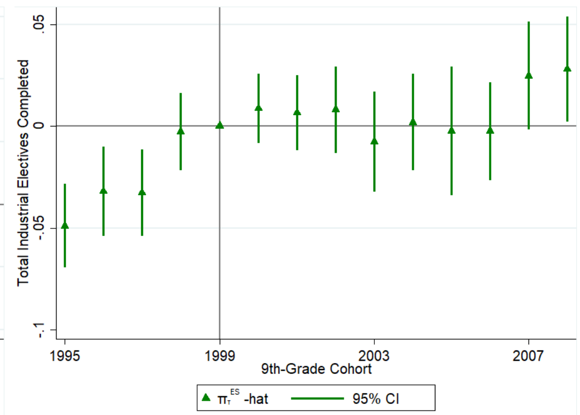
(d) Per-Pupil K-12 Expenditure

Figure A3: The Labor Demand Shock Reduced Opportunity Costs But Did Not Affect K-12 Spending

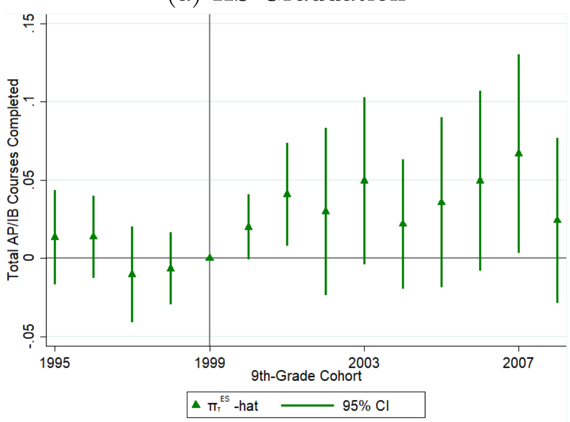
Notes: These figures present estimates of the effect of exposure to Chinese import competitionon opportunity costs and school spending using wage data broken down by age and education levels from the Quarterly Workforce Indicators dataset and K-12 finance data from the National Center for Education Statistics 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 and 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.



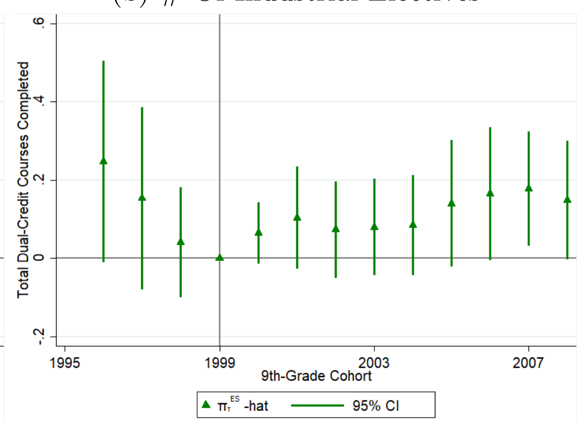
(a) HS Graduation



(b) # Of Industrial Electives



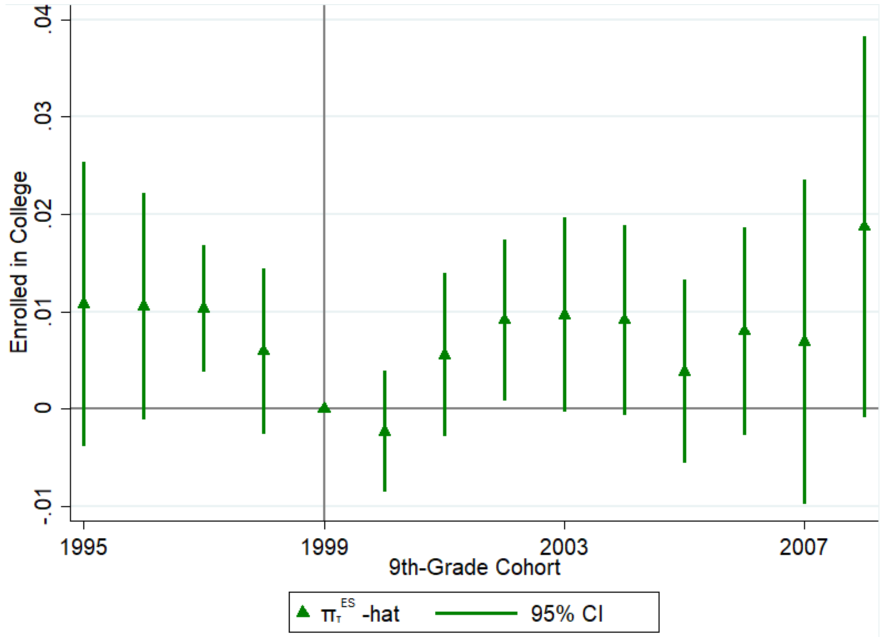
(c) # of AP/IB Courses



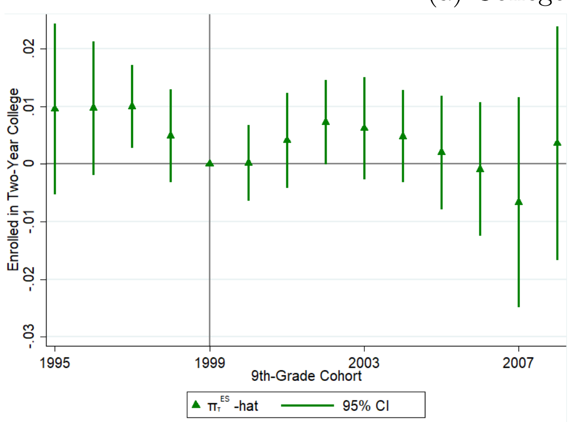
(d) Dual Enrollment

Figure A4: Effects of the Labor Demand Shock on Human Capital Accumulation in HS

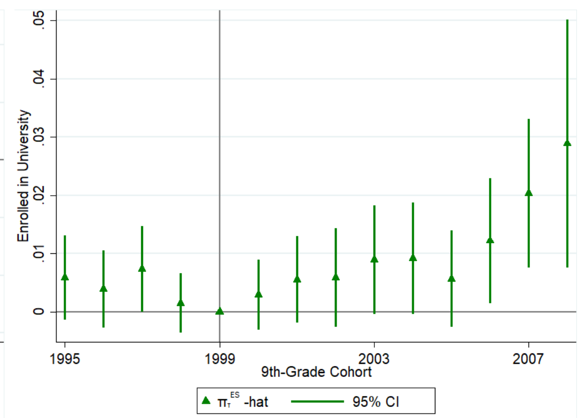
Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on high school graduation and course selection. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from 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) AP/IB courses 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



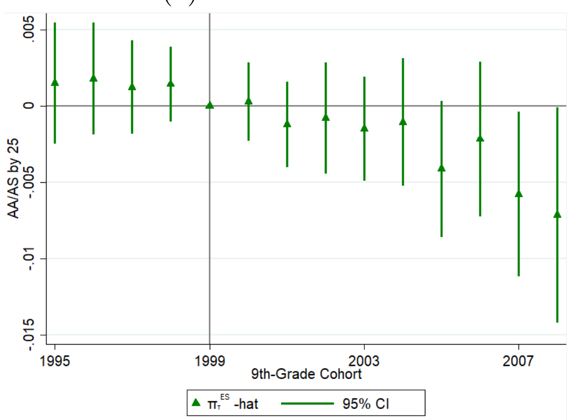
(c) University Enrollment

Figure A5: Effects of the Labor Demand Shock on College Enrollment

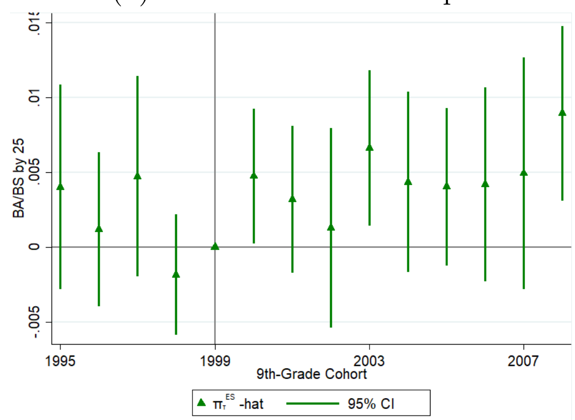
Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on college enrollment. Estimates reflect coefficients from event study regressions (equation (1)) that compare the difference in outcomes between students from counties that were more 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 A6: 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.

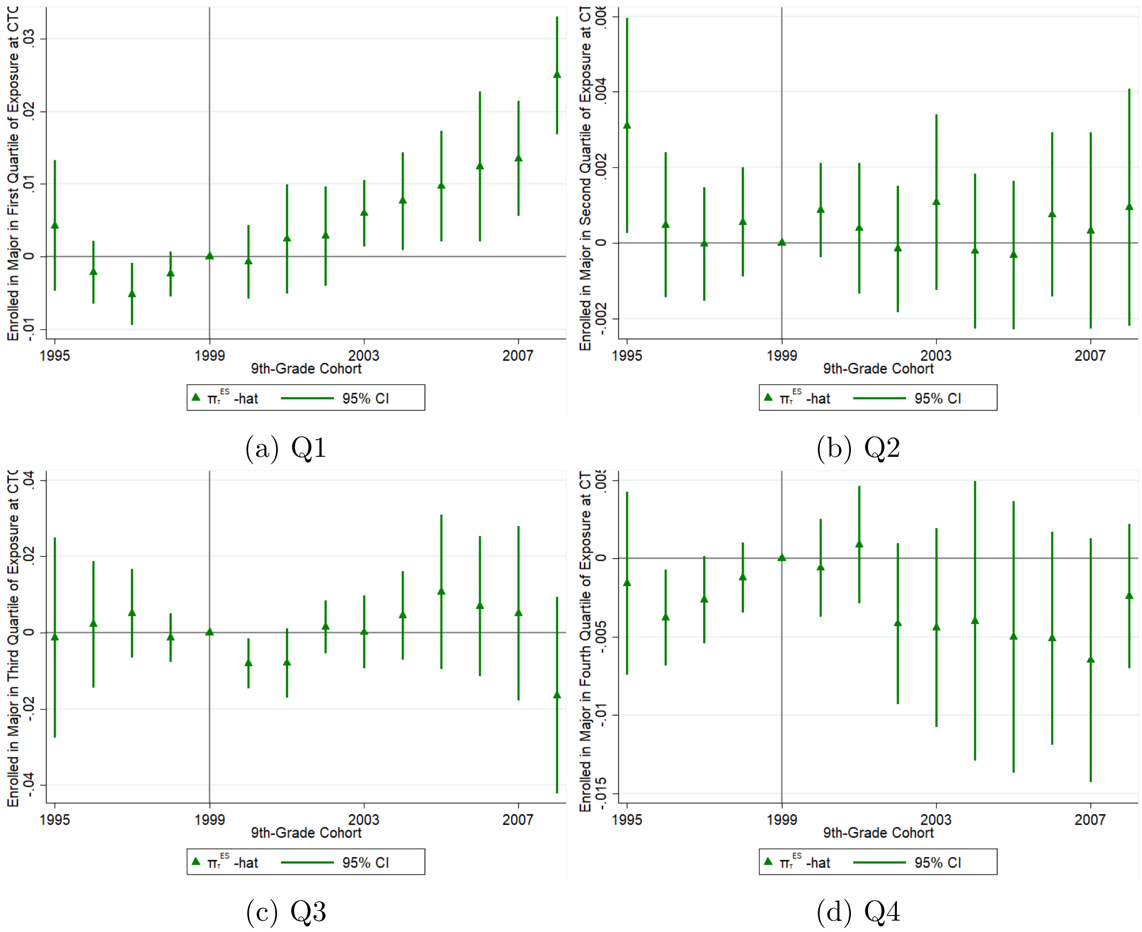
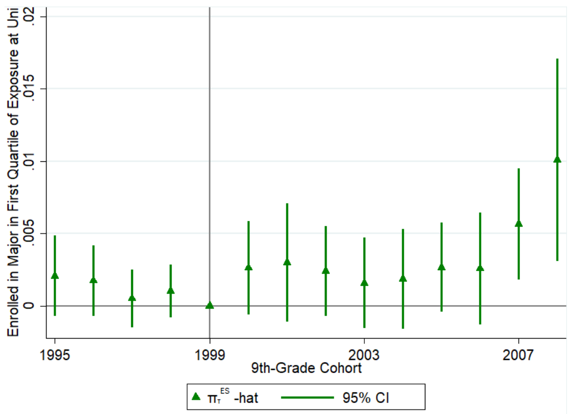
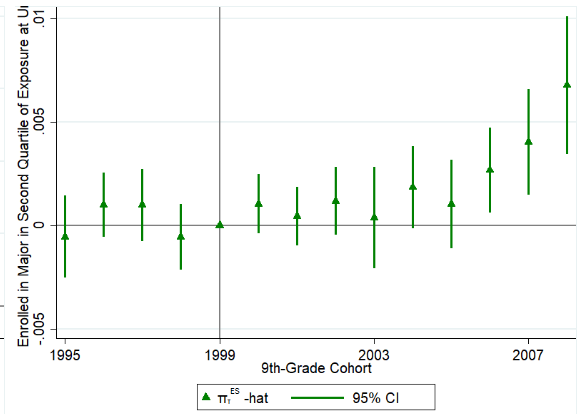


Figure A7: Effects on Enrollment by Field Exposure Quartile at Two-Year Colleges

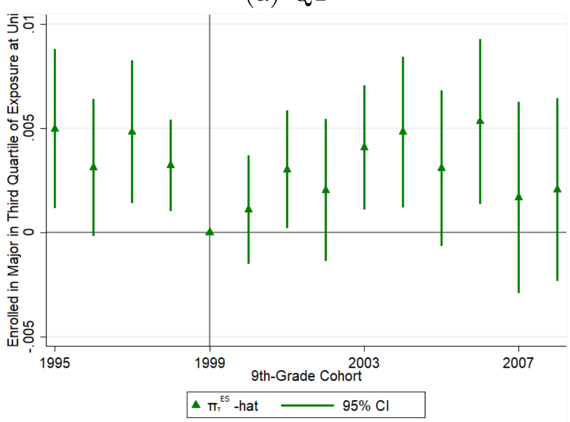
Notes: These figures present estimates of the effect of exposure to adverse local shocks during youth and adolescence on enrollment by major at public two-year colleges. I define shock exposure for each major (two-digit CIP code) as the employment-weighted average tariff gap of industries employing recent 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.



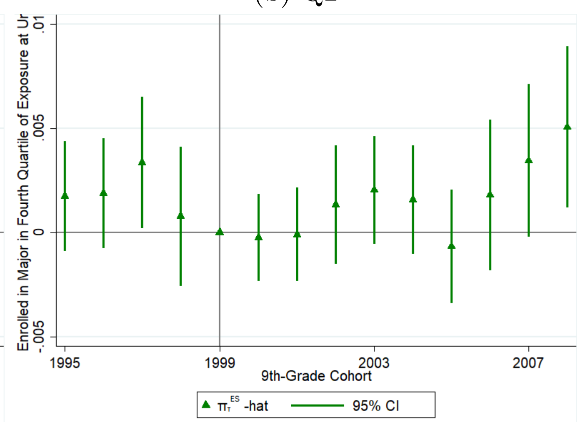
(a) Q1



(b) Q2



(c) Q3



(d) Q4

Figure A8: 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.[50] 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 A1 presents estimates of parametric 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

[50] 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 (2016) 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 parametric 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.[51] 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

[51] 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).[52] 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., 2021), 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

[52] 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. Estimations of the effects of exposure to local shocks on course completions by vocational course category are presented in Table A4.

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 7.

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 7.

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 A5.

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.[53]

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).[54] Property-poor districts that do not meet the

[53]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.

[54]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: 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.