Do Human Capital Adjustments Protect Youths from Structural Change?

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December 12, 2025

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Abstract

This paper examines how exposure to structural labor demand shocks during youth affects human capital and later-life earnings. Using Texas administrative data and leveraging variation in exposure to import competition, I show that the average affected student made modest educational adjustments and experienced minimal earnings protection. However, students unbound by credit constraints or given salient signals of rising returns to education were 11 – 24% more likely to attend community college, were 22 – 43% more likely to earn an associate's degree, and were shielded from earnings losses. These results suggest that educational adjustments can buffer against structural change.

JEL Classification: F16, H73, I24, I26, J23, J24, O33

Keywords: Human Capital, Returns to Education, Structural Change, Import Competition, Labor Demand

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Previously posted as "Human Capital Adjustments and Labor Market Resilience." For their helpful comments, I wish to thank Brian Beach, Sandra Black, Chris Candelaria, Kitt Carpenter, Luis Carvajal-Osorio, Christa Deneault, Sam Dodini, Mitchell Downey, Andrew Foote, Michel Grosz, Jessica Kiser, Michelle Marcus, Camila Morales, Pia Orrenius, Matthew Pesner, Joel Rodrigue, Jesse Rothstein, Peter Schott, Bryan Stuart, Lesley Turner, and Patrick Turner; seminar participants at the Federal Reserve Bank of Dallas, University of Alabama, University of North Carolina at Charlotte, University of South Carolina, Vanderbilt University, and Western Kentucky University; and participants at the AEFP 48th Annual Conference, SEA 93rd and 94th Annual Meeting, North Texas Economic Conference, the Federal Reserve System Conference on Economic Heterogeneity, and the Federal Reserve System Regional Conference. All remaining errors are my own. This research was supported by a grant from the American Educational Research Association which receives funds for its "AERA-NSF Grants Program" from the National Science Foundation under NSF award NSF-DRL 1749275. Opinions reflect those of the author and do not necessarily reflect those of the AERA or NSF. The conclusions of this research also do not necessarily reflect the opinion or official position of the University of Houston Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board, the Texas Workforce Commission, or the State of Texas. This research does not represent the views of the Federal Reserve Bank of Dallas or the Federal Reserve System.

1 Introduction

Structural changes to an economy often create clear winners and losers. While substantially lowering consumer prices (Jaravel and Sager, 2019), trade liberalization with China caused lasting declines in employment and earnings for low-skilled workers in exposed industries and local labor markets (Autor et al., 2014; Pierce et al., 2022). Whether individuals coming of age avoided the same fate likely depended on their ability to make adjustments to their human capital investments.

Canonical human capital theory implies that Chinese import competition's negative effects on labor market opportunities for less-educated workers would have incentivized students in exposed local labor markets to pursue further education (Becker, 1962; Greenland and Lopresti, 2016). However, the large direct costs of college may have prevented otherwise-optimal adjustments in the presence of financial frictions (e.g., Friedman, 1955; Brown et al., 2011). Moreover, how well teenagers internalize the returns to education across attainment levels and fields is not clear (Arcidiacono et al., 2025; Baker et al., 2018). As continued automation, the rapid deployment of artificial intelligence, and other looming changes to the U.S. economy threaten to bear asymmetric benefits and costs across workers and local labor markets, understanding the degree to which human capital adjustments can protect against lasting harm in the labor market is particularly important.

This paper examines how youths adjusted their educational decisions in response to declines in local labor demand caused by Chinese import competition, and whether educational adjustments yielded labor market benefits. Linked administrative K-12, postsecondary, and wage data from Texas allow me to overcome a key challenge that has limited prior work – namely, that the researcher must observe an individual's "exposure" to the shock during youth, their educational decisions, *and* their later-life earnings. Using a difference-in-differences design, I first show relatively modest average changes in educational decisions by students from counties that were more-exposed to the "China shock" that are consistent with prior literature (Ferriere et al., 2018; Burga and Turner, 2022). I then provide novel evidence that, as a whole, individuals coming of age in such counties fared nearly as poorly in the labor market as those who had already recently entered the workforce and were

directly exposed to the shock. However, I show evidence that financial and informational frictions hindered adjustments. Students with plausibly relaxed credit constraints and those who received more salient signals of the increase in the returns to education were 11 - 24% more likely to enroll in a two-year college, were 22 - 43% more likely to earn an associate's degree, and were protected from the shock's adverse earnings effects. My results align with a simple conceptual framework in which students weigh the earnings premia from human capital with direct and opportunity costs and are among the first empirical evidence that sufficient human capital adjustments can substantially shield individuals coming of age from structural change (Branco et al., 2025).

My research design uses quasi-random variation in exposure to local labor demand changes from the October 2000 establishment of "Permanent Normal Trade Relations" (PNTR) with China. PNTR exposed subsets of domestic manufacturing firms to increased competition from Chinese exporters (Pierce and Schott, 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 employment, but with firms with less-exposed specialties (e.g., processed foods) (Pierce and Schott, 2020; Greenland et al., 2019). Counties in Texas with a standard deviation greater exposure to the shock experienced a 3.6 percentage point decline in their employment-to-population ratio and a 1.5 percentage point increase in their unemployment rate.[1] Employment losses were strongest in manufacturing – but spread outside of the sector – and for less-educated workers. I provide evidence suggesting the shock represented both a decline in the opportunity cost of education and an increase in the local college earnings premia.

Using linked administrative K-12, college, and earnings records for students from all public K-12 schools and postsecondary institutions in Texas, I leverage this variation in a difference-in-differences specification that compares changes in educational and labor market outcomes across cohorts of students who reached critical ages before and after the policy change (first difference) in counties that were more- and less-exposed to PNTR (second differ-

[1]These magnitudes are comparable to estimates of the effect of the China shock on local labor markets across a nationwide sample (Pierce and Schott, 2020) and to the effects of a typical recession on more-exposed local labor markets (Hershbein and Stuart, 2024).

ence). The panel nature of the my dataset enables an intent-to-treat framework that assigns treatment based on where students resided prior to PNTR, preventing bias from migratory responses to the shock (Greenland et al., 2019). Identification requires the assumption that trends in outcomes across cohorts of students from counties that experienced large and small shocks would have responded the same on average for a given dose of exposure to the shock (Callaway et al., 2021; Goodman-Bacon, 2021).[2] I provide support for causal interpretation by showing parallel trends in outcomes before the onset of PNTR and that exposure is not correlated with trends in pre-determined student characteristics across cohorts. Moreover, I show robustness to relaxing this assumption by allowing smooth deviations in potential outcomes from existing linear trends (Rambachan and Roth, 2023).[3]

I first examine average effects of the China shock on educational decisions. I find no evidence that students from more-exposed counties were more likely to graduate from high school – consistent with prior estimates using school-level graduation counts (Burga and Turner, 2022) – or that students shifted course loads away from those aligned with employment in manufacturing. However, exposure to larger local shocks caused a 3.5 percentage-point increase (11%, relative to the mean among pre-period cohorts) in two-year college enrollment and a 0.8 percentage-point (18%) increase in associate's degree receipt. Consistent with changes in the returns to education across fields, students also shifted from technical to academic programs at two-year colleges. Following students into the labor market, I find no statistically significant evidence that students young enough to have adjusted their educational decisions in response to the China shock fared better than those who entered the labor market before the shock, and the point estimates imply earnings gains only large enough to offset just over 20% of the sustained decline in earnings the shock caused for recent high school graduates.

Motivated by a simple conceptual framework, I explore three potential explanations why

[2]Formally, the specification identifies an Average Causal Response parameter under the stated "strong" parallel trends assumption (Callaway et al., 2021). Interpretation of the identified parameter differs from that of an Average Treatment Effect by representing marginal effects of a change in treatment intensity, rather than the effects of treatment relative to its absence. The identifying assumption differs from the standard parallel trends assumption by requiring potential outcomes to trend in parallel on average for all given doses of treatment, rather than only under the absence of treatment.

[3]I show that my primary results are also robust to alternative control sets and dropping the observations from any single county in Section 7.

the overall student population made relatively modest human capital adjustments in response to the labor demand shock: financial frictions, imperfect information, and mitigating changes in low-skilled labor demand. I leverage geographic variation in the cost of college from a unique feature of Texas' community college system (Denning, 2017) to consider whether financial frictions prevented credit-constrained students from enrolling in college. I find that exposure to larger local shocks caused students eligible for reduced tuition at a local community college to be 24% more likely to enroll in two-year colleges, 43% more likely to earn associate's degrees, and 35% more likely to earn bachelor's degrees. These strong responses contrast with economically and statistically insignificant estimated responses by students who were ineligible for reduced tuition, consistent with the presence of binding credit constraints.[4]

Next, I consider whether the shock was salient enough for students to internalize its effects on the returns to education. Using linkages to earnings records, I proxy shock salience by the share of high school graduates and dropouts from older cohorts who worked in manufacturing prior to PNTR. I find that the shock's effect on high school graduation and two-year college enrollment were each 3 percentage-points higher for students from high schools with a 10 percentage-point greater share of older peers who worked in manufacturing. Although the China shock reduced employment in and outside of manufacturing in more-exposed counties in my setting, these results suggest that students for whom the direct impact on manufacturing employment was most visible more easily internalized changes in the returns to education.

The China shock would not change the local returns to education if there were coincident increases in labor market opportunities for less-educated workers. Kovalenko (2023) shows that such opportunities arose in local labor markets overlaying shale plays during the 2000s fracking boom, and students responded by entering the labor market earlier. I find that the China shock had no significant effect on educational attainment in counties overlaying shale reserves, consistent with the offsetting emergence of low-education labor market

[4]These results also align with (Ferriere et al., 2018), who find evidence of the China shock only positively affecting college enrollment among students from the top three quartiles of the wealth distribution. Both sets of analyses suggest credit-constraints prevented adjustments by poor households, and, my results support that additional financial aid could enable adjustments by otherwise-constrained students.

opportunities.

To assess whether sufficiently large human capital adjustments protected against earnings depressions, I estimate effects of exposure to the shock on earnings across these three dimensions of heterogeneity. I find that students who were plausibly not bound by credit constraints and exposed to larger local shocks exhibited sustained increases in earnings later in life relative to cohorts who made educational decisions prior to the shock. The estimated earnings gain of over $3,000 in annual earnings at age 35 is large enough to represent full protection from the declines in earnings the shock caused for cohorts who entered the labor market just before PNTR. Furthermore, I show a dollar-for-dollar increase in *imputed* earnings based on educational attainment and industry of employment, supporting the notion that human capital adjustments enabled the earnings gains. I also find substantial earnings gains for students who faced a more salient change in the returns to education that align with dollar-for-dollar increases in imputed earnings. Unsurprisingly, students from counties that experienced counteracting shocks to labor demand from the China shock and fracking boom experienced relative earnings gains following the boom that could not be statistically explained by increases in educational attainment. Taken together, these three exercises suggest education as a critical "escape route" from skill-biased structural change – which may only not be necessary in the presence of countervailing labor demand shocks.

This paper contributes to a growing literature on the effects of structural changes to labor demand on individuals coming of age. An extensive group of research finds evidence of varying degrees of human capital adjustments in response to trade (Atkin, 2016; Greenland and Lopresti, 2016; Li, 2018; Burga and Turner, 2022; Ferriere et al., 2018; Choi, 2024), energy booms and busts (Black et al., 2005; Weinstein, 2022; Cascio and Narayan, 2022; Kovalenko, 2023), and technological change (Di Giacomo and Lerch, 2023; Branco et al., 2025).[5] My dataset's linkages to earnings records allow me to provide among the first empirical evidence that such educational adjustments can provide meaningful protection

[5]A related strand of literature examines how *cyclical* shocks affect educational attainment (Charles et al., 2018; Stuart, 2022; Weinstein, 2022; Deneault, 2025). Most notably, Stuart (2022) finds that individuals growing up in local labor markets hit hardest by the 1980-1982 recession were less likely to earn a college degree and earned less as results. The larger presence of need-based financial aid and the presence of Texas' "Robin Hood" K-12 finance system, which prevented declines in local property values from manifesting in declines in school spending, may contribute to why only I find evidence of increased average educational attainment – and earnings benefits, for students who sufficiently adjusted their human capital investments.

against structural change, alongside Branco et al.'s (2025) study of educational adjustments to routine-biased technological change.[6] The authors find that individuals growing up in states with greater adoption of industrial robots were more likely to earn a bachelor's degree and experienced relative increases in adult earnings. The magnitude of my estimated earnings effects across the full sample as a proportion of the estimated earnings declines for individuals who entered the labor market just before the onset of the shock is comparable to the authors' finding that human capital adjustments only offset 30% of the direct adverse earnings effects caused by the technological change. Furthermore, my evidence of educational adjustments providing *full* earnings protection for students who were eligible for reduced tuition at local two-year colleges supports the results of their simulation of more generous financial aid. My granular administrative dataset also allows me to highlight additional factors that aided or inhibited educational responses, including shock salience.

My findings also add to the literature on barriers to college education. Empirical work provides evidence that financial frictions (Brown et al., 2011; Denning, 2017; Lovenheim, 2011; Lovenheim and Reynolds, 2013; Belzil et al., 2021; Black et al., 2023), physical distance (Acton et al., 2024), and imperfect information on the returns to education (Betts, 1996; Oreopoulos and Dunn, 2013; Wiswall and Zafar, 2014; Hurwitz and Smith, 2018; Baker et al., 2018; Arcidiacono et al., 2025) may inhibit many individuals from enrolling in college. My results demonstrate that by constraining students from responding to *changes* in the returns to education, such barriers take on a heightened importance during periods of significant economic change. From this perspective, the interaction between increases in the returns to education and unequal constraints may exacerbate inequality in educational attainment and earnings (Autor et al., 2020; Athreya and Eberly, 2021).

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 exposure to Chinese import competition on exposed local labor markets and workers (e.g., Autor et al., 2014, 2021; Pierce and Schott, 2020). Recent work attempts to distinguish the

[6]My full-sample results also align with those of Choi (2024), who finds that exposure to deindustrialization in the Northeast during the 1920s caused students to obtain more education. Both papers find that *average* adjustments did not yield labor market benefits. I show that average responses to the China shock mask substantial heterogeneity along adjustment frictions and that students who made sufficient adjustments experienced sustained increases in adult earnings.

differential impacts of the China shock on *people* (i.e., workers in exposed manufacturing subsectors) versus *places* (i.e., local labor markets with large initial employment shares in exposed manufacturing subsectors). Autor et al. (2025) find that exposed local labor markets experienced full employment recovery in levels – but not relative to population – but that incumbent workers never fully recover earnings losses. However, no prior work examines the effect of the shock on the long-term outcomes of *youths growing up* in affected areas. I provide evidence, at least in Texas, such individuals avoided lasting harm *if* they acquired sufficient human capital. A relatively modest average effect on college enrollment, despite evidence of large returns to doing so, mirrors evidence of limited uptake but substantial benefits of retraining through the Trade Adjustment Assistance program (Hyman, 2018). Taken together, these results suggest benefits to public subsidies to train both directly displaced workers and the next generation in the wake of trade and other structural shocks.

2 Background

2.1 Policy Variation

My research design leverages variation in declines in local labor demand resulting from a change in U.S. trade policy toward China in 2000.[7] The U.S. historically subjected goods imported from foreign countries to one of two sets of tariff rates. Goods imported from fellow members of the World Trade Organization (WTO) were subject to relatively low "column 1" rates (hereafter, "preferred" tariff rates), while goods from nonmarket economies were subject to relatively high "column 2" rates (hereafter, "punitive" tariff rates) that followed closely with those set by the Smoot-Hawley Tariff Act of 1930. In 1999, preferred tariff rates averaged 4% and punitive rates averaged 37%. Moreover, the difference between the preferred and punitive tariff rates (the "tariff gap") varied widely by type of good, ranging from a 0 to 80 percentage-point difference.

The President may annually extend preferred tariff rates to nonmarket economies, although Congress can pass legislation to block such an extension. The U.S. first granted

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

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.[8] The political uncertainty surrounding trade policy with China permeated into the operations of U.S. firms, and those producing goods subject to large tariff gaps were particularly unlikely to outsource production to China due to the prospect of reversal to punitive tariff rates in any given year (Pierce and Schott, 2016).[9]

In October 2000, Congress passed a bill to establish "Permanent Normal Trade Relations" (PNTR) with China, removing the need for annual approval and the corresponding uncertainty. 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.[10] These effects persisted through at least 2016 (Autor et al., 2021). Additionally, more

[8]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.

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

[10]Using a shift-share design and aggregate Census data, Autor et al. (2013) find that local labor markets

exposed labor markets experienced reductions in housing prices and public spending (Feler and Senses, 2017) and increases in fatal drug overdoses (Pierce and Schott, 2020; Autor et al., 2019).

2.2 Conceptual Framework

To motivate my empirical analyses, I outline a simple conceptual framework of how the local labor demand shocks caused by PNTR may have affected human capital accumulation, mimicking recent work that draws on canonical human capital theory (Charles et al., 2018; Becker, 1962). I consider young adults in high school deciding whether to continue their education or enter the labor force by weighing gains in discounted lifetime earnings from obtaining a given level of education versus the monetary (i.e., tuition and fees), psychic (i.e., effort), and opportunity (i.e., forgone wages) costs of doing so. The above literature on the local labor market effects of the China shock suggest multiple channels through which exposure to the shock could affect human capital decisions in this framework. 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. Concurrently, decreases in expected earnings for workers in sectors directly exposed to the labor demand shocks,

(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.

such as manufacturing, would reduce the expected returns for studying fields that lead to employment in these industries and incentivize students to adjust their course and major selection away from such fields.

I note three complexities beyond this basic intuition that are relevant to my empirical analysis. Because private markets under-provide student debt (Friedman, 1955) and and federal undergraduate loans have limits, some students may face borrowing constraints. For such credit-constrained students, the monetary costs of college prohibit enrollment regardless of opportunity costs.[11] In a local average treatment effect framework, credit-constrained students serve as "never-takers," while "compliers" are comprised of students induced to attend college by the reduction in local wages for non-college-educated workers (Imbens and Angrist, 1994).[12]

Second, students may not perfectly observe changes in labor demand. A robust literature supports that young adults possess incomplete or biased information on the labor market prospects associated with different education levels and fields (Betts, 1996; Oreopoulos and Dunn, 2013; Wiswall and Zafar, 2014; Hurwitz and Smith, 2018; Baker et al., 2018; Arcidiacono et al., 2025). Like credit-constrained students, students who do not observe changes in the returns to education may be considered "never-takers" of college enrollment as a response to PNTR.

Finally, a negative shock to local labor demand may tighten constraints on external investments in human capital (Stuart, 2022; Burga and Turner, 2022). A particularly important channel of such investments is K-12 spending, and my setting allows me to leverage negative shocks to local labor demand that do not affect school spending. 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.[13] As a result of this policy, even though K-12 education

[11]Empirical and experimental work provides evidence that credit constraints prevent college enrollment for some students (Brown et al., 2011; Denning, 2017; Lovenheim, 2011; Lovenheim and Reynolds, 2013; Belzil et al., 2021; Black et al., 2023).

[12]In my setting, students for whom the lifetime earnings gains from college are larger than psychic, monetary, and both pre- and post-shock opportunity costs are "always-takers."

[13]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 details the specific formula

in Texas 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 shields students in counties exposed to adverse local shocks from reductions in school spending that would typically accompany declines in local property wealth.

As described further in Section 4, I empirically test the predictions of this framework by estimating treatment effect heterogeneity along proxies for credit constraints and the salience of changes in opportunity costs the in Section 6.

2.3 Generalizability of Setting

Although unique in some manners, my research setting of Texas is inherently important. Nearly 11% of all kindergarten through 12th-grade (K-12) students and 10% of full-time college students in the U.S. attend school in Texas, making the state an inherently relevant setting to study the effects of labor demand shocks on human capital accumulation. Public K-12 schools in Texas differed from those of the U.S. as a whole in the years before the trade shock in terms of student demographics – 40% of students in public K-12 schools in Texas were Hispanic, as opposed to only 16% across the country overall – but received similar per-pupil funding levels and produced similar high school graduation rates. Students at both two- and four-year colleges in Texas also exhibited similar differences in demographics from U.S. college students as a whole. Although graduation rates trailed at Texas colleges, funding levels mirrored nationwide averages.[14]

3 Data

3.1 Data Sources

My primary dataset consists of administrative student-level data from the University of Houston Education Research Center (UHERC), featuring linked individual-level K-12, post-secondary, and workforce records from 1994 to present. K-12 data from the Texas Education

used and its updates over time.

[14]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.

Agency include standardized test scores, course schedules, graduation records, and demographic information for all students who attended public schools in Texas. Postsecondary data from the Texas Higher Education Coordinating Board include enrollment, declared majors, and degree and certificate records from all public colleges and universities in the state.[15] 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 state unemployment insurance.[16]

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 dataset Eckert et al. (2020), population counts from the Surveillance, Epidemiology, and End Results program, and school district finance data from the National Center for Education Statistics. I provide additional details on these supplemental data sources in Appendix B.

3.2 Variable Definitions

My analysis estimates effects of exposure to the China shock during youth on educational attainment and adult earnings. I use the following measures of "extensive-margin" educational attainment:

- An indicator for graduating from a Texas public high school
- Indicator variables for enrolling in a public two-year college, public four-year college or university, or any public college or university in Texas within two years of expected high school graduation
- Indicator variables for earning a short-length certificate, associate's degree, or bachelor's degree by age 25

[15] The dataset also includes enrollment records for private colleges in Texas but not for my entire sample period. I exclude private colleges from my primary analysis and provide evidence suggesting the shock did not cause substitution between private and public colleges in Section **??**.

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

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

I also examine adjustments along the "intensive" margins of course and major selection. I use THECB's classification of community college programs as academic or technical programs as a broad measure of an intensive-margin choice. For a more granular measure of field choice, I use pre-period employment records of high school and community college completers to define the "relevance" of high school courses (by two-digit TEA subject area) and community college majors (by two-digit CIP code) to employment in industries more affected by the shock. Formally, I define course relevance by regressing the industry-level exposure (as detailed in Section 4) of an individuals' four-digit NAICS code of employment on counts of course completions across TEA-defined subjects and grouping courses into quartiles from the most negatively to most positively associated with employment in industries that were directly exposed to import competition following PNTR. I similarly define major m's shock relevance as the average industry-level exposure of four-digit NAICS codes of employment for individuals earning a community-college degree in m observed prior to the onset of the shock.

To contextualize the size of educational adjustments, I construct an imputed earnings variable in dollar terms (2020$) that for individual i working in industry k at age a is equal to the mean wages that individuals of the same education level and industry as i that entered the labor market before the onset of the shock earned at age a. I interpret estimated effects on the imputed earnings as the expected change in earnings associated with an individual's human capital adjustments and compare these effects to estimates of effects on realized adult earnings. Likewise, this measure can be thought of as a dollar-scaled human capital index.

3.3 Sample Selection

To build my analysis sample, I start with cohorts of students who entered ninth grade at Texas public high schools since Fall 1995. Two factors related to the nature of the administrative data determine further selection. First, I only observe students attending public schools, colleges, and universities in Texas; hence, students who permanently leave Texas attrit from the dataset. I discuss the implications of this for identification in Section 4. Second, student linkages across time allow me to define exposure to the trade shock based on a student's county of residence prior to 2000 to avoid bias due to endogenous migration out

of counties exposed to larger shocks. Thus, I limit my analysis samples to students observed prior to the start of the shock, which also bounds the time horizon over which treatment effects can be estimated. The panel begins with students in ninth grade in Fall 1995 and ends with those in ninth grade in Fall 2008.[17]

4 Research Design

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

4.1 Identification

My research design seeks to isolate changes in local labor demand caused by PNTR (Pierce and Schott, 2020) and compare the difference in outcomes of students in cohorts who made high school and college decisions after vs. before the policy change in counties that were more vs. less exposed to the labor demand shock. I follow Pierce and Schott (2020) in measuring county c's exposure to the establishment of Permanent Normal Trade Relations with China ($Exposure_c$, i.e., their treatment "dose") as its employment-weighted average tariff gap using 1990 employment counts from the harmonized County Business Patterns Database (Eckert et al., 2020).

Figure 1 presents maps of county-level exposure to PNTR, grouped by quintiles. Panel (a) shows that raw exposure to import competition is highly spatially correlated, with county-level tariff gaps in East Texas far exceeding those of more rural and oil-rich West Texas. The raw exposure measure largely reflects levels of overall manufacturing employment, and numerous confounding factors (e.g., automation) may differentially affect local labor demand and human capital decisions over time across counties with more or less overall manufacturing employment. Panel (b) instead groups counties into quintiles based on their tariff gap,

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

conditional on their industrial make-up across broader industries. I construct this measure by residualizing a county's tariff gap by its employment shares across two-digit NAICS industry codes. The remaining variation in shock exposure reflects idiosyncratic differences in the *products* local manufacturers produced, rather than broader differences in industrial makeup. Panel (b) reflects the variation that I seek to leverage in my research design.

Specifying treatment timing (τ) as when a cohort entered high school, I estimate the following difference-in-differences specification:[18]

$$y_{ict} = \Pi^{DD} Exposure_c * \mathbf{1}\{t \geq 2000\} + \alpha_c + \alpha_t + \Phi S_i + \Gamma \mathbf{1}\{t \geq 2000\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \tag{1}$$

In equation (1), I regress outcome y_{ict}, such as an indicator variable for graduating high school, for student i in county c and ninth-grade cohort t on $Exposure_c$ interacted with an indicator variable for entering high school after the start of the shock. County (α_c) and cohort (α_t) fixed effects 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.[19] First, I include a vector of pre-period measures of county demographic characteristics and employment shares across two-digit NAICS codes (X_c) interacted with the post-period indicator 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}).[20] Because

[18] 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 my analysis includes estimating effects on adjustment mechanisms that occur during high school (course-taking and diploma receipt).

[19] 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 and to account for the differences in student demographics across more- and less-exposed counties shown in Section 4.1.1.

[20] X_c consists of 1990 measures of median household income, population share without a college degree, the foreign-born population share, and employment shares across two-digit NAICS codes, 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, the county's exposure to changes in tariffs on imports into China and Chinese domestic production subsidies, and the county's exposure to

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 (i.e., the variation presented in Figure 1b). Π^{DD} (Event Study) represents the difference in outcomes between such students among cohorts entering high school after PNTR relative to this difference among earlier ninth grade cohorts.

$Exposure_c$ measures the "dosage" of a treatment that is inherently continuous. Thus, equation (1) compares changes in outcomes across cohorts of students from counties exposed to exogenously "big" and "small" labor demand shocks without a group of "pure" control counties that are completely unaffected by PNTR. Such comparisons cannot identify the causal effect of a labor demand shock relative to the shock's absence (Callaway et al., 2021). However, they can identify the causal effect of exposure to a large local labor demand shock relative to a small one. Identification requires the assumption that students from high- and low-exposure counties would deviate from trends the same on average for a given dose of exposure to the shock (Callaway et al., 2021).[21]

To assess pre-trends and dynamic responses to treatment across cohorts, I also estimate an event study specification:

$$y_{ict} = \sum_{\substack{\tau=1995 \\ \tau \neq 1999}}^{2008} \pi_\tau^{ES} \mathbf{1}\{t=\tau\} * Exposure_c + \alpha_c + \alpha_t + \Phi S_i + \sum_{\substack{\tau=1995 \\ \tau \neq 1999}}^{2008} \Gamma_\tau \mathbf{1}\{t=\tau\} \times X_c + \Theta Z_{ct} + \epsilon_{ict} \tag{2}$$

where I interact $Exposure_c$ (and control measures, X_c) with an indicator variable for belonging to a particular cohort τ. π_τ^{ES} (Event Study) represents the difference in outcomes

NAFTA (Choi et al., 2024). Both sets of covariates are adopted from Pierce and Schott (2020), with the addition of two-digit industry shares and the volume 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 all controls besides two-digit industry shares.

[21] Formally, the specification identifies an Average Causal Response parameter under the stated "strong" parallel trends assumption (Callaway et al., 2021). Interpretation of the identified parameter differs from that of an Average Treatment Effect by representing marginal effects of a change in treatment intensity, rather than the effects of treatment relative to its absence. The identifying assumption differs from the standard parallel trends assumption by requiring potential outcomes to trend in parallel on average for all given doses of treatment, rather than only under the absence of treatment.

between students from high- vs. low-exposure counties among cohort τ relative to this difference among the 1999 ninth grade cohort.

Greenland et al. (2019) and Burga and Turner (2022) find evidence of the China shock increasing outmigration from exposed local labor markets, particularly among young adults and the less-educated. Such out-migration among lower-ability students would yield selection into treatment in a naive framework based on contemporaneous residence, biasing estimates of the effects of exposure to the labor demand shock on educational attainment and later-life earnings upward. Therefore, I assign treatment status and county-level covariates to students based on their county of residence *prior* to the onset of the shock in an Intent-to-Treat (ITT) framework. This approach is made possible by my ability to link students across years and datasets in the individual-level UHERC data. Because I cannot observe students who 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 18 through 35.[22]

4.1.1 Empirical Support for Required Assumptions

Although the required assumption for identification – that a given shock size would cause the same average deviations from trends for students from high-exposure and low-exposure counties – is fundamentally untestable, I provide evidence that testable implications hold true. Table 1 shows descriptive statistics of the characteristics and outcomes of students who entered high school prior to PNTR. Column (5) presents estimates of the correlation between the standardized measure of exposure to the labor demand shock, $Exposure_c$, and the student-level measures. Although balance on *levels* of outcomes is not required for a difference-in-differences design, differences in pre-treatment student characteristics and

[22]Figure A1 presents separate estimates of equation (1) with indicator variables equal to one if an individual is not observed in the data again from that age through age 35 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 who 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.

outcomes across counties with varying realized treatment dosages would suggest that the same dose of the shock itself may systematically affect both groups of students differently. Column (5) shows that counties with larger shares of workers in subsectors of manufacturing that were more affected by PNTR had fewer students who were eligible for free-or-reduced-price lunch (FRPL), more non-Hispanic white students, and fewer Hispanic students prior to 2000. I control for student demographics in my main estimations to minimize concern over heterogeneous potential outcomes along these dimensions. Notably, $Exposure_c$ is not correlated with educational attainment measures among pre-period cohorts, supporting that students from high- and low-exoosure counties would respond similarly to the same labor demand shock.

Identification of equation 1 would more explicity be threatened by differential *trends* in student characteristics, such that changes in outcomes after PNTR could merely reflect compositional shifts.[23] To test for such selection into treatment, I predict later-life earnings for all cohorts based on the relationship between adult earnings and pre-measured student FRPL eligbility, race, ethnicity, gender, and English-Language Learner status for pre-shock cohorts and estimate the "effects" of local shocks on the predicted earnings index. Evidence of significant relationships between treatment and trends in this predicted earnings index would warrant concern that changes in trends of outcomes for students from high- and low-exposure counties could reflect demographic shifts, rather than true effects of the shock on human capital decisions. An event study of the relationship between shock exposure and earnings predicted on observables in Figure A2 show that this relationship remains stable before and after PNTR, supporting the validity of the design.

Finally, I present event study estimates in Section 5 showing that outcomes of students from high- and low-exposure counties trended in parallel prior to PNTR. Differential pre-trends would suggest the presence of confounders even before the onset of the China shock. In Section 7, I test the robustness of my main results to allowing for the continuation of any existing pre-trends into the post-period, following Rambachan and Roth (2023).

[23]My intent-to-treach approach that assigns students to counties based on where they attended school prior to the onset of the shock should prevent within-state migratory responses to the shock from causing selection into treatment but would still be threatened by selective migration out of Texas that results in attrition from the sample.

4.2 Effects of PNTR on Local Labor Markets

Using county-level data from the County Business Patterns Database (Eckert et al., 2020), unemployment rates from the BLS, and individual-level earnings records from the Texas Workforce Commission and UHERC, I characterize the local labor demand shock caused by PNTR in Texas before examining student responses. This serves dual purposes of assessing the validity and relevance of the research design and guiding interpretation of the channels through which the shock may have affected student behavior.

Figure 2 presents event study estimates of the relationship between $Exposure_c$ and overall and manufacturing-specific employment counts relative to population over time. Both employment measures trended similarly across high-exposure and low-exposure counties prior to PNTR, suggesting that they would have continued to do so in the absence of the policy. After the policy change, both measures fell sharply in more-exposed relative to less-exposed counties. Difference-in-difference estimates in Columns (1) and (2) of Table 2 indicate that a standard deviation greater exposure to the shock is significantly correlated with a 3.6 percentage point reduction in overall employment relative to population, nearly half of which came from declines in manufacturing employment. These magnitudes are similar to previous estimates of the effects of the China shock on local labor markets using nationwide samples and to the effects of a recession on more- exposed local labor markets.[24]

I next provide evidence that recent labor market entrants were subject to similar economic harm. Using Texas Workforce Commission earnings records and a sample of high school dropouts and graduates and two- and four-year college graduates that entered the labor market prior to the onset of the shock, I estimate individual fixed effects specifications comparing changes in earnings after vs. before the onset of shock across individuals from

[24] Autor et al. (2021) finds a 1.9 percentage-point decline in employment relative to population in exposed local labor markets. Hershbein and Stuart (2024) show that each recession since the 1970s corresponded to approximately a 3 percentage-point employment decline in counties with above-median exposure.

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

high-exposure vs low-exposure counties.[25] Estimates in Table 3 indicate that young adults from counties with a standard deviation greater exposure to the China shock were employed for 3 percentage points fewer quarters and earned nearly $1,670 less annually over the next 10 + years after the onset of the shock. Event study estimates in Figure A3 highlight the persistence of the shock's negative effects on recent labor market entrants, which is consistent with findings of the persistence of the China shock's effects on local labor markets (Autor et al., 2021) and with the literature on the "scarring" effects of entering the labor market during economic downturns (e.g., Kahn, 2010). The estimated effects of the shock on individuals that had recently made key educational decisions also serve as useful benchmarks of the counterfactual economic harm the shock may cause on individuals coming of age if they did not adjust their human capital decisions.

The above declines in local labor demand may have entered into educational decisions by decreasing the opportunity cost of schooling. Column (4) of Table 3 shows that the labor demand shock dramatically reduced earnings for individuals without a college education, a proxy for the opportunity cost of attending college. Notably, estimates in Column (5) suggest that the shock did not significantly affect earnings of college-educated young adults. The resulting relative increase in the local college earnings premia would have further encouraged individuals coming of age in areas more exposed to the shock to pursue a college education.

On the other hand, local economic shocks also may affect human capital accumulation by decreasing family income and school funding (Stuart, 2022; Burga and Turner, 2022). If property values fell in exposed counties, as shown with a nationwide sample by Feler and Senses (2017), accompanying reductions in school spending could negatively affect short- and long-run student outcomes.[26] 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 2 show that despite district revenues from local property taxes falling by $569 per-pupil (13%) in more-exposed counties, school spending was unaffected

[25]The specification regresses annual earnings on $Exposure_c * \mathbf{1}\{t \geq 2000\}$ and controls for individual and degree-by-year-by-cohort fixed effects, along with the same individual and county-level controls from equation 1.

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

compared to that of less-exposed counties. 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, a dollar change in local revenues resulted in a 67 cent change in school spending.

All together, these results support that, in Texas, the China shock increased the incentives for marginal students to acquire more education and did not reduce public investments that support such attainment (i.e., K-12 funding). However, as explored in Section 6, adjustment frictions may have hindered some students from responding to increases in the returns to education.

5 Average Adjustments to the Shock

The first-stage results discussed in section 4.2 suggest that the China shock incentivized marginal students to pursue additional education by reducing the opportunity cost of schooling and increasing the returns to higher levels of education. I present event study and difference-in-differences estimates of the effects of exposure to the labor demand shock on measures of educational attainment in Figure 3 and Table 4.

Despite evidence of reductions in opportunity costs, I find no evidence that larger local shocks caused economically or statistically significant effects on high school graduation.[27] However, students exposed to a standard deviation larger shock were 3.5 percentage points more likely to enroll in a two-year community or technical college (11%, relative to the pre-period mean) after graduating high school and 2.2 percentage-points (5%) more likely to

[27]The null estimate adds to a mixed literature on the effects of the China shock on high school graduation. Greenland and Lopresti (2016) find that greater local exposure to the China shock caused a 3.6 percentage-point increase in the number of high school diplomas awarded in a commuting zone relative to the count of 17 year old residents. Burga and Turner (2022) find increases in sustained high school enrollment, but much smaller (0.9 percentage points) effects on diploma counts relative to population counts and evidence of negative effects on the share of young adults residing in a commuting zone with a high school degree. Burga and Turner propose weak instrument bias and outmigration as reasons for the discrepancy between their results and that of Greenland and Lopresti (2016). Although limited to the setting of Texas, my administrative data allows me to assign treatment to students based on where they attended school prior to the onset of the shock, minimizing concerns over selective migration. My null result supports the evidence of limited graduation effects found in Burga and Turner (2022) and suggests that the China shock had little, if any, effect on the likelihood that the average student graduated from high school.

enroll in a two- or four-year postsecondary institution.[28] These effect sizes are comprable to existing estimates of the effects of placement in smaller elementary school classrooms (Chetty et al., 2011) or a $770 decline in two-year college tuition (Denning, 2017).[29] The shock also caused a significant 0.8 percentage-point (18%) increase in associate's degree receipt. Imprecise point estimates of a 0.3 percentage-point (18%) decline in shorter-length certificate receipt and of a 0.5 percentage-point (4%) increase in bachelor's degree receipt suggest a general shift toward pursuing credentials that reflect larger investments in human capital.

Students may have also adjusted their human capital investments along *intensive* margins in both high school and college in response to the labor demand shock (Acton, 2021; Di Giacomo and Lerch, 2023; Weinstein, 2022).[30] Vocational courses can give high school 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).[31] Similarly, community college certificates and degrees often directly prepare students for specific occupations and industries. Although the China shock manifested in a broader decline in labor demand for less-skilled workers in exposed areas, the shock's origin in manufacturing may have caused students to shift their fields of study away from those aligned with employment in directly affected sectors. Table 5 shows that students exposed

[28]Because I define enrollment outcomes based on attending a college at any point within two years of expected high school graduation, my measure for enrollment in a two- or four-year college is the *union* of the respective measures for community college enrollment and four-year enrollment, rather than the sum of the two.

[29]Chetty et al. (2011) find that assignment to small classes (averaging 15 students, rather than 22) for grades K-3 in the Tennessee STAR experiment caused a 1.8 percentage-point increase in college enrollment. Denning (2017) examines the effects of expanding access to reduced tuition at local community colleges in Texas finds that a $1,127 (2020$) reduction in tuition caused a 5.1 percentage-point increase in enrollment by high school completers. Assuming linearity, my estimated effect on community college enrollment of 3.5 percentage-points scaled by his coefficient corresponds to a $774 increase in aid.

[30]Acton (2021) finds that high school graduates responded in the short-term to local plant closures by shifting community college program choices from those related to jobs lost in the closure and toward programs occupations that required similar skill sets. Di Giacomo and Lerch (2023) study human capital adjustments to the introduction of industrial robots and show that colleges in robot-exposed local labor markets produced relatively more degrees in computer science and engineering, business and economics, and social science – all fields that were either complementary to or relatively unaffected by local automation. Weinstein (2022) shows evidence that localities more exposed to the dot-com crash, fracking boom, 2008 financial crisis, and the Delaware headquarters boom exhibited larger changes in completions of sector-relevant majors.

[31]Brunner et al. (2023) provide evidence that large earnings returns to participation in career and technical education programs may also reflect development of general, rather than purely specialized, skills.

to larger local shocks shifted from technical to academic programs at two-year colleges, consistent with a decline in opportunities for blue-collar work.

I also explore whether students made more precise adjustments in field choices in high school and at two-year colleges. I use pre-period employment records of high school and community college completers to define the "relevance" of high school courses (by two-digit TEA subject area) and community college majors (by two-digit CIP code) to employment in industries more affected by the shock. Table A1 presents estimates of the shock on high school course completions and two-year college major choice by quartile of field relevance to the shock. I find no evidence at either education level of shifts in precise fields away from those that were previously studied by individuals who found employment in industries exposed to the policy change. However, estimated effects on major *offerings* at two-year colleges in Table A2 suggest that schools in areas exposed to larger shocks *increased* their offerings of programs that historically produced graduates that worked in manufacturing, while decreasing offerings of less-exposed programs. This countervailing shift in offerings likely reflects increased demand for manufacturing-based upskilling from directly displaced workers (Foote and Grosz, 2020) and may have limited the ability of students to switch to less-exposed majors.[32]

The above evidence of relatively modest effects of the China shock on educational decisions and attainment by the average student are consistent with prior work (Burga and Turner, 2022; Ferriere et al., 2018), but whether the estimates represent adjustments large enough to substantially buffer against the shock's negative effects on labor demand for low-skilled workers is unclear. Using linkages to wage records from the Texas Workforce Commission, I estimate the effects of exposure to the shock during youth on adult earnings. Figure 4a presents estimates from individual difference-in-difference estimates on earnings at each age from 25 through 35.[33] All coefficients are positive and statistically insignificant, ranging from \$30 to \$566. Notably, the coefficients reflect comparisons of cohorts that made key

[32]Foote and Grosz (2020) examine the effects of mass layoffs on enrollment and program choice at local community colleges. They find positive effects on enrollment that is largest in manufacturing-based programs.

Available seats in courses and majors are preferable measures of major "supply" than the number of programs offered but are not observed.

[33]Age 35 is the oldest age at which I can observe the earnings of at least three cohorts that entered high school after the onset of the China shock.

educational decisions after PNTR ("adjusting cohorts") to those that made them before it ("non-adjusting cohorts"). Therefore, the null results imply that the average individual coming of age in a county that was exposed to a larger shock fared nearly as poorly as their older peers who were directly harmed in the labor market by the shock. A back-of-the-envelope calculation presented in Table 8 indicates that the point estimates only represent earnings gains large enough to offset 21% of the sustained decline in earnings the shock caused for recent labor market entrants, as estimated in Section 4.2.[34] In addition to the null effect on earnings, estimates in Table 6 suggests that the average human capital adjustments also did not lead to gains in employment.

The lack of evidence for relative earnings gains could reflect that students did not make sufficiently large adjustments on average or that sustained depressions in local labor demand "scarred" even students who obtained meaningful human capital (Kahn, 2010). I provide evidence for the former by estimating effects on imputed earnings that are assigned based on a student's educational attainment and industry of employment and the mean earnings for education-by-industry cells at individual ages from the earliest pre-period cohort. Estimates in Table 6 show that a standard deviation greater exposure to the shock caused only a $392 increase in earnings predicted on educational attainment and industry. This suggests that the modest educational adjustments by the average student exposed to the shock were not large enough to provide substantial protection against the shock's lasting negative effects on local labor demand and motivates an examination of the mechanisms that explain the muted average response in the following section.

In addition to adjusting their human capital investments – and in many cases, complementing such adjustments, individuals exposed to local shocks during youth may have moved across geographies, industries, or occupations as adults as manners of adjustment. I provide evidence that PNTR did not cause students to later migrate to less-exposed counties and caused only small changes in their industries of adult employment in Appendix A.1.[35]

[34]The 95% confidence interval of estimated earnings gains rules out relative earnings gains large enough to offset more than 69% of the sustained decline in earnings the shock caused for recent labor market entrants.

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

6 Mechanisms & Heterogeneity: Who Escaped from Structural Change?

Estimated average effects of the China shock on educational decisions of youths reveal that enrollment in two-year colleges was a primary channel of adjustment to the shock; however, relatively few students made this or other adjustments. Motivated by the conceptual framework in Section 2.2, I explore the role of financial and information frictions in hindering adjustments to educational decisions. In addition, I examine whether the emergence of other labor market opportunities that required little education reduced incentives to invest in human capital in some areas affected by the China shock. I estimate heterogeneous effects of the shock on educational attainment and adult earnings along each dimension of interest. I then compare discounted earnings gains implied by the estimates to the persistent earnings losses experienced by young adults in the labor market that were directly affected by the shock to approximate the degree of protection from the shock that human capital adjustments provided.

6.1 Credit Constraints

Experimental (Belzil et al., 2021) and empirical (Brown et al., 2011; Lovenheim, 2011) evidence – including prior studies of students in Texas (Denning, 2017; Black et al., 2023) supports that borrowing constraints prevent some students from pursuing a costly college education. Likewise, students who were exposed to the China shock but bound by such constraints would not have been able to respond to increased incentives to enroll in college. I test for the presence of borrowing constraints by leveraging geographic variation in the price of college created by community college "tax districts" in Texas: because community colleges in Texas are funded in-part by local property taxes, students living in tax-paying municipalities pay "in-district" tuition that is less than half of what out-of-district students pay, on average (Denning, 2017).[36]

[36]Figure 5 presents a map of community college tax districts in Texas, as of 2023. During the 2014-2015 school-year, schools charged out-of-district students 63% (approximately $1,000, for two 12-credit-hour semester) higher tuition than they charged in-district students (Denning, 2017).

Formally, I estimate heterogeneous effects of exposure to the labor demand shock on the outcomes of students with access to discounted tuition at local community colleges by interacting equation 1 with an indicator variable for attending K-12 in a community college tax district. If the direct costs of college substantially hindered some students from adjusting their human capital investments in response to the shock, then the estimated differential effect on educational attainment should be larger for students from tax districts.[37]

I present estimates of the heterogeneous effects of the labor demand shock by tax district residence in Panel A. of Table 7. Estimates in the first row represent the effects of greater exposure to the labor demand shock on students from outside of community college tax districts. The null results on community college enrollment and degree receipt support the notion of binding constraints from the direct costs of college preventing adjustments to the shock. Additionally, that greater exposure to the shock caused significant *declines* in four-year enrollment among students outside of tax districts suggests that the negative labor demand shock may have tightened borrowing constraints by reducing family income, a dynamic found to occur during recessions (Stuart, 2022; Anstreicher, 2025).

Estimates of the interaction coefficient in the second row indicate that unlike plausibly constrained students, those with access to cheap local college obtained substantially more human capital in response to the shock. Greater exposure to the shock caused significant 7.0 percentage-point and 1.6 percentage-point differential increases in community college enrollment and two-year degree receipt, respectively, among students residing in tax districts. Significant 5.7 percentage-point and 3.2 percentage-point relative increases in four-year enrollment and bachelor's degree receipt also suggest that initial enrollment in low-cost two-year colleges served as a pathway to four-year enrollment and degrees for many students. This is consistent with Denning (2017), who provides evidence that that expansion of tax districts increased both initial two-year enrollment and eventual transfers to four-year universities.

Compared to the estimated human capital adjustments by the average student across the full sample, adjustments by those with access to cheap local colleges are larger both in

[37]In addition to being eligible for discounted tuition, students residing in tax districts are also physically closer, on average, to local community colleges than those residing outside districts. Physical distance in of itself may serve as a barrier to college enrollment (Acton et al., 2024), but researchers have also considered distance as a "direct" cost of college through a pecuniary lense in the context of credit constraints (Cameron and Taber, 2004).

magnitude – i.e., the estimated total effects on community college enrollment and two-year degree receipt are twice as large – and breadth – i.e., the shock also caused students in tax districts to pursue four-year degrees. Evidence that these students experienced improved relative earnings in adulthood would support that the adjustments provided substantial protection against the shock, while the absence of differential earnings benefits would suggest that youths growing up in areas harmed by structural change were "scarred," regardless. I present strong evidence of the former scenario in Columns (6) and (7) of Table 7. Students from outside tax districts – who made no significant upward adjustments on their human capital investments – did not experience any earnings gains relative to recent labor market entrants that were directly harmed by the shock. On the other hand, students with access to low-cost local community colleges exhibited significant relative annual earnings gains of over $3,000 in adulthood. The estimated effect on realized earnings matches nearly dollar-for-dollar with effects on imputed earnings based on educational attainment and industry of employment, providing further support that the earnings gains reflect benefits from human capital adjustments.

To assess how much protection human capital adjustments provided relative to those directly affected by the shock, I calculate a back-of-the-envelope net present value (NPV) of the human capital investments and compare it to the sustained $1,668 annual earnings losses the shock caused for individuals that recently entered the labor market prior to the shock. Figure 6 presents estimates of the differential effects of exposure to the shock during youth on earnings at each age from 25 through 35. The discounted cumulative total effects on earnings across these ages for individuals from tax districts are equal to 115% of the discounted stream of earnings losses over the same number of years that the shock caused for individuals that had already entered the labor force.[38] This comparison suggests that the sizable human capital adjustments made by individuals with access to cheap local provided *full protection* against the shock's negative labor market effects.

[38]Table 8 presents the results of this exercise. Using a discount rate of 3% and only compare earnings gains and losses from age 25 to 35 yields an NPV of the earnings gains equal to $18,220 compared to the estimated earnings losses of $15,896. Under a further assumption that exposed individuals from tax districts would continue to experience the same earnings gain that they exhibited at age 35 until retirement at age 65 and that directly exposed recent labor market entrants would continue to experience a $1,668 annual loss until retirement, the discounted lifetime earnings gains of $63,931 far exceed the discounted lifetime earnings losses of $40,223.

6.2 Information Frictions

In order to adjust to the labor demand shock, students needed to internalize changes in the returns to education. Thus, information frictions may have hindered the average student's adjustments to the labor demand shock.[39] I proxy for the salience of the shock to a student's information set by measuring the share of recent dropouts and graduates from their high school that worked in manufacturing before the shock. If the receipt of a stronger signal of the change in labor market opportunies made students more likely to update their beliefs over the returns to different education levels, then the estimates should show larger adjustments by students with more older peers that were directly affected by the shock.

Panel B of Table 7 presents heterogeneous effects of the labor demand shock by high school shock salience.[40] Estimates in the first row indicate that exposure to larger local shocks did not significantly affect educational attainment of students from high schools with no alumni working in manufacturing before the onset of the shock. Consequently, these students experienced no significant earnings gains in adulthood relative to students from similar high schools in counties that were less exposed to the labor demand shock.

Estimates in the second row of Panel B represent differential effects of greater exposure to the shock for students from high schools with a 10 percentage-point larger share of previous dropouts and graduates that worked in manufacturing prior to the shock.[41] The results support that student's responsiveness increased with shock salience: greater exposure to the shock caused a differential 2.7 (0.8) percentage-point increase in two-year (four-year) college enrollment and a 1.0 (1.4) percentage-point increase in two-year (four-year) degree receipt for students from schools with more alumni in manufacturing. These adjustments corresponded to a significant $1,265 increase in imputed earnings and a dollar-for-dollar increase in realized earnings at age 35. However, a comparison of discounted earnings benefits

[39] A robust literature supports that young adults possess incomplete or biased information on the labor market prospects associated with different education levels and fields (Betts, 1996; Oreopoulos and Dunn, 2013; Wiswall and Zafar, 2014; Hurwitz and Smith, 2018; Baker et al., 2018; Arcidiacono et al., 2025)

[40] Because I can only observe a student's high school when they reach 9th grade, the estimating specification differs slightly from my primary "intent-to-treat" approach that assigns treatment based on where I observe students attending school prior to the shock. $HSMan_i$ is instead measured as of 9th grade.

[41] The average student in the sample attended a high school where 11% of graduates and dropouts from 1994-1999 worked in manufacturing at the time of the shock. A 10 percentage-point increase in the share of such students is approximately equal to moving from the 10th to 75th percentile or to two standard deviations.

and direct "scarring" effects of the shock on young adults in Table 8 suggests that students who received salient signals of the shock still only made adjustments large enough to offset 56% of the shock's direct harm on earnings. Incomplete adjustments even by students with more salient signals of changes in the returns to education also further supports that some students were constrained from enrolling in college by the direct pecuniary costs.

6.3 Alternate Labor Market Opportunities

Coincident increases in labor market opportunities for less-educated workers could at least partially offset increases in the local returns to education caused by the China shock. Technological improvements in hydrolic fracturing and horizontal drilling resulted in a sharp increase in labor demand in local labor markets overlaying shale plays around 2005, known as the "fracking boom" (Feyrer et al., 2017; Winters et al., 2019). Kovalenko (2023) shows that students in fracking areas in Texas responded to the increase in opportunity costs by exiting high school early to enter the labor market. I test for differential responses to the China shock in the presence of new labor market opportunities requiring little education by interacting equation 1 with an indictor for the presence of shale underneath a student's county. Because I explicitly control for Kovalenko's (2023) measure of local exposure to the fracking boom in my main estimations, this exercise is not testing for an explanation for relatively modest student responses. Rather, this exercise is an implicit test of the conceptual framework in which students weigh the expected benefits and costs of a college education and a test of whether mitigating changes to labor demand provide buffers to structural change.

Panel C of Table 7 shows that greater exposure to the shock caused significant differential *declines* in four-year college enrollment by students from shale counties and no statistically significant increases in other measures of educational attainment. The results in Columns (7) and (8) are consistent with fracking opportunities negating the need for educational adjustments: students from shale counties that were more exposed to the shock experienced significant relative improvements in realized earnings that could not be explained by their educational attainment. Examining the dynamic effects on earnings across ages in Figure 8 and Table 8 suggests that annual earnings gains for exposed students from shale counties matched the size of estimated direct effects of the shock by age 32, but that in total such

students were only protected from 54% of earnings losses caused by the shock from ages 25 and 35. Still, these results are consistent with the development of new local opportunities for less-educated workers providing a buffer against the China shock.

7 Robustness

I test the robustness of my main results to alternative specifications, focusing on my estimates of the differential effects of the shock on imputed and realized earnings for individuals with access to low-cost local colleges. My primary specification controls for student demographics, 1990 county employment shares by 2-digit NAICS industry interacted with cohort dummies, 1990 county demographics interacted with cohort dummies, shale and natural gas presence interacted with cohort dummies, and exposure to other changes to U.S. trade policy.[42] I first test the robustness of my main results to excluding these covariates and to sequentially adding back each set of control variables and present the results in Table A3. Column (5) corresponds to the preferred specification utilized throughout the paper.

The results in columns (1) through (5) show that my main findings are robust to excluding any control group, with the exception of the baseline industry shares. The null estimates in Column (1) should not be surprising. Without conditioning on baseline industry shares, the estimate is largely identified off comparisons of students from counties with larger or smaller *overall* employment shares in manufacturing. Structural differences in the economic profiles of these two groups of counties could subject such comparisons to confounders from numerous other economic shocks, and Figure A4 shows that the first-stage relationship between shock exposure and local employment does not exist without conditioning on baseline industrial composition. In this sense, the null results in Column (1) can almost be interpreted as a falsification exercise. Across the other four specifications, estimates of the differential increase in imputed earnings reflecting educational attainment and industry of employment range from \$2,357 to \$3,318 and are all statistically significant ($p < 0.01$). Estimates of the effects on realized earnings similarly range from \$3,091 to \$4,689 and are also all statistically

[42] These covariates all follow Pierce and Schott (2020), with the exception of controlling for baseline industry shares and the fracking boom. Both of these additions serve to account for Texas-specific confounders related to the energy industry.

significant ($p < 0.01$).

The China shock was not the only labor demand shock to occur during my sample period, and correlations in the incidence of shocks across counties could confound my estimations and violate the required assumption that differences in outcomes across counties that were more or less exposed to the China shock would trend in parallel if both groups experienced the same treatment dosage. In Columns (6) through (8) of Table A3, I sequentially add controls for county-level exposure to the the 2000 dot-com bubble crash, the 2000s housing boom and bust, and the 2007-2008 financial crisis, – three labor demand shocks which prior research has shown affected educational attainment (Charles et al., 2018; Weinstein, 2022).[43] Across the three additional specifications, the estimated increase in imputed earnings ranges from \$2,612 to \$2,863, and the estimated increase in earnings ranges from \$2,513 to \$2,996. All six estimates are statistically significant ($p < 0.05$).

My research design requires assuming that potential outcomes would trend in parallel across more- and less-exposed counties. I test for robustness of my main results to allowing for deviations from parallel trends in the post-period, following Rambachan and Roth (2023). The estimated differential effects of shock exposure on college enrollment and adult earnings for students eligible for reduced two-year college tuition are both robust to controlling for continuations of linear pre-trends. The estimated effect on college enrollment is also robust to allowing additional "smooth" deviations equal to 5% of the estimated treatment effect each period after treatment, and the estimated effect on earnings is robust to allowing such deviations equal to 15% of the estimated treatment effect.

I find evidence that students exposed to the China shock with access to cheap local community colleges were also more likely to later enroll in public four-year 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

[43] 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 the 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 measures with cohort dummies to allow them to flexibly affect outcomes across cohorts.

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.[44] I also note that if local shocks caused substitution from enrollment in more-expensive colleges to less-expensive colleges, we would expect such an enrollment shift to manifest across the cost distribution *within* Texas, too. I group Texas public universities by their stated in-state tuition and fees using data from the NCES Integrated Postsecondary Education Data System and estimate the effects of exposure to local shocks on enrollment across gross tuition quartiles. Table A5 presents results from both of these exercises. Contrary to a story of substitution, estimated effects on college enrollment are positive (although noisy) for private schools and for all but the third tuition quartile.

Finally, I provide evidence that no single county is driving my results. Figure A5 presents histograms of the estimated effects of exposure to the labor demand shock on imputed and realized adult earnings when iteratively dropping each county from the sample. All estimated effects on both measures exceed $1,500.

8 Conclusion

This paper examines whether youths can shield themselves from structural labor market shocks by adjusting their human capital investments. Exploiting quasi-random variation in local labor demand created by the establishment of Permanent Normal Trade Relations with China, I show that the average student in Texas made only modest educational adjustments in response to the China shock and subsequently did not experience improved labor market outcomes. However, I find substantial heterogeneity underlying this muted average response that aligns with a simple conceptual framework of human capital decisions. Students who were plausibly unconstrained by college costs or received more salient information about the declining returns to low-skilled work made larger adjustments – enrolling in and completing

[44] 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.

both two-year and four-year programs at significantly higher rates – and were protected from the shock's adverse effects on earnings. In contrast, youths facing tighter credit constraints or weaker information signals failed to adjust and experienced earnings streams comparable to those of exposed workers.

Existing research on structural declines in labor demand – including the China shock – paints a gloomy picture for exposed local labor markets, which still exhibit elevated nonemployment and reduced income more than a decade after the onset of local shocks (Autor et al., 2021, 2025; Choi, 2024). Such labor demand shocks cause lasting declines in the earnings of affected prime-aged workers, who have already made critical educational decisions (Pierce et al., 2022; Autor et al., 2014). My findings provide evidence suggesting that when individuals coming of age are able to make considerable adjustments to their human capital investments, they can avoid suffering the same fate.

Critically, my findings underscore the importance of barriers to educational attainment in hindering effective adaptation to structural change. Financial aid programs and policies promoting access to postsecondary education can likely ease the distributional burden of structural change by ensuring that individuals coming of age are able to develop newly demanded skills. Moreover, information interventions that make shifts in labor market demand more salient could help students make forward-looking educational decisions. As continued automation and the rapid development of artificial intelligence threaten to dramatically reshape the labor market, policies that enhance the resilience not only of displaced workers, but also those on the cusp of entering the labor force will be critical to minimizing the costs of economic transformation.

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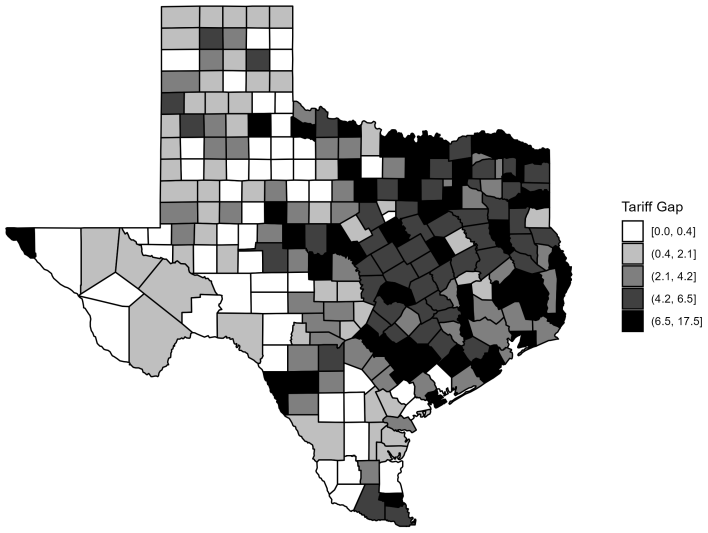
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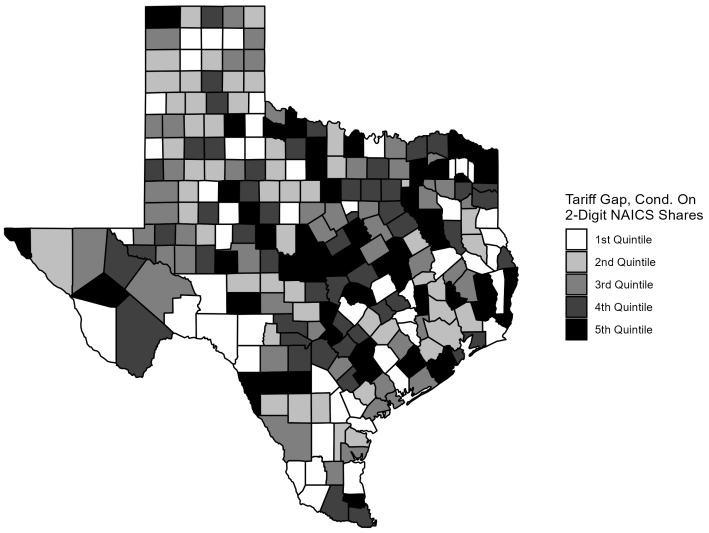
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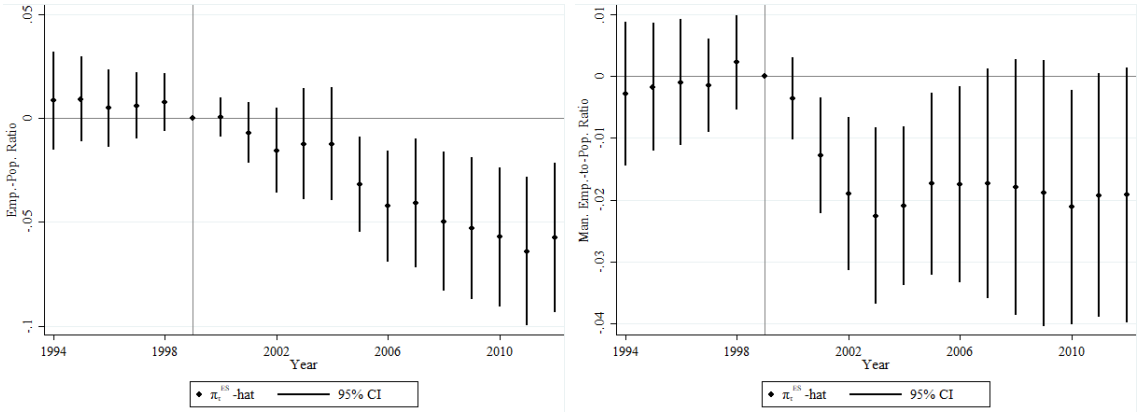
(a) Shock Exposure



(b) Shock Exposure, Conditional on 2-Digit NAICS Employment Shares

Figure 1: County-Level Exposure to the China Shock in Texas

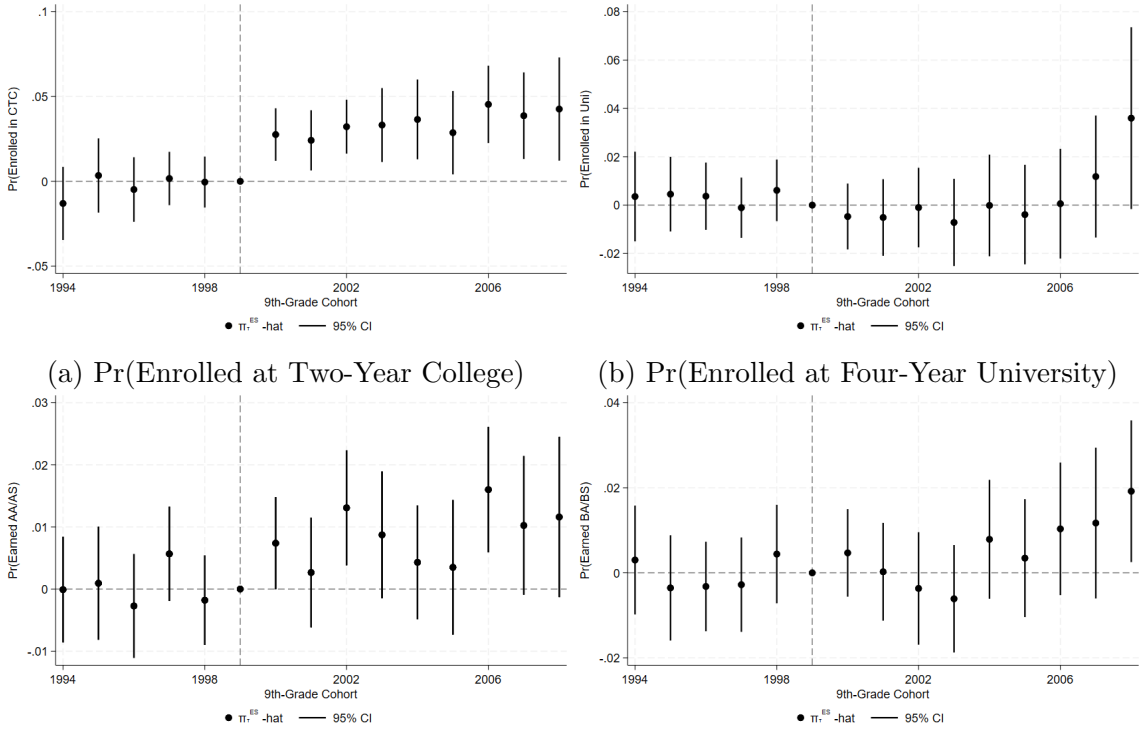
Notes: These maps show county-level variation in exposure to adverse local shocks caused by Chinese import competition in Texas, as measured by a county's employment-share weighted "tariff gap" across the industries (i.e., product specialties) present in the county. Each industry's tariff gap is defined 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. Panel (b) residualizes the exposure measure by a county's employment shares across two-digit NAICS codes, reflecting idiosyncratic variation in exposure based on the types of products that local manufacturers produced, rather than overall manufacturing presence.



(a) Employment-to-Population Ratio (b) Manufacturing Emp.-to-Pop. Ratio

Figure 2: 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. Estimates reflect coefficients from 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 this difference one year prior to the onset of PNTR. 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) Pr(Enrolled at Two-Year College)

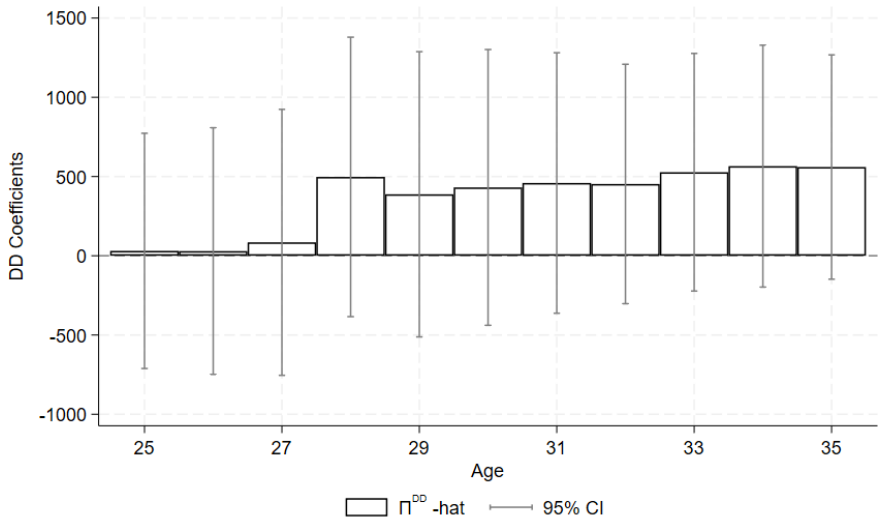
(b) Pr(Enrolled at Four-Year University)

(c) Pr(Earned Associate's Degree)

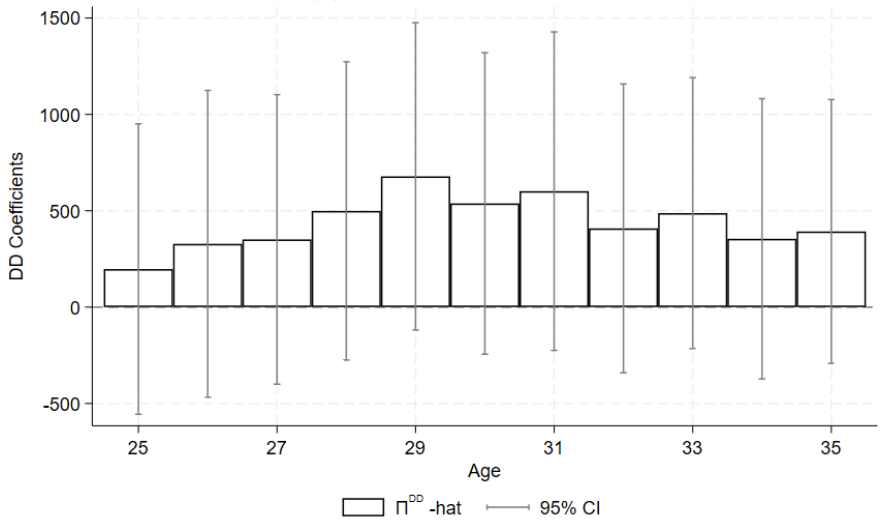
(d) Pr(Earned Bachelor Degree)

Figure 3: Effects of the Labor Demand Shock on College Enrollment & Degree Receipt

Notes: These figures present estimates of the effect of exposure to negative labor demand shocks during youth and adolescence on college enrollment and degree receipt. Estimates reflect coefficients from 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 this difference one year prior to PNTR. The outcomes are (a) an indicator for enrolling at a public two-year community or technical college (CTC) in Texas within two years of expected high school graduation, (b) enrolling at a public four-year university in Texas, (c) earnings an associate's degree by age 25, and (d) earning a bachelor's degree by age 25 . 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) Earnings, by Age



(b) Earnings Predicted from Education & Industry, by Age

Figure 4: Effects on Imputed and Realized Adult Earnings

Notes: This figure presents estimates of the effects of exposure to negative labor demand shocks during youth and adolescence on realized and imputed earnings from ages 25 - 35. Each box and whisker reflects the coefficient of interest from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000). 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 in Panel (a) is annual earnings from employment in occupations covered by unemployment insurance in Texas. Imputed earnings in Panel (b) are imputed as the mean earnings at that age by individuals who entered the labor market before the onset of the shock with the same educational attainment level and worked in the same industry . Standard errors are clustered by county.

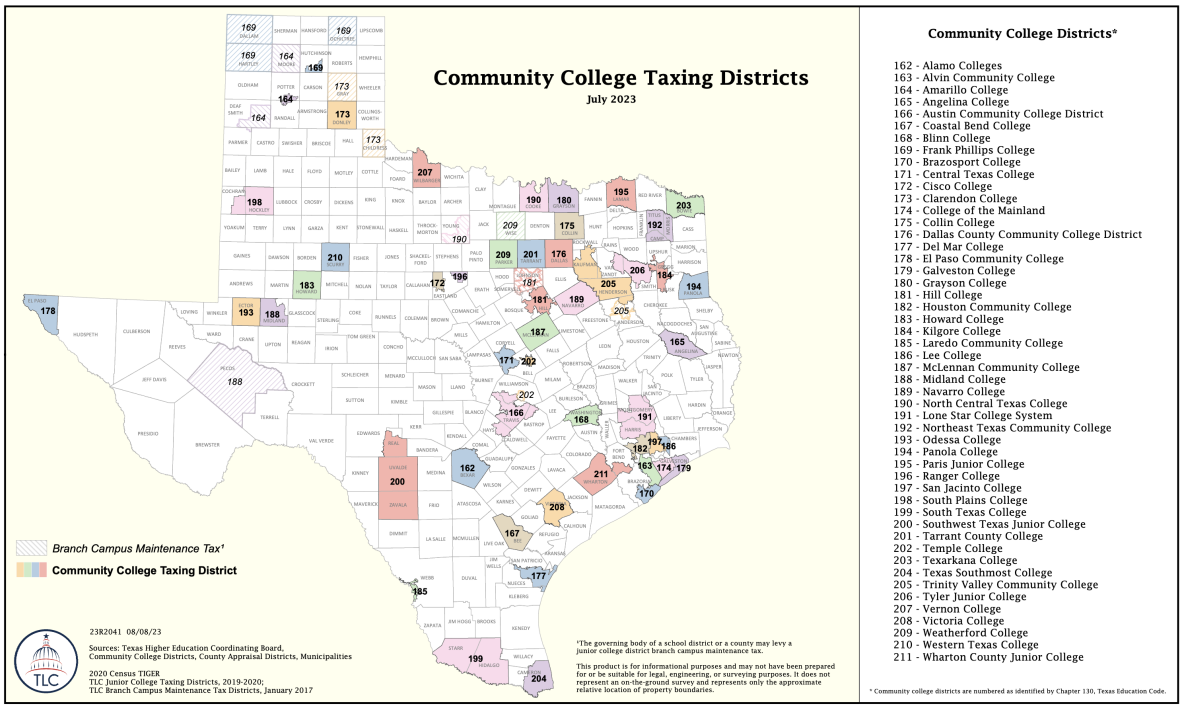
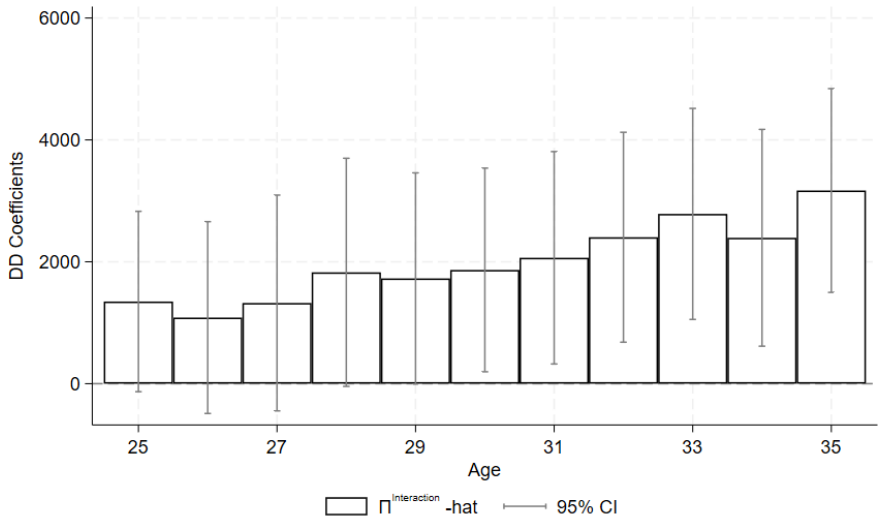
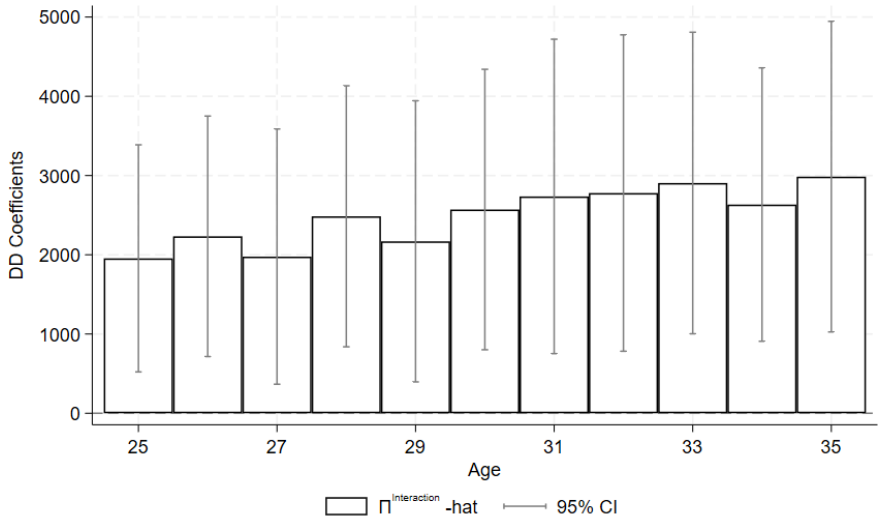


Figure 5: Community College Tax Districts in Texas

Notes: This figure presents a map of community college tax districts in Texas as of 2023. The figure was originally created by the Texas Legislative Commission and the Texas Higher Education Coordinating Board.



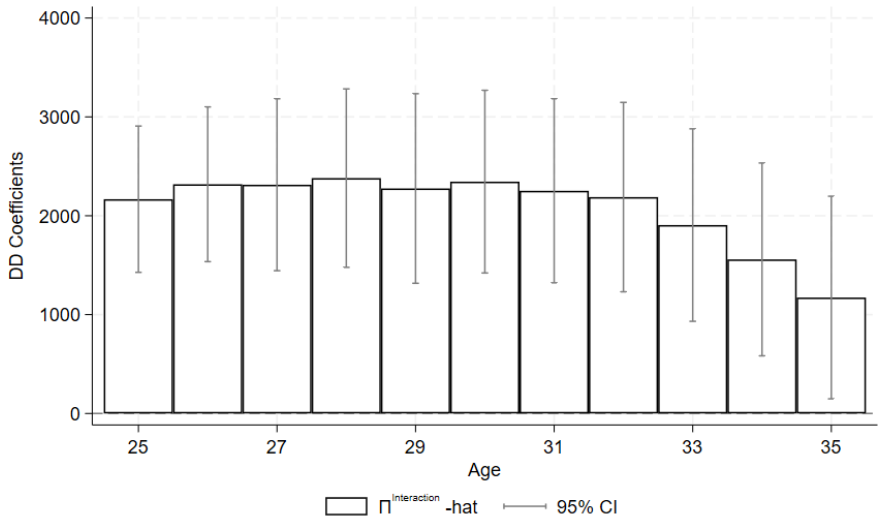
(a) Earnings, by Age



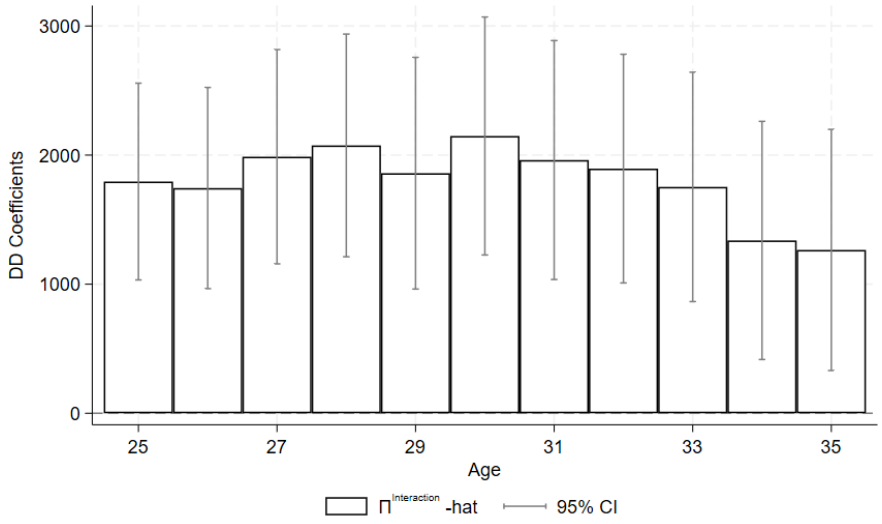
(b) Earnings Predicted from Education & Industry, by Age

Figure 6: Differential Effects on Imputed and Realized Adult Earnings for Students from Two-Year College Tax Districts

Notes: This figure presents estimates of the differential effect of exposure to negative labor demand shocks during youth and adolescence on realized and imputed earnings from ages 25 - 35 for individuals with access to reduced tuition at local community colleges. Each estimate reflects the interaction coefficient from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000), fully interacted with an indicator variable for a student residing in a public two-year college "tax district" and being eligible for reduced tuition. 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 in panel (a) is annual earnings from employment in occupations covered by unemployment insurance in Texas. Imputed earnings in panel (b) are imputed as the mean earnings at that age by individuals who entered the labor market before the onset of the shock with the same educational attainment level and worked in the same industry. Standard errors are clustered by county.



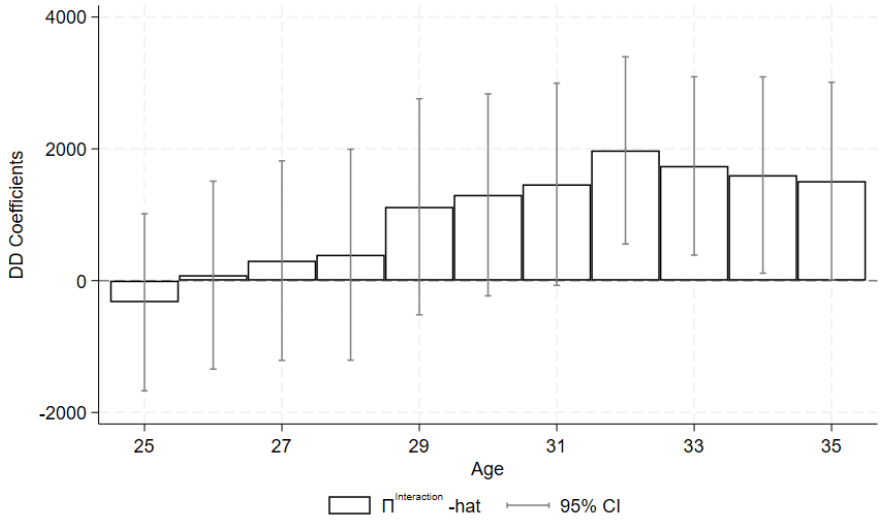
(a) Earnings, by Age



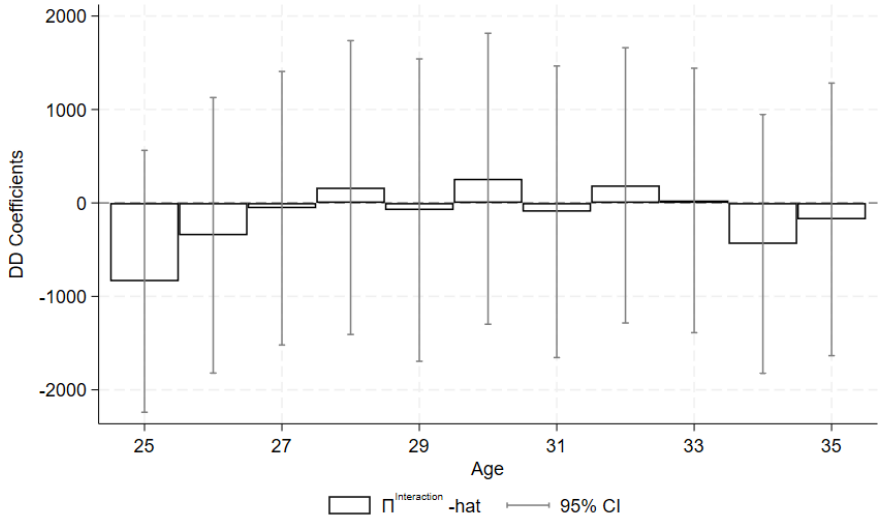
(b) Earnings Predicted from Education & Industry, by Age

Figure 7: Differential Effects on Imputed and Realized Earnings by Share of HS Alumni Working in Manufacturing

Notes: This figure presents estimates of the differential effect of exposure to negative labor demand shocks during youth and adolescence on realized and imputed earnings from ages 25 - 35 for individuals from high schools with 10 percentage-points more alumni working in manufacturing. Each estimate reflects the interaction coefficient from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000), fully interacted with the share of a high school's 1994 graduates and dropouts that worked in manufacturing before PNTR. 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 in panel (a) is annual earnings from employment in occupations covered by unemployment insurance in Texas. Imputed earnings in panel (b) are imputed as the mean earnings at that age by individuals who entered the labor market before the onset of the shock with the same educational attainment level and worked in the same industry. Standard errors are clustered by county.



(a) Earnings, by Age



(b) Earnings Predicted from Education & Industry, by Age

Figure 8: Differential Effects on Imputed and Realized Earnings of Students from Fracking Counties

Notes: This figure presents estimates of the differential effect of exposure to negative labor demand shocks during youth and adolescence on realized and imputed earnings from ages 25 - 35 for individuals from counties overlaying shale plays. Each estimate reflects the interaction coefficient from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000), fully interacted with an indicator variable for a student residing in a county overlaying shale reserves. 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 in panel (a) is annual earnings from employment in occupations covered by unemployment insurance in Texas. Imputed earnings in panel (b) are imputed as the mean earnings at that age by individuals who entered the labor market before the onset of the shock with the same educational attainment level and worked in the same industry. Standard errors are clustered by county.

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

	(1) Mean	(2) SD	(3) Min	(4) Max	(5) $Exposure_c$
FRPL	0.441	0.497	0	1	-0.074*
					(0.044)
White	0.485	0.500	0	1	0.165***
					(0.063)
Hispanic	0.035	0.478	0	1	-0.165**
					(0.082)
Black	0.137	0.344	0	1	0.005
					(0.029)
Graduated HS	0.701	0.458	0	1	0.012
					(0.010)
Enrolled at Postsecondary Institution	0.425	0.494	0	1	0.005
					(0.010)
Enrolled at CTC	0.327	0.469	0	1	0.012
					(0.013)
Enrolled at University	0.179	0.469	0	1	-0.006
					(0.010)
Certificate by 25	0.019	0.136	0	1	0.002
					(0.003)
Associate's by 25	0.041	0.199	0	1	0.007
					(0.004)
Bachelor's by 25	0.441	0.497	0	1	-0.000
					(0.008)
Share of Qtrs Employed in TX at 35	0.555	0.474	0	1	-0.003
					(0.013)
Employed All Qtrs in TX at 35	0.499	0.500	0	1	-0.003
					(0.012)
Earnings at 35	30,075	38,087	0	210,836	-494
					(1,263)

Standard errors in parentheses are clustered at the county level

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

Notes: This table presents descriptive statistics for students from the 1995 to 1999 cohorts of ninth graders attending public schools in Texas. 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 and earnings (2020$) are measured at age 35. Column (5) presents estimates of the correlation between $Exposure_c$ and the student-level outcome of interest.

Table 2: Effects of Import Competition on Local Labor Markets & School Resources in Texas

	(1) Emp. to Pop.	(2) Man. Emp.-to-Pop.	(3) Unemployment Rate	(4) Per-pupil Prop. Tax Revenue	(5) Per-pupil School Spending
$Exposure_{ct}$	-0.036***	-0.016**	1.507**	-569**	-31
	(0.012)	(0.007)	(0.696)	(270)	(156)
Percent Change	-5.7	-16.0	28.2	-12.6	-0.4
Pre-period Mean	0.638	0.103	5.336	4,536	8,529
N	4,275	4,275	4,275	4,275	4,275

Standard errors in parentheses are clustered at the county level

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

Notes: This table presents estimates of the effects of exposure to Chinese import competition on local economic conditions and school resources, using employment counts from the County Business Patterns Database, population counts by age group from the Survey of Epedemiology and End Results, unemployment rates from the Bureau of Labor Statistics, and school district finances from the National Center for Education Statistics. Estimates reflect coefficients from a continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative before PNTR (2000). The specification controls for county and year fixed effects, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean. The county-level outcome variables across columns are as follows: (1) employment-to-population, (2) manufacturing employment-to-population (3) unemployment ratio, (4) per-pupil district property tax revenue, and (5) per-pupil current expenditure.

Table 3: Direct Effects of the China Shock on Annual Earnings of Young Workers

	(1) Share of Qtrs Employed	(2) Earnings	(3) ln(Earnings)	(4) ln(Earnings), No College	(5) ln(Earnings), College
$Exposure_{ct}$	-0.030***	-1,668**	-0.288***	-0.350***	-0.038
	(0.009)	(701)	(0.086)	(0.116)	(0.107)
N	4,076,162	4,076,162	4,076,162	2,261,934	1,813,679

Standard errors in parentheses are clustered at the county level

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

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on employment earnings of recent labor market entrants. Estimates reflect coefficients from an individual fixed effects model that compares within-person changes in earnings of young workers from counties that were more vs. less exposed to the China shock. The specification controls for county and 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. The specification also includes additional degree-type-by-year fixed effects. Standard errors are clustered by county. Estimates in (1) through (3) reflects an estimating sample of high school dropouts, high school graduates, two-year college graduates, and four-year college graduates that entered the labor force between 1995 and 1999, while the estimate in (4) only reflects high school dropouts and graduates and the estimate in (5) only reflects two- and four-year college graduates. High school dropouts and graduates are assigned to the county of their high school, and college and university graduates are assigned to their reported county of residence. Annual earnings are winsorized at the 99th percentile. Natural logs are taken of earnings +1 to still include $0 earners.

Table 4: Effects of the Labor Demand Shock on College Enrollment and Degree Receipt

	(1) HS Grad	(2) Enrolled	(3) Enrolled at CTC	(4) Enrolled at Uni	(5) Certificate	(6) AA/AS	(7) BA/BS
$Exposure_{ct}$	0.003	0.022**	0.035***	-0.002	-0.003	0.008**	0.005
	(0.008)	(0.010)	(0.009)	(0.008)	(0.002)	(0.004)	(0.004)
Percent change	0.4	5.1	10.8	-1.1	-18.6	18.3	3.6
Pre-period mean	0.702	0.423	0.328	0.173	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 in parentheses are clustered at the county level

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

Notes: This table presents estimates of the effects of exposure to Chinese import competition on educational attainment attainment using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from a continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative before PNTR (2000). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean. The outcome in column (1) is an indictor for graduating high school. The outcome in college (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., for a student that enrolls at both a two-year and a four-year institution within two years of expected HS graduation, both indicators will populate as 1). The outcomes in columns (5) through (7) are indicator variables for receiving a certificate, associate's degree, and bachelor's degree by age 25, respectively.

Table 5: Effects of the Shock on Enrollment in Academic vs Technical Programs at CTCs

	(1) Enrolled, Academic	(2) Enrolled, Technical	(3) CTC Degree, Academic	(4) CTC Degree, Technical
$Exposure_{ct}$	0.057***	-0.024**	0.009***	-0.005**
	(0.011)	(0.011)	(0.003)	(0.002)
Percent change	24.7	-26.7	29.4	-28.6
Pre-period mean	0.231	0.090	0.031	0.018
N	3,678,707	3,678,707	3,678,707	3,678,707

Standard errors clustered by county in parentheses

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

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on enrollment and degree receipt in academic vs. technical programs at two-year community or technical colleges. The difference-in-differences specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean. The outcomes in Columns (1) and (2) are indicator variables for enrolling in an academic and technical program at a community or technical college within two years of expected HS graduation, and the outcomes in Columns (3) and (4) are indicator variables for earnings degrees from a community or technical college in an academic or technical program, respectively.

Table 6: The Average Student Did Not Make Large Enough Adjustments to Yield Significant Earnings Benefits

	(1) Share of Qtrs Employed	(2) Employed Every Qtr	(3) Earnings (2020$)	(4) Imputed Earnings
$Exposure_{ct}$	0.007	0.004	560	392
	(0.006)	(0.005)	(361)	(349)
Percent change	1.3	0.8	1.9	1.3
Pre-period mean	0.553	0.496	29,318	31,062
N	2,135,226	2,135,226	2,135,226	2,135,226

Standard errors in parentheses are clustered at the county level

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

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks during youth and adolescence on employment and earnings outcomes at age 35 using individual-level linked data from the University of Houston Education Research Center. Estimates reflect coefficients from a continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative before PNTR (2000). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean. The outcome in column (1) is the share of quarters at age 35 an individual was employed in Texas. The outcome in column (2) is an indicator for being employed in every quarter at age 35 in Texas. The outcome in column (3) is annual earnings (2020$) at age 35. The outcome in column (4) is imputed earnings (2020$) at age 35 based on educational attainment and industry of employment.

Table 7: Heterogeneous Effects of the Labor Demand Shock on Human Capital and Earnings

	(1) HS Grad	(2) Enrolled at CTC	(3) Enrolled at Uni	(4) AA/AS	(5) BA/BS	(6) Realized Earnings	(7) Imputed Earnings
A. Reduced CTC Tuition							
$Exposure_{ct}$	-0.002	0.006	-0.014**	0.001	-0.005	-4	-196
	(0.007)	(0.008)	(0.007)	(0.003)	(0.004)	(427)	(446)
$Exposure_{ct} * CTCDist$	0.041**	0.070***	0.057***	0.016***	0.032***	3,091***	2,985***
	(0.018)	(0.016)	(0.016)	(0.006)	(0.011)	(854)	(1,000)
Percent change, $CTCDist = 1$	5.7	24.0	27.9	43.0	24.5	11.0	9.3
Pre-period mean, $CTCDist = 1$	0.682	0.319	0.152	0.038	0.111	28,030	29,920
B. Share of HS Alumni in Manufacturing							
$Exposure_{ct}$	-0.021**	0.010	-0.007	-0.001	-0.006	-260	- 443
	(0.009)	(0.009)	(0.009)	(0.004)	(0.005)	(512)	(428)
$Exposure_{ct} * HSMan_i$	0.029***	0.027***	0.008*	0.010***	0.014***	1,245**	1,265***
	(0.006)	(0.008)	(0.005)	(0.002)	(0.004)	(505)	(477)
Percent change, $HSMan_i = 0.1$	1.0	11.4	0.5	22.7	6.1	3.4	1.3
Pre-period mean	0.702	0.321	0.174	0.040	0.126	29,342	31,062
C. Shale Presence							
$Exposure_{ct}$	-0.004	0.036***	0.011*	0.008***	0.006*	237	704
	(0.007)	(0.008)	(0.007)	(0.003)	(0.004)	(421)	(349)
$Exposure_{ct} * Shale_c$	0.014	-0.005	-0.033**	-0.001	0.000	1,512**	-176
	(0.018)	(0.016)	(0.016)	(0.006)	(0.011)	(765)	(744)
Percent change, $Shale_c = 1$	1.5	9.5	-13.7	16.8	5.1	6.0	1.7
Pre-period mean, $Shale_c = 1$	0.707	0.326	0.161	0.039	0.124	29,339	30,921

Standard errors in parentheses are clustered at the county level

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

Notes: This table presents estimates of the differential effects of exposure to Chinese import competition on educational attainment and earnings of students attending high schools eligible for reduced tuition at local two-year colleges using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000), fully interacted with an indicator variable for a student residing in a public two-year college "tax district" and being eligible for reduced tuition (Panel A), the share of older graduates and dropouts from a student's high school that worked in manufacturing before PNTR (Panel B), and an indicator variable for the presence of a shale play underneath the county (Panel C). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient and total effect (DD + interaction coefficient) divided by the pre-period mean for individuals from inside and outside community college districts. The outcomes across columns are as follows: indicator variables for (1) graduating high school, (2) enrolling in a public two-year college, (3) enrolling in a public university, (4) earning a two-year degree, (5) earning a bachelor's degree, (6) realized earnings at age 35, and (7) imputed earnings based off educational attainment and industry of employment. Earnings for the estimation reflected in Column (7) are imputed as the mean earnings at age 35 of individuals that entered the labor market before the onset of the shock with the same educational attainment level and employed in the same industry.

Table 8: Back-of-the-Envelope Comparison of Discounted Earnings Benefits with "Scarring" Estimates

(1) Age	(2) Discounted Cumulative Earnings Gains, Full Sample	(3) Discounted Cumulative Earnings Gains, $CTCDist_i = 1$	(4) Discounted Cumulative Earnings Gains, $HSMan_i = 0.1$	(5) Discounted Cumulative Earnings Gains, $Shale_c = 1$	(6) Discounted Cumulative "Scarring" Effects
25	31	556	705	-248	-1,668
26	60	1,309	1,433	-291	-3,287
27	140	2,802	2,145	-197	-4,860
28	596	4,182	3,233	302	-6,386
29	941	5,702	4,107	1,108	-7,868
30	1,313	7,411	5,014	2,118	-9,307
31	1,697	9,329	5,944	3,260	-10,704
32	2,065	11,599	6,788	4,703	-12,060
33	2,481	13,486	7,556	6,090	-13,377
34	2,915	15,888	8,243	7,398	-14,655
35	3,332	18,220	8,920	8,699	-15,896
NPV	-12,565	2,324	-6,976	-7,197	
Protection Share	0.21	1.15	0.56	0.55	

Notes: This table presents discounted sums of the estimated effects of greater exposure to the China shock during youth on earnings from age 25 through 35, in comparison to discounted sums of the estimated earnings losses that the shock caused for individuals that had recently entered the labor market before the onset of the shock. Discounted earnings gains in (2) - (5) reflect sums of estimated effects on earnings through individual ages, discounted at a rate of 3%. Estimates in Columns (3) through (5) reflect the total effect for the specified subgroup from heterogeneity estimations. Column (6) shows a discounted sum of the estimated earnings losses for individuals that entered the labor market just before the onset of the shock from Table 3 as a benchmark. Net Present Value is calculated as the difference between earnings gains and the losses in Column (6) and Protection Share is calculated as the ratio of earnings gains to losses.

A Additional Results

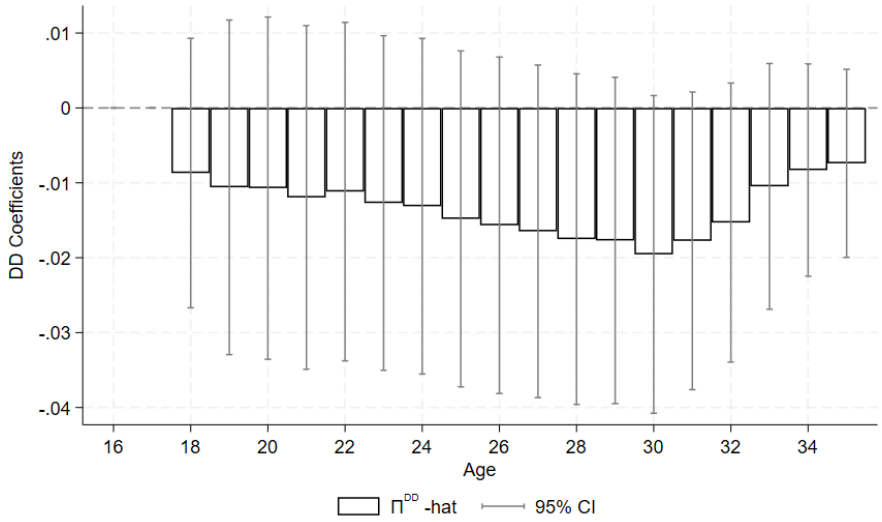


Figure A1: No Statistically Significant Evidence of Differential Attrition from the Dataset

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 35. Each estimate reflects the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000). 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. Standard errors are clustered by county.

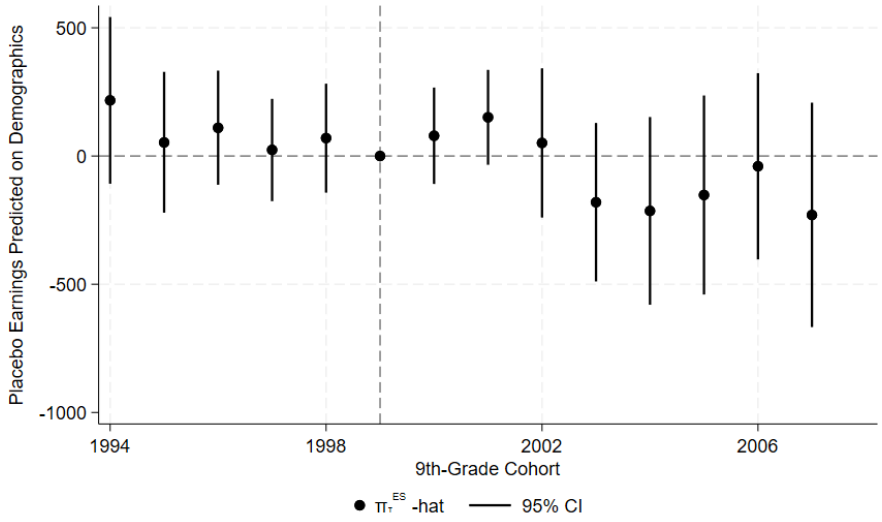


Figure A2: Shock Exposure Did Not "Affect" Pre-Determined Student Characteristics

Notes: This figure present estimates of the dynamic relationship between county-level exposure to Chinese import competition and earnings predicted from pre-determined student characteristics. Placebo earnings are defined using coefficients from an OLS regression of adult earnings on pre-measured FRPL eligibility, race, ethnicity, gender, and English-Language Learner status for individuals that entered high school before PNTR. The event study specification controls for county and year fixed effects, 1990 county characteristics interacted with post, county exposure to the fracking boom, and other changes to U.S. trade policy. Standard errors are clustered by county.

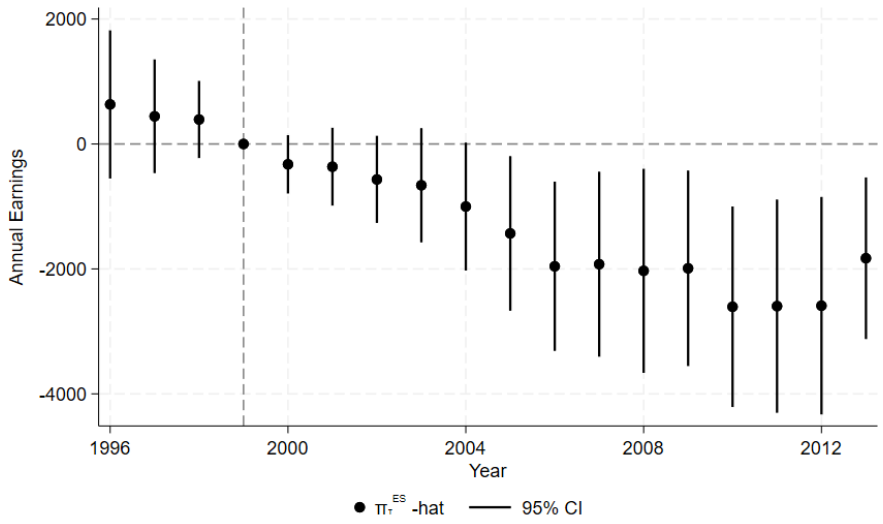
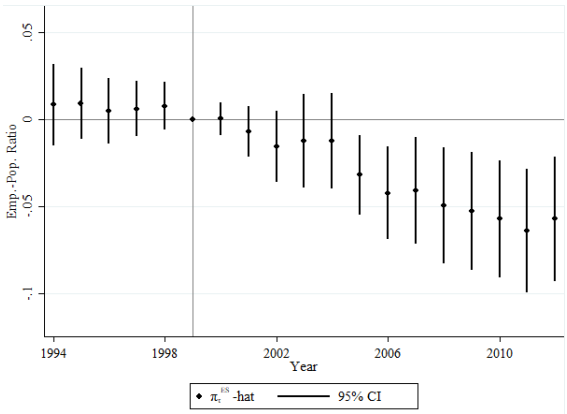
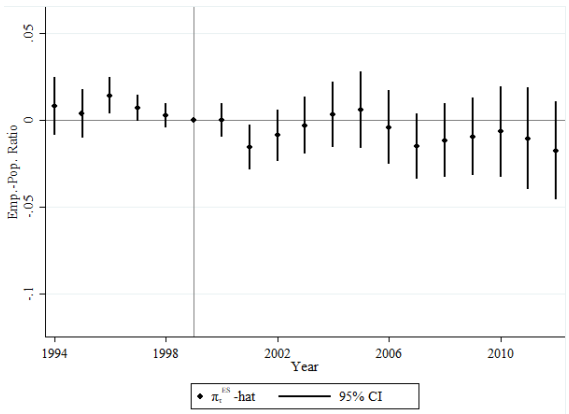


Figure A3: Effects of the Labor Demand Shock on Earnings of Recent Labor Market Entrants

Notes: This figure presents estimates of the effect of exposure to the labor demand shock earnings of recent labor market entrants. Estimates reflect coefficients from individual fixed effects event study regressions that compare changes in earnings of young adults from counties that were more or less exposed to negative labor demand shocks across cohorts relative to this difference one year prior to PNTR. The sample includes high school graduates, dropouts, and two- and four-year college graduates that entered the labor market before 2000. The outcome is annual earnings (2020$). The specification controls for individual and cohort-by-degree-by-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.



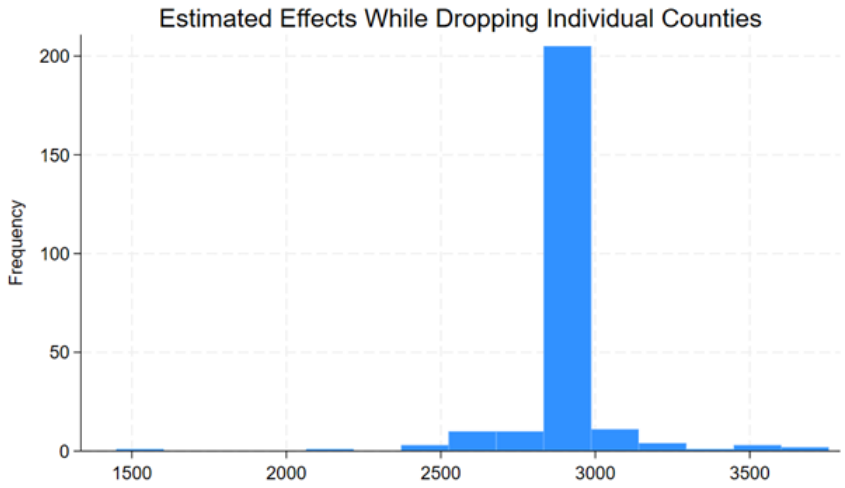
(a) Employment-to-Population Ratio; Preferred Specification



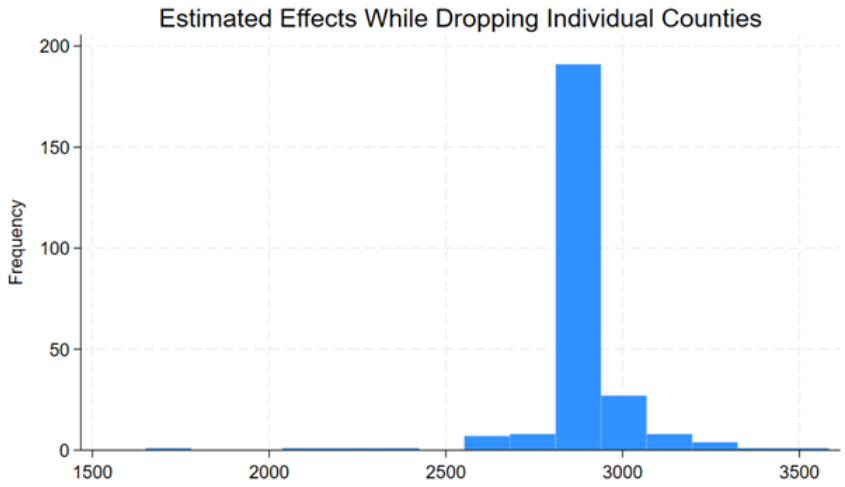
(b) Emp.-to-Pop. Ratio; No Controls

Figure A4: First-Stage Relationship Between Shock Exposure and Local Labor Demand Requires Controlling for Industrial Makeup

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. Estimates reflect coefficients from 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 this difference one year prior to the onset of PNTR. The specification in (a) 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. The specification in (b) only controls for county and year fixed effects. Standard errors are clustered by county.



(a) Imputed Earnings



(b) Earnings

Figure A5: Robustness to Dropping Each County from the Sample

Notes: These figures present histograms of the estimated differential effect of exposure to the labor demand shock on imputed and realized earnings for individuals in community college tax districts while dropping individual counties from the sample.

Table A1: Intensive-Margin Adjustments: Relevance of Field of Study to Directly Affected Industries

	(1) Q1	(2) Q2	(3) Q3	(4) Q4
A. HS Course Shares				
$Exposure_{ct}$	-0.001	0.004	0.001	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Percent change	-0.5	1.6	0.6	-0.8
Pre-period mean	0.218	0.247	0.168	0.367
N	3,678,707	3,678,707	3,678,707	3,678,707
B. Two-Year College Major				
$Exposure_{ct}$	0.002	0.001	0.008	0.002
	(0.006)	(0.002)	(0.009)	(0.003)
Percent change	5.5	14.6	11.7	9.3
Pre-period mean	0.031	0.007	0.071	0.024
N	3,678,707	3,678,707	3,678,707	3,678,707

Standard errors clustered by county in parentheses

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

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on high-school course choices (Panel A) and two-year college enrollment (Panel B) by field of study where courses majors are grouped according to field-specific shock exposure. I use a sample of pre-period high school graduates to define shock relevance for two-digit Texas Education Agency course subject areas by regressing the average tariff gap of an individual's four-digit NAICS code of employment on counts of course completions. I then group courses into quartiles from the most negatively to most positively associated with employment in industries that were directly exposed to import competition following PNTR. I similarly define shock exposure for each major (two-digit CIP code) as the employment-weighted average tariff gap of industries employing recent two-year graduates from that major in the pre-period. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean. The outcomes in Panel A are the share of courses completed across each quartile, and the outcomes in Panel B are indicator variables for enrolling at a two-year college and choosing a major in that quartile.

Table A2: Did Local Two-Year Colleges Adjust Program Offerings?

	(1) Q1	(2) Q2	(3) Q3	(4) Q4
$Exposure_{ct}$	-0.080	-0.044	0.062	0.062*
	(0.098)	(0.034)	(0.070)	(0.036)
Percent Change	-29.6	-48.6	14.1	31.1
Pre-period Mean	0.270	0.0911	0.439	0.200
N	862	862	862	862

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 the share of programmatic offerings (six-digit CIP codes) at two-year public colleges by the broader major's (two-digit CIP code) exposure to the shock. I define shock exposure for each major as the employment-weighted average tariff gap of industries employing recent graduates from that major in the pre-period. Estimates reflect coefficients from a difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after vs. before the start of the shock (2000). 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. The outcome variables in each column are the share 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.

Table A3: Robustness of Main Results to Sequentially Adding Covariates

	(1) No Controls	(2) + Industry Shares	(3) + 1990 Demos	(4) + Trade Policies	(5) + Fracking)	(6) + Dot-Com Crash	(7) + Housing Bubble	(8) + Financial Crisis
A. Imputed Earnings								
$Exposure_{ct}$	255	-50	77	-185	-161	-406	97	-48
	(200)	(304)	(445)	(462)	(451)	(492)	(696)	(691)
$Exposure_{ct} * CTCDist$	144	2,357***	3,318***	2,942***	2,888***	2,863***	2,612**	2,778**
	(666)	(808)	(954)	(972)	(998)	(1,057)	(1,302)	(1,130)
Percent Change, Non-district	0.8	-0.2	0.2	-0.6	-0.5	-1.2	0.3	-0.1
Percent Change, District	1.3	7.7	11.3	9.2	9.1	8.2	9.1	9.1
Pre-period Mean, Non-district	32,715	32,715	32,715	32,715	32,715	32,715	32,715	32,715
Pre-period Mean, District	29,920	29,920	29,920	29,920	29,920	29,920	29,920	29,920
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								
$Exposure_{ct}$	392*	-36	-69	-55	-4	-187	56	-28
	(234)	(346)	(452)	(438)	(427)	(452)	(613)	(627)
$Exposure_{ct} * CTCDist$	819	3,604***	4,689***	3,269***	3,091***	2,996***	2,513**	2,601***
	(572)	(792)	(787)	(815)	(854)	(846)	(979)	(947)
Percent Change, Non-district	1.3	-0.1	-0.2	-0.2	-0.0	-0.6	0.2	-0.1
Percent Change, District	4.3	12.7	16.5	11.5	11.0	10.0	9.2	9.2
Pre-period Mean, Non-district	31,183	31,183	31,183	31,183	31,183	31,183	31,183	31,183
Pre-period Mean, District	28,030	28,030	28,030	28,030	28,030	28,030	28,030	28,030
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

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 differential effects of exposure to Chinese import competition during youth on human capital and later-lafe earnings of students from K-12 districts that were eligible for reduced tuition at local two-year colleges using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000), fully interacted with an indiator variable for a student residing in a public two-year college "tax district" and being eligible for reduced tuition. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient and total effect (DD + interaction coefficient) divided by the pre-period mean for counties inside and outside community college districts. Column (1) includes no controls and columns to the right sequentially add control variables. Column (2) controls for baseline industry shares interacted with a post dummy. Column (3) controls for 1990 county demographics interacted with a post dummy. Column (4) controls for changes to other trade policies. Column (5) controls for exposure to the fracking boom. Column (6) controls for exposure to the dot-com crash. Column (7) controls for exposure to housing bubble. Column (8) controls for exposure to the financial crisis.

Table A4: Robustness to Smooth Deviations from Linear Trends (Rambachan and Roth, 2023)

	(1) Deviation Size (m), as a % of Treatment Effect	(2) Deviation Size (m), Absolute	(3) 90% Confidence Set Lower Bound	(4) 90% Confidence Set Upper Bound
A. College Enrollment				
	Parallel		0.018	0.069
	0 (Linear)		0.024	0.070
	5%	0.003	0.010	0.064
	10%	0.005	-0.005	0.059
	15%	0.008	-0.012	0.061
	20%	0.011	-0.015	0.066
B. Earnings at 35				
	Parallel		808	5,033
	0 (Linear)		2,951	6,871
	5%	146	2,603	8,191
	10%	292	1,740	9,492
	15%	438	554	10,340
	20%	584	-608	11,077

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

Table A5: Four-Year Enrollment Responses Across Tuition Quartiles

	(1) Q1 Tuition	(2) Q2 Tuition	(3) Q3 Tuition	(4) Q4 Tuition	(5) Private
$Exposure_{ct}$	0.002	-0.000	-0.017	0.006	-0.001
	(0.016)	(0.013)	(0.014)	(0.009)	(0.003)
$Interaction_{ict}$	0.021	-0.016	0.006	0.022	0.008
	(0.020)	(0.018)	(0.016)	(0.014)	(0.006)
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 differential effects of exposure to Chinese import competition on college enrollment outcomes of students attending high schools eligible for reduced tuition at local two-year colleges using individual-level data from the University of Houston Education Research Data. Estimates reflect coefficients from the primary continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative to before PNTR (2000), fully interacted with an indiator variable for a student residing in a public two-year college "tax district" and being eligible for reduced tuition. The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry 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. 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 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.

A.1 Other Forms 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 geographies, industries, or occupations as adults as manners of adjustment. I first examine whether students exposed to large local shocks migrated to less-exposed counties after high school. Using the location of a student's college or university as a proxy for their adult residence, I estimate effects of greater exposure to the shock on attending college in any high-exposure (low-exposure) county and on attending college in a high-exposure (low-exposure) county other than where a student attended K-12.[45] Estimates in Table A6 suggest that students exposed to larger shocks did not adjust to the shock by moving to less-exposed counties. Greater exposure to the shock significantly increased the likelihood students enrolled in colleges in high-exposure counties without affecting the likelihood they did so in low-exposure counties. Although students exposed to larger shocks were significantly more likely to enroll in colleges outside of their home county, they did so in other high-exposure counties.

I next estimate the effects of exposure to the local shocks on later-life employment across groupings of two-digit NAICS industry codes (Table A7). Exposure to shocks during K-12 significantly increased the likelihood that students worked in food services and accommodations and decreased the likelihood they worked in retail at age 35.

Due to data limitations, assessing whether exposure to local shocks during youth and the ensuing human capital adjustments affected occupation choice is beyond the scope of this paper.[46]

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

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

Table A6: Students Did not Migrate to Less-Exposed Counties for College

	(1) Enrolled in High-Exp. County	(2) Enrolled in Low-Exp. County	(3) Moved for College	(4) Moved to High-Exp. for College	(5) Moved to Low-Exp. for College
$Exposure_{ct}$	0.026**	-0.011	0.012	0.012*	-0.000
	(0.010)	(0.008)	(0.009)	(0.007)	(0.007)
Percent Change	9.0	-3.9	3.9	7.1	-0.1
Pre-period Mean	0.439	0.134	0.305	0.198	0.107
N	0.291	0.275	0.302	0.167	0.135

Standard errors in parentheses are clustered at the county level

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

Notes: This table presents estimates of the effects of exposure to negative local labor demand shocks on migration patterns, proxying for a student's adult residence with the location of the college or university they attended. Estimates reflect coefficients from a continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative before PNTR (2000). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are initially 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. The outcome in column (1) and (2) are indicators for enrolling in a public two- or four-year college or university within two years of expected HS graduation in a county with above-median or below-median exposure to PNTR, respectively. The outcome in column (3) is an indicator for enrolling in a college or university outside of the county where a student attended K-12. The outcomes in columns (4) and (5) are indicator variables for enrolling in a college or university in a high-exposure (low-exposure) county other than where a student attended K-12. In all outcomes, students are assigned the location of the modal postsecondary institution they attended.

Table A7: Effects of Exposure to the Labor Demand Shock on Later-Life Employment by Industry

	(1) Man.	(2) Const./Trans.	(3) Oil and Gas	(4) Retail	(5) Food and Accomm.
DD	0.001	0.001	0.000	-0.003*	0.004***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Percent change	1.5	2.0	0.2	-6.7	12.5
Pre-period mean	0.038	0.055	0 0.016	0.052	0.032
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
$Exposure_{ct}$	0.001	0.000	-0.001	0.002	0.001
	(0.001)	(0.000)	(0.001)	(0.002)	(0.003)
Percent change	1.4	0.9	-5.0	5.0	1.4
Pre-period mean	0.041	0.037	0.010	0.048	0.081
N	2,135,226	2,135,226	2,135,226	2,135,226	2,135,226

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 35 using individual-level linked data from the University of Houston Education Research Center. Estimates reflect coefficients from a continuous difference-in-differences regression that compares changes in the differences between outcomes of students from more- and less-exposed counties entering high school after relative before PNTR (2000). The specification controls for county and year fixed effects, student demographics, 1990 county characteristics and employment shares by industry interacted with post, shale and natural gas presence interacted with post, and other changes to U.S. trade policy. Students are assigned to the county of the first school where they appear in the dataset. Standard errors are clustered by county. Percent changes are calculated as the estimated difference-in-differences coefficient divided by the pre-period mean.

B Data

B.1 Sample Selection

My primary analysis sample consists of 3,678,707 students who 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 who I ever observe starting ninth grade.[47] In order to an adopt an intent-to-treat framework that accounts for endogenous migration, I also limit the sample to students who 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. I drop students participating in special education, because of their unique job prospects relative to the rest of the sample and the labor demand shock.

Not all students who 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 difference-in-difference specifications representing changes in the difference in attrition rates between students from more- and less-exposed counties across the policy change. 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 35. No coefficient is statistically distinguishable from zero, suggesting attrition only causes classical measurement error. I drop all students from the analysis sample that attrit at age 17 or earlier and measure employment and earnings outcomes for stayers that

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

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 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.[48] I further control for employment shares across two-digit NAICS codes to flexibly account for labor demand shocks outside of manufacturing. The first three measures come from the Census Bureau's 1990 Decennial Census, while the latter two are derived using data from the County Business Patterns database and the Surveillance, Epidemiology, and End Results Program.

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

[48] 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.

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. I also control for local exposure to NAFTA using the shares from Choi et al. (2024).

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 less-educated workers, 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 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).[49] I interact each of these cross-sectional exposure measure with year dummies to allow them to flexibly affect outcomes across cohorts.

B.2.3 County-Level Outcomes

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

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

[49]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.

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 All Urban Consumers Consumer Price Index to convert all nominal measures to 2020 dollars.

B.3 College Major Categories

Table B1: 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 A1.

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.[50]

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

[50]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.

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

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

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

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

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

C.2 Generalizability

Table C1 presents descriptive statistics on the composition, funding levels, and graduation rates of public K-12 and postsecondary institutions in Texas and the United States as a whole. Public K-12 schools in Texas differed from those of the U.S. as a whole in the years before the trade shock in terms of student demographics – 40% of students in public K-12 schools in Texas were Hispanic, as opposed to only 16% across the country overall – but

received similar per-pupil funding levels and produced similar high school graduation rates. Students at both two- and four-year colleges in Texas also exhibited similar differences in demographics from U.S. college students as a whole. Although graduation rates trailed at Texas colleges, funding levels mirrored nationwide averages.

Relative to the rest of the country, Texas employs a fairly unique school finance system, known as the "Robin Hood" plan. Under the Robin Hood plan, excess property tax revenues from wealthy school districts are collected by the state government and redistributed to poor districts. In addition to promoting equitable funding *levels* across rich and poor districts, the system mechanically makes local school funding levels essentially orthogonal to *changes* to local property values (in relative terms). I use data from the National Center for Education Statistics (NCES) District Finance Survey to estimate the effects of local shocks on district revenues and expenditures in Table C2. I find that despite district revenues from local property taxes falling by nearly $600 per-pupil in more-exposed counties, school spending was unaffected. Moreover, estimates in Panel B of the effects of the China shock on property tax revenues and school spending in districts across the rest of the country support the uniqueness of this "insurance value" provided by the Robin Hood formula: while school spending in Texas fell by just 5 cents per dollar lost in district property tax revenues due to local shocks, the pass-through of changes in local property taxes to school spending was 67 cents in the rest of the country.

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			
$Exposure_{ct}$	-569**	-31	0.05
	(270)	(156)	
Percent Change	-12.6	-0.4	
Pre-period Mean	4,536	8,529	
N	4,275	4,275	
Rest of U.S.			
$Exposure_{ct}$	100***	67	0.67
	(39)	(107)	
Percent Change	2.92	0.66	
Pre-period Mean	3,416	10,190	
N	52,729	52,729	

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 difference-in-differences regression that compares changes in the differences between county-level outcomes in more- and less-exposed counties after PNTR (2000). 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 in 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.