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SHORT-TERM AND LONG-TERM EFFECTS OF TRADE LIBERALIZATION

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Short-Term and Long-Term Effects of Trade Liberalization*

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Abstract

Free trade increases economic activity in the long run but produces significant labor market disruptions. I study the short-term and long-term effects of trade liberalization on workers by examining how young adults in the United States responded to the post-2000 U.S.-China trade boom. Because adjustment frictions increase with age, the long-term impacts of trade are determined by not only how but also where in the lifecycle workers respond to trade shocks. To document these adjustment mechanisms, I assemble several datasets and employ multiple identification strategies. The main empirical approach leverages the geographic variation in local exposure to China's obtaining permanent normal trade relation (PNTR) status in 2000. Overall, I find that young people's short-term responses to trade liberalization were overwhelmingly negative. In particular, I find that PNTR essentially had no college attainment effects but in fact raised the incidence of several undesirable outcomes. Those negative outcomes include lower geographic and industry mobility and increased engagement in criminal activities and risky health behaviors. I also show that PNTR significantly diminished young adults' chances of long-term economic success. My findings imply that without government intervention, the disruptive effects of trade will likely remain high in the long run.

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JEL Classification: F13, F16, J24

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1 Introduction

Trade liberalization creates significant economic disruptions; as a result, international trade remains politically contentious in the United States. Historically, mainstream economists have emphasized the positive effects of trade, such as lowering consumer prices, increasing product variety, and lifting economic activity in the long run, but mostly treated the uneven labor market impacts as short-term disturbances. Only with the mounting evidence on the slow U.S. labor market adjustments to the China import shock in the early 2000s (Autor, Dorn, and Hanson 2013) has the field begun to grapple with the potential persistence of trade's disruptive effects. Whether the disruptions are enduring or short-lived depends on how workers respond to trade-induced labor demand changes.

In this study, I present new evidence on the short-term and long-term effects of trade by studying how young adults in the United States responded to the post-2000 U.S.-China trade boom and how their responses relate to long-term economic outcomes.[1] The population I consider consists of college-goers (aged 18 to 24) and recent college graduates (aged 25 to 34). To my knowledge, this is the first study to explicitly assess the impact of trade on young people's transitions into adulthood and the labor market. The study of these workers is important for two main reasons. First, older workers have already made various forms of irreversible investments in human, social, and health capital—the main channels of trade adjustment that I consider—and thus face higher adjustment frictions.[2] Conversely, young adults have considerable latitude in potential adjustment mechanisms, especially along the educational attainment margin. Second, as the returns on those investments compound over time and young people have more periods for the benefits to accrue, young people also have strong incentives to make those investments.

My consideration of workers' potential channels of trade adjustment follows the literature on worker responses to local labor demand shocks. From a theoretical perspective, this vast literature centers on how changes in local economic conditions—through altering foregone earnings and future expected earnings—affect workers' time allocation between labor market and non-labor market activities (Becker 1962, 1965). To aid the discussion of my results, I conceptualize the types of responses identified in the literature as either "productive" or "nonproductive." Productive responses correspond to actions that can improve economic outlook. Nonproductive responses are actions that can either harm or do not directly improve labor market prospects. To analyze a wide array of outcomes, I assemble data from several sources and employ multiple identification strategies leveraging the same trade shock.

1. China offers a rich case study as it constitutes most of the import growth from low-wage countries in the United States since 2000 (Autor, Dorn, and Hanson 2016).

2. For purposes of this discussion, adjustment frictions refer to factors that impede or deter worker reallocation across industries and geographies.

My main empirical approach leverages the cross-commuting zone (CZ) variation in manufacturing declines induced by China's obtaining permanent normal trade relation (PNTR) status in 2000. This approach is motivated by a growing body of work that documents the contribution of PNTR to the surge in U.S. imports from China in the early 2000s (Pierce and Schott 2016a; Handley and Limão 2017). PNTR, which went into effect with China's accession to the World Trade Organization (WTO) in 2001, formalized China's normal trade relation (NTR) status. The trade policy was unusual in that it promoted trade by reducing the uncertainty surrounding future tariff rates, not by lowering actual tariff rates. Reductions in such trade policy uncertainty have been shown to increase trade flows by fostering and accelerating exporter entry (Handley 2014; Handley and Limão 2015, 2017) and inducing more productive firms to export (Feng, Li, and Swenson 2017).

I measure local PNTR exposure using the CZ employment-weighted average of the Pierce and Schott (2016a) industry-level "NTR gaps." They define the NTR gaps as the difference between two sets of tariff rates. The first set of tariff rates is the relatively high non-NTR tariff rates set by the Smoot-Hawley Act of 1930. These rates would have been applied to Chinese goods had China lost its NTR status. The second set is the relatively low NTR tariff rates reserved for WTO members set in 1999. A higher NTR gap implies that the industry experienced a more substantial decline in tariff uncertainty following China's WTO entry, which likely led to faster growth in imports from China and steeper employment declines. For ease of interpretation, the results are the implied effects of a one standard deviation increase in PNTR, which is about 2.78 percentage points.

In using PNTR-induced variation in trade exposure for identification, I rely on the implicit assumption that local exposure to PNTR is uncorrelated with unobserved secular trends or shocks in labor supply and demand. Using a difference-in-difference approach and several Bureau of Labor Statistics (BLS) local employment datasets from 1990 to 2015, I first establish that the China import shock had strong displacement effects, particularly in the manufacturing sector. Next, I perform event studies to show that local employment outcomes exhibited parallel pre-event trends and sharp breaks at precisely the time the trade policy went into effect. Because labor demand changes associated with productivity growth and labor supply shocks are smooth and continuous (Charles, Hurst, and Notowidigdo 2018), evidence from these analyses suggests that alternative economic shocks, such as automation, were unlikely to be the main factor. Further, I show that other shocks that coincided with China's WTO entry and could have impacted Chinese import growth, such as the tech bust, fail to explain the observed employment trends.

Having established the plausible exogeneity of PNTR, I begin the analysis by showing that rising trade pressures from China created strong incentives for young people to attend college. I show that PNTR not only reduced young people's current job opportunities but also significantly diminished their economic prospects. I measure young adults' current and future employment opportunities using the labor market outcomes of noncollege-educated workers between 18 and 34 years old and between 35 and 54 years old, respectively. The latter group corresponds to a group of "prime working-age" (hereinafter "prime-aged") workers. Using the 1990, 2000, 2005–2015 individual-level Census and American Community Survey (ACS) data, I show that both groups of noncollege workers experienced similar trade-induced reductions in employment rates and earnings. I also show that college-educated workers, especially bachelor's degree holders, experienced much less adverse labor market effects, especially on the employment margin, compared to noncollege workers.

Despite the positive returns to college degree receipt, I find that young adults did not have strong skill acquisition responses to the China import shock. Using the aggregated institution-level college enrollment and completion data from the Integrated Postsecondary Education System (IPEDS), I show that PNTR slightly increased college enrollment by 4 percent relative to the sample mean, with the effects concentrated at two-year colleges and public institutions.[3] While this finding verifies the prediction of the human capital theory (Becker 1962), my results also show that there were no other economically meaningful or statistically significant educational responses. In particular, I find no evidence of a four-year college enrollment effect or any completion effects. Possible foregone earnings and student debt accumulation make enrollment without attainment an undesirable outcome.

Could young people's geographic relocation or industry transitions—which could have allowed them to find new jobs even without significant skill acquisitions—explain the finding? To assess these employment transitions, I exploit the unique breadth of outcomes covered in the 1997 sample of the National Longitudinal Survey of Youth (NLSY97). In an effort to provide a concise analysis of the various outcomes, I create a summary index for each outcome category and formulate z-scores of the summary indices such that larger positive values indicate more desirable outcomes. My NLSY97 identification strategy leverages the individual-panel structure of the dataset and young adults' place of residence in 1997. This strategy, distinct from repeated cross-sectional analysis (e.g., Pierce and Schott 2016b), allows me to track trade's impact on a group of 6,772 young adults from 2001, when they were aged between 16 and 20, to their early 30s.

Using the NLSY97, I show that migration and industry-switching were unlikely to be the main channels of trade adjustment. Specifically, young workers initially living in higher PNTR CZs experienced significantly higher cumulative exposure to PNTR after 2000 not only at the CZ level but also at the industry level, indicating lower worker mobility on those margins. Trade-exposed workers also were more likely to be employed in routine-intensive occupations—that

3. "College" refers to all postsecondary institutions.

is, jobs high in repetitive and codifiable content that make them susceptible to automation and offshoring (Autor and Dorn 2013), though the occupation-level estimates are much smaller and not statistically significant.

In addition to lower geographic and industry mobility, youth exposure to the import shock significantly raised the incidence of several unappealing risky behavior outcomes. Those outcomes include criminal behaviors, such as being arrested and being in jail, as well as risky health behaviors, such as alcohol consumption and illegal drug use. By contrast, I find much weaker evidence for family and childrearing responsibilities impacting young adults' schooling and employment choices. Together, the evidence suggests that nonproductive trade adjustment mechanisms may have reinforced and prolonged the negative labor market effects of Chinese import competition.

Lastly, I show that the import shock strongly diminished young people's chances of future economic success, even more than a decade after the trade policy change. I reference Chetty et al. (2011) and create an adult economic success measure using five outcomes: total assets at age 30, has owned any home by age 30, has been married by age 30, has moved out of state by age 30, and live in a higher socioeconomic status community at age 30 as measured by the county college population share. As many of the individual components are positively correlated with future earning trajectories conditional on income, I interpret the outcome not only as a measure of young adult's economic self-sufficiency at age 30 but also a broad approximation to their future economic success. Further, because the Chinese import surge had largely subsided by the early 2010s, the outcome measure arguably provides a good indication for trade's potential effects over the next few decades as the NLSY97 respondents begin to retire. I show that PNTR's effect on future earnings trajectories is significant, negative, and ranges between -4.26 and -6.43 percent relative to the standard deviation. This finding shows that without changes to current trade adjustment policies, the geographic disparities in workers' outcomes resulting from trade liberalization will likely persist.

This paper adds to an active and growing number of studies that document the negative non-labor market effects of trade. Those adverse effects include the increase in political polarization (Autor et al. 2016; Che et al. 2016), the fall in local public good provision (Feler and Senses 2017), and the rise in the incidence of single mothers (Autor, Dorn, and Hanson 2017). In this emerging body of work, this study is closest in scope to Pierce and Schott (2016b) who document trade's negative effects on health and mortality.[4] My analysis extends the literature in several important ways. First, while prior studies have examined young adults as part of their investigations, this is the first study to document comprehensively young people's transitions

4. One distinction is that they measure the mortality rates associated with different risky health behaviors, whereas I directly measure the incidence of those behaviors.

into adulthood as well as their responses to trade shocks across multiple related dimensions. Second, I am able to quantify the transitional costs of trade liberalization in terms of its effect on workers' chances of future economic success. Finally, my analysis improves on current estimates of the long-term impacts of trade by leveraging longitudinal data rather than repeated cross-sections. By combining a series of datasets in a novel way, I am therefore able to avoid the concerns around the effects of worker mobility, especially among young people, on estimated impacts—issues that were not fully addressed by the previous literature.[5]

My work is also related to the literature on young people's human capital adjustments to local labor demand shocks (Black, McKinnish, and Sanders 2005; Cascio and Narayan 2015; Charles, Hurst, and Notowidigdo 2018).[6] Among the studies, only Charles, Hurst, and Notowidigdo (2018) study college enrollment and attainment.[7] This study differs from prior work in several aspects. First, my local labor demand shock is induced by a change in trade policy. Second, the shock is negative. Third, I do not find any completion effects. The distinctions in the findings highlight potential differences in workers' responses to positive and negative local labor demand shocks. They also underscore how the geographic concentration of manufacturing industries, which propagates the adverse effects of trade shocks, and the lower baseline schooling attainment in those communities can impact the size of educational responses. In terms of scope within this literature, this paper most resembles the work of Greenland and Lopresti (2016), who document a positive relationship between rising Chinese import competition and U.S. high school graduation rates.[8] While these results are instructive, whether the new high school graduates went on to obtain a college degree remains unclear. As economic outcomes continue to decline for U.S. noncollege graduates, my study bridges an important gap in this literature. My results, which do not show a significant college attainment effect, also highlight potential frictions in degree attainment in the postsecondary education market that are absent in the secondary education market.

Together, my findings—the absence of significant productive responses and the surprising number of nonproductive responses by young adults—have several strong policy implications. They imply that current trade adjustment policies, such as the Trade Adjustment Assistance (TAA) program, have not been effective in easing the labor market transitions of many young

5. Although I did not find a strong migration response in my sample, it was not clear *ex ante* that this would be the case. For example, Autor, Dorn, and Hanson (2013) find no evidence of population adjustments to rising import competition, while Greenland, Lopresti, and McHenry (2018) find evidence of small and delayed migration responses.

6. A related body of work studies how local economic conditions during early childhood affect educational attainment and income in the long run (e.g., Stuart 2017).

7. Black, McKinnish, and Sanders (2005) and Cascio and Narayan (2015) examine the impact of technology shocks on secondary educational attainment.

8. Using a the Census/ACS data, which is a different dataset from theirs, I also find an increase in the number of high school graduates, though my estimate is smaller.

people.[9] A related implication is that the long-term fiscal costs of trade liberalization may be higher than what the current estimates indicate (e.g., Autor, Dorn, and Hanson 2013). This is because persistent joblessness among young people, compared to among older workers, will result in higher lifetime usage of public assistance. Depressed local economic activity can also lead to declines in state and local government revenues, which can in turn decrease public college funding, lower student-oriented resources, and further dampen the educational attainment of future generations of workers (Bound, Lovenheim, and Turner 2010).[10] Lastly, my results strongly suggest that economic policies that fail to consider the interplay between the labor market and non-labor market factors that impact worker adjustment will achieve limited success.

The rest of this paper is organized as follows. Section 2 describes the data sources and defines the key variables. Section 3 presents the labor market results using the BLS and Census/ACS data. Section 4 presents the IPEDS college enrollment and attainment results. Section 5 quantifies the significance of additional adjustment channels and discusses the long-term implications of trade's disruptive effects. Section 6 concludes.

2 Data and Measurement

Section 2.1 defines the local labor market. Section 2.2 describes the measurement of the main outcomes in the three datasets I use to study young adults. Section 2.3 discusses the construction of the trade variables.

2.1 Local Labor Markets

I approximate local labor markets using commuting zones (CZs). A commuting zone is a cluster of counties with strong commuting ties internal to the clusters (Tolbert and Sizer 1996). I prefer CZs as my treatment unit because they cover both urban and rural areas and minimize labor mobility across treatment units. Because of the distinct industrial structure of Alaska and Hawaii, I restrict the analysis to the 48 contiguous U.S. states, leaving 722 CZs.

CZ demographic and economic information come from the 100 percent Census data, Quarterly Census of Employment and Wages (QCEW), Local Area Unemployment (LAU), County Business Patterns (CBP), and Surveillance, Epidemiology, and End Results (SEER). Table A1 in the Appendix documents the sample periods and the outcome variables in each dataset.

9. However, there is evidence to suggest that, conditional on program participation, TAA has been effective in re-training workers (Hyman 2018).

10. I present suggestive evidence of trade-induced declines in public college funding and resources, though my estimates are not always precisely measured.

2.2 Main Outcomes

2.2.1 Census/ACS Data

The main purpose of using the individual-level public-use 1990, 2000, 2005–2015 Census and American Community Survey (ACS) data (Ruggles et al. 2019) is to provide the link between trade-induced local labor demand changes and young people's skill acquisition responses. To this end, I collect data on the employment and earnings of 18–54 year-old college workers and noncollege workers, which include both high school dropouts and high school graduates (or equivalent). Earnings are inflated to 2015 dollars using the Consumer Price Index (CPI). Weekly wages are calculated by dividing total income from wages and salary by the total number of weeks worked last year.[11]

I impose several sample restrictions to the Census/ACS data to increase their comparability with the other datasets in my analysis. I restrict the samples to noninstitutionalized workers living in their state of birth at the time of the interview. This approach follows Charles, Hurst, and Notowidigdo (2018) and aims to focus on the subset of the local population whose CZ of residence during youth is likely the same as their current residence. One drawback of this strategy is that I will miss the portion of workers who have moved away from trade-exposed CZs. If migration among young people is an important margin of worker response, as shown by the previous literature (e.g., Bound and Holzer 2000), then neglecting population adjustments may over-state the magnitude of the negative effects of trade. I address this concern in Section 5 using a longitudinal dataset that allows me to track the same group of workers over time.

For the analysis on college-educated workers, I exclude respondents aged between 18 and 24. This restriction addresses two data constraints. One constraint is that the Census/ACS data only record the highest degree attained by the respondents. Another constraint is that the Census/ACS data are unavailable at the CZ level between 2001 and 2004, during a surge of Chinese imports. As workers aged between 18 and 24 were likely to be enrolled in school, excluding them from the analysis arguably better captures workers' complete skill acquisition responses to the China import shock.

2.2.2 IPEDS Data

Next, I examine the human capital impacts of trade liberalization using the 1990–2015 Integrated Postsecondary Education System (IPEDS). IPEDS is an aggregated institution-level dataset covering both public and private colleges eligible for Federal Title IV student aid. The advantages of the dataset are its extensive coverage of postsecondary education-related outcomes across time and space. These features make the IPEDS data ideal for examining young adults' skill

11. Hourly wages are not used because the ACS data only report them in intervals starting in 2008.

acquisition responses. One disadvantage of the dataset is that the IPEDS data leave out both demographic information, such as age, and labor market information on college enrollees.

In the IPEDS data, I restrict the sample of postsecondary institutions to all two-year and four-year colleges with geographic (county) identifiers. "Two-year colleges" are colleges whose highest degree offered is less than a bachelor's degree, which include both community colleges and less than two-year institutions. "Four-year colleges" include all remaining colleges. To focus on the local response to trade shocks, I follow the literature (Charles, Hurst, and Notowidigdo 2018) and drop selective four-year colleges from the main analysis.[12] I show in Table B6 in the Appendix that the IPEDS results are not sensitive to using different sample definitions. Those differences include the inclusion of selective four-year colleges, the exclusion of less-than-two-year colleges, whose sampling issues have been well-documented (Cellini 2009), or the exclusion of for-profit colleges.

College enrollment is measured by the first-time full-time fall enrollment (hereinafter "enrollment") for undergraduate degree or certificate-seeking students. College completion is measured by the total number of undergraduate degrees and sub-baccalaureate certificates. Both enrollment and completion counts are aggregated to the CZ-institution level and adjusted by CZs' 18 to 34 population from the SEER data. Results remain unchanged when I adjust by 18 to 24 population or use alternative measurements of enrollment (total first-time, total full-time, total fall) or completion (associate's, bachelor's, certificates); for example, see Table B7 in the Appendix.[13]

2.2.3 NLSY97 Data

The 1997 sample of the National Longitudinal Survey of Youth (NLSY97) dataset is a longitudinal survey of 8,984 individuals who were interviewed annually until 2011 and biennially thereafter. In 2001, respondents were aged 16 to 20 years old, that is, around college-attending age. The age range, in addition to the wealth of information on young adults, makes the NLSY97 data ideal for studying young workers.

The restricted-use version of the NLSY97 dataset contains data on respondents' county of residence. I use this information to match to CZ and assign the intensity of PNTR exposure. To guard against endogenous migration over the sample period, I assign individuals to CZs based on

12. A selective four-year college is defined as having a Barron's 2009 selectivity index between 1 (most competitive) and 3 (very competitive).

13. My method of adjustment follows the convention in the literature (e.g., Pierce and Schott 2016b; Autor, Dorn, and Hanson 2017; Charles, Hurst, and Notowidigdo 2018). The advantage of using first-time full-time fall enrollment is that this measure is unaffected by changes in time to completion, which has been shown to increase over my sample period (Bound, Lovenheim, and Turner 2012). Other measures of local human capital accumulation, such as total full-time enrollment or total fall enrollment, are not to immune to those changes.

their county of residence in 1997. This method of assignment allows me to link workers' trade exposure during youth to their adult outcomes.[14] Outcomes are measured between 2001 to the first survey round after young adults have turned 30, during which important asset information such as home ownership is collected.[15] Further, I restrict the sample to young adults in the contiguous 48 U.S. states in 1997 to keep the sample consistent with the IPEDS and Census/ACS analyses.

Using this dataset, I assess the multidimensional effects of trade. Here, the innovation is to provide a comprehensive analysis of young adults' responses and the long-term implications of the import shock's short-term disruptions. As such, my analysis not only covers a wide range of outcomes (over 20 distinct outcomes), many of which are novel in the literature, but also spans multiple outcome categories, including several worker mobility outcome categories (geographic, industry, and occupation mobility), multiple risky behavior outcome categories (criminal behavior and risky health behavior), one life event outcome category, and one long-term economic outcome category.

To aid multiple hypotheses testing and to present the analysis in a succinct manner, I create a summary index for each outcome category. Summary indices are created in three steps. First, I create z-scores for the individual components of each summary index and then rescale the z-scores such that higher values indicate more desirable outcomes. Second, I create summary indices of the outcome categories by averaging the rescaled z-scores. Third, I standardize the summary indices again to have a mean of zero and a standard deviation of one. Below, I briefly discuss the individual components of each summary index. The complete list of the summary indices and the individual components can be found in Table A4 in the Appendix.

First, to study worker mobility, I relate youths' cumulative exposure to the China import shock at the geographic and industry levels between 2001 and age 30 to their initial place of residence. The geographic mobility summary index combines four geographic mobility outcomes: average annual PNTR at the CZ level; has lived in a different state as 1997; has lived in a different CZ as 1997; and has lived in a different state as 1997. The industry mobility summary index consists of two outcomes: average annual PNTR at the industry level and has been employed in manufacturing. I also separately examine whether trade-exposed youths were more likely to be employed in occupations that were vulnerable to trade shocks as measured by an occupation summary index. The occupation summary index averages annual routine, social, and math skill intensities of employment. Occupation skill intensities are constructed using the 1998 Occupational Information Network (O*NET). Routine-intensive jobs are susceptible to offshoring, whereas social-skill and math-skill intensive jobs require localized knowledge and proximity to

14. The estimated effect of PNTR has the interpretation of the "intent-to-treat" (ITT) treatment effect.
15. The NLSY97 survey collects asset and home ownership information at five-age intervals starting at age 20.

other skill-intensive workers, making them more difficult to send overseas (Autor, Levy, and Murnane 2003; Autor and Dorn 2013; Deming 2017).

Next, the two risky behavior summary indices concern criminal and risky health behaviors. I use these two summary indices to document the undesirable worker responses that negatively influence educational attainment and future employment outcomes.[16] The criminal behavior summary index includes the incidence of arrests, incarcerations, and joblessness associated with incapacitation (has left work because of incapacitation; has not looked for work because of incapacitation). The risky health behavior summary index measures behavioral responses associated with "deaths of despair" (Case and Deaton 2017). The index includes four individual components: monthly alcohol consumption (days); monthly illegal drug use (times); monthly alcohol consumption before/during school or work (days); and monthly illegal drug use before/during school or work (days).

The life event summary index measures the extent to which childrearing-related factors affected young people's schooling and employment decisions. Prior work (Lindo 2010) shows that negative shocks to expected future earnings and lifetime returns on human capital investment can accelerate childrearing. Early childrearing increases family-related obligations and can widen the college attendance-attainment gap (Cohen, Brawer, and Kisker 2013). To assess these relationships, the summary index combines outcomes such as fertility choices as college-goers and joblessness resulting from childcare or family reasons (has left work because of childcare or family reasons; has not looked for work because of childcare or family reasons). I also include a measure of the intensity of childrearing activities using an indicator for whether respondents have had at least three children (one standard deviation above the mean) as college-goers.

Lastly, I measure young adults' chances for long-term economic success. Because the NLSY97 respondents were still relatively young in the last year of my survey (aged between 30 to 34 in 2015), I measure their lifetime earnings over the next few decades until retirement (assuming they retire at age 65) using a method analogous to Chetty et al. (2011). I create an adult economic success summary index using five outcomes positively associated with future earnings trajectories and a higher socioeconomic status (SES): total assets at age 30, has owned a home by age 30, has been married by age 30, has moved out of state by age 30, and lives in a higher SES community at age 30, which is measured by the percent of county population with college education.[17] I interpret the summary index as a broad measure capturing workers' early economic success in young adulthood.

16. As summarized in the literature review by Cawley and Ruhm (2011), although the relationships between some risky health behaviors and other labor market outcomes, such as wages and earnings, are ambiguous, their effects on educational attainment and employment are generally negative.

17. This measure is based on the 2000 Census and thus should be unaffected by PNTR-induced changes in local demographic composition.

2.3 International Trade

Credible estimates of the impact of trade requires a plausibly exogenous measure of trade exposure. To this end, my identification variation uses the Pierce and Schott (2016a) industry-level trade exposure to China's PNTR status, which went into effect at the end of 2001.[18] Beginning in 1980, the U.S. Congress began annually granting China normal trade relation (NTR) status. The annual renewal gave China access to the relatively low NTR tariff rates reserved for WTO members, but it also created substantial uncertainty surrounding future tariff rates. For instance, in 1999, a year before China's PNTR status, failure of renewal would have increased the mean tariff rates from 4 percent to 37 percent.

Recent empirical evidence (e.g., Pierce and Schott 2016a; Handley and Limão 2017) shows that although the U.S. tariffs applied on Chinese goods varied little prior to 2001, China's WTO accession—by eliminating the annual threat of high U.S. tariffs established under the Smoot-Hawley Act of 1930—was still able to substantially increase Chinese exports to the United States in the early 2000s. Figure 1 provides compelling visual evidence of PNTR's impact. It shows an inflection point in the relationship between U.S. manufacturing imports from China and U.S. manufacturing decline at precisely the time of the policy change, with the imports doubling as a share of the U.S. Gross Domestic Product in less than five years following trade liberalization. Both the size and the rapidity of the China import shock in the early 2000s visibly surpassed those of the Japan import shock in the 1980s and the Mexico import shock in the 1990s.

The regional specialization of manufacturing industries implies that the impact of PNTR on local employment should be relatively larger in CZs that specialized in industries with higher tariff uncertainty prior to the policy change. That is, high PNTR CZs should exhibit labor market trends similar to those of low PNTR CZs in the pre-PNTR period. Further, because they experienced relatively larger increases in trade, they also should display relatively steeper declines in manufacturing employment after the trade policy change. Using this intuition, I construct a measure of CZs' PNTR exposure in two steps. First, I reference Pierce and Schott (2016a) and measure industry-level PNTR exposure as the difference between the non-NTR and NTR tariff rates, the "NTR gap," in the year prior to the policy change. Formally, for an industry j, $\textit{NTR gap}_j = \textit{Non-NTR}_j - \textit{NTR}_j$. NTR gaps capture industry-level tariff uncertainty, as a higher NTR gap implies that the failure of NTR tariff renewal would have led to higher tariff hikes. Pierce and Schott (2016a) show that manufacturing industries with higher NTR gaps experienced significantly faster employment declines post PNTR. Second, I aggregate the

18. While there were other trade liberalization policies around the same time, such as the 1994 North American Free Trade Agreement (NAFTA), their trade liberalization effects were likely to be smaller. For example, in 1993, over half of the U.S. imports from Mexico were already entering the United States duty-free (Villareal and Fergusson 2017).

NTR gaps to the CZ level, weighted by local manufacturing employment composition in 1990:

$$PNTR_c = \sum_{j \in \text{Mfg}} \frac{Emp_{jc1990}}{Emp_{c1990}} NTR\ gap_j. \tag{1}$$

Local industry composition is compiled using 1990 County Business Patterns.[19] The base year is chosen to be a decade before the policy change to alleviate concerns about the correlation between local industry mix and contemporaneous shocks that affect local employment declines. The geographic distribution of exposure to PNTR is plotted in Figure 2. The figure shows high regional concentration of PNTR exposure in the Appalachian region in the Southeast and parts of the Rust Belt in the Midwest. I also report the twenty most and least trade-exposed CZs in Table A5.

The exogeneity of $PNTR_c$ relies on its orthogonality to unobserved local labor supply and labor demand shocks, which requires plausibly controlling for confounding drivers of local manufacturing decline. To this end, I include an extensive set of baseline 1990 CZ covariates, such as demographic and labor market composition, in all my regressions. Demographic information is calculated using the 100 percent Census data provided by the Missouri Census Data Center.[20] The list of variables includes log population; share of the population employed in manufacturing; share of the female population in the labor force; share of the population without a college degree; share of the population that is black, Asian, and of other races (Native American and Pacific Islander); share of population that is foreign-born; and average household income. To alleviate the concern that there may still exist spurious correlation between PNTR and the CZ controls, even though they are set in 1990, I show that the employment trends estimated without controls (Figure B1 in the Appendix) are qualitatively similar to the employment trends estimated using the full set of controls presented in the next section. Summary statistics of the CZ controls are reported in Table A2.

3 Trade-Induced Changes in Local Economic Conditions

In this section, I show that trade-induced local labor demand declines created strong economic incentives for young adults to go to college. Section 3.1 introduces the estimation equation and the identification assumptions; Section 3.2 presents evidence of the validity of those assumptions; Section 3.3 uses the Census/ACS data to document the negative impacts of the China import shock on young people's job opportunities.

19. Suppressed CBP employment counts are imputed using the method in Acemoglu et al. (2015).
20. These datasets can be found on the Missouri Census Data Center website: http://mcdc.missouri.edu.

3.1 Difference-in-Difference Methodology

I estimate how trade affects outcomes of interest between 1990 and 2015 using a difference-in-difference methodology. The estimation equation is given by

$$y_{crt} = \beta^{\text{DD}} PNTR_c \times Post_t + \gamma' X_{c,1990} \times Post_t + \varphi_{rt} + \varphi_c + \varepsilon_{crt}, \tag{2}$$

where y_{ct} is an outcome in commuting zone c at time t. Robust standard errors are clustered at the CZ level.

The regressor of interest is the interaction of $PNTR_c$ with the post-2001 dummy $Post_t$. This "dose-response" difference-in-difference specification allows PNTR to differentially affect CZs according to initial exposure intensity. The vector $X_{c,1990}$ includes CZs' demographic and employment composition in 1990, as discussed in Section 2.3. I also include region-by-year fixed effects (φ_{rt}) and CZ fixed effects (φ_c) to capture time-varying changes across Census regions (Northeast, Midwest, South, and West) and time-invariant differences across local labor markets, including $PNTR_c$, respectively.[21]

The difference-in-difference identification conditions for using PNTR-induced variation in trade exposure amounts to two assumptions. First, outcomes exhibit similar trends in the absence of the trade policy change. Second, no confounding labor demand or labor supply shocks coincide with the timing of the policy change. Although counterfactual outcome trends cannot be observed, I perform several event studies on local employment outcomes to show the absence of pre-event trends. Futher, by controlling for the initial manufacturing share of employment, identification comes from comparing high PNTR CZs to low PNTR CZs *with similar manufacturing intensities*. For confounding shocks to significantly bias my estimates, they would have to be both uncorrelated with local manufacturing employment share and coincide with the timing of the trade policy change. Below, I argue that alternative explanations such as productivity shocks are unlikely to drive the sharp breaks in the employment trends observed in the event studies.

My use of trade policy-induced local labor demand shocks for identification also resembles the growing number of "shift-share instrument" research designs. Although the difference-in-difference methodology is distinct from those research designs, the unconfoundness condition for using PNTR is related to the exogeneity conditions underlying the use of shift-share instruments. Recent studies on this topic (e.g., Borusyak, Hull, and Jaravel 2018; Goldsmith-Pinkham, Sorkin, and Swift 2018) make clear that instrument validity can be derived from either the orthogonality in the "shifts," in the "shares," or both, depending on the context of the study.

For my purposes, I argue that the orthogonality in the "shifts," that is, the orthogonality of

21. Results are similar when using Census division-by-year or state-by-year fixed effects (see Table B4).

industry-level NTR gaps to contemporary drivers of local manufacturing decline, is a plausible assumption. Under this identification condition, the shift-share instrument is valid when industry-level shocks do not correlate with other industry unobservables, such as technology shocks, or unobservables that correlate with local industrial composition. As demonstrated by Pierce and Schott (2016a), the NTR tariff rates varied little in the years leading up to the trade policy change. In fact, most of the variation in the NTR gaps comes from non-NTR tariff rates that were set by the Smoot-Hawley Tariff Act of 1930. This insight implies that the manufacturing decline induced by PNTR is a result of trade policies set over seventy years ago, which long predate the secular decline in manufacturing and the advancements in information and communication technologies, both of which began in the early 1980s. It is, therefore, difficult to imagine that the tariff uncertainty created by the NTR gaps somehow spuriously correlate with contemporaneous industry-level confounders in the manufacturing sector. Lastly, the "shift" in my instrument is induced by a national trade policy change, making it likely to be exogenous to local confounding shocks.

3.2 Local Labor Market Effects

Before turning to Census/ACS results, I first establish PNTR's adverse employment effects on local economies and check the validity of my identification assumptions.[22] For this analysis, I collect annual local employment data from the 1990–2015 Quarterly Census of Employment and Wages (QCEW) and Local Area Unemployment (LAU). Table 1 presents the employment results. Outcomes are scaled (multiplied by 100) such that the coefficients have the interpretation of percent, percentage point, or per capita changes. Further, because a unit of PNTR has no direct economic interpretation, the discussion hereinafter focuses on the implied effects of a one standard deviation increase in PNTR (2.78 percentage points), which are reported in brackets.[23]

The estimates indicate that the China import shock significantly reduced local employment opportunities. I estimate a one standard deviation increase in PNTR changed total employment by -4.38 percent, with the strongly adverse effects in the manufacturing sectors (-6.53 percent) and smaller and less precisely measured, though still negative, effects in the nonmanufacturing sectors (-1.05 percent). The implied effects of a similar-sized increase in PNTR on (log) unemployment, the unemployment rate, and the labor force-to-population ratio are 2.46 percent, 0.42

22. Several such relationships between local employment and trade have been examined in detail in prior studies (e.g., Autor, Dorn, and Hanson 2013; Pierce and Schott 2016b), I report the analysis here for completeness. Differences in the measurement of trade exposure, treatment units, and sample periods complicate direct comparison of the employment estimates between studies. Nonetheless, the implied effects of my estimates are relatively similar to the estimates in Pierce and Schott (2016b), whose methodology most resembles mine.

23. This magnitude is similar to the size of the interquartile range of PNTR in my sample (about 2.70 percentage points), another commonly used scale in the literature.

percentage points, and -0.52 per capita, respectively.[24]

Figure 3 shows that the employment results are not driven by secular local labor market trends, which could invalidate the causal interpretation of my results. The graphs plot the estimated coefficients from an event study model, in which I replace the post-2001 dummies in Equation (2) with year dummies and normalize the coefficient associated with 2000 to be zero. All of the graphs either show no pre-event trends or trends in the opposite direction that disappeared several years before the trade policy change.

One remaining concern is potential confounding shocks to the manufacturing sector. The timing of China's WTO entry raises the possibility that my estimates are contaminated by the effects of the tech bust in the early 2000s. Changes in other trade policies during the same period, such as changes in the NTR tariff rates or the phase-out of MFA import quotas, may also influence my estimates. Lastly, the Great Recession of 2008 could have led to differential labor market trends not captured by my current set of controls. I follow the literature (e.g., Pierce and Schott 2016a; Charles, Hurst, and Notowidigdo 2018) in addressing these concerns: I remove high-tech industries that were likely exposed to the dot-com bubble in the measurement of PNTR; I control for annual NTR tariff rates and MFA fill rates; I control for CZ exposure to the housing boom using structural breaks in local housing prices.[25] These estimates, shown in Table B2 in the Appendix, are comparable to my preferred labor market estimates.

3.3 Opportunity Costs and Expected Lifetime Earnings

I now show that trade led to the disappearance of both current and future employment opportunities for young people, especially for those without college education. For this analysis, I rely on the Census/ACS data because they contain worker demographics, such as age and educational attainment, that are necessary for my purposes. The estimation equation is the same as Equation (2). I measure the opportunity costs of going to college using the labor market outcomes of noncollege workers in the same age group (18 to 34 year-olds). This choice follows from the assumption that those workers' labor market outcomes reflect the foregone employment opportunities and earnings of marginal students deciding whether to exit the local labor force in pursuit of a college degree. To measure young adults' expected future earnings, I assume young adults approximate their labor market trajectories using the current labor market outcomes of

24. The labor force-to-population ratio is calculated as the ratio of the labor force to 18 to 64 population. Because Hurricane Katrina interrupted LAU data collection between 2005 and 2006, in my LAU analysis I exclude all CZs that contained counties with missing unemployment statistics.

25. The Census identifies ten industries as Advanced Technology Production industries: biotechnology, life science, opto-electronics, information and communications, electronics, flexible manufacturing, advanced materials, aerospace, weapons, and nuclear technology. Fill rates are actual imports as a percentage of allowable imports under the MFA import quotas. They can be interpreted as a measure of the extent to which the quotas were binding (Pierce and Schott 2016a).

older workers in the same CZ.[26] I focus on the labor market outcomes of prime-aged workers (aged 35 to 54) as they have higher labor force attachment than older cohorts.

The results in Table 2 show that noncollege workers, both young and old, experienced strongly adverse trade-induced labor market impacts. Column 1 and 2 show that PNTR lowered young people's employment rate (-1.38 percent point) and wages (-2.00 percent).[27] The next two columns report the same outcomes for prime-aged noncollege workers. The employment and wage estimates in Column 3 and 4 are generally comparable to those in the first two columns, also showing strong and statistically significant negative effects.

Further, I show that college education investment had significant and positive long-term returns, especially on the employment margin. The outcomes in Column 5 and 6 are the college-noncollege employment and wage gaps among prime-aged workers. The estimates reflect the relative changes in the labor market outcomes of college and nocollege workers in a particularly CZ. I estimate a one standard deviation increase in PNTR significantly increased the college-noncollege employment gap by 0.33 percentage points. The effect of a similar-sized increase in PNTR on the college-noncollege wage gap is essentially zero, indicating no statistically discernible differences between the changes in the college premium in trade-exposed locales and elsewhere. As the national college-noncollege wage gap rose precipitously over my sample period, one interpretation of this result is that I cannot rule out possible long-term wage gains from going to college for young workers without college education in trade-affected communities.

4 Effect of Trade on Human Capital Accumulation

Falling foregone earnings of college attendance and rising college labor market premium should have created upward pressure on college enrollment and attainment in trade-exposed locales. In particular, college attainment as a lever for upward economic mobility has been widely recognized. The positive impacts of college degree receipt also extend to geographic mobility, especially over long distances (Wozniak 2010). Increased labor flows can help mitigate trade's negative labor market effects that arise from regional industry specialization. In this section, I use the Integrated Postsecondary Education System (IPEDS) data to study how the import shock changed the stock of local human capital.

26. Charles, Hurst, and Notowidigdo (2018) make similar assumptions.

27. The wage results are based on employed workers with identifiable occupations and had positive wages in the previous year. This sample selection likely *attenuates* my estimates; that is, my estimates can be interpreted as the lower bounds of the true adverse wage effects.

4.1 College Enrollment and Attainment

My analysis in this section shows that the import shock had uneven effects on human capital accumulation and ultimately failed as a lever to raise the educational trajectories of young people. In particular, I do not find any evidence of college attainment effects despite some increases in college enrollment. As most of the returns on education investments, especially in the long run, are contingent upon degree completion, enrollment without completion is arguably an undesirable outcome, particularly if enrollment not only led to forgone earnings but also increased student debt.

Table 3 reports the IPEDS results estimated using Equation (2). Column 1 to 3 report the results for total college enrollment and separately for two-year and four-year colleges. The estimates show that rising Chinese import competition increased total enrollment, with the effect concentrated at two-year colleges. A one standard deviation increase in exposure to PNTR increased total enrollment by 0.11 per capita, which translates to a modest 4 percent increase relative to the sample mean. A similar-sized increase in PNTR raised two-year enrollment by 0.09 per capita, or a 7 percent increase. By contrast, the effect on enrollment at four-year colleges is about half the size (0.04 per capita, or a 3 percent increase).[28] The next three columns report the results for college completion. The estimates range from -0.04 to 0.07 per capita, or between -3 and 4 percent changes, but they are generally smaller and very imprecisely measured.

Figure 4 plots the estimated coefficients from the event study models. The graphs provide visual evidence of the enrollment and completion effects documented in Table 3. First, none of the event study graphs exhibits differential trends in schooling outcomes over the entire decade prior to 2000. Second, Panel A and B, respectively, show clear upward trends in total and two-year enrollment per capita after 2000. None of the other figures exhibits evidence of treatment effects in either direction.

As public colleges subsidize in-state college enrollment with lower tuition fees, the skill acquisition response may be larger at public colleges than at private colleges. If such differential effects exist, then the capacity of public colleges to absorb the increase in demand for education and provide financial support for large cohorts can significantly impact the size of the human capital response (Bound and Turner 2007; Bound, Lovenheim, and Turner 2010). In Table 4, I show that the positive two-year enrollment effect is concentrated at public colleges, especially at two-year institutions, though I also observe a small and positive four-year enrollment effect. By contrast, the results on private colleges are much smaller in magnitude and not statistically

28. In general, my enrollment estimates yield similar conclusions as Charles, Hurst, and Notowidigdo (2018). My findings, as do theirs, suggest that the relationships between college enrollment and local employment opportunities differ significantly by college institution level. In the Appendix, I also show that, like Charles, Hurst, and Notowidigdo (2018), I fail to find any gender differentials in the college enrollment and college attainment effects.

significant.[29] Lastly, even after separating by postsecondary sector, I still fail to find any statistically significant or economically meaningful college attainment effects.

4.2 Are There Short-Term Gains from College Education?

Overall, my IPEDS results suggest that there was a statistically significant, albeit small, college enrollment response to trade exposure. However, the lack of any completion effects makes it unclear whether college attendance improved the labor market outcomes of young workers. To answer this question, I again turn to the Census/ACS data. This time I examine the labor market outcomes of 25–34 year-old college workers, that is, the age cohort of recent college graduates. Their labor market outcomes arguably provide an estimate of the short-term gains to college education.

In Table 5, I show that trade had strongly significant and negative effects on the employment and wages of young college workers, especially on college dropouts. I report the results separately for college dropouts and college graduates (those with at least a bachelor's degree) separately. The estimates in Column 1 and 2 indicate that PNTR significantly lowered the employment rates of young college dropouts and young college graduates by -0.72 and -0.44 percentage points, respectively. The next two columns show that young college workers had significantly higher employment rates than their noncollege counterparts, with the employment gap being smaller for college dropouts (0.53 percentage points) than for college graduates (0.81 percentage points), though the difference is not statistically distinguishable.

I also show that PNTR had *negative* effects on the college-noncollege wage gap for young workers. Those negative results indicate that college attendance, especially without college degree receipt, unlikely led to significant *short-term* wage gains. The estimates in Column 5 and 6, though not all consistently precisely measured, show that the negative effects on the college-nocollege wage gaps were -0.67 and -0.23 percent for dropouts and graduates, respectively. The negative effects on college premium go against the secular rise in returns to college degree receipt at the national level. Nevertheless, they are consistent with trade-induced displacements of workers from the relatively high paying manufacturing jobs to the low paying low-skilled service

29. In Table B9 in the Appendix, I provide suggestive evidence that public institutions' reliance on public funding makes them vulnerable to economic shocks and compromises their ability to absorb trade-induced increases in demand for college education. Because finance outcomes are less consistently observed than enrollment and completion in IPEDS, to increase the precision of the estimates I restrict the sample to a balanced panel of institutions between 1990 and 2015 with nonmissing outcomes in the main finance categories: revenues from federal, state and local sources, revenues from tuition, total education expenditures, and instruction expenditures. I find that there are negative relationships between PNTR and public college funding, mostly driven by the declines in state and local funding. The estimates on revenues from tuition and education and instruction expenditures are less precisely measured and either close to null or slightly negative. The results in Table B9 are consistent with trade-induced declines in state and local tax base as documented by Feler and Senses (2017).

occupations, which led to a narrowing in the college-noncollege wage gap among young workers in trade-exposed locales. They are also consistent with the stagnant job and wage growth in areas that were most impacted by the China import shock, which created local economic conditions akin to recessions (Autor, Dorn, and Hanson 2016).

In Table B3 in the Appendix, I perform additional analyses with the Census/ACS data to validate my findings in this section. First, I find that the import shock increased the supply of potential college-goers as measured by the percent of 18 to 19 population that is a high school graduate.[30] Next, I show that trade simultaneously increased the share of college dropouts and decreased the share of college graduates among young people aged 25 to 34. These findings are consistent with my IPEDS results. Lastly, I show that PNTR's negative effects on college workers wages and college premium remain quantitatively similar when estimated using earnings, indicating that changes in workers' labor supply are not driving my Census/ACS results.

5 Other Outcomes and Adult Economic Success

Deteriorating local economic conditions and the lack of educational adjustments by young adults, at least in any economically meaningful way, create a puzzle. The findings naturally lead to the consideration of young adults' alternative pathways of trade adjustment. In the rest of the paper, I employ the 1997 sample of the National Longitudinal Survey of Youth (NLSY97) data to provide evidence for potential mechanisms.

5.1 NLSY97 Methodology

The econometric model in this section is similar to Equation (2), with some slight modifications. The changes are necessary because of two important differences between the NLSY97 data and the previous datasets. First, because NLSY97 respondents became of college age around the time of the trade policy change and because I examine their long-term outcomes, the respondents in my sample all eventually became exposed to PNTR during the sample period. Second, a well-known weakness with the NLSY97 dataset is its small sample size. After restricting the sample to respondents with nonmissing education, labor market, and summary index outcomes, my NLSY97 sample consists of 6,772 respondents and covers approximately 20 percent of the 722 available CZs.[31] The issue of the small sample size is exacerbated by the geographic con-

30. The estimated increase associated with a one standard deviation change in PNTR (0.35 percentage points) is smaller than what Greenland and Lopresti (2016) found in their study. However, a direct comparison of the magnitude is complicated by the different datasets and estimation methodologies used in the two studies, though the qualitative conclusion remains similar.

31. That is, I keep the NLSY97 respondent if any of the individual components of a summary index is nonmissing. Restricting the sample to only respondents without any missing outcomes decreases the sample size to $N = 6,147$; this

centration of PNTR exposure and manufacturing intensity; in the NLSY97 data, the correlation coefficient between the two variables is over 0.77. Together, these data features raise concerns about low statistical power when I include both variables in the econometric model.

My preferred approach for addressing the NLSY97 data issues is to relate the evolution of workers' outcomes to the cross-CZ variation in exposure to PNTR based on respondents' place of residence in 1997. I use two methods to measure PNTR exposure. The first method applies the continuous measure $PNTR_c$ as in the previous sections. The second method applies a binary measure that indicates whether the respondent lived in a high PNTR CZ as measured by whether the CZ is in the top third of PNTR distribution. Using the indicator variable increases the precision of the estimates and addresses the possibility of low statistical power arising from the high correlation between $PNTR_c$ and manufacturing intensity. As a robustness check, I also estimate models using indicators for both the middle and top-third PNTR CZs. The results from these regressions, which are discussed in the next section, indicate that the adverse effects of trade are concentrated in the most exposed CZs. Guided by this finding, I prefer to use the top tercile as my threshold, as opposed to alternative thresholds (e.g., the median), which would attenuate the estimates.

Formally, the estimation equation is of the following form:

$$y_{icrt} = \alpha + \beta^{\text{NLSY}} Exposure_c + \zeta' X_{c,1990} + \delta' D_i + \phi_{rt} + \varepsilon_{icrt}, \tag{3}$$

where y_{icrt} is an outcome of interest for respondent i in CZ c in region r at time t (year of age 30 survey round). Most of the outcomes are measured cumulatively or averaged annually from 2001 to the first survey round after respondents have turned 30. In some cases, I measure outcomes between 2001 and age 25 and between ages 25 and 30. Compared to using cross-sectional data, using the full history of information spanning over a decade arguably improves the measurement of workers' long-term behavior. The variable $Exposure_c$ is either the continuous or the binary measure of PNTR exposure.

The vector X_{ct} contains all the 1990 CZ characteristics from Equation (2). In addition to the CZ covariates, I include an extensive set of individual baseline controls to adjust for cross-individual differences in characteristics. The vector of individual controls D_i includes indicators for demographic information (e.g., age, sex, black, Hispanic), family structure (e.g., two-headed household, single-father household, single-mother household, other household types), parents' educational attainment (e.g., less than high school, high school, some college, college graduates, ungraded/missing), and mother's age when the individual was born (e.g., 23 and younger, between 24 and 28, 29 and older, missing). I also include controls for individuals' household

sample restriction does not change my results significantly.

income in 1996 and Armed Forces Qualification Test (AFQT) z-scores.[32] In some of the robustness checks, I further control for baseline peer and school-related factors and young adults' noncognitive skills. In instances in which control variables have missing values, I code the values as zero and create indicator variables documenting the missing information. Finally, I include region-by-year fixed effects (ϕ_{rt}).

The main identification assumption is that exposure to PNTR is exogenous conditional on the vector of observables. Besides the extensive set of individual baseline controls, Equation (3), as in the previous sections, includes CZs' manufacturing intensity; thus, confounding economic shocks to the manufacturing sector is not a big concern. Nevertheless, two threats to identification remain. One worry is potential selection on unobservables, such as unobserved skill differentials that influence school behaviors. The second concern is the possibility of regional economic shocks outside of manufacturing industries, such as the Great Recession. In the next section, I present evidence showing that these factors are unlikely to be driving the results.[33]

5.2 Comparison with Census/ACS and IPEDS Results

Because of the differences in the samples and methodologies, I first validate my previous schooling and labor market results with the NLSY97 data in Table 6. Broadly, the NLSY97 results are comparable to the Census/ACS and IPEDS results. Panel A of Table 6 reports the estimates using the continuous measure of PNTR exposure; Panel B, the binary measure; Panel C, indicators for middle-third and top-third PNTR CZs.[34] The outcomes in the first four columns are workers' college enrollment and attainment by institution level; the outcomes in the last two columns are workers' hourly wages when not enrolled in school and whether they have ever received unemployment insurance, a measure of worker displacement.[35]

Although the estimates in Column 1 to 4 of Panel A are somewhat noisy, they generally indicate no significant skill acquisition responses, especially on the college attainment margin. The implied effect on two-year college enrollment translates to a marginally significant 8 percent increase relative to the sample mean, which is comparable to the implied effect of the IPEDS estimate (a 7 percent increase). The other education estimates in Panel A are either null or noisy,

32. AFQT test scores approximate cognitive skills and have been widely used in the literature as a proxy for scholastic ability (e.g., Cameron and Heckman 2001; Lovenheim and Reynolds 2011). AFQT is computed using standard scores from four components: arithmetic reasoning, mathematics, knowledge, paragraph comprehension, and word knowledge. The tests are designed to help "predict future academic and occupational success in the military." See http://official-asvab.com/index.htm for details.

33. As mentioned earlier, I do not have unexposed NLSY97 cohorts and thus cannot estimate event study models.

34. The standard deviation of the PNTR variable in the NLSY97 is 3.28 percentage points.

35. I use the receipt of unemployment insurance as a measure of unemployment because NLSY97 only reported the labor force status of the respondents based on the Current Population Survey questions for years 1997, 2000, and 2006.

similar to the IPEDS findings. The estimates in the next two columns, though also imprecisely measured, show that PNTR had negative labor market effects.

Turning to Panel B, the estimates are generally qualitatively similar to those in Panel A, showing no attainment effects and strong negative labor market effects. One exception is the finding associated with two-year college enrollment (Column 1), in which the Panel A and Panel B coefficients differ in sign. Heterogeneous enrollment effects across the PNTR distribution provide a possible explanation. In Panel C of Table 6, I test this hypothesis by using a more flexible econometric specification that allows for differential effects. I find that the two-year college enrollment effect is larger for young adults who lived in the middle-third PNTR CZs (3.24 percentage points, $t = 2.02$) than those who lived in the top third (1.09 percentage points, $t = 0.45$), though the difference is not statistically distinguishable. Further, the estimates in Column 5 and 6 of Panel C show that the adverse labor market effects of trade are concentrated in most PNTR-exposed CZs. Severe economic deterioration in those local labor markets could have created negative social and fiscal spillovers that impeded educational adjustments. Nonetheless, even with this more flexible specification, for the most part, I cannot reject that the high PNTR CZ estimates in Panel C are statistically different from the implied effects of the Panel A estimates or the Panel B estimates.

Table B10 in the Appendix reports the results from several robustness checks. The results are generally quite similar to my preferred estimates.[36] For ease of comparison, Panel A of Table B10 reports my preferred estimates from the main article. In Panel B of the same table, I additionally control for several skill measures and contextual variables. Skill measures, which include social and noncognitive skills, act as proxies for individual-level unobservables.[37] Contextual variables, which include measures of peer and school factors in 1997, serve as proxies for unobserved youth environmental factors that influence schooling and employment decisions.[38] Research has shown the significant positive associations between these variables and academic and labor market performance (Heckman, Stixrud, and Urzua 2006; Chetty et al. 2011; Deming 2017). In Panel C of Table B10, I exclude individual baseline controls from the regressions.

36. To save space, I only report the results for the binary PNTR measure. I have also checked the robustness of the continuous PNTR measure. The robustness results are very similar to the estimates in Panel A of Table 6.

37. The definition of these skills follow Deming (2017). Social skills average extroversion and reservedness. Noncognitive skills are measured using seven personality trait measures: disorganized, conscientious, undependable, thorough, trusting, disciplined, and careless. Some z-scores (disorganized, undependable, carless) are rescaled such that higher values indicate more desirable personality traits.

38. The peer summary index averages the z-scores of the percent of school peers who participate in church going, smoking, getting drunk, sports, gang, volunteering, using drugs, and skipping class, and those who plan to go to college. The school summary index averages the z-scores of school-related characteristics (frequency of having something stolen; frequency of being threatened; frequency of being in fights; frequency of being late without an excuse; frequency of being absent) and respondents' attitude toward school ("teacher is good," "teacher is interested in students," "students disrupt learning," "grade is fair," "students cheat on homework," "discipline is fair," "feels safe in school"). As before, the z-scores of negative school characteristics and respondent attitudes are multiplied by -1.

This exclusion tests the significance of demographic and household characteristics in driving the results. Even with a battery of tests, I fail to find any evidence of significant changes in the results. In Panel D and E of Table B10, I test for the existence of confounding economic shocks by separately controlling for the CZ unemployment rate at age 19, the modal age for high school degree receipt, and CZ exposure to the housing boom.[39] Again, I fail to find any significant changes in the estimates. For confounders to significantly bias my results, they would have to be uncorrelated with the rich set of observables, several proxies for individual and environmental unobservables, local unemployment rate when youths first became of college-age, and one of the largest economic shocks during the sample period, and yet spatially correlated with youths' place of residence in 1997. Although spurious correlation remains possible, the evidence from the robustness checks suggests that this is unlikely.

Lastly, I present further evidence of trade's effects on workers' transition from school to work by relating PNTR to the timing of schooling and employment choices. I separate the young adults' outcomes into two sub-periods: from 2001 to age 25 and after age 25. The first sub-period approximates the period of on-time college attendance; the second sub-period, the early-career employment of recent college graduates. Because the returns to human capital investment compound over time, and adjustment frictions increase with age, enrollment during the on-time college attendance period is arguably a more desirable outcome. On the contrary, I find that exposure to rising trade pressures from China is associated with "delayed" college and earlier onset of adverse economic outcomes. The results in Table B11 in the Appendix show that PNTR exposure is linked to adverse schooling and labor market outcomes between 2001 and age 25, including being less likely to have enrolled at four-year colleges, had lower wages, and being more likely to have received unemployment insurance. The results also show that the same workers were more likely to have enrolled at two-year colleges after 25. The findings, though not consistently precisely estimated, provide supportive evidence for the claim that the trade shock was a lever for raising college enrollment, though not college attainment.

5.3 Worker Mobility and Cumulative Exposure to Trade

Such significant geographic disparities in young people's prospects and educational trajectories suggest the existence of substantial geographic and labor market frictions. Table 7 assesses this possibility by studying workers' cumulative exposure to PNTR across multiple dimensions. The outcomes are standardized summary indices multiplied by 100. As such, the coefficients can be interpreted as percentage changes relative to the standard deviation (which is one by design).

39. Analogous to Charles, Hurst, and Notowidigdo (2018), I use structural breaks in housing prices to provide an exogenous measure of the effect of housing boom. Structural breaks in CZ housing prices are made publicly available by Greenland, Lopresti, and McHenry (2018).

Larger positive values indicate "better" outcomes. Total mobility summary index combines mobility at the geographic and industry levels, i.e., the labor market adjustment channels associated with trade. The results for the individual components of the summary indices are reported in the Appendix. Compared to the summary indices, the effects of PNTR on the individual components are less precise but generally have a similar interpretation.

Absent any significant geographic or sectoral frictions, as assumed in many standard long-run models of trade, I am unlikely to find any meaningful relationships between the China import shock and young people's cumulative exposure. On the contrary, I consistently find that the import shock had large and significant negative impacts on worker mobility. The estimates in Column 1 of Table 7 show that youth exposure to PNTR was strongly associated with less total mobility and higher cumulative exposure from 2001 to age 30, with implied effects ranging between -29.19 and -33.29 percent changes relative to the standard deviation.

The estimates in Column 2 to 4 indicate that the large cumulative trade exposure mostly stems from the significant geographic and industry-level frictions, especially the former. The implied effects of PNTR on geographic mobility are between -28.56 to -35.33 percent changes relative to the standard deviation. In addition, Column 3 shows that trade-exposed young adults were significantly less likely to have ever left their 1997 county of residence by age 30 (a change of -3.60 to -4.85 percentage points).[40] The estimates in Column 4 indicate that the effect of PNTR on industry mobility was smaller but still significant and ranged between -13.58 and -14.31 percent relative to the standard deviation.

The estimates in Column 5 show that PNTR also negatively affected the skill content of young people's employment between 2001 and age 30. Although the implied effects are negative, they are much smaller (between -2.79 and -5.41 percent) and less precisely measured. The negative coefficients suggest that young adults were much less likely to be employed in math-skill and social-skill intensive occupations. Rather, they were more likely to be employed in routine-intensive occupations that had become susceptible to technology shocks and offshoring. The smaller magnitude of the coefficients in Column 5 compared to the coefficients in Column 2 is consistent with the industry-level concentration of trade shocks, which pervade occupation and skill levels (Autor, Dorn, and Hanson 2015). The evidence is also consistent with employment losses in the manufacturing sector, particularly after 2000, being increasingly concentrated in plant closures (Holmes and Stevens 2014; Asquith et al. 2019), making the impacts of trade industry-specific rather than occupation-specific.

40. I also find that the low geographic mobility in trade-exposed regions extends to cross-CZ and cross-state moves, though those estimates are less precisely measured (see Table B12). The imprecision of those estimates reflect the mixed evidence on the effect of Chinese import competition on population adjustments.

5.4 Risky Behaviors, Life Events, and Adult Economic Success

Without significant population adjustments or increases in the local human capital stock, trade-affected communities likely experienced a persistent deterioration in local economic conditions and a steady rise in long-term unemployment and joblessness.

I show that, faced with the bleak economic circumstances, young adults adopted unappealing adjustment mechanisms, including engaging in crime and risky health behaviors. The participation in those activities not only were unlikely to improve young adults' current labor market outcomes but also likely had scarring effects on their prospects for employment. The estimates in Table 8 indicate that PNTR's effects on criminal activities (Column 1) and risky health behaviors (Column 2 and 3) were as large as -11.20 percent and -12.22 percent relative to the standard deviation, respectively. The coefficients on the individual components confirm these findings. In Table B13, I show that trade significantly raised the incidence of youth arrests, incarceration, and joblessness from incapacitation. I also find strong effects of PNTR on intensive alcohol consumption and illegal drug use, including consumption of such substances before/during school and work (see Table B14). The link between PNTR and other risky health behaviors, such as cigarette smoking and marijuana use, is less strong. This evidence is consistent with that of Pierce and Schott (2016b), who find that PNTR is generally positively associated with increases in alcohol and drug-related mortality.

The findings on young adults' risky behaviors starkly contrast with their life event outcomes. In Column 4 of Table 8, I fail to find any evidence of significant family-related adjustment frictions, such as an increase in childrearing responsibilities as college goers. Table B15 in the Appendix also presents evidence to suggest that family-related responsibilities likely played a less significant role in young adults' slow skill acquisition and persistent joblessness.[41] The imprecision of my estimates may reflect the countervailing effects of the China import shock on marriage, maternity, and fertility. In particular, Autor, Dorn, and Hanson (2017) show that a negative shock to the market value of men deterred family formation and fertility for women aged 18 to 39, whereas a negative shock to women's own labor market outcomes had the opposite effect.

Lastly, from a policy perspective, we are interested in how trade impacts the long-term economic success of young adults. Whether they can find jobs and become self-sufficient adults has significant implications for both the duration and intensity of young adults' reliance on public assistance. Persistent jobless among young people also leads to sustained declines in state and local funding for public goods and services, such as public education, which can further stunt the educational growth of future generations of workers. In the final column of Table 8, I present

41. I have also examined whether teen pregnancy played a role by examining the relationship between PNTR exposure and mothers' age at the birth of first child. The result did not support this explanation.

direct evidence on the potential long-term effects of trade by examining NLSY97 respondents' economic success at early adulthood. I assume that early adult economic success at age 30 is a good predictor of future economic success. This assumption is supported by the empirical evidence that shows income ranks within cohorts stabilize around age 30 and thus unlikely to suffer from lifecycle bias (Chetty et al. 2014). Further, the China import shock had largely disappeared by the early 2010s when the outcomes are measured. Therefore, assuming workers' earnings trajectories continue on the same paths over the next thirty or forty years until retirement, the estimates in Column 4 of Table 8 show that exposure to the import shock strongly reduced young people's chances of future economic success, even more than a decade after the trade policy change. The implied impacts range between -4.26 and -6.43 percent relative to the standard deviation, though not all consistently precisely measured. Together, the results in this section suggest that the geographic disparities in economic opportunity and economic mobility will continue to diverge and the disruption effects of trade liberalization may be more enduring than commonly recognized in the literature.

6 Conclusion

As the transformation to a service economy accelerates, low-skilled workers in the United States must adjust to the rapid changes in labor demand or risk being left behind. Evidence suggests that large frictions in the labor market have prevented the smooth employment transitions of displaced workers and contributed to almost a decade of anemic employment growth at the turn of this century. The impetus for this paper is to examine whether young adults, who have more margins of adjustment than older workers, are able to acquire new skills and respond to these changes.

By examining a wide array of outcomes—ranging from college attainment to risky behaviors to life events—I find robust evidence to suggest that various non-labor market frictions likely prolonged the slow skill acquisition response of young workers and amplified the negative labor market effects of trade. In particular, I show that while it induced young people to attend college, rising import competition failed to raise educational attainment. Further, I find that youth exposure to trade not only lowered worker mobility across multiple dimensions but also led to a number of detrimental risky behavioral outcomes. Lastly, I show that trade-exposed workers' future economic success is expected to change by -4.26 to -6.43 percent over the next few decades, widening the current geographic disparities in economic outcomes.

The implications of my findings raise concerns over not only the regressive distributional consequences of rapid trade liberalization but also their durability and possible transmission across generations. To effectively address the trade-induced regional divergence in economic opportunity and to raise upward economic mobility, my results suggest that policymakers must consider a broad range of noneconomic factors, in addition to the income maintenance of trade-affected workers. Those factors include the significance of non-labor market frictions and the potential declines in funding and student resources at public schools and colleges. My results also indicate that trade adjustment policies, which currently target trade-affected workers and firms, could target distressed communities as a whole; in particular, the policies could expand coverage to include young people who are not directly impacted by trade. But perhaps more important, as my findings suggest that even college-aged youths struggled to adjust to the rapid changes in labor demand, trade adjustment policies could also include programs that invest in the early human capital development of young children in distressed communities. These are important avenues for future research.

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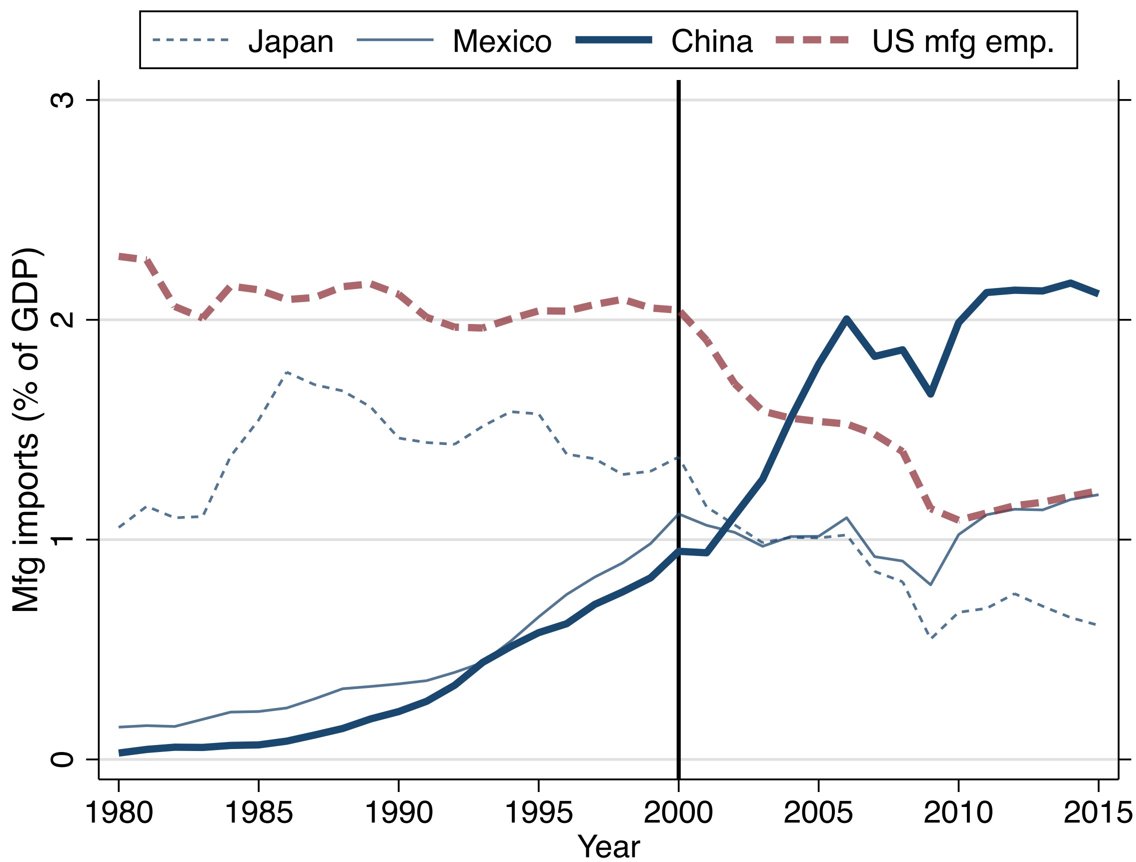

FIGURE 1. Relationship Between U.S. Imports from China and U.S. Manufacturing Employment, 1990–2015

Source: Author's calculations from the U.S. Census and Burea of Labor Statistics Data.

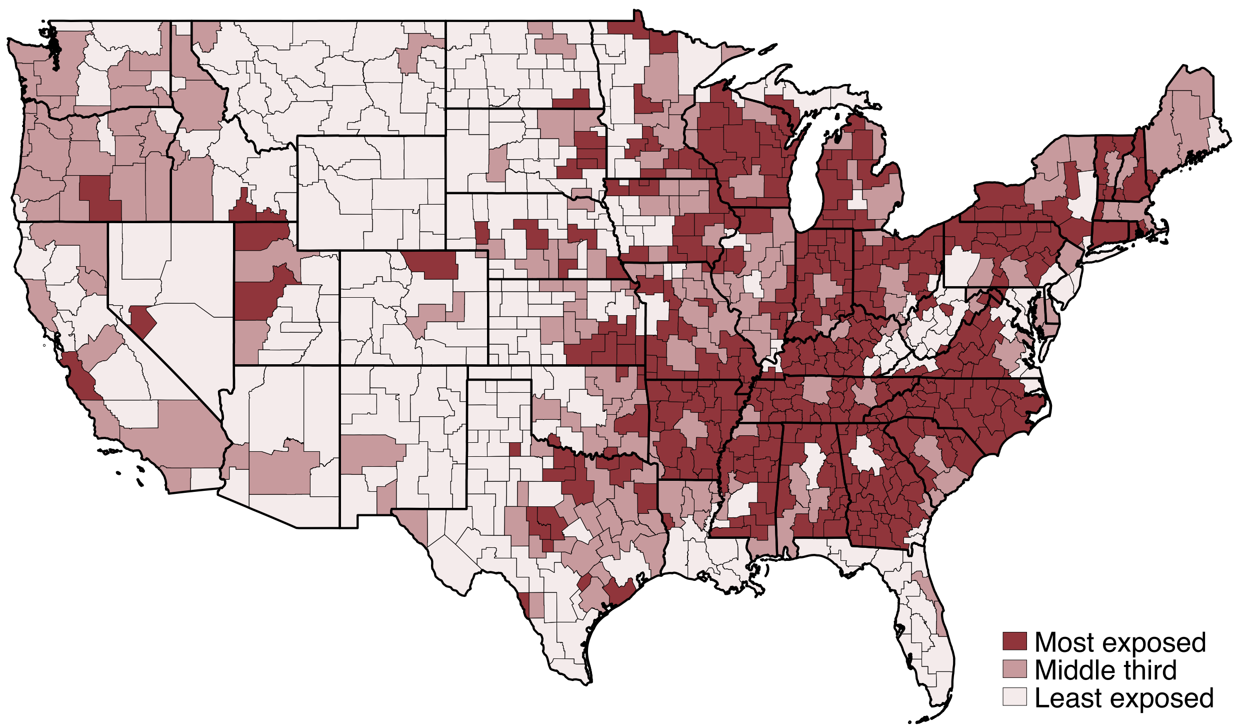

FIGURE 2. Geographic Distribution of PNTR Exposure.

Source: Author's calculations from the 1990 County Business Patterns and the Pierce and Schott (2016a) NTR Gaps.

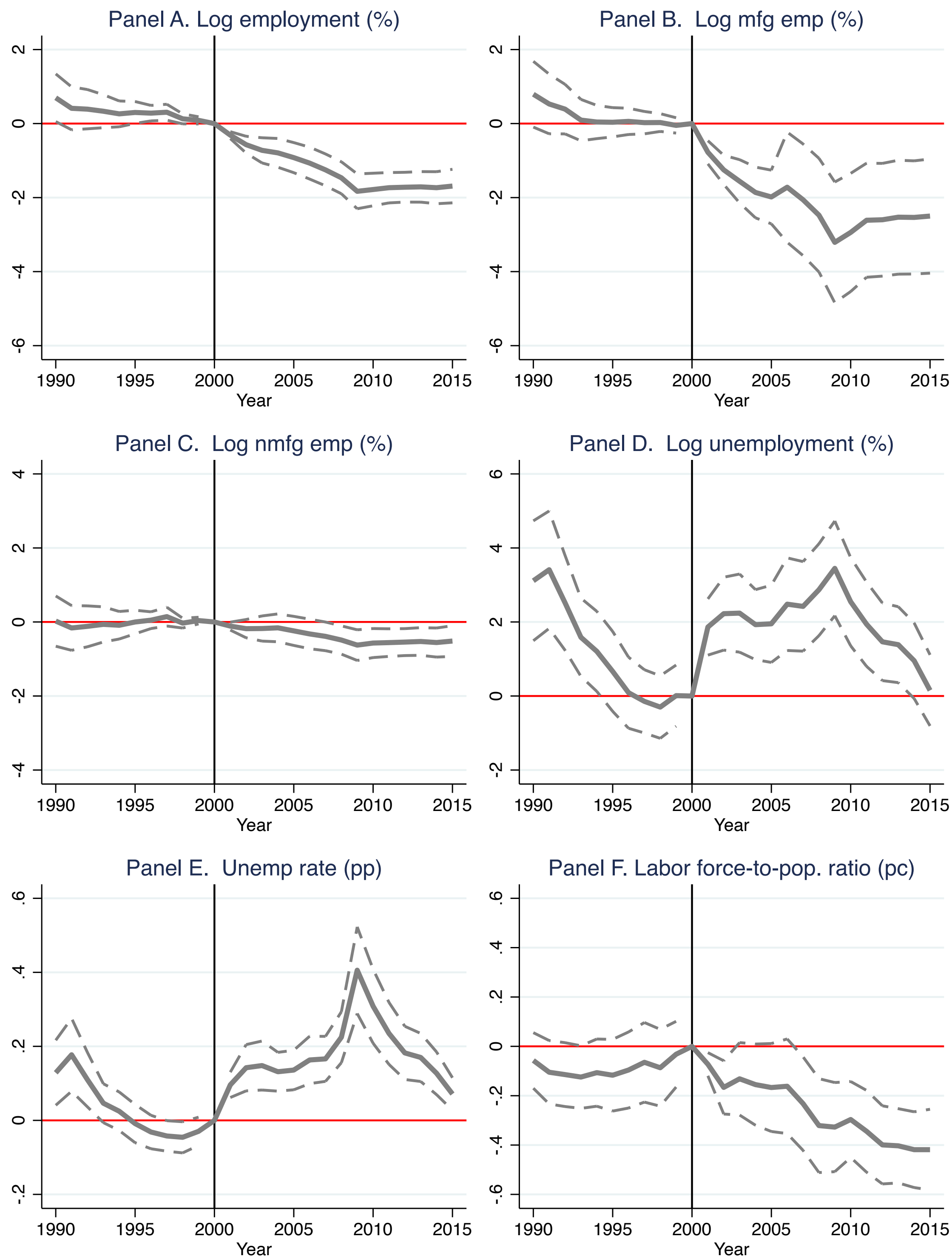

FIGURE 3. Relationships Between PNTR and Local Employment Trends

Source: Author's calculations from the 1990 to 2015 Bureau of Labor Statistics, Local Area Unemployment, and Quarterly Census of Employment and Wages Data.

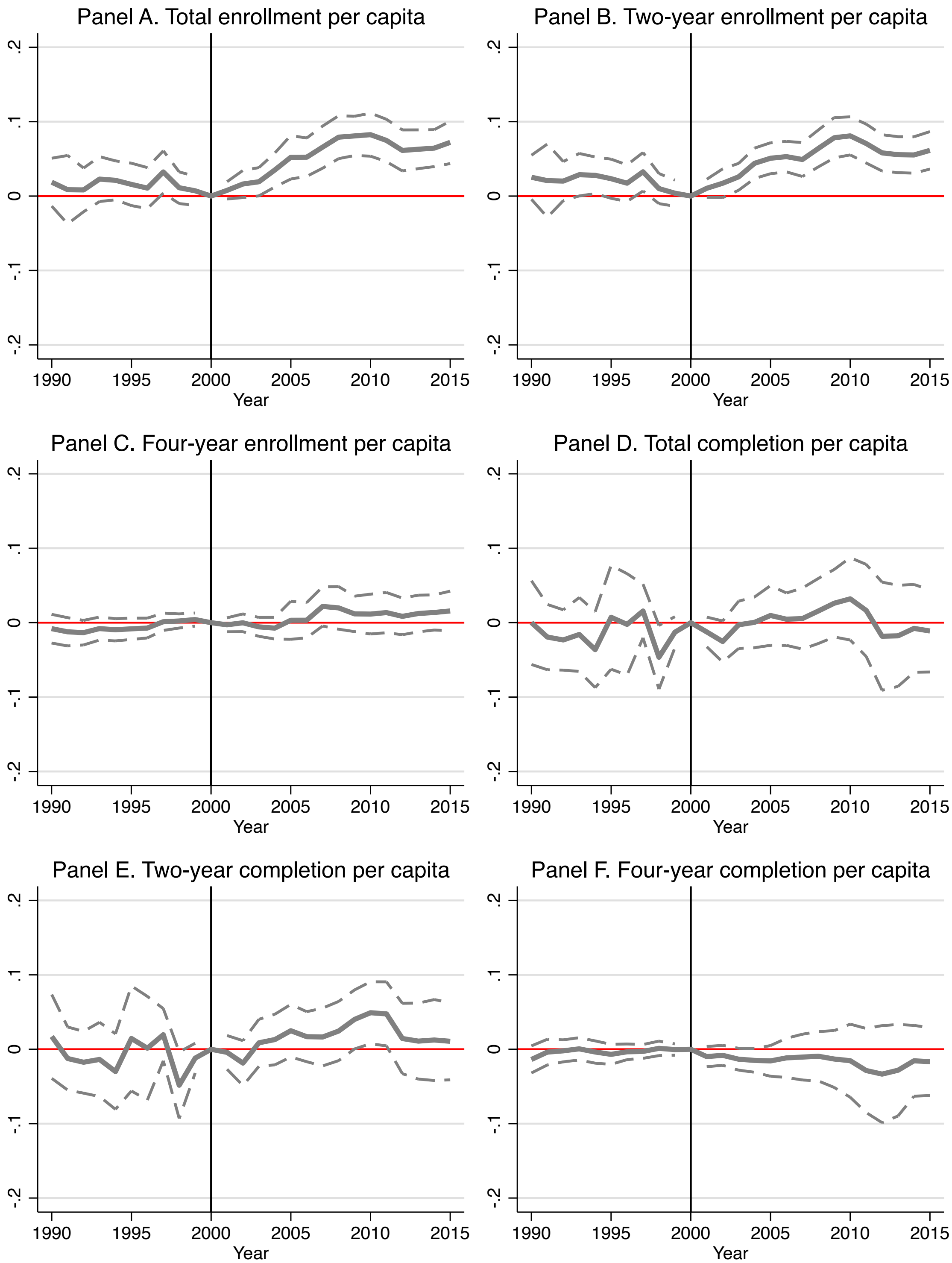

FIGURE 4. Relationships Between PNTR and College Outcomes

Source: Author's calculations from the 1990 to 2015 Integrated Postsecondary Eduaction System Data.

TABLE 1. Effect of PNTR on Commuting Zone Employment, BLS Data, 1990–2015

Dependent variable: employment and labor force participation	Log total employment (1)	Log mfg employment (2)	Log nmfg employment (3)	Log unemp. (4)	Unemp. rate (5)	Labor force-to-pop. ratio (6)
PNTR × Post	-1.58***	-2.35***	-0.38	0.89**	0.15***	-0.19**
	(0.24)	(0.62)	(0.23)	(0.41)	(0.02)	(0.09)
	[-4.38]	[-6.53]	[-1.05]	[2.46]	[0.42]	[-0.52]
R^2	1.00	1.00	1.00	0.99	0.89	0.89
Mean dep. var.	1315.74	1110.48	1299.28	1064.84	6.15	80.50
Observations	18726	17742	18726	18726	18726	18726

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates includes interactions of the post-2001 dummy with several 1990 CZ characteristics: log population; share of the population employed in manufacturing; share of the female population in the labor force; share of the population without a college degree; share of the population that is black, Asian, and of other races (Native American and Pacific Islander); share of population that is foreign-born; and average household income. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 2. Labor Market Effects of PNTR on Noncollege Workers, Census/ACS Data, 1990, 2000, 2005–2015

Dependent variable: employment rate and weekly wages of noncollege worker aged 18–34 and 35–54	Percent noncollege pop. that is employed, aged 18–34 (1)	Noncollege avg. log weekly wages, aged 18–34 (2)	Percent noncollege pop. that is employed, aged 35–54 (3)	Noncollege avg. log weekly wages, aged 35–54 (4)	College-noncollege employment gap, aged 35–54 (5)	College-noncollege weekly wage gap, aged 35–54 (6)
PNTR × Post	-0.49***	-0.72***	-0.40***	-0.75***	0.12**	-0.03
	(0.07)	(0.14)	(0.06)	(0.14)	(0.05)	(0.09)
	[-1.38]	[-2.00]	[-1.11]	[-2.09]	[0.33]	[-0.09]
R^2	0.84	0.80	0.82	0.85	0.58	0.64
Mean dep. var.	59.77	593.73	68.90	647.61	14.84	41.14

Note: $N = 9386$. All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 3. Effect of PNTR on College Enrollment and Attainment, IPEDS Data, 1990–2015

Dependent variable: college enrollment and completion per capita	Total enrollment (1)	Two-year enrollment (2)	Four-year enrollment (3)	Total completion (4)	Two-year completion (5)	Four-year completion (6)
PNTR × Post	0.04***	0.03***	0.01	0.01	0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
	[0.11]	[0.09]	[0.04]	[0.04]	[0.07]	[-0.04]
R^2	0.85	0.67	0.92	0.73	0.64	0.76
Mean dep. var.	2.51	1.36	1.23	3.11	1.73	1.48
Observations	15145	14001	10561	15072	13860	10688

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 4. College Enrollment and Attainment Effects of PNTR by Postsecondary Sector, IPEDS Data, 1990–2015

Dependent variable: college enrollment and completion per capita by institution sector	Total enrollment (1)	Two-year enrollment (2)	Four-year enrollment (3)	Total completion (4)	Two-year completion (5)	Four-year completion (6)
Panel A. Public colleges						
PNTR $\times$ Post	0.04***	0.03***	0.02**	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
	[0.10]	[0.08]	[0.05]	[0.02]	[0.03]	[0.02]
R^2	0.88	0.71	0.96	0.82	0.69	0.95
Mean dep. var.	1.75	1.00	0.91	2.09	1.24	1.03
Observations	14012	12805	7430	13959	12700	7494
Panel B. Private colleges						
PNTR $\times$ Post	0.01	0.01	0.01	0.01	0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
	[0.03]	[0.02]	[0.02]	[0.02]	[0.05]	[-0.04]
R^2	0.76	0.66	0.77	0.55	0.51	0.49
Mean dep. var.	0.81	0.41	0.46	1.09	0.55	0.62
Observations	11055	9430	7642	10821	9073	7753

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 5. Effect of PNTR on the Employment, Weekly Wages, and College Premium of College Workers Aged 25–34, Census/ACS Data, 1990, 2000, 2005–2015

Dependent variable: employment rate, weekly wages, and college premium of college workers aged 25–34	Percent college dropout that is employed (1)	Percent college graduate that is employed (2)	College dropout-noncollege employment gap (3)	College graduate-noncollege employment gap (4)	College dropout-noncollege weekly wage gap (5)	College graduate-noncollege weekly wage gap (6)
PNTR × Post	-0.26***	-0.16***	0.19***	0.29***	-0.24*	-0.08
	(0.06)	(0.05)	(0.07)	(0.07)	(0.13)	(0.18)
	[-0.72]	[-0.44]	[0.53]	[0.81]	[-0.67]	[-0.23]
R^2	0.57	0.35	0.29	0.51	0.20	0.46
Mean dep. var.	79.83	88.71	12.64	21.52	15.57	52.31
Observations	9386	9382	9386	9382	9386	9381

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 6. Effect of PNTR on Schooling and Labor Market Outcomes, NLSY97 Data

Dependent variable: college attainment, mean hourly wages, and UI receipt from 2001 to age 30	Has enrolled at 2-year colleges (1)	Has enrolled at 4-year colleges (2)	Has received associate's degree (3)	Has received bachelor's degree (4)	Hourly wages, not in school (5)	Has received unemp. insurance (6)
Panel A. Continuous PNTR						
PNTR	0.69*	-0.43	0.41	0.12	-0.09	0.38
	(0.41)	(0.32)	(0.28)	(0.26)	(0.07)	(0.47)
	[2.26]	[-1.41]	[1.34]	[0.39]	[-0.30]	[1.25]
R^2	0.04	0.32	0.02	0.32	0.11	0.04
Panel B. Binary PNTR						
Lived in high PNTR CZs in 1997	-1.09	-0.71	-1.11	1.64	-0.76**	4.55**
	(2.34)	(1.60)	(1.31)	(1.46)	(0.37)	(2.17)
R^2	0.04	0.32	0.02	0.32	0.11	0.04
Panel C. PNTR terciles						
Lived in high PNTR CZs in 1997	1.09	-4.13**	-1.67	-0.11	-1.02**	5.91***
	(2.42)	(1.86)	(1.46)	(1.49)	(0.42)	(2.26)
Lived in middle PNTR CZs in 1997	3.24**	-5.11***	-0.80	-2.63	-0.38	2.16
	(1.61)	(1.79)	(1.10)	(1.62)	(0.36)	(1.68)
R^2	0.04	0.33	0.02	0.33	0.11	0.05
Mean dep. var.	28.36	42.00	11.15	29.27	17.32	27.05

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 7. Relationships Between PNTR and Mean Exposure, NLSY97 Data

Dependent variable: summary index *z*-score of geographic, industry, and occupation exposure from 2001 to age 30	100 × Total exposure index z-score (1)	100 × CZ exposure index z-score (2)	Has moved out of county (3)	100 × Industry exposure index z-score (4)	100 × Occupation exposure index z-score (5)
Panel A. Continuous PNTR					
PNTR	-10.15***	-10.77***	-1.48***	-4.14***	-0.85
	(0.97)	(0.92)	(0.54)	(0.88)	(0.61)
	[-33.29]	[-35.33]	[-4.85]	[-13.58]	[-2.79]
R^2	0.19	0.21	0.09	0.07	0.25
Panel B. Binary PNTR					
Lived in high PNTR CZs in 1997	-29.19***	-28.56***	-3.60	-14.31***	-5.41*
	(6.09)	(5.47)	(2.45)	(5.16)	(3.25)
R^2	0.17	0.18	0.09	0.07	0.25
Mean dep. var.	0.00	0.00	66.92	0.00	0.00

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE 8. Effect of PNTR on Risky Behaviors, Life Events, and Adult Economic Success, NLSY97 Data

Dependent variable: summary index z-score of risky behavior outcomes, life event outcomes and, adult economic success from 2001 to age 30	100 × Criminal behavior index z-score (1)	100 × Risky health behavior index z-score (2)	100 × Life event index z-score (3)	100 × Adult economic success index z-score (4)
Panel A. Continuous PNTR				
PNTR	-0.69	-2.24***	0.05	-1.96**
	(0.89)	(0.80)	(0.73)	(0.80)
	[-2.26]	[-7.35]	[0.16]	[-6.43]
R^2	0.12	0.04	0.17	0.19
Panel B. Binary PNTR				
Lived in high PNTR CZs in 1997	-11.20***	-12.22***	-2.97	-4.26
	(3.98)	(3.66)	(3.51)	(5.85)
R^2	0.13	0.04	0.17	0.19
Mean dep. var.	0.00	0.00	0.00	0.00

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

Online Appendix for "Short-Term and Long-Term Effects of Trade Liberalization" by Gary C. Lin

A Data and Measurement

TABLE A1. Data Sources, Sample Periods, and Variables

Data Sources	Sample	Outcomes
Panel A. Main Outcomes		
Integrated Postsecondary Education Data System	1990–2015	College enrollment; college completion
Census/American Community Survey	1990, 2000, 2005–2015	Employment and earnings by educational attainment and age group
National Longitudinal Survey of Youth 1997	1997–2011, 2013, 2015	College attainment; employment and wages; marriage and fertility; criminal behavior; risky health behaviors; mobility (geographic, industry, occupation); asset and home ownership
Panel B. Supplemental Outcomes		
100 Percent Sample Census	1990	Population; share of the population employed in manufacturing; share of the female population in the labor force; share of the population without a college degree; share of the population that is black, Asian, and of other races (Native American and Pacific Islander); share of population that is foreign-born; average household income
Quarterly Census of Employment and Wages	1990–2015	Employment counts
Local Area Unemployment	1990–2015	Unemployment rate; unemployment counts; labor force population counts
County Business Patterns	1990	Employment counts by industry
Surveillance, Epidemiology, and End Results	1990–2015	Population counts (aged 18–34, aged 18–24)

TABLE A2. Summary Statistics of Commuting Zone Variables, 1990–2015

	Pre-PNTR (1990–2000)			Post-PNTR (2001–2015)		
	Mean	SD	N	Mean	SD	N
Panel A. Per-capita college outcomes						
Four-year enrollment	0.987	1.047	4370	1.400	1.239	6199
Four-year male enrollment	0.902	0.990	4357	1.239	1.146	6191
Four-year female enrollment	1.074	1.127	4360	1.566	1.373	6194
Four-year completion	1.145	1.067	4443	1.721	1.573	6251
Four-year male completion	0.978	1.013	4427	1.375	1.287	6237
Four-year female completion	1.317	1.155	4428	2.077	1.944	6244
Two-year enrollment	1.334	0.912	6033	1.388	0.777	7969
Two-year male enrollment	1.178	1.102	5897	1.220	0.790	7746
Two-year female enrollment	1.497	0.856	6026	1.568	0.830	7951
Two-year completion	1.418	1.438	5875	1.955	1.210	7993
Two-year male completion	1.143	1.847	5757	1.493	1.341	7778
Two-year female completion	1.703	1.268	5869	2.439	1.295	7986
Panel B. Demographic and economic covariates						
PNTR exposure in 2000				8.361	2.777	722
Total population in 1990 (millions)				3.090	3.985	722
Percent of population that is Asian in 1990				2.703	3.125	722
Percent of population that is black in 1990				12.285	9.838	722
Percent of population that is of other races in 1990				4.101	5.409	722
Percent of population that is foreign-born in 1990				8.226	8.457	722
Percent of population that is non-college in 1990				54.281	8.745	722
Female labor force participation rate in 1990				44.511	4.280	722
Percent employed in manufacturing in 1990				6.933	3.630	722
Average household income in 1990 (thousands)				50.691	10.566	722

TABLE A3. Summary Statistics of Individual Baseline Covariates, NLSY97 Data

	Mean	SD	N
Panel A. Individual covariates			
Age at 30	29.47	0.60	6772
Born in U.S. (%)	81.94	38.47	6772
Female (%)	50.18	50.00	6772
Race white (%)	68.99	46.26	6772
Race black (%)	17.85	38.29	6772
Race Hispanic (%)	13.16	33.81	6772
AFQT score	166.89	31.41	5418
$100 \times$ Non-cognitive skills index	-0.76	98.07	6601
$100 \times$ Social skills index	2.58	102.04	6772
$100 \times$ School summary index	4.73	97.32	6763
$100 \times$ Peer summary index	5.34	98.28	6761
Father's HGC 1–11 years (%)	12.24	32.78	6772
Father's HGC 12 years (%)	23.42	42.35	6772
Father's HGC 13–15 years (%)	14.38	35.09	6772
Father's HGC 16+ years (%)	18.80	39.08	6772
Mother's HGC 1–11 years (%)	16.32	36.96	6772
Mother's HGC 12 years (%)	32.11	46.69	6772
Mother's HGC 13–15 years (%)	22.86	42.00	6772
Mother's HGC 16+ years(%)	18.97	39.21	6772
Live with both parents (%)	52.03	49.96	6749
Live with single father (%)	3.58	18.59	6749
Live with single mother (%)	24.68	43.12	6749
Live with other HH members (%)	19.70	39.78	6749
Mom's age $\geq$ 29 (%)	28.22	45.01	6314
Mom's age 24–28 (%)	34.17	47.43	6314
Mom's age $\leq$ 23 (%)	37.61	48.44	6314
Household income in 1996 (thousands)	50.16	43.22	5009
Panel B. CZ covariates			
PNTR exposure (pp)	8.54	3.28	6772
Living in top-third PNTR CZs (%)	30.28	45.95	6772

Note: $N = 6,772$. All summary statistics are weighted by the NLSY97 sampling weights. Noncognitive skills, social skills, 1997 school index, and 1997 peer index are defined in the main article. Residence parents' educational attainment are categorized by highest grade completed in 1997.

TABLE A4. Summary Indices, NLSY Data

Outcome Category	Summary Index Individual Components
Adult Economic Success	Total assets at age 30 (+); has owned a home by age 30 (+); has been married by age 30 (+); has lived in a different state as 1997 by age 30 (+); percent of county population with at least a bachelor's degree at age 30 (+).
Criminal Behavior	Has been arrested by age 30 (-); has been incarcerated by age 30 (-); has not looked for work because of incapacitation by age 30 (-); has left job because of incapacitation by age 30 (-).
Geographic Mobility	Average CZ-level exposure to PNTR by age 30 (-); has lived in a different state as 1997 by age 30 (+); has lived in a different CZ as 1997 by age 30 (+); has lived in a different county as 1997 by age 30 (+).
Industry Mobility	Average industry-level exposure to PNTR by age 30 (-); has been employed in manufacturing by age 30 (-).
Life Event	Has had a child as a college-goer by age 30 (-); has had at least three children as a college-goer by age 30 (-); has been a single parent (unmarried with child) as a college-goer by age 30 (-); has left job because of family reasons by age 30 (-); has not looked for work because of family reasons by age 30 (-).
Risky Health Behavior	Average number of days drank alcohol in the last 30 days by age 30 (-); average number of times used illegal drugs since last interview by age 30 (-); average number of days drank before/during school or work hours in the last 30 days by age 30 (-); average number of times used illegal drugs before/during school or work hours in the last 30 days by age 30 (-).
Occupation	Average routine-intensity of employment by age 30 (-); average social skill-intensity of employment by age 30 (+); average math-intensity of employment by age 30 (+).

Note: This table documents the individual components of each summary index used in the main article. The parentheses document whether an individual component is a "desirable" outcome (+) or an "undesirable" outcome (-). Undesirable outcomes are rescaled so that larger positive values correspond to more desirable outcomes.

TABLE A5. Top and Bottom 20 PNTR-Exposed Commuting Zones

Rank	Commuting Zone	State	Rank	Commuting Zone	State
1	Morganton	North Carolina	703	Mobridge	South Dakota
2	Bennettsville	South Carolina	704	Colstrip	Montana
3	Galax	Virginia	705	Miller	South Dakota
4	Hickory	North Carolina	706	Winner	South Dakota
5	Washington	Georgia	707	Burlington	Colorado
6	Lexington	Tennessee	708	O'Neill	Nebraska
7	McMinnville	Tennessee	709	Carrington	North Dakota
8	Gastonia	North Carolina	710	Welch	West Virginia
9	Crossville	Tennessee	711	Hazard	Kentucky
10	Rome	Georgia	712	Center	Kansas
11	New Albany	Mississippi	713	Haskell	Texas
12	Tupelo	Mississippi	714	Coldwater	Kansas
13	Corinth	Mississippi	715	Wano	Kansas
14	Henderson	North Carolina	716	East Grant	North Dakota
15	Martinsville	Virginia	717	East Corson	South Dakota
16	Griffin	Georgia	718	Steele	North Dakota
17	Cleveland	Tennessee	719	Loa	Utah
18	Talladega	Alabama	720	Ekalaka	South Dakota
19	Toccoa	Georgia	721	Mission	South Dakota
20	Starkville	Mississippi	722	Murdo	South Dakota

B Additional Tables and Figures

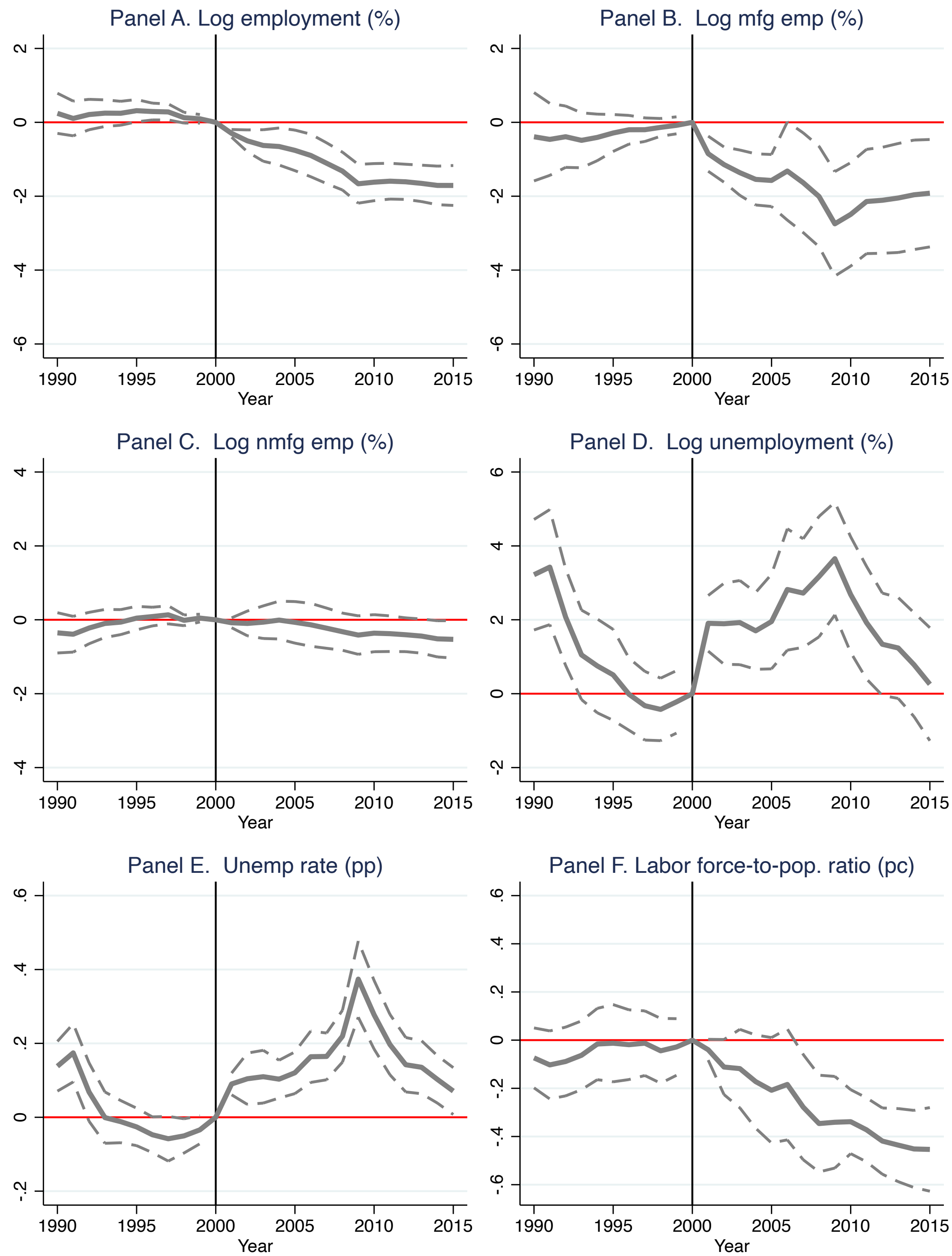

FIGURE B1. Effect of PNTR on Local Employment Trends, No CZ Controls, BLS Data,1990–2015

Source: Author's calculations from the 1990 to 2015 Bureau of Labor Statistics, Local Area Unemployment, and Quarterly Census of Employment and Wages Data.

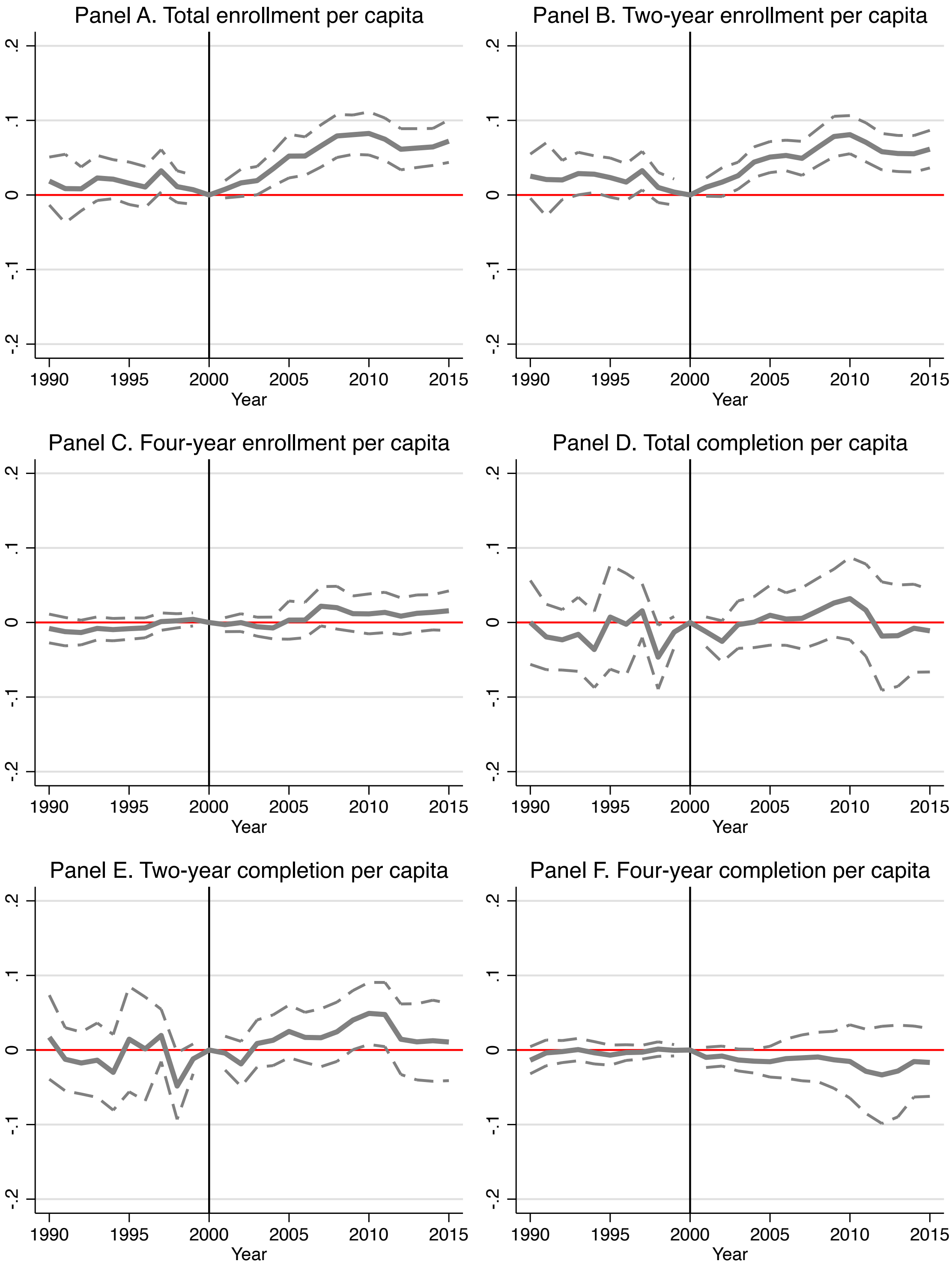

FIGURE B2. Effect of PNTR on College Enrollment and Attainment Trends, No CZ Controls, IPEDS Data,1990–2015

Source: Author's calculations from the 1990 to 2015 Integrated Postsecondary Eduaction System Data.

TABLE B1. Pre-Great Recession Effect of PNTR on Commuting Zone Employment, BLS Data, 1990–2007

Dependent variable: employment and labor force participation	Log total employment (1)	Log mfg employment (2)	Log nmfg employment (3)	Log unemp. (4)	Unemp. rate (5)	Labor force-to-pop. ratio (6)
PNTR × Post	-1.10***	-1.79***	-0.21	1.05**	0.11***	-0.07
	(0.21)	(0.43)	(0.21)	(0.41)	(0.02)	(0.10)
	[-3.05]	[-4.97]	[-0.57]	[2.93]	[0.31]	[-0.20]
R^2	1.00	1.00	1.00	0.99	0.88	0.90
Mean dep. var.	1313.40	1119.66	1295.10	1051.38	5.49	81.20
Observations	12965	12211	12965	12965	12965	12965

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B2. Sensitivity Analysis of Labor Market Effects of PNTR, BLS Data, 1990–2015

Dependent variable: local employment, with varying regional and labor market control variables	Preferred estimates (1)	Column 1, Census division-year fixed effects (2)	Column 1, state-year fixed effects (3)	Column 1, exclude high-tech industries (4)	Column 1, include additional trade-policy variables (5)	Column 1, include additional housing boom variable (6)
Panel A. Log employment						
PNTR × Post	-1.58***	-1.30***	-1.32***	-1.91***	-1.32***	-1.59***
	(0.24)	(0.23)	(0.23)	(0.29)	(0.27)	(0.25)
Panel B. Log mfg employment						
PNTR × Post	-2.35***	-2.10***	-2.38***	-2.45***	-2.10***	-2.22***
	(0.62)	(0.62)	(0.39)	(0.70)	(0.61)	(0.62)
Panel C. Log nmfg employment						
PNTR × Post	-0.38	-0.13	-0.16	-0.59**	-0.40	-0.38
	(0.23)	(0.22)	(0.23)	(0.27)	(0.25)	(0.25)
Panel D. Log unemployment						
PNTR × Post	0.89**	1.10***	-0.14	0.83*	1.44***	0.78*
	(0.41)	(0.40)	(0.39)	(0.44)	(0.45)	(0.44)
Panel E. Unemployment rate						
PNTR × Post	0.15***	0.14***	0.10***	0.17***	0.17***	0.15***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Panel F. Labor force-to-pop. ratio						
PNTR × Post	-0.19**	-0.15	-0.16	-0.18*	-0.13	-0.24***
	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.09)

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates includes interactions of the post-2001 dummy with several 1990 CZ characteristics: log population; share of the population employed in manufacturing; share of the female population in the labor force; share of the population without a college degree; share of the population that is black, Asian, and of other races (Native American and Pacific Islander); share of population that is foreign-born; and average household income. All regressions include commuting zone fixed effects. Except for columns 2 and 3, all regressions use region-by-year fixed effects; column 2 uses Census division-year fixed effects; column 3 uses state-year fixed effects. Column 4 uses PNTR net of high-tech industries as the regressor of interest. Column 5 additionally includes NTR tariff rates and MFA fill rates. Column 6 additionally includes estimates of structural breaks in local housing prices. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B3. Sensitivity Analysis of Employment and Earnings Estimates, Census/ACS Data, 1990, 2000, 2005–2015

Dependent variable: population, employment earnings, and earnings gap	Population share that is high school graduates, aged 18–19 (1)	Population share that is college dropouts, aged 25–34 (2)	Population share that is college graduates, aged 25–34 (2)	College dropout avg. log earnings, aged 25–34 (4)	College graduate avg. log earnings, aged 25–34 (5)	College dropout-noncollege earnings gap, aged 25–34 (6)	College graduate-noncollege earnings gap, aged 25–34 (7)
PNTR × Post	0.13*	0.11**	-0.32***	-1.19***	-0.99***	-0.24	-0.04
	(0.07)	(0.05)	(0.05)	(0.19)	(0.17)	(0.17)	(0.21)
	[0.35]	[0.30]	[-0.90]	[-3.30]	[-2.75]	[-0.65]	[-0.11]
R^2	0.48	0.74	0.93	0.74	0.72	0.17	0.41
Mean dep. var.	35.97	34.35	27.67	1024.49	1063.41	19.19	58.11
Observations	9386	9386	9386	9386	9381	9386	9381

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B4. Sensitivity Analysis of College Enrollment and Attainment Effects of PNTR, IPEDS Data, 1990–2015

Dependent variable: college enrollment and completion per capita, with varying regional and labor market control variables	Preferred estimates (1)	Column 1, Census division-year fixed effects (2)	Column 1, state-year fixed effects (3)	Column 1, exclude high-tech industries (4)	Column 1, include additional trade-policy variables (5)	Column 1, include additional housing boom variable (6)
Panel A. Total enrollment						
PNTR × Post	0.04***	0.04***	0.03**	0.04**	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel B. Two-year enrollment						
PNTR × Post	0.03***	0.04***	0.03***	0.04***	0.04***	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel C. Four-year enrollment						
PNTR × Post	0.01	0.01	0.00	0.01	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel D. Total completion						
PNTR × Post	0.01	0.01	0.00	-0.00	0.02	0.02
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Panel E. Two-year completion						
PNTR × Post	0.02	0.01	0.01	0.03	0.03	0.02
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Panel F. Four-year completion						
PNTR × Post	-0.01	0.00	-0.01	-0.03*	-0.02	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates includes interactions of the post-2001 dummy with several 1990 CZ characteristics: log population; share of the population employed in manufacturing; share of the female population in the labor force; share of the population without a college degree; share of the population that is black, Asian, and of other races (Native American and Pacific Islander); share of population that is foreign-born; and average household income. All regressions include commuting zone fixed effects. Except for columns 2 and 3, all regressions use region-by-year fixed effects; column 2 uses Census division-year fixed effects; Column 3 uses state-year fixed effects. Column 4 uses PNTR net of high-tech industries as the regressor of interest. Column 5 additionally includes NTR tariff rates and MFA fill rates. Column 6 additionally includes estimates of structural breaks in local housing prices. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B5. Pre-Great Recession Effect of PNTR and College Enrollment and Attainment, IPEDS Data, 1990–2007

Dependent variable: college enrollment and completion per capita	Total enrollment (1)	Two-year enrollment (2)	Four-year enrollment (3)	Total completion (4)	Two-year completion (5)	Four-year completion (6)
PNTR × Post	0.02*	0.02*	0.01	0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
	[0.06]	[0.05]	[0.02]	[0.02]	[0.04]	[-0.02]
R^2	0.85	0.68	0.95	0.80	0.67	0.95
Mean dep. var.	2.41	1.37	1.12	2.73	1.55	1.27
Observations	10527	9798	7200	10451	9640	7303

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B6. Sensitivity Analysis of IPEDS Sample Selection, IPEDS Data, 1990–2015

Dependent variable: college enrollment and completion per capita, alternative samples and population adjustments	Total enrollment, exclude for-profit colleges (1)	Total completion, exclude for-profit colleges (2)	Total enrollment, exclude L2 colleges (3)	Total completion, exclude L2 colleges (4)	Total enrollment, include selective colleges (5)	Total completion, include selective colleges (6)
PNTR × Post	0.04***	0.01	0.04***	0.01	0.05***	0.02
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
	[0.11]	[0.04]	[0.12]	[0.03]	[0.14]	[0.05]
R^2	0.88	0.82	0.86	0.81	0.88	0.81
Mean dep. var.	2.11	2.52	2.28	2.81	3.32	3.99
Observations	14836	14819	14836	14701	15164	14825

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B7. Sensitivity Analysis of College Outcome Measurement, IPEDS Data, 1990–2015

Dependent variable: college enrollment and completion per capita, alternative definitions	First-time full-time enrollment per 18–24 capita (1)	Total award per 18–24 capita (2)	Total first-time enrollment per 18–34 capita (3)	Total full-time enrollment per 18–34 capita (4)	Total fall enrollment per 18–34 capita (5)	Total certificate awards per 18–34 capita (6)	Total associate's degrees per 18–34 capita (7)	Total bachelor's degrees per 18–34 capita (8)
PNTR × Post	0.08***	0.01	0.04*	0.11**	0.09	0.02	0.00	-0.00
	(0.03)	(0.06)	(0.02)	(0.06)	(0.09)	(0.02)	(0.01)	(0.01)
	[0.22]	[0.02]	[0.11]	[0.31]	[0.25]	[0.04]	[0.01]	[-0.00]
R^2	0.79	0.68	0.75	0.76	0.68	0.57	0.77	0.82
Mean dep. var.	6.06	7.53	3.39	9.75	17.60	0.97	1.01	1.22
Observations	15145	15072	15147	15148	15148	14067	13931	10597

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B8. Gender Differentials in College Enrollment and Attainment Effects of PNTR, IPEDS Data, 1990–2015

Dependent variable: college enrollment and completion per capita by gender	Total enrollment (1)	Two-year enrollment (2)	Four-year enrollment (3)	Total completion (4)	Two-year completion (5)	Four-year completion (6)
Panel A. Men						
PNTR × Post	0.04***	0.03***	0.01	0.01	0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
	[0.11]	[0.09]	[0.03]	[0.03]	[0.05]	[-0.02]
R^2	0.78	0.57	0.93	0.69	0.57	0.84
Mean dep. var.	2.22	1.20	1.10	2.47	1.34	1.21
Observations	15007	13640	10541	14936	13527	10659
Panel B. Women						
PNTR × Post	0.04***	0.03***	0.02	0.01	0.03	-0.02
	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)
	[0.11]	[0.08]	[0.05]	[0.03]	[0.09]	[-0.06]
R^2	0.85	0.72	0.90	0.73	0.70	0.70
Mean dep. var.	2.80	1.54	1.36	3.77	2.13	1.76
Observations	15127	13974	10546	15062	13847	10666

Note: All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B9. Effect of PNTR on Public College Revenues and Expenditures, IPEDS Data, 1990–2015

Dependent variable: public college revenues and expenditures	Total public funding (1)	Federal funding (2)	State and local funding (3)	Tuition and fees (4)	Total education spending (5)	Instruction spending (6)
Panel A. Log dollars per capita (percent)						
PNTR × Post	-0.94*	0.37	-1.00*	-0.04	-0.39	-0.24
	(0.51)	(0.65)	(0.59)	(0.48)	(0.35)	(0.44)
	[-2.62]	[1.02]	[-2.77]	[-0.12]	[-1.09]	[-0.66]
R^2	0.90	0.90	0.87	0.95	0.90	0.90
Mean dep. var.	932.72	779.92	904.35	843.80	939.20	884.14
Panel B. Log total value (percent)						
PNTR × Post	-0.85*	0.46	-0.91	0.05	-0.30	-0.15
	(0.51)	(0.74)	(0.56)	(0.54)	(0.36)	(0.44)
	[-2.37]	[1.27]	[-2.52]	[0.13]	[-0.84]	[-0.41]
R^2	0.99	0.98	0.99	0.99	1.00	1.00
Mean dep. var.	1965.60	1812.80	1937.23	1876.69	1972.09	1917.02
Panel C. Dollars per capita ($1,000 pc)						
PNTR × Post	-0.16**	-0.04	-0.12**	-0.01	-0.03	0.01
	(0.07)	(0.03)	(0.06)	(0.03)	(0.05)	(0.03)
	[-0.45]	[-0.11]	[-0.34]	[-0.02]	[-0.10]	[0.02]
R^2	0.91	0.91	0.87	0.92	0.91	0.91
Mean dep. var.	11.97	2.92	9.05	5.08	12.51	7.28

Note: $N = 11880$. The sample is restricted to a balanced panel of two-year and four-year colleges. Finance outcomes are aggregated to the commuting zone-level, weighted by the institution's full-time-equivalent student population. Institutions whose per-full-time equivalent revenues or expenditures are less than $100 or greater than $1,000,000 are dropped. Panel A reports log revenues and expenditures per capita (multiplied by 100); Panel B reports log revenues and expenditures per capita (multiplied by 100); Panel C reports revenues and expenditures per capita. All regressions are weighted by CZs' 18 to 34 population in 1990. The vector of CZ covariates are defined at the bottom of Table 1. All regressions include commuting zone and region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Brackets include the implied effect of a one standard deviation increase in exposure to PNTR. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B10. Sensitivity Analysis of Schooling and Labor Market Estimates, NLSY97 Data

Dependent variable: college attainment and labor market outcomes from 2001 to age 30	Has enrolled at 2-year colleges (1)	Has enrolled at 4-year colleges (2)	Has received associate's degree (3)	Has received bachelor's degree (4)	Hourly wages, not in school (5)	Has received unemp. insurance (6)
Panel A. Preferred						
Lived in high PNTR CZs in 1997	-1.09	-0.71	-1.11	1.64	-0.76**	4.55**
	(2.34)	(1.60)	(1.31)	(1.46)	(0.37)	(2.17)
R^2	0.04	0.32	0.02	0.32	0.11	0.04
Panel B. Proxy for unobservables						
Lived in high PNTR CZs in 1997	-1.00	-0.84	-1.11	1.46	-0.75**	4.67**
	(2.33)	(1.59)	(1.31)	(1.40)	(0.36)	(2.12)
R^2	0.05	0.34	0.02	0.35	0.12	0.05
Panel C. No baseline controls						
Lived in high PNTR CZs in 1997	-1.71	-1.57	-1.47	1.03	-0.74*	4.85**
	(2.33)	(2.69)	(1.28)	(2.48)	(0.43)	(2.30)
	0.02	0.02	0.01	0.02	0.03	0.02
Panel D. CZ unemp. rate						
Lived in high PNTR CZs in 1997	-1.17	-0.73	-1.12	1.51	-0.77**	4.34**
	(2.31)	(1.60)	(1.31)	(1.44)	(0.37)	(2.12)
R^2	0.04	0.32	0.02	0.32	0.11	0.05
Panel E. Housing boom						
Lived in high PNTR CZs in 1997	-0.96	-1.07	-1.20	1.48	-0.76**	4.29**
	(2.32)	(1.56)	(1.30)	(1.45)	(0.37)	(2.14)
R^2	0.04	0.32	0.02	0.32	0.11	0.05

Note: $N = 6,772$ except for Panel E $(N = 6,760)$. All regressions are weighted by NLSY97 sampling weight. Except for Panel C, all regressions include the full set of CZ and individual covariates as in Table B10. Panel B additionally includes measures of social and noncognitive skills and 1997 peer and school summary indices; Panel D additionally includes CZ unemployment rate at age 19; Panel E additionally includes estimates of structural breaks in local housing prices. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

Table B11. Relationships Between PNTR and School to Work Transitions, NLSY97 Data

Dependent variable: timing of college enrollment and labor market outcome, from 2001 to age 30	Has enrolled at 2-year colleges, age $\leq$ 25 (1)	Has enrolled at 2-year colleges, age $>$ 25 (2)	Has enrolled at 4-year colleges, age $\leq$ 25 (3)	Has enrolled at 4-year colleges, age $>$ 25 (4)	Hourly wages, not in school, age $\leq$ 25 (5)	Hourly wages, not in school, ages $>$ 25 (6)	Has received unemp. insurance, age $\leq$ 25 (7)	Has received unemp. insurance, age $>$ 25 (8)
Panel A. Continuous PNTR								
Implied effect of PNTR (1SD)	0.61	0.43*	-0.51	-0.11	-0.13*	-0.12	0.45	-0.04
	(0.38)	(0.24)	(0.31)	(0.21)	(0.08)	(0.10)	(0.39)	(0.36)
	[2.00]	[1.41]	[-1.67]	[-0.36]	[-0.43]	[-0.39]	[1.48]	[-0.13]
R^2	0.05	0.02	0.34	0.02	0.06	0.09	0.04	0.03
Panel B. Binary PNTR								
Lived in high PNTR CZs in 1997	-1.67	0.70	-0.74	-0.25	-1.07**	-0.66	4.93**	1.57
	(2.05)	(1.26)	(1.60)	(1.14)	(0.42)	(0.54)	(2.19)	(1.65)
R^2	0.05	0.02	0.34	0.02	0.06	0.09	0.04	0.03
Mean dep. var.	22.98	9.77	37.88	9.36	15.53	19.38	14.57	18.29
Observations	6772	6764	6772	6764	6772	6395	6772	6771

Note: All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level.

TABLE B12. Effect of PNTR on the Individual Components of the Various Exposure Summary Indices, NLSY97 Data

Dependent variable: geographic, industry, and occupation exposure, from 2001 to age 30	100 × CZ PNTR z-score (1)	Has lived in a different CZ as 1997 (2)	Has lived in a different state as 1997 (3)	100 × industry PNTR z-score (4)	Has been employed in mfg (5)	100 × Social skills z-score (6)	100 × Routine skills z-score (7)	100 × Math skills z-score (8)
Panel A. Continuous PNTR								
PNTR	25.92***	-0.29	-0.41	4.04***	1.52***	-0.63	0.80	-0.55
	(0.67)	(0.52)	(0.49)	(0.99)	(0.33)	(0.67)	(0.74)	(0.73)
	[85.02]	[-0.95]	[-1.34]	[13.25]	[4.99]	[-2.07]	[2.62]	[-1.80]
R^2	0.79	0.11	0.08	0.05	0.07	0.25	0.08	0.19
Panel B. Binary measure								
Lived in high PNTR CZs in 1997	71.76***	-0.20	-0.54	14.38**	5.07***	-4.64	4.72	-3.26
	(7.93)	(1.98)	(2.48)	(5.70)	(1.86)	(3.51)	(4.49)	(4.29)
R^2	0.65	0.11	0.08	0.05	0.07	0.25	0.08	0.19
Mean dep. var.	0.00	51.50	33.55	0.00	24.31	0.00	0.00	0.00

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B13. Effect of PNTR on the Individual Components of the Criminal Behavior Summary Index, NLSY97 Data

Dependent variable: individual component of criminal behavior summary index (nonstandardized)	Has been arrested (1)	Has been incarcerated (2)	Has left job as a result of being in jail (3)	Has not looked for work as a result of being in jail (4)
Panel A. Continuous PNTR				
PNTR	0.25	0.13	0.06	0.15
	(0.32)	(0.24)	(0.11)	(0.19)
	[0.82]	[0.43]	[0.20]	[0.49]
R^2	0.12	0.08	0.04	0.08
Panel B. Binary PNTR				
Lived in high PNTR CZs in 1997	3.41**	3.18***	0.83	2.06**
	(1.57)	(1.02)	(0.51)	(0.85)
R^2	0.12	0.08	0.04	0.08
Mean dep. var.	26.36	7.94	2.44	5.34

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B14. Effect of PNTR on the Individual Components of the Risky Health Behavior Summary Index, NLSY97 Data

Dependent variable: individual component of health risk behavior summary index (nonstandardized) and incidences of cigarette and marijuana use	Days drank alcohol in last 30 days (1)	Times used drugs since last interview (2)	Days drank alcohol before/during school/work in last 30 days (3)	Times used drugs before/during school/work in last 30 days (4)	Days smoked cigs. in last 30 days (5)	Times used marij. in last 30 days (6)	Time used marij. before/during school/work in last 30 days (7)
Panel A. Continuous PNTR							
PNTR	0.02	0.23**	0.01*	0.01***	0.06	-0.01	-0.01
	(0.04)	(0.09)	(0.01)	(0.00)	(0.08)	(0.04)	(0.01)
	[0.07]	[0.75]	[0.03]	[0.03]	[0.20]	[-0.03]	[-0.03]
R^2	0.11	0.02	0.04	0.02	0.11	0.03	0.02
Panel B. Binary PNTR							
Lived in high PNTR CZs in 1997	0.29*	0.99**	0.09**	0.05**	0.07	0.37*	0.02
	(0.16)	(0.45)	(0.04)	(0.02)	(0.40)	(0.21)	(0.08)
R^2	0.11	0.02	0.04	0.02	0.11	0.04	0.02
Mean dep. var.	4.76	2.33	0.32	0.09	8.86	2.17	0.57

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B15. Effect of PNTR on the Individual Components of the Life Event Summary Index, NLSY97 Data

Dependent variable: individual component of life event summary index (nonstandardized)	Has left job because of family reasons (1)	Has not looked for work because of family reasons (2)	Has had at least 1 child as a college-goer (3)	Has had at least 3 children as a college-goer (4)	Has been a single parent as a college-goer (5)
Panel A. Continuous PNTR					
PNTR	-0.03	0.02	0.04	0.09	-0.30
	(0.31)	(0.10)	(0.22)	(0.20)	(0.29)
	[-0.10]	[0.07]	[0.13]	[0.30]	[-0.98]
R^2	0.06	0.02	0.07	0.12	0.19
Panel B. Binary PNTR					
Lived in high PNTR CZs in 1997	0.24	0.87	1.23	0.39	-1.48
	(1.32)	(0.54)	(0.97)	(0.96)	(1.41)
R^2	0.06	0.02	0.07	0.12	0.19
Mean dep. var.	11.59	1.97	7.00	11.67	27.15

Note: $N = 6,772$. All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level. Significance level is denoted * at 10 percent, ** at 5 percent, and *** at 1 percent.

TABLE B16. Effect of PNTR on the Individual Components of the Adult Economic Success Summary Index, NLSY97 Data

Dependent variable: individual component of adult economic success sumary index (nonstandardized)	Total assets at age 30 (thousands) (1)	Has owned a home by age 30 (2)	Has been married by age 30 (3)	Has lived in a different state as 1997 by age 30 (4)	Live in a high SES county at age 30 (5)
Panel A. Continuous PNTR					
PNTR	-0.46	0.11	-0.37	-0.41	-0.20***
	(0.67)	(0.34)	(0.34)	(0.49)	(0.07)
	[-1.51]	[0.36]	[-1.21]	[-1.34]	[-0.66]
R^2	0.10	0.09	0.08	0.08	0.24
Panel B. Binary PNTR					
Lived in high PNTR CZs in 1997	-2.63	-0.13	-1.58	-0.54	-0.21
	(3.74)	(1.80)	(2.35)	(2.48)	(0.49)
R^2	0.10	0.09	0.08	0.08	0.24
Mean dep. var.	61.63	32.69	52.16	33.55	15.71
Observations	6656	6759	6772	6772	6769

Note: All regressions include CZ and individual baseline covariates and are weighted by NLSY97 sampling weights. The vector of CZ covariates are defined at the bottom of Table 1. Individual baseline covariates include demographic information, family background, household structure, household income, and AFTQ scores as defined in the main article. All regressions include region-by-year fixed effects. Robust standard errors are clustered at the CZ level.

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