The Surprising Intergenerational Effects of Manufacturing Decline*

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Abstract

This paper analyzes the impact of manufacturing decline on children. To do so, it considers local employment structure—characterizing lost manufacturing jobs and left-behind places—high-school dropout rates, and college access in the US over 1990–2010. To establish a basis for causal inference, the paper uses variations in trade exposure from China, following its entry to the WTO, as an instrument for manufacturing decline in the US. While the literature on job loss has emphasized negative effects on children, the main conclusion of this research is that the rapid US manufacturing decline decreased high-school dropout rates and possibly increased college access. The magnitudes of the estimates suggest that for every 3-percentage-point decline in manufacturing as a share of total employment, the high-school dropout rate declined by 1 percentage point. The effects are largest in the areas with high racial and socioeconomic segregation and in those with larger African American populations. The results are consistent with the idea that the manufacturing decline increased returns and decreased opportunity costs of education, and with sociological accounts linking working-class environment and children's education.

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1 Introduction

This paper is about children of the left-behind places of America—the children of crisis. It asks what happens to children in the many declining manufacturing towns and cities of the US. The main focus is whether the local decline in manufacturing employment has increased—or decreased—the high-school dropout rate. The paper also explores the consequences of manufacturing decline on educational mobility—that is, the chances that a child born to poor parents enrolls in a college, and the factors that characterize the places with the largest effects on children.

The American middle class has declined across the country, affecting places from Detroit to Boston, from Middletown, Ohio to Washington, DC. The main causes of this— technology and trade—have eliminated a large part of US manufacturing jobs, and plausibly continue to do so (Acemoglu et al. 2016). The effects are most visible at the geographical level: some places have been left-behind while some places prosper. The haves and have-nots live in different places (Moretti 2013, Florida 2017). This is well documented: geographically uneven manufacturing decline and shrinking middle incomes are the key factors in America's deepening divide between rich and poor (Autor et al. 2015).

The previous research on globalization, technology, and inequality focuses primarily on adult males' labor market prospects. While this is undoubtedly important, the long-term effects—the future of work—depend on children, the next generation. This aspect of labor market adjustment—children—has received surprisingly little attention in the literature. But it could be the most important margin.

To study this, the paper uses county-level data on the employment structure and children's educational outcomes from the U.S. 1990–2010. To establish causal inference, the paper uses variations in trade exposure from China following its entry to the World Trade Organization (WTO) as an instrument for local manufacturing decline in the US. The instrument is computed from detailed product-level trade data from the UN Comtrade database. To explore the local factors correlated with the effects, the paper uses a large set of data on community characteristics, from segregation to educational resources. The idea of the empirical setup is that, conditional on the instrumental variables strategy, otherwise similar places faced different levels of manufacturing decline. This identifies the effects on children.

The literature on manufacturing decline of the 21st century paints a bleak picture. In places that have been hit the hardest, workers—especially adult men—have been slow to adjust (Autor et al. 2014, Yagan 2017). These places are characterized by job losses, lower employment and wages, and increased applications for social assistance (Autor et al. 2013, Balsvik et al. 2015). Contemporary evidence also suggests that manufacturing decline is a source of social distress. When factory jobs vanish, men become less desirable partners and divorces more common (Autor et al. 2017). Violent- and property-crime tend to increase (Pierce and Schott 2016b, Feler and Senses 2015, Deiana 2015). In places that experienced trade-induced manufacturing decline, children become more likely to be raised in poor single-headed households, making childhood poverty more prevalent (Autor et al. 2017). Based on this evidence, it would be reasonable to conjecture that manufacturing decline could make teenagers more prone to drop out of high-school and direct them away from college.

This paper finds the opposite. In places where manufacturing has declined, children drop less out of high-school. The relationship appears to be causal: comparing places within the same US region, with similar initial share of workers employed in manufacturing, and with similar demographic characteristics; those places that saw manufacturing decline because they were historically specialized in the particular industries that China started to export in 2001, saw sizable decreases in high-school dropout rates—compared to the otherwise similar places that were not exposed to competition with China. This paper also finds that when manufacturing employment declines, chances that poor children enroll in college increase. The causal evidence on the second observation is less conclusive but it is consistent with the first finding.

The paper also analyzes the local characteristics that could mediate, mitigate, or amplify the effects. To do so, it estimates interactions between manufacturing decline and a large set of factors that have been discussed in the sociology and economics literature, such as segregation and inequality. In contrast with the literature on the determinants of upward income mobility, I find that the effects are larger in areas with higher segregation and with larger African American populations. Local educational resources, such as school spending or student-teacher ratios show no significant correlations with the size of the effect. If anything, their predictive effect is negative. These are new and puzzling findings.

The main results are consistent with the idea that the manufacturing decline increased returns and decreased opportunity costs of education, and with sociological accounts linking working-class environment and children's education. In the classical Becker (1964) model of human capital investment, the decision-maker—in this case a teenager—compares the marginal costs and benefits of education. Complementary evidence by Autor et al. (2013) shows that trade-induced US manufacturing decline reduced the wages for individuals with low levels of education, compared to those with more, plausibly increasing the relative benefits of schooling. On the opportunity cost side, a reduction in available manufacturing jobs may have reduced the outside options for high-school dropouts.

From sociological perspective, Willis (1977), in the landmark research "*Learning to La-*

bor: How Working Class Kids Get Working Class Jobs", highlights how children inherit occupations and class from their parents and community. In working-class communities, Willis (1977) notes, counter-school culture of resistance and opposition to academia are prevalent. But possibly a decline in working-class jobs, as in this paper, could lead to a decline in working-class culture. Following Willis' (1977) argument, this could lead to an increase in children's education. Willis' theory could also help reconcile the interaction effects between local segregation and manufacturing decline: more segregated places could be the ones supporting stronger and more uniform working-class culture. When factory jobs vanish, the culture fostering high-school dropout behavior could dissolve, especially so in segregated places where the local culture may have been stronger.

In contrast to the group-level analysis of this study, a body of literature studies the individual-level effects of parental job loss. Most of it finds negative effects. For example, Oreopoulos et al. (2008) find that children whose fathers were displaced face long-lasting effects into adulthood: lower earnings, higher social assistance, and lower college attendance. Other longitudinal studies find that parental job loss decreases school grades (Rege et al. 2011) and increases grade repetition (Stevens and Schaller 2011).[1] But these opposite results do not need to be contradictory. Those children whose parent lost a job tend to be negatively affected, but—at the local level—the other children could primarily respond to the changed incentives and local environment—returns to education and the opportunity cost of it—while avoiding the cost of job loss in the family.

When factories closed in the US, some new factories opened in the developing world. In line with the results of this study, Atkin (2016) finds that local factory *openings* in export-manufacturing industries lead to *higher* school dropout rates in Mexico. Young people dropped out of school to work in manufacturing. This is a mirror image to what appears to have happened in the US. The effect is reasonably identified: Atkin (2016) uses the variation in the timing of factory openings across commuting zones in Mexico during a period of major trade reforms 1986–2000. Atkin (2016) argues that the effects are driven by the increased opportunity cost of schooling.[2]

This study's results are also consistent with the available local evidence from the US. Us-

[1] Much fewer and less strongly identified studies focus on the community-level effects of job losses. A series of papers by Ananat et al. (2011, 2017) explore this aspect by comparing U.S. states. In line with individual-level effects, they find large negative effects on student achievement and college mobility from state-level job losses. The correlations they document at the state level, however, may not need to be causal. Another interpretation is that they focus on different type of variations in job losses.

[2] Similarly, Shah and Millet Steinberg (2017) observe that in India children dropped out of school into productive work when rainfall was higher. In their setting, the opportunity cost of schooling, even for fairly young children, appears to have been an important factor in determining overall human capital investment. Munshi and Rosenzweig (2006), Shastry (2012), Jensen (2012) and Oster and Steinberg (2013) provide complementary evidence on the arrival of high-skill service jobs in India.

ing historical data, Goldin and Katz (1997), show that industrialization slowed the growth in high school attendance in the early 20th century United States. Focusing on the Appalachian coal boom and bust of the 1970s and 1980s, Black et al. (2005) find that the boom lead to increases in school dropout rates and the bust decreased them.

This analysis on the intergenerational effects of manufacturing decline builds on the work of Autor et al. (2013)—and related studies by Acemoglu et al. (2016), Pierce and Schott (2016), and Bloom et al. (2015)—by using the rapid expansion of China's exports in manufacturing goods for empirical identification. Among other results, Autor et al. (2013) confirm the classical prediction Heckscher–Ohlin model of international trade: there are winners and losers from the geography of globalization. This research paper expands their work to include analysis on the consequences of the manufacturing decline more generally, a dimension they do not consider, and by characterizing the intergenerational effects on children's education. In short, Autor et al. (2013) focus on the causes and consequences of the 1990–2007 US manufacturing decline on adult men. This paper looks at the intergenerational effects of it.

The contribution of this paper is empirical: it answers the question of how children have been affected by the rapid manufacturing decline of the 2000's in the US. To date, this question has had no attention, or an answer in the literature. This research matters because the future of work critically depends on the labor market prospects of the next generation. The paper provides new evidence on how people and communities adjust to the structural transformation of work.

The article is organized as follows. Section 2 describes the data set and the empirical methodology. Section 3 reports the primary ordinary least squares (OLS) and two-stage least squares (2SLS) estimates of the impact of manufacturing decline shocks on high-school dropout rates and college mobility. Section 4 explores the robustness of the main results through several tools. Section 5 takes the analysis further and explores interactions between the effects of children and observable characteristics of commuting zones. Section 6 discusses the findings, provides interpretations, and connects the results to earlier empirical literature. Section 7 concludes.

2 Empirical Approach

2.1 Local Labor Markets

The unit of the analysis is regional economies—the local labor markets of the United States. The idea of the geographical analysis is that strong regional variations in the industry specialization make different places differentially exposed to shocks in manufacturing employment. Decline in manufacturing has varied by region and over time, not at the individual level, making local economies a natural observation unit. The operational geographical units are 722 commuting zones (CZ) developed by Tolbert and Sizer (1996). They approximate the areas where the population of interest works.[3] The CZ:s cover all metropolitan and nonmetropolitan areas, both urban and rural, of the mainland United States. The CZ:s are based on economic geography rather than administrative borders, are time-consistent, provide more granular measurement than state-level analysis, and can be matched to various official statistics (Autor and Dorn 2013). Table 2 summarizes descriptive statistics for the CZs.[4] Figure 3 displays a map of the CZs, with the key variables of this study.[5]

2.2 Manufacturing Decline

A. Descriptive Data

The main data source on the US employment structure is the County Business Patterns (CBP) from 1991 to 2011 provided by the US Census Bureau. The CBP provides annual data on employment and payroll by county and industry. The data cover all US private employment, excluding most government employees, agricultural workers, self-employment, private household employment, and railroad workers.[6]

To complement the employment statistics, the paper uses population data from the Census Population Estimates. It provides data on the total and working-age (ages 15–64) US population at the county level. The county level data are mapped to CZs using the matching strategy detailed in Dorn (2009).

The main explanatory variable is the (annualized) decadal change in the share of manufacturing employment E_i^{MF} within total employment E_i^{TOT} in a CZ i:

[3] Tolbert and Sizer (1996) measure commuting ties between US counties and define commuting zones as collections of counties with strong commuting ties between them.

[4] An average CZ had a population of 350,000 in 1991. The largest commuting zone, New York, NY, had a population of 10.4 million and the smallest had 1,311.

[5] The object of interest in this study is the childhood environment, and therefore this analysis treats CZ:s as the observation units of interest without weighting them.

[6] For confidentiality reasons CBP reports employment by industry as an interval. I compute employment in these cases using the fixed-point imputation strategy developed by Autor et al. (2013).

$$\Delta MF_{it} = \Delta \left(\frac{E_{it}^{MF}}{E_{it}^{TOT}} \right). \tag{1}$$

Figure 1 describes the evolution of the US manufacturing employment based on CBP data. The US manufacturing employment was approximately constant in 1991–2000, but declined rapidly by 33.3 percent in 2000–2011.[7] Manufacturing's share of total employment was 19.1 percent in 1991 and fell to 10.4 percent in 2011. The rate of change in the manufacturing employment also had large variations between CZ:s and over time, as shown in the map of Figure 3 and in Table 3. While the manufacturing share of employment decreased on average in the US over 1991–2011, some places saw even increases in it. Table 2 summarizes descriptive statistics of manufacturing-to-total employment ratios, as well as employment-to-population ratios and the population size of the CZ—key baseline control variables in the estimation.

B. IV Strategy

To identify plausibly exogenous variations in manufacturing decline, I use an instrumental variables strategy (IV) based on the local industry exposure to China's imports.[8] Between 1990 and 2011, the share of US manufacturing imports from China increased over four-fold, from 4.5 percent to 23.1 percent (Fig 2).[9] This increase coincides with a sharp drop in the US manufacturing employment after 2000 (Fig 1).

The general idea is that China's entry to the world market is close to an exogenous shock to US manufacturing labor demand (Autor et al. 2013). The increase in China's exports to the US originates from China, not the US. It was sparked by China's large economic reforms in 1980–2000, and made possible by two sudden policy changes in 2001: China's accession to the World Trade Union (WTO) and a change in a US trade policy that eliminated potential tariff increases on Chinese imports (Pierce and Schott 2016, Hanson 2012, Naughton 2006). China's exports to the US were almost exclusively in manufacturing goods. This translated to a negative shock to US manufacturing labor demand in the 1999–2011 (Autor et al. 2013).[10]

The particular implementation of the IV strategy originates from the approach of Autor

[7] US manufacturing employment was 17.0 million in 1991, 17.1 million in 2000, 13.9 million in 2007, and 11.4 million in 2011, according to CBP data.

[8] The methodology draws from research by Autor et al. (2013), and related studies by Autor et al. (2014), Acemoglu et al. (2016), Bloom, Draca, and Van Reenen (2015), and Pierce and Schott (2015). For a review on the identification strategy and the related literature, see Autor et al. (2016). Dix-Carneiro and Kovak (2016) and Edmonds et al. (2013) use similar strategies based on geographic variations and trade opening in Brazil and India.

[9] UN Comtrade Database 1990–2011.

[10] Autor et al. (2016) provide a comprehensive survey on the factors behind the increase in China's trade.

et al. (2013) using local labor market variations in the US industry exposure to Chinese import competition. The measure of exposure to China's imports leverages the fact that commuting zones vary in their distribution of industrial employment, making some commuting zones more exposed to the China's import competition than others. In the data, these variations are large, as illustrated in the map of Figure 3 and quantified in Table 3. The key idea is that each US commuting zone specializes in a set of industries but not in all of them (Ellison et al. 2010). Similarly, and centrally to this analysis, China's opening affected a narrow set of industries more heavily and much less some (Pierce and Schott 2016, Autor et al. 2013). For example, places specialized in textiles and plastic goods saw sharply larger increases in China's import competition compared to places specialized in the steel, chemical, or paper industries (Autor et al. 2013).

The baseline measure of trade exposure at the CZ level (the instrument) is the local employment-weighted average of changes in the US industry import exposure ratio:

$$\Delta IP_{i\tau}^{CZ} = \sum_j \frac{L_{ijt}}{L_{it}} \times \frac{\Delta M_{j\tau}^{UC}}{M_{j,t_0} - E_{j,t_0} + Y_{j,t_0}}. \tag{2}$$

The key component of this measure is $\Delta M_{j\tau}^{UC}$, the change in imports from China in a US manufacturing industry j over the selected period τ (most estimations are performed in stacked annualized decadal differences 1991–1999 and 1999–2011).[11] It is divided by the initial absorption $Y_{j,t_0}+M_{j,t_0}-E_{j,t_0}$ at the baseline year; where M_{j,t_0} is the industry imports, E_{j,t_0} is the industry exports, and Y_{j,t_0} is the industry shipments. The industry-measure tracks export supply shocks from China to US manufacturing output demand in industries where China and the US started to compete after 2001. The industry-level measure is mapped into geographical commuting zones by constructing local industry-employment-weighted sums of industry changes: L_{ijt}/L_{it} is industry j's baseline period share of total employment in CZ i. The variations in the geographical instrument $\Delta IP_{i\tau}^{CZ}$ come from variations in the local industry employment structure in the baseline year.

An alternative measure of trade exposure at the CZ level (the alternative instrument) is analogous but based on China's imports to eight developed countries excluding the US:

$$\Delta IPO_{i\tau}^{CZ} = \sum_j \frac{L_{ijt}}{L_{it}} \times \frac{\Delta M_{j\tau}^{OC}}{M_{j,t_{0-k}} - E_{j,t_{0-k}} + Y_{j,t_{0-k}}}, \tag{3}$$

where $\Delta M_{j\tau}^{OC}$ is the change in imports from China in the manufacturing industry j in a set of eight high-income countries that excludes the US.[12] The denominator $M_{j,t_{0-k}} - E_{j,t_{0-k}} +$

[11] The year 1991 is the earliest where high-quality disaggregated bilateral trade data are available.

[12] The countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

$Y_{j,t_{0-k}}$ is defined as above but for the eight other countries.[13] The trade volumes are simply summed over the countries. The employment weights refer to the CZ:s industry employment structure as in the baseline measure. The alternative instrument is motivated by a concern that the baseline US measure can, in part, reflect US-based shocks to US import demand. The alternative instrument aims to capture the supply-component of China's exports to the US, and eventually its impact on US manufacturing industries. The identifying assumption is that the other high-income economies were similarly exposed to China's trade opening and that their industry demand shocks are uncorrelated with each other.[14] Intuitively, the supply component is correlated between the countries, while the demand component is less so. A large literature, surveyed by Autor et al. (2016), highlights that still the main source of variations in China's exports to the US comes from factors internal to China. But the alternative instrument can potentially clean US industry demand shocks from the estimation.

Data on international trade for 1991–2011 come from the UN Comtrade Database. It provides bilateral imports and exports data harmonized at the six-digit HS product level. I match the product-level data to four-digit SIC industries using the crosswalk of Pierce and Schott (2012). The crosswalk assigns 10-digit HS products to four-digit SIC industries (at that level each HS product maps into a single SIC industry). The data from UN Comtrade are at the level of six-digit HS products. At that level some HS products map into multiple four-digit SIC industries. To weight product data to industries, I use US import data at the 10-digit HS level, averaged over 1995–2005. This process aggregates the four-digit SIC industries to 397 manufacturing industries that all have product codes assigned to them. As in Autor et al. (2013), to match other industry data, I merge a few industries together, resulting in 392 manufacturing industries. All trade amounts are inflated to 2007 US dollars using the Personal Consumption Expenditure (PCE) deflator obtained from the US Federal Reserve. Table 3 summarizes the CZ-level changes in exposure to China's imports.

Intuitively, the main estimates come from comparing changes in high-school dropout rates between places with different patterns in manufacturing employment share over time. I focus on the variations in manufacturing employment that come from the exposure to Chinese imports. As argued earlier, these variations plausibly came from outside the system unexpectedly (Autor et al. 2013, 2016). This makes the comparisons between changes in

The specific set of countries is based on data availability: these are the only high-income countries that have bilateral trade data available in 1991 at a level that can be harmonized to HS classification.

[13]The local industry employment data are from 1988 (not 1991) to reduce the error covariance between the dependent and independent variables.

[14]This assumption, made in Autor et al. (2013, 2014, 2015, 2018) and Acemoglu et al. (2016), among others, is rather strong, and unlikely to hold literally. But the alternative instrument can still help overcome some part of the endogeneity issues regarding US import demand shocks.

CZ high-school dropout rates and changes in CZ manufacturing employment potentially informative.

To make the comparisons cleaner, I control for a set on baseline characteristics of the places: the baseline manufacturing share of employment, region of the US,[15] employment–to–population ratio, and the population size of the CZ. The baseline manufacturing share control induces comparisons between places that had a similar share of manufacturing employment but saw different declines in it due to differential exposure to China's opening to the world market. This control is important, since variations in the instrument are especially pronounced within the manufacturing sector (see, Tab. 4). The regional controls narrow the comparisons to within-region differences, so that the results are not driven by differential trends between regional areas of the US. The controls for employment–to–population ratio and the population size of the CZ narrow further the comparisons to between places with similar employment rates and labor market size. The commuting zone baseline controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 periods. In the analysis, treatment is the manufacturing decline, and the comparison group is the otherwise similar places that had a smaller decline in manufacturing. The specifications also include a control for a time-trend.

This research focuses on manufacturing decline, instrumenting it by changes in China's import shares, in contrast with a large research literature initiated by Autor et al. (2013) that studies the labor market consequences of trade with China. That is, the approach of this study creates variations in manufacturing rather than only in trade exposure. From this perspective, Autor et al. (2013) trade exposure estimates depict the reduced form relationship, and my estimates are the IV estimate of interest, scaling up the trade exposure with the induced variations in manufacturing. The estimates are interpretable as the local average treatment effects (LATE) of the manufacturing decline if the China shock works exclusively through its effect on manufacturing employment. Extensive previous research suggest that this is the case (see, Autor et al. 2016, for a review). To be clear, the rapid rise in China's imports to the US had various effects on local labor markets.[16] In the previous literature, these effects have been interpreted to be working through trade exposure's effect on manufacturing industry. But with imperfections in labor and other markets, China's trade shock may have had an independent effect on manufacturing firm revenues, without working through changes in the manufacturing employment, translating to incomes and tax revenues that can both affect children's outcomes. While this is unlikely to be qualitatively important, I report both reduced form and IV estimates.

[15] The regional controls indicate nine regional census divisions.

[16] These effects vary from reductions in employment rates to increases crime (Autor et al. 2013, Deiana 2016).

In terms of interpretation, the analysis at the CZ level jointly estimates the reallocation and aggregate demand effects of the manufacturing labor demand shock (as pointed out by Acemoglu et al. 2016). The reallocation effect works through the movement of production factors from the declining sectors to new sectors. The aggregate demand effect multiplies the negative direct and indirect effects of the manufacturing decline stemming from import growth from China. The instrument combines induced employment shifts in both trade-exposed and non-exposed industries. Put simply, the estimates capture the total effect of China-induced manufacturing decline working through many potential channels, including employment-, wage-, and public finance effects, and social and psychological responses within the community.

The IV strategy estimates the local average treatment effect (LATE). It is the effect of treatment on the population of compliers. The compliers are those places that saw a decline in the manufacturing precisely due to China's opening to the world market. The effects of manufacturing decline in these places may differ from the effects in some other places where manufacturing employment declined for some other reason. But this effect in left-behind places hurt by globalization is exactly what this study and many policy makers are interested in (see, for example, The Economist 2017). In particular, the LATE may reflect the effect of unexpected manufacturing decline, while the OLS estimates could capture more secular trends. The IV estimates reflect the effect of differential exposure to manufacturing decline, which may differ from the effect from aggregate US manufacturing decline.

Critical threats to the validity of the estimates come from omitted variables correlated with the instruments. A key threat is selective mobility. That is, the empirical strategy essentially considers synthetic cohorts over time in different places. But this idea does not work if the cohorts are significantly unstable over time. Validating this aspect of the identification strategy, evidence from the US suggests that mobility responses to labor market shocks in 1991–2011 have been small and incomplete (Glaeser and Gyourko 2005). Less educated workers and their families—many of which work in manufacturing and are subject to the largest variations of the treatment—are even less mobile (Notowidigdo 2011). In particular, investigating mobility and trade shocks, Autor et al. (2013) find little impact of regional trade exposure on changes in mobility. Furthermore, Autor et al. (2014) consider whether workers initially employed in more trade-exposed industries are more likely to change their place of residence, and find little effects.

In summary, the IV strategy constructs plausibly exogenous variations in manufacturing employment between places that without being exposed to the instrument could have had similar trends in educational outcomes. Using this strategy, I can evaluate the effects of trade-induced manufacturing decline on places and children.

2.3 Education

A. High-School Dropouts

The main educational outcome is the high-school dropout rate. Data on high-school dropout rates come from the US Census for the years 1990 and 2000, and from the American Community Survey (ACS) for the year 2011.[17] It is defined as the share of civilian 16 to 19 year-old population that is not enrolled in school nor is a high school graduate. The benefit of high-school dropout rate as an outcome is that it captures activity rather than a cumulative stock value. The US Census and ACS report the data at the county level. I match the counts on 16-19 year-old total population and high-school dropout population to the CZ-level using the matching strategy detailed in Dorn (2009), and compute the CZ-level high-school dropout rates. The US Census and ACS are particularly useful data sources for geographical analysis due to their full coverage and large sample size.

For estimation, the main outcome variable is the (annualized) decadal change in the high-school dropout rate in a CZ i over time period τ:

$$\Delta HS_{i\tau} = \Delta \left(\frac{HS_{i\tau}^{16-19}}{POP_{i\tau}^{16-19}} \right) \tag{4}$$

where $HS_{i\tau}^{16-19}$ is the number of 16 to 19-year-old residents of the commuting zone (CZ) i that are not in high school nor high-school graduates, and $POP_{i\tau}^{16-19}$ is the population of 16 to 19-year-olds in the same CZ. Most analyses focus on time period τ over 1990–2011.

Table 1 shows descriptive statistics for high-school dropout rates in CZs. On average, high-school dropout rate was 10.3 percent in 1991 and decreased to 6.0 percent in 2011. These averages mask large geographical variations in the trends. A map in the Figure 3 visualizes the geography of changes in high-dropout rates, and compares it to the changes in the manufacturing employment share. A simple visual comparison suggests that places where high-school dropout rates declined are also places where manufacturing declined.

B. College Mobility

As an alternative measure, I use the college-income gradient developed by Chetty et al. (2014).[18] This outcome variable—college mobility—measures the degree to which a child's college attendance at age 19 is predicted by parental income. It captures one aspect of college access of the young people who were born in a given commuting zone.

[17] Starting in 2010, the Census stopped using the long form survey and reports education data in the American Community Survey. The American Community Survey measure is computed as a five-year average over 2009–2013. Additional analyses use high-school dropout rates over 1970–1990 from the US Census.

[18] No publicly available US database captures college attendance by the place of birth, previous schooling location, or parental place of residency.

College mobility is computed from the restricted access universe of individual tax returns from the U.S. Internal Revenue Service (IRS). In the underlying data, college attendance is defined as an indicator whether the child has a 1098-T form filed on her behalf when she is 18–21. All colleges and universities, vocational schools, and other postsecondary institutions that are eligible for student aid—are require to file 1098–T forms that report the tuition payments or scholarships received by the student. The 1098-T forms are reported by the universities independently of individual tax returns and plausibly cover the college attendance for all US children. Chetty et al. (2014) document that the tax records capture college attendance quite accurately. The parental income data come similarly from the US tax records, and is defined as the pre-tax adjusted gross income plus tax-exempt interest income and the non-taxable portion of Social Security and Disability (SSDI) benefits. The income measure includes labor earnings, capital income, unemployment insurance, Social Security, and disability benefits, but excludes nontaxable cash transfers, such as food stamps.

This paper uses the public-use summary statistics on intergenerational mobility at the CZ-level provided by Chetty et al. (2014) with an agreement from the IRS. The data are available by CZ for cohorts born between 1984 and 1993.[19] The data include two summary statistics for each CZ and cohort: the estimated slope of a linear equation that predicts college attendance based on parental income, and an intercept. In particular, Chetty et al. (2014) estimate the slope and the intercept of the conditional expectation that a child is attending college given her parents' national income rank for each CZ i and cohort c:

$$C_{jic} = \alpha_{ic} + \beta_c P_{ic} + \varepsilon_{ic} \tag{5}$$

where C is an indicator for a child j being enrolled in college at age 19. The slope of the college–income relationship (β_c) measures the degree of relative college mobility in CZ i and for cohort c.[20] The linear conditional expectation fits the data remarkably well (Chetty et al 2014).

For the analysis of this paper, I construct a measure of "absolute upward mobility" (Chetty et al. 2014) at percentile p in CZ i for cohort c, as the expected probability of attending college for a child who grew up in CZ i with parent who have a national income rank of p: $\bar{c}_{pic} = \alpha_{ic} + \beta_{ic} p_c$. In particular, I focus on the CZ-cohort average of college attendance of children with parents at the 25th percentile in the national distribution, $\bar{c}_{25,ic} = \alpha_{ic} + 0.25\beta_{ic}$.

As the outcome variable, I use an annualized decadal change in the 25th percentile

[19] To preserve confidentiality, values for CZ-cohort cells with fewer than 250 observations are omitted.

[20] Note that this reverses the notation of Chetty et al. (2014) to maintain consistency with respect to other notation in this paper.

college mobility (CM) between the cohorts born in 1984 and 1993 in the CZ i:

$$\Delta CM_{25,i\tau} = \Delta \left(\alpha_{i\tau} + 0.25\beta_{i\tau}\right). \tag{6}$$

The idea behind using changes between cohorts 1984 and 1993 is that most of the manufacturing decline and increase in the China's imports began after 2001, the year the cohort born in 1984 turned 17, and a year before the median starting-age of college. In contrast, the cohort born in 1993 turned 17 in 2010, a year before the end-line of our analysis period. While the control group may also have been affected by the manufacturing decline, the difference between the cohorts captures the change in treatment intensity.[21] In line with this idea, I define this measure as the change in college mobility over 1999–2011, the college starting years of each cohort. Tables 1 and 3, and the map in Figure 3 report descriptive statistics on college mobility over 1999–2011.

The drawback of the college mobility data is that it is only available for a single 9-year change. This reduces statistical power and prevents from including controls for time-trends and the pre-period evolution of college mobility. For data confidentiality reasons, the measure is only available for 616 CZs.

3 Estimates

The main specification is a stacked first-difference model for annualized decadal changes in the CZ-level variables 1991–2011:

$$\Delta Y_{i\tau}^{CZ} = \alpha_\tau + \beta \Delta MF_{i\tau}^{CZ} + \gamma X_{i0} + e_{i\tau} \tag{7}$$

The dependent variable is either ΔHS_{it}, the annual change in the high-school dropout rate in CZ i over time period τ, or $\Delta CM_{25,i}$, the annual change in the 25th percentile college mobility (CM) between the birth cohorts of 1984 and 1993 in CZ i. The term X_{i0} is a set of CZ start-of-period controls; α_τ is the time effect; and $e_{i\tau}$ is the error term. The key explanatory variable in this model is $\Delta MF_{i\tau}^{CZ}$, the annual change in the manufacturing–to–total employment ratio over period τ in CZ i. The coefficient β reveals the impact of manufacturing decline on educational outcomes. The standard errors are clustered by commuting zone to allow for over-time error correlations.

To establish plausibly causal interpretation, I instrument for the decline in manufacturing employment share using the contemporaneous growth of China's imports to the US,

[21] Research by Chetty et al. (2014) and Chetty and Hendren (2017) suggests that the effects from the exposure to local conditions come mostly when the children are young. This supports the approach of comparing these cohorts.

$\Delta IP_{i\tau}^{CZ}$, as specified in Section 2.2, or alternatively using the growth of China's imports to the eight other high-income countries, $\Delta IPO_{i\tau}^{CZ}$, specified in Section 2.2. The variations in the instrument come from variations in local industry employment structure, making some places more exposed to rise in China's exports. Table 3 summarizes the CZ-level changes in the key variables: manufacturing share, import exposure, high-school dropout rate, and college mobility.

A. The First Stage

The analysis begins by estimating a first stage relationship between the commuting zone exposure to China's imports and manufacturing decline. The first stage is estimated from stacking changes in CZ manufacturing-to-total employment ratio and exposure to Chinese imports within local industries over the periods 1991–99 and 1999– 2011:

$$\Delta MF_{i\tau}^{CZ} = \alpha_\tau + \beta \Delta IP_{i\tau}^{CZ} + \gamma X_{i0} + e_{i\tau} \tag{8}$$

The term X_{i0} is a set of CZ-by-sector start-of-period controls, α_τ is the time effect; and $e_{i\tau}$ is the error term. Table 4 details the estimates obtained with this approach. The sizable F-statistics for the excluded instruments indicate that regional variations in import exposure have a strong influence on the likelihood of manufacturing decline for CZ:s. The columns 1–3 are estimated without the control for the baseline manufacturing employment share, while the columns 4–6 include that control. Within the CZ:s with the same start-of-period share of manufacturing employment and other baseline controls, the coefficient of trade exposure variable is smaller (-.87 vs. -2.18) but its explanatory power is larger (adjusted R^2 of 0.40 vs. 0.29).

As a visual illustration of the first stage relationship, Figure 4 plots the value of the instrument, import exposure as detailed in the Equation 2, against the value of the explanatory variable, manufacturing decline as in Equation 1, for all US commuting zones over 1991–2011, which is equivalent to the first-stage regression in Table 4 but without additional controls and performed in single annual change over 1991–2011. The slope coefficient is –2.80 with standard error 0.21 and t-statistic –13.4. The regression has an R-squared of 0.35, again indicating a relatively strong predictive power of import growth from China for the US manufacturing decline (as also reported by Autor et al. 2013).

B. High-School Dropout Rate Estimates

The OLS and 2SLS estimates of manufacturing decline effects on commuting zone high-school dropout rates 1991–2011 are presented in Table 5.

Columns 1–4 present the OLS estimates, progressively including additional baseline controls in the specification. These estimates do not have a causal interpretation, but show a negative relationship between manufacturing decline and high-school dropout rates. In places where manufacturing has declined, high-school dropout rates have declined, too. In columns 1–3, the estimates of the predictive effect vary from −.109 to −.0733 with $p < 0.01$. However, including regional controls for nine US Census divisions make the effect smaller and in the most restrictive model the coefficient is statistically insignificant.

Columns 5–8 present the 2SLS estimates. Column 5 of Panel A considers the relationship between CZ manufacturing decline and changes in CZ high-school dropout rates without additional controls, except for a control for a time trend. The strongly negative and statistically significant point estimate in this column indicates that a 1 percentage point decrease in the manufacturing share of total employment decreases the high-school dropout rate among CZ's 16- to 19-year-old population by .227 percentage points. The OLS and IV estimates are different possibly because the IV estimates capture the effect of unexpected manufacturing decline, while OLS estimates reflect the more predictable secular decline in manufacturing that may have had less impact.

The last three columns of Panel A and Panel B, refine the estimates and explore the robustness, by controlling for the initial manufacturing employment share in a local labor market (Panel B), the initial population (col. 6), the employment-to-population ratio at the baseline (col. 7), and for nine census divisions (col. 8).

By controlling for local manufacturing intensity in Panel B, I allow for differential employment trends in the manufacturing and non-manufacturing sectors. This creates (thought) comparisons between with places with the same manufacturing intensity but saw different changes in it, due to exposure to China's trade. The control for initial population allows for different time trends in local labor markets with different sizes. Similarly, the control for employment-to-population ratio allows for separate trends for labor markets with different levels of activity. The controls for census divisions allow for heterogeneity in regional time trends. The control for the baseline manufacturing employment share has a sizable impact on the estimates. It increases the estimate from –.227 to –.433, without additional covariates. Adding the other covariates has a modest impact on the manufacturing decline coefficient. Among these covariates, the regional controls seem to matter the most. The most restrictive, and preferred, estimate remains sizable and statistically significant at –.366 in column 8 of Panel B.

Taking together the OLS and 2SLS estimates suggests that manufacturing decline is associated with a reduction in high-school dropout rates. In this data, this effect varies between –.16 and –.37 percentage points per a 1 percentage point decrease in the manufacturing share of total employment. In terms of magnitude, the average high-school dropout rate in 1991 was 10.3 percent; and the decline in manufacturing share of total employment across CZs was 7.9 percentage points over 1991–2011. Using the preferred estimate of –.366, this translates to a 2.9 percentage point reduction in the commuting zone high-school dropout rate over 1991–2011—a large but reasonably sized effect.

C. College Mobility Estimates

The OLS and 2SLS estimates of manufacturing decline effects on commuting zone college mobility 1999–2011 are presented in Table 6. As described in detail in Section 2.3, college mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records.

Columns 1–4 present the OLS estimates. They show that, on average, places that saw declines in manufacturing as a share of total employment were also the places that saw increases in college mobility. The predictive effect is smaller but stays statistically and economically significant after controlling for a set of baseline characteristics of these places. The estimates of the predictive effect vary from .397 to .254 with $p < 0.05$.

Columns 5–8 present the 2SLS estimates. In Panel A, without additional controls, the estimate in the column 5 considers the relationship between CZ manufacturing decline and changes in CZ college mobility. Consistent with the results of the high-school dropout rate analysis, the 2SLS estimate in column 5 implies a positive and statistically significant effect from manufacturing decline to college mobility. In particular, the estimate in this column indicates that a 1 percentage point decrease in the CZ's manufacturing share of total employment increases the CZ average of college attendance of children with parents at the 25th percentile in the national distribution by .36 percentage points. Controlling for the baseline population size and employment-to-population ratios leaves the 2SLS estimates largely unchanged. Including the nine regional census indicators makes the estimate insignificant, but keeps its sign unchanged and the magnitude in the ballpark.

In Panel B, focusing on the variations within a set of places with similar manufacturing start-of-period share of total employment, the estimates are not anymore statistically significant. However, most coefficients do have the same sign and, while smaller, fit into the range of the estimates of Panel A. The college mobility variable covers only a one observation per CZ. A plausible interpretation is that including the manufacturing share control leaves too few degrees of freedom to produce precise estimates.

Although, in general, the estimates highlight a negative descriptive relationship between manufacturing share of employment—working class jobs—and college mobility, the most restrictive causal estimates are inconclusive.

In terms of interpretation, a drawback of the college mobility measure is that it leaves a few possibilities for the mechanism driving the increases (or decreases) in it.[22] The simple case is that relatively poorer children enroll more in college. However, the focus on the national distribution creates a complication when looking at changes over time. A decline in local income moves the residents left in the national income distribution. But if income is not an important determinant of college access in that place, this decline in incomes translates to an increase in the college mobility measure: now poorer children (that were previously rich) are more likely to go college. The drawback aside, supporting a non-mechanical interpretation, Chetty and Hendren (2017) provide evidence that the given college mobility rates of a CZ are largely interpretable as causal effects of the place. While the most restrictive estimates are inconclusive, the research of this paper suggest a potential causal chain from lost manufacturing jobs to a place that provides higher college access to poor children. To establish or dispute the causal chain, more research is needed.

4 Robustness

A. Pretrends

US high-school dropout rate has been declining since the 1970s,[23] and manufacturing as a share of employment has also trended downward since the 1950s.[24]A visual inspection of the maps in Figure 3 suggest that in the period 1991–2011, these trends tended to be stronger in the same places. This association could, however, be a result of a long-standing secular trend. The correlation this study documents between declining manufacturing share of employment and contemporaneous declines in high-school dropout rates during 1991–2011 could potentially predate the recent decline in manufacturing. In that case, the estimates would likely overstate the impact of manufacturing decline in the current period. To address this concern, I include measures of pretrends in high-school dropout rates in Table 7, specifically two terms for the change in the CZ high-school dropout rates, measured over the intervals 1970–80 and 1980–1990.[25]

Formally, the pre-trend controls mean including lagged dependent variables to the stacked first-difference specification:

[22] This complication is not highlighted in Chetty et al. (2014). Unfortunately, the raw data on college attendance by place of birth, or similar data, are not available.

[23] Source: US Census.

[24] Source: Community Business Patterns.

[25] Data on college mobility are only available over 1999–2011 and thus does not allow for testing pretrends in that outcome variable.

$$\Delta Y_{i\tau}^{CZ} = \alpha_\tau + \beta \Delta MF_{i\tau}^{CZ} + \gamma X_{i0} + \delta_1 \Delta Y_{i1980\text{–}90}^{CZ} + \delta_2 \Delta Y_{i1970\text{–}80}^{CZ} + e_{i\tau} \tag{9}$$

Table 7 replicates the main set of results on high-school dropout rates but including the pretrends. The pretrend variables have no important effect on the magnitude or precision of the coefficient of interest: the estimates are close to that found in the main Table 5. The measured effects are slightly larger, increasing from −.366 to −.418 with $p < 0.01$ in the preferred and most restrictive specification. However, this hints that even with the IV strategy, there is some temporal dependence left in the local high-school dropout series.

B. Falsification Test

As a falsification test, Table 8 reports results from a 2SLS regression of changes in high-school dropout rates in earlier decades on the instrumented manufacturing decline between 1999 and 2011. For the identification strategy, it would be a concern if *future* declines in CZ manufacturing due to China's trade opening predicted *past* changes in local high-school dropout rates—in the time periods before China had affected US manufacturing. Operationally, I estimate a set of models:

$$\Delta MF_{i\tau}^{CZ} = \alpha_i + \beta \Delta IP_{i1999\text{–}2011}^{CZ} + \gamma X_{i0} + e_{i\tau}, \tag{10}$$

where τ takes four different values: 1970–80, 1980–90, 1990–2000, and 1999–2011.

In Panel A, the first row performs the estimation without additional controls. The rows 2–4 go through combinations of regional controls and the controls for baseline share of manufacturing in total employment. The results from the specifications that include baseline controls, either the regional controls or manufacturing share, show largely that future instrumented manufacturing decline does not predict past changes in high-school dropout rates. Conversely, the estimate is large and significant in the contemporaneous period 1999–2011 where it should be. Adding demographic covariates keep the estimates essentially unchanged (not reported). This pattern of results is consistent with the identifying assumption that the within-industry and CZ correlation between declining manufacturing employment and import penetration from China in 1991–2011 that seems to translate to reductions in high-school dropout rates, originates from trade shocks rather than long-term secular trends between manufacturing employment and high-school dropout rates.

However, the specifications that do not include any covariates show some evidence of temporal dependence in the high-school dropout rate series. The predictive effect is visible for the 1970–1980 period.[26] But including heterogeneous regional trends makes this effect

[26] A significant predictive effect for 1970–1980 from China's imports to manufacturing employment is

disappear. This pattern of findings suggests that the regional baseline covariates are necessary for the identification of the empirical results. In either pretrend analysis or falsification test, the most restrictive manufacturing employment control does not make a difference.

C. Alternative IV

A reasonable concern is that the measured US imports from China, used to construct the main instrument, could be correlated with domestic US demand shocks rather than reflecting external supply factors external to the US labor and product markets, possibly resulting in biased estimates. As detailed in Section 2.2, an alternative instrument uses China's import growth in eight other high-income countries as detailed in Equation 3. The idea is that the other high-income face a similar supply shock from China, while are possibly subject to different idiosyncratic industry-specific demand shocks. Intuitively, China's trade flows to other countries than the US are plausibly less determined by factors internal to the US and more by factors related to China's opening to the world market.

The alternative instrument is explored by estimating the main specification,

$$\Delta Y_{i\tau}^{CZ} = \alpha_\tau + \beta \Delta MF_{i\tau}^{CZ} + \gamma X_{i0} + e_{i\tau} \tag{11}$$

but instrumenting the changes the manufacturing as a share of total employment, $\Delta MF_{i\tau}^{CZ}$, with the contemporaneous change in China's imports elsewhere, $\Delta IPO_{i\tau}^{CZ}$.

Table 11 reports the results from the alternative IV estimation both for high-school dropout rates 1991–2011 and college mobility 1999–2011. The point estimates are almost identical to the main estimates of Tables 5 and 6. The first stage relationship is equally strong with F-statistic 155.0 and adjusted R^2 of .22 without baseline controls, and F-statistic 71.7 and adjusted R^2 of .39 with a full set of baseline controls.

D. Reduced Form Estimates

Interpreting the IV estimates as the effect of manufacturing decline requires assuming that China's trade exposure affected educational outcomes exclusively through its effect on manufacturing employment. Recall, that the variations in the instrument come exclusively from differential manufacturing industry compositions between places. Therefore, from this perspective, the assumption is not unreasonable (see Section 2.2 for further discussion). But although most of the effect are likely to translate through the manufacturing industry, the increased competition could affect the community and thus education through manufacturing industry profits and reduced demand for suppliers rather than employment. Based on

similarly found in Autor (2013) p. 2135.

other studies these income- and public finance effects would likely reduce human capital investment, and bias the estimates downwards (see, for example, Davis and Von Wachter 2011, for a review).[27] For conceptual clarity, in addition to main IV estimates, I provide the reduced form estimates from trade to educational outcomes, both for high-school dropout rates and college mobility.[28] In particular, I estimate the following model:

$$\Delta Y_{i\tau}^{CZ} = \alpha_\tau + \beta \Delta IP_{i\tau}^{CZ} + \gamma X_{i0} + e_{i\tau} \tag{12}$$

where the dependent variable is either ΔHS_{it}, the annual change in the high-school dropout rate in CZ i over time period τ, or $\Delta CM_{25,i}$, the annual change in the 25th percentile college mobility (CM) between the cohorts of 1984 and 1993 in CZ i. The term X_{i0} is a set of CZ-by-sector start-of-period controls; α_τ is the time effect; and $e_{i\tau}$ is the error term. The explanatory variable in this model is $\Delta IP_{i\tau}^{CZ}$, the annual change in exposure to Chinese imports within local industries over period τ in CZ i. The coefficient β reveals the impact of trade exposure on educational outcomes. The standard errors are clustered by commuting zone to allow for over-time error correlations. Table 12 presents the results.

In Columns 1 and 2, the reduced form estimates have the same signs and similar magnitudes than the IV estimates for both high-school dropout rates and college mobility. As before, including baseline control for the manufacturing employment share makes the college mobility coefficient insignificant. However, the coefficient is still large, positive, and significant when including all other baseline controls.

In Columns 3 and 4, the import exposure instrument itself is instrumented with the alternative instrument constructed from Chinese imports to eight other high-income countries, as in Autor et al. (2013). This specification produces larger results, the estimates increase almost by a factor of two. This suggests that quantitative results are somewhat sensitive to the choice of particular instrument, but qualitatively show the same pattern.

E. Log-Log Specification and Baseline Education

So far, the analysis has adjusted for the baseline differences by considering first differences of the variables, controlling for some baseline characteristics of the places, and using the IV strategy. A concern might be still that the places with initially higher high-school dropout rates might have larger response to manufacturing decline. And these places could be the same places where manufacturing declined due to exposure to China's trade. This could bias the results upwards. Now, I consider two extensions to address this. The following

[27] The reduced form estimates are also less sensitive to measurement error.

[28] Note that simply controlling for, say changes in the unemployment rate would be bad control, since those changes would most likely be caused by the manufacturing industry exposure to China's trade.

discussion focuses on the high-school dropout rates, because the college mobility results with this research design were inconclusive.

First, I estimate the main specification in logarithms:

$$log(\Delta HS_{i\tau}^{CZ}) = \alpha_\tau + \beta log(\Delta MF_{i\tau}^{CZ}) + \gamma X_{i0} + e_{i\tau}, \tag{13}$$

with the same notation, variables, and instrumentation as earlier. This specification considers relative changes in manufacturing and the high-school dropout rate. Table 13 reports the results. The estimates are similar in sign, significance, and magnitude to the main results that were estimated in percentage points. The estimate −.498 in column 2 means that a 1 percentage (relative) decline in the manufacturing share of total employment decreases the high-school dropout rate among a CZ's 16- to 19-year-old population by .498 percentages.

Second, I control for the baseline high-school dropout rate:

$$\Delta HS_{i\tau}^{CZ} = \alpha_\tau + \beta \Delta MF_{i\tau}^{CZ} + \gamma X_{i0} + HS_{it}^{CZ} + e_{i\tau}, \tag{14}$$

where HS_{it}^{CZ} controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 period. Now initial high-school dropout rate is used both to compute the dependent variable and as a control variable. While this is a different model than the main specification, the key idea is making treated and control units comparable on lagged outcomes.[29] Table 13 reports results from this specification. The estimates show more dispersion, but are in line with the earlier results. The preferred estimate with full set of baseline controls is almost unchanged.

5 Exploring the Mechanism

A. Rural vs. Urban

Are the effects of manufacturing decline on children's education similar around the US, or are the effects different in rural versus urban America? To study this, I estimate interactions between CZ manufacturing decline and the CZ being located in a rural part of the US. The US Census measures the share of rural population in each US county based on where people work (Ratcliffe et al. 2016). I match this data to CZs weighting by the population of each county. I define a rural CZ as a place where more than 50 percent of the population lives in a rural setting, and compute an indicator variable for it. I explore different thresholds up until 90 percent, with no large impact on results.

Table 9 presents the results for rural-urban interaction analysis. The rural-interaction

[29] Imbens and Wooldridge (2009) provide an informed discussion on the details of this.

coefficient is small, insignificant, and has only a minor effect on the main coefficient. While not decisive, the results suggest the rural-urban distinction does not play a key rule in channeling the effect. This is a substantial finding: many commentators feel that manufacturing decline and the issues associated with it are particularly an issue of rural America (The Economist 2017). But the implications for children's education appear similar in both.

B. Correlates of the Intergenerational Effects

What characterizes the places where manufacturing decline tends to lead to lower high-school dropout rates? One would expect that some place-based characteristics would matter. For example, places with high income- and racial segregation might not be able to channel students to high-school after a decline in manufacturing employment. Again, places equipped with generous educational resources might see larger decreases in high-school dropout rates after manufacturing jobs have declined. But the following results show that neither is true—the opposite is.

To produce these results, I estimate interactions between CZ manufacturing decline and a large set of CZ's baseline community factors that have been discussed in the sociology and economics literature, such as segregation and inequality. Because most of these factors are relatively stable over time, and I only have data for essentially one period, I focus on cross-sectional characteristics. The estimation is in stacked annualized decadal differences over 1991–2011:

$$\Delta HS_{i\tau}^{CZ} = \alpha_\tau + \beta_1 \Delta MF_{i\tau}^{CZ} + \beta_2(\Delta MF_{i\tau}^{CZ} \times K_i) + \beta_3 K_i + \gamma X_{i0} + e_{i\tau} \tag{15}$$

The outcome variable is ΔHS_{it}, the annual change in the high-school dropout rate in CZ i; K_i is the time-invarying interacted community variable included in each model one at a time, X_{i0} is a set of CZ start-of-period controls; α_τ is the time effect; and $e_{i\tau}$ is the error term. Again, the main explanatory variable in this model is $\Delta MF_{i\tau}^{CZ}$, the annual change in the manufacturing–to–total employment ratio over period τ in CZ i. The key parameter of interest in this model is β_2, the coefficient of the interaction term. With two endogenous variables, I instrument for the decline in manufacturing employment share using the contemporaneous growth of China's imports to the US, $\Delta IP_{i\tau}^{CZ}$, as specified in Section 2.2, and with the interaction term between the fixed community variable and China's imports, $\Delta IP_{i\tau}^{CZ} \times K_i$. The analysis of community characteristics is limited to the 2SLS estimates on high-school dropouts since the estimates for college mobility are considerably more sensitive to specific controls and regional trends.

Tables 14 and 15 describes the set of interacted variables and their sources. The data on local factors was compiled by Chetty et al. (2014). The authors provide a comprehensive overview on variable definitions and measurement.

Two main results emerge (Tab. 10). First, segregation and share of black population strongly interact with the positive effects of manufacturing decline on education. That is, the effects of manufacturing decline are largest in the areas with high segregation and in those with larger African American populations. This is true for several different measures of segregation. While the risk of false rejections of the null is present with multiple testing, the fact that many different measures of segregation produce a similar result supports this finding.

Second, educational resources—student-teacher ratio and school expenditure per student—do not significantly interact with the main effects. An exception is the number of colleges in CZ, which has a predictive effect of making the manufacturing effect smaller. Additionally, some other insignificant coefficients are noteworthy. For example, religion and social capital (Putnam 1995, measured as activities related to civil society) are both strong predictors of upward income mobility (Chetty et al. 2014). However, they do not interact with the effects of manufacturing decline on children's education.

What the main results seem to suggest is that manufacturing jobs—or a broader working-class community—keep a pathway open for teenagers to drop out of high school. When those jobs decline, the pathway declines, too. Now this effect appears to be stronger in more segregated places. Perhaps the factors behind segregation, or segregation itself, support the pathway. Sociological work by Willis (1977), among others, supports this hypothesis.

Compared to the previous literature on the determinants of children's outcomes, these interactions show significance for very different factors than, for example, for which Chetty et al. (2014) find positive predictive power. In particular, segregation strongly correlates with low upward mobility in a cross section of CZs. And educational resources strongly predict high mobility (Chetty et al. 2014). Perhaps surprisingly, manufacturing decline has the highest effect in those places that on average fail to produce upward mobility of income.[30]

The results suggest that trade-induced manufacturing decline leads to lower high-school dropout rates—especially so in segregated places and those with larger share of black residents. As matter of correlation, local investment level in schooling does not predict the effect. These are new and puzzling findings.

[30] In the cross-sectional evidence of Chetty et al. (2014), the share of manufacturing employment only weakly and negatively correlates with upward mobility.

6 Discussion

What explains the main results? The standard Becker (1964) human capital investment model compares the marginal costs and benefits of education. The key primitive is the economic returns to education. The model has lead many labor economists to argue that educational investment would increase vis-à-vis increasing income inequality (Ananat et al. 2017). The simplest model argues that manufacturing workers' children would notice that manufacturing no longer offers stable jobs, and would obtain higher education than what their parents had.[31]

The sociological account of Willis (1977) offers a complementary view, highlighting how children inherit occupations and class from their parents. It argues that predominantly working class communities—places where the share of manufacturing employment is high—help children embrace an anti-school mentality and prepare them for low-education working-class employment. For example, Willis (1977) argues, working-class fathers may act as role-models to their children and through that channel affect the children's educational choices. Extrapolating from Willis' (1977) observations in working-class communities, a decline in manufacturing could lead to a decrease in such role models and translate to an increase in children's education. The results suggests that this may have been the case.[32] Willis' theory could also help reconcile the interaction effects between local segregation and manufacturing decline. Perhaps working-class culture was stronger in the areas with higher segregation.

In contrast, available evidence on job loss and income shocks indicates that negative shocks lead to negative effects on children's education (Davis and Von Wachter 2011). This is clearly seen in the scarring effects of parental job loss observed by Oreopoulos et al. (2008). That literature suggests that education investment benefits from the resources that are available to the child. In addition to parental effects, William Julius Wilson (1996) in "*When Work Disappears*" and earlier Whyte (1943) point to the loss of jobs, fuelled by decline in manufacturing, as a driver of social anomie and community-level distress in poor neighborhoods and increasing childhood poverty (Autor et al. 2017).

[31] Goldin and Katz (2000) describe the lack of manufacturing jobs and its consequences on education—in the US prairie states of 1910: "Youths in these states could not have worked in industry, for there was scant manufacturing – –. And although many farmers would have preferred that their children remain on the land, most knew it would prove impossible for all but one. The best they could do was to endow their children with education to be mobile."

[32] Commenting on the work in the line of Willis (1977), Vance (2016 pp. 246) confirms this observation in his personal memoir of growing up in rural America: "working-class boys like me do much worse in school because they view schoolwork as a feminine endeavor." Vance (2016 pp. 244) also suggests—from his own experience and observations—that psychological and social factors could be much more important than traditional economic factors: "My elementary and middle schools were entirely adequate – –. I had Pell Grants and government subsidized low-interest student loans that made college affordable. The real problem for so many of these kids is what happens (or doesn't happen) at home."

These two lines of thinking—positive factors of Becker (1964) and Willis (1977), and the negative factors of Oreopoulos (2008) and Wilson (1996)—highlight a tension between *income* and *opportunity* shocks. In the empirical literature, this tension is perhaps clearest in the case of Indian casino openings. Federal legislation in 1988 allowed Indian tribes to open casinos in many states, leading to the opening of nearly new 400 casinos in the US. It had both components: the income shock—the casino openings initiated a government transfer scheme giving a portion of the casino profits to individuals with preexisting American Indian status—and the opportunity shock—change in the local employment opportunities. The two components appear to have had opposite effects (Akee et al. 2010, Evans and Kim 2008). Akee et al. (2010) find that children in the households affected by the the government income transfer program had higher levels of education in their young adulthood. In contrast, Evans and Kim (2008) find that—within the same communities—young adults responded to the increased employment and wages of low-skilled workers by dropping out of high school and reducing college enrollment rates. This was despite presence of the income transfer scheme and additional college tuition subsidy programs of many tribes. This paper and a larger literature suggest that both results could be true at the same time (see, for example, Black et al. 2005, Atkin 2016, and Shah and Millet Steinberg 2017).[33]

In summary, while places experiencing manufacturing job losses face reductions in the monetary and social resources available to children, perhaps changing incentives and social structure counteract that effect to produce the positive results on education found in this paper. This adjustment, however, may be incomplete. This paper contributes to the growing evidence on the interplay between local labor market conditions and educational decisions (Atkin 2016, Shah and Millet Steinberg 2017), going beyond the direct effects of parental job loss.

Looking from a different perspective, a large literature discusses regional divergence of the US (Ganong and Shoag 2017), and why we tend to observe a permanent decline in a place hit by a negative economic shock (Blanchard and Katz 1992, Dix-Carneiro and Kovak 2017). Previous scholars have investigated the role of social distress (Wilson 1996, Ananat et al. 2017), imperfect mobility, declining housing prices, generous social welfare payments (Ganong and Shoag 2017), and human capital externalities (Dix-Carneiro and Kovak 2017). The evidence is inconclusive. This paper explores a new channel: human capital investment of the next generation. Local job destruction could lead the youth off the path to high school and college. In the long run, this could lead to lower local productivity and long-term

[33]The tension between income and opportunity shocks has not been generally clear in the research literature. For example, Akee et al. (2010) suggest that the contrary results by Evans and Kim (2008) arise from identification issues from focusing on community-level variations (this is possible). But local employment opportunity effects exist at the community level and therefore the natural observation unit is the community, not the individual.

decline. But the results of this research suggest otherwise: a decline in formal education after an economic shock does not seem to be a channel for local long-term decline and regional divergence. Something else is.

7 Conclusion

This paper provides new evidence on the impact of manufacturing decline on children. To do so, it considers variations in local employment structure—characterizing left-behind places and lost manufacturing jobs—high-school dropout rates, and college access in the US over 1990–2010. To establish causal inference, the paper uses variations in trade exposure from China following its entry to the WTO as an instrument for local manufacturing declines in the US.

The results suggest that negative shocks to manufacturing labor demand, measured at the local labor market level, had large positive effects on children's education, decreasing high-school dropout rates and possibly increasing college access. The magnitudes of the estimates suggest that for every 3-percentage-point decline in manufacturing as a share of total employment, high-school dropout rate declined by 1 percentage point. These findings contrast with the literature on job loss that has emphasized negative effects from economic shocks on children. The results are consistent with the idea that the manufacturing decline increased returns and decreased opportunity costs of education, and with sociological accounts linking working-class environment and children's education. These effects perhaps counteract the negative effects from income loss. The effects are largest in the areas with high segregation and in those with larger African American populations. This set of findings is new—and a first step in quantifying the intergenerational effects of lost manufacturing jobs due to technological change and globalization.

Children face the collateral damage from the adults' world. And the long-run consequences depend on them. That's why this research matters.

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A Figures

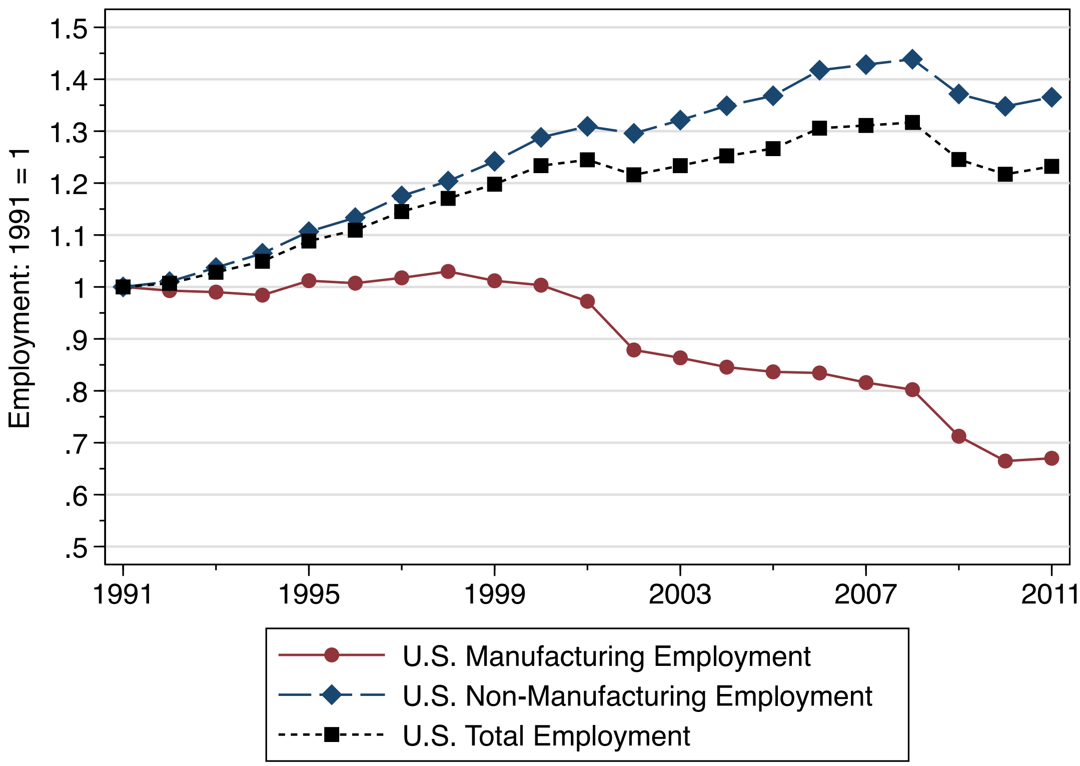


Figure 1: Changes in US manufacturing and non-manufacturing employment, 1991–2011. Employment data are normalized to 1991. Source: CBP.

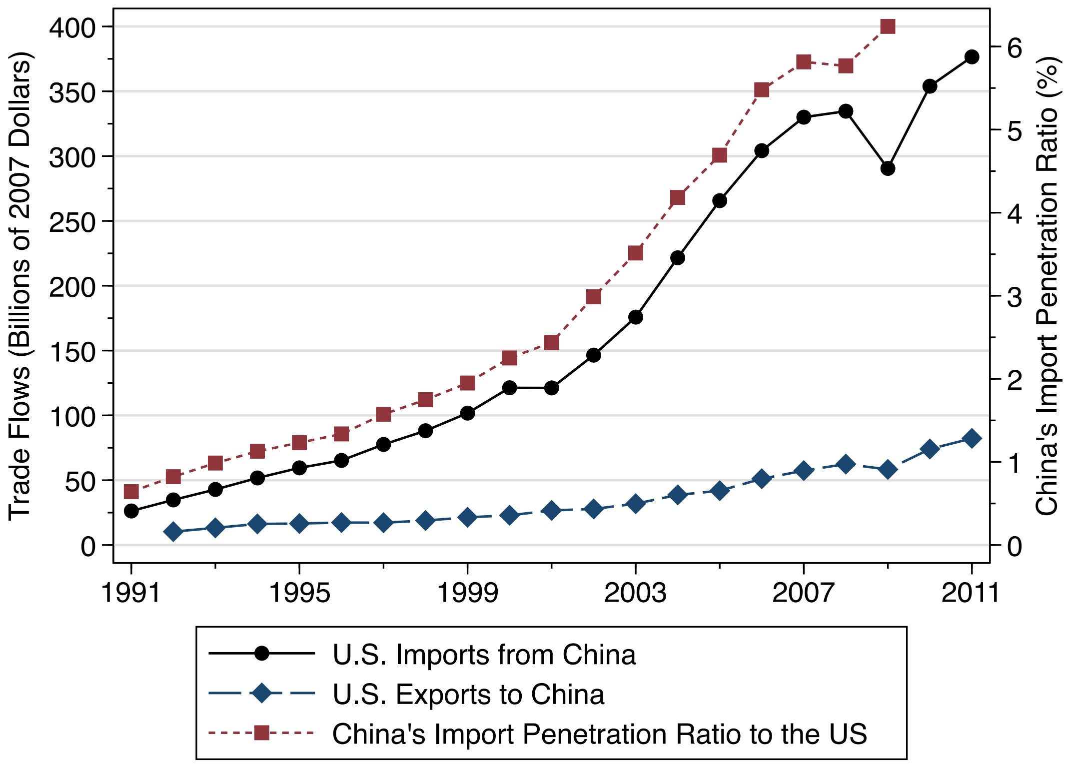
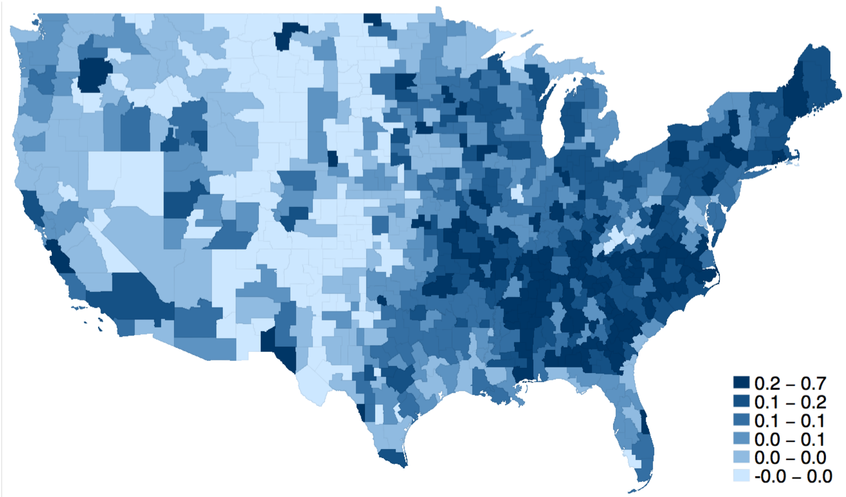
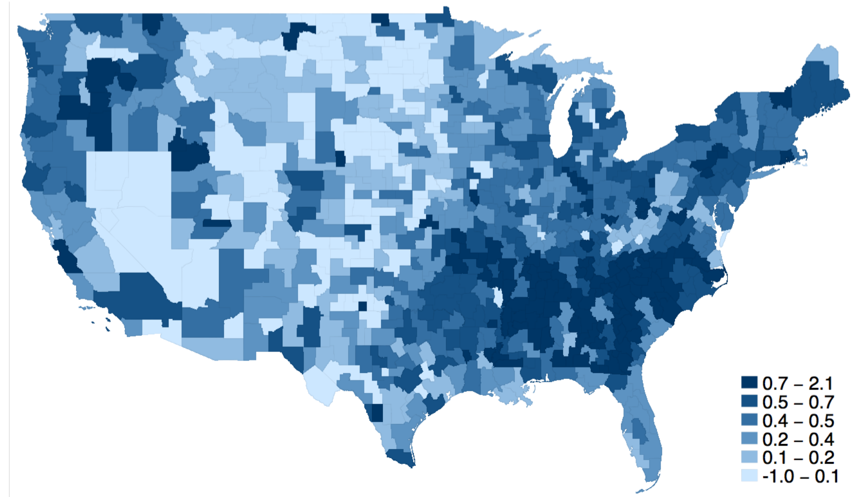


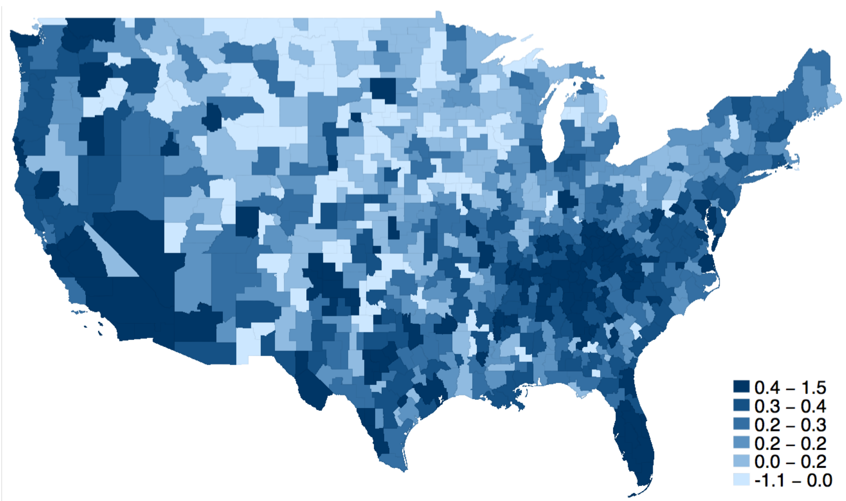
Figure 2: US-China bilateral trade flows, 1991–2011. Source: UN Comtrade Database. Trade volumes are deflated to 2007 US dollars using the PCE price index. China's import penetration is defined as China's manufacturing imports to the US divided by US domestic manufacturing output plus imports minus exports. Export data are available only from 1992 onward. The import penetration ratio series ends in 2009 because the NBER-CES Manufacturing Industry Database ends in 2009.



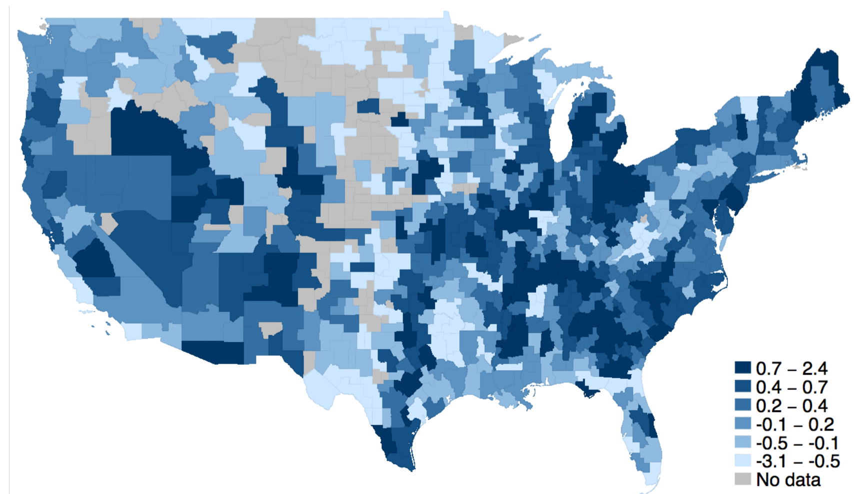
(a) Decline in Manufacturing Share of Employment (–) (CBP, US Census).



(b) Increase in Import Exposure from China (+) (UN Comtrade).



(c) Decline in High-School Dropout Rate (–) (US Census, ACS).



(d) Increase in College Mobility (+) (US Tax Records via Chetty et al. 2014).

Figure 3: Maps. All variables are in 100 $\times$ annual changes 1991–2011, except college mobility 1999–2011. The (+) and (−) signs indicate whether the heat map refers to increases or decreases in the variable. The variables are constructed as detailed in text.

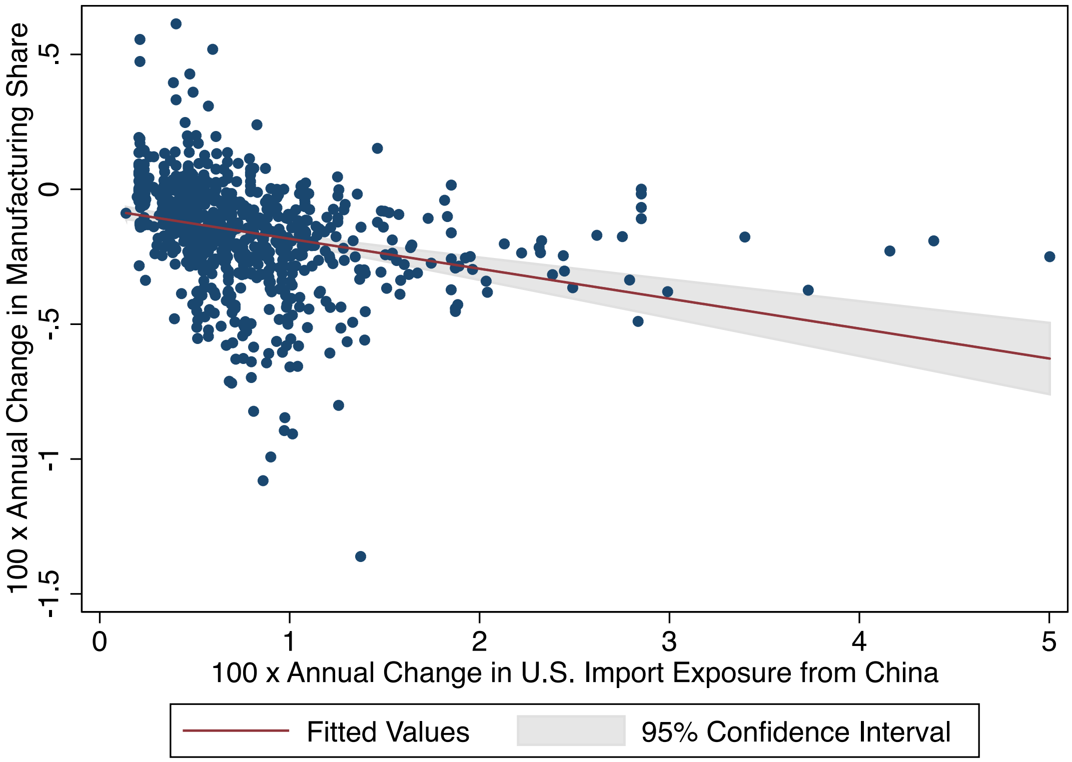


Figure 4: First-stage regression, 1991–2011. Each point represents a commuting zone (N = 722). Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The annual change in commuting zone exposure to Chinese imports is a weighted average of changes in US import exposure in 392 four-digit manufacturing industries, where the weights are start-of-period employment shares within the commuting zone. Imports are deflated to constant dollars using the PCE price index. Lines are fitted by OLS regression. The 95 percent confidence interval is based on standard errors clustered on 722 commuting zones. The slope coefficient is −2.80 with standard error 0.21 and t-statistic −13.4; the regression has an R-squared of 0.35.

B Tables

Table 1: Descriptive Statistics for Manufacturing, Employment, and Population in CZs.

		Mean	Std. Dev.	Min	Max
Manufacturing–to-Total Employment Ratio (%)	1991	21.9	12.8	.10	61.4
	1999	19.3	11.3	.13	57.7
	2011	13.9	8.7	.26	51.3
Employment-to-Population Ratio (%)	1991	42.1	10.6	11.0	76.8
(Working age)	1999	48.01	11.8	16.3	83.0
	2011	44.9	10.4	16.5	80.0
Population	1991	350,000	.95 M	1311	10.4 M
(Total)	1999	380,000	1.04 M	1213	16.6 M
	2011	430,000	1.16 M	1017	18.1 M

Notes: N = 722 commuting zones. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. Working-age population is those between the ages of 15 and 64.

Table 2: Descriptive Statistics for Education in CZs.

		Mean	Std. Dev.	Min	Max
High-School Dropout Rate	1990	10.3	4.2	.36	31.7
	2000	9.2	3.9	.41	22.4
	2011	6.0	3.3	.38	30.2
College Mobility	2002	32.5	8.4	13.7	61.2
	2011	33.2	7.7	12.1	58.2

Notes: N = 722 commuting zones for high-school dropout rates, 616 for college mobility, The variables are expressed in percentages. High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. College mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution of income. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records. The years 2002 and 2011 refer to the standard college-starting years of cohorts born in 1984 and 1993.

Table 3: Changes in Commuting Zone Manufacturing Share, Import Exposure, High-School Dropout Rate, and College Mobility.

	1991–99				1999–2011			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Δ in Manufacturing-to-Total Employment Ratio	-.31	.57	-5.34	3.14	-.45	.43	-2.83	.97
Δ in Exposure to China's Imports	.06	.08	.00	.95	.09	.09	.00	.69
Δ in High-School Dropout Rate	-.10	.29	-1.96	1.43	-.30	.35	.35	1.58
Δ in College Mobility	–	–	–	–	.11	.75	-3.12	2.38

Notes: N = 1444 = 2 $\times$ 722 commuting zones. All variables are 100$\times$ annual change in the measure. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The annual change in commuting zone exposure to Chinese imports is a weighted average of changes in US import exposure in 392 four-digit manufacturing industries, where the weights are start-of-period employment shares within the commuting zone. Imports are deflated to constant dollars using the PCE price index. High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. College mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution of income. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records. The annual change in college mobility in 1999–2011 refers to the annual change in college mobility between cohorts born in 1984 and 1993.

Table 4: The First Stage: Estimates of China's Import Effects on Commuting Zone Manufacturing Decline over 1991–2011.

Manufacturing Share	(1)	(2)	(3)	(4)	(5)	(6)
Commuting zone import exposure	-2.58***	-2.54***	-2.18***	-1.25***	-.91***	-.87***
	(.23)	(.24)	(.24)	(.24)	(.23)	(.22)
F-Statistics	117.4	76.5	36.1	216.4	180.6	70.5
Adjusted R^2	0.21	0.21	0.29	0.32	0.33	0.40
Time effect controls	–	Yes	Yes	–	Yes	Yes
Baseline controls	–	–	Yes	–	–	Yes
Manufacturing share baseline	–	–	–	Yes	Yes	Yes

Notes: First stage regression. Each column reports results from stacking changes in commuting zone manufacturing-to-total employment ratios and in exposure to Chinese imports within local industries over the periods 1991–99 and 1999– 2011. The dependent variable is the annual change in the manufacturing-to-total employment ratio (N = 1,444 = 722 commuting zones $\times$ 2 periods). The explanatory variable an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. Baseline controls include population counts, employment-to-population ratios, and region controls for nine regional census divisions. The commuting zone baseline controls, including the manufacturing share control, are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 periods. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 5: OLS and 2SLS Estimates of Manufacturing Decline Effects on Commuting Zone High-School Dropout Rates 1991–2011.

High-School Dropout Rate	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Excluding Manufacturing Share Control at the Baseline								
Commuting zone manufacturing decline	-.109***	-.107***	-.107***	-.044**	-.227***	-.228***	-.232***	-.162***
	(.018)	(.018)	(.018)	(.021)	(.031)	(.030)	(.030)	(.034)
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population counts at the baseline	–	Yes	Yes	Yes	–	Yes	Yes	Yes
Employment-to-population ratios at the baseline	–	–	Yes	Yes	–	–	Yes	Yes
Census division indicators	–	–	–	Yes	–	–	–	Yes
B. Including Manufacturing Share Control at the Baseline								
Commuting zone manufacturing decline	-.082***	-.075***	-.0733***	-.030	-.433***	-.416***	-.415***	-.366**
	(.021)	(.021)	(.021)	(.025)	(.131)	(.132)	(.130)	(.125)
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population counts at the baseline	–	Yes	Yes	Yes	–	Yes	Yes	Yes
Employment-to-population ratios at the baseline	–	–	Yes	Yes	–	–	Yes	Yes
Census division indicators	–	–	–	Yes	–	–	–	Yes

Notes: Each column reports results from stacking changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 1,444 = 722 commuting zones × 2 periods). The manufacturing decline is instrumented with the commuting zone import exposure from China's imports. The instrument is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The commuting zone baseline controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 periods. Census division indicators control for nine regional census divisions. Standard errors are clustered by commuting zone.
$*p < 0.10$
$**p < 0.05$
$***p < 0.01$

Table 6: OLS and 2SLS Estimates of Manufacturing Decline Effects on Commuting Zone College Mobility 1999–2011.

College Mobility	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Excluding Manufacturing Share Control at the Baseline								
Commuting zone manufacturing decline	.397***	.409***	.407***	.316***	.360***	.378***	.329***	.232
	(.076)	(.076)	(.076)	(.082)	(.124)	(.123)	(.124)	(.142)
Population counts at the baseline	–	Yes	Yes	Yes	–	Yes	Yes	Yes
Employment-to-population ratios at the baseline	–	–	Yes	Yes	–	–	Yes	Yes
Census division indicators	–	–	–	Yes	–	–	–	Yes
B. Including Manufacturing Share Control at the Baseline								
Commuting zone manufacturing decline	.387***	.361***	.395***	.254**	.236	.138	.122	-.122
	(.101)	(.101)	(.102)	(.103)	(.278)	(.285)	(.283)	(.301)
Population counts at the baseline	–	Yes	Yes	Yes	–	Yes	Yes	Yes
Employment-to-population ratios at the baseline	–	–	Yes	Yes	–	–	Yes	Yes
Census division indicators	–	–	–	Yes	–	–	–	Yes

Notes: Each column reports results from regressing changes in commuting zone measures of absolute college mobility on decline in manufacturing-to-total employment ratios over the period 1999–2011. The dependent variable is the annual change in college mobility between cohorts born in 1984 and 1993 (N = 616 commuting zones). College mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records. The manufacturing decline is instrumented with the commuting zone import exposure from China's imports. The instrument is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The commuting zone baseline controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 periods. Census division indicators control for nine regional census divisions. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 7: Pretrends: 2SLS Estimates of Manufacturing Decline Effects on High-School Dropout Rates over 1991–2011.

High-School Dropout Rate	(1)	(2)	(3)	(4)	(5)	(6)
Manufacturing decline	-.227***	-.216***	-.172***	-.433***	-.481***	-.418***
	(.031)	(.034)	(.036)	(.142)	(.126)	(.129)
Pretrend controls	–	Yes	Yes	–	Yes	Yes
Baseline controls	–	–	Yes	–	–	Yes
Manufacturing share baseline	–	–	–	Yes	Yes	Yes

Notes: Pretrends. Each column reports results from stacking changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 1,444 = 722 commuting zones $\times$ 2 periods). The manufacturing decline is instrumented with the commuting zone import exposure from China's imports. The instrument is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. All models include a control for time trend. Pretrend controls are annual changes in the high-school dropout rate over 1970–80 and 1980–90 computed from the US Census. Baseline controls include population counts, employment-to-population ratios, and region controls for nine regional census divisions. The commuting zone baseline controls, including the manufacturing share control, are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 periods. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 8: Falsification Test: 2SLS Estimates of Manufacturing Decline Effects on High-School Dropout Rates over 1970–2011.

High-School Dropout Rate	1970–80 (1)	1980–90 (2)	1991–99 (3)	1999–2011 (4)
A. Excluding Manufacturing Share Control				
Manufacturing decline 1999–2011	-.314***	.016	-.088**	-.288***
	(.055)	(.058)	(.042)	(.045)
With regional controls	-.044	.048	-.062	-.179***
	(.055)	(.060)	(.046)	(.050)
B. Including Manufacturing Share Control				
Manufacturing decline 1999–2011	-.080	-.213*	-.145	-.487***
	(.141)	(.126)	(.107)	(.138)
With regional controls	.206	-.091	-.111	-.361***
	(.131)	(.126)	(.114)	(.141)

Notes: Falsification test. Each column reports results from a separate specification regressing changes in commuting zone high-school dropout rates in the specified decade and declines in manufacturing-to-total employment ratios over the period 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 722 commuting zones over one decade). The manufacturing decline is instrumented with the commuting zone import exposure from China's imports. The instrument is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. High-school dropout rate is computed from the US Census for 1970–2000, and from the ACS for 2011 as a five-year average. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The commuting zone baseline manufacturing controls are computed in 1999 for the 1999–2011 period. Region controls control for nine regional census divisions. Panels A and B contain no additional controls. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 9: Rural vs. Urban: 2SLS Estimates of Trade Exposure Effects on High-School Dropout Rate 1991–2011 and College Mobility 1999–2011.

Rural vs. Urban	2SLS	
	(1)	(2)
	A. High-School Dropout Rate	
Commuting zone manufacturing decline	-.224***	-.407***
	(.048)	(.129)
Interaction: manufacturing decline × rural	-.027	.029
	(.061)	(.067)
Baseline manufacturing emp. share	–	Yes
Other baseline controls	–	Yes
	B. College Mobility	
Commuting zone manufacturing decline	.566***	.100
	(.142)	(.261)
Interaction: manufacturing decline × rural	-.167	-.21
	(.235)	(.235)
Baseline manufacturing emp. share	–	Yes
Other baseline controls	–	Yes

Notes: Rural vs. Urban. In Panel A, each column reports results from stacking the logarithms of changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 1,444 = 722 commuting zones × 2 periods). High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. In Panel B, each column reports results from regressing changes in commuting zone measures of absolute college mobility and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in college mobility between cohorts born in 1984 and 1993 (N = 616 commuting zones). College mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records. In both Panels A and B, manufacturing decline is instrumented with the commuting zone import exposure from China's imports. The instrument is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. Both panels include interaction terms with US Census rural area indicator as in text. The commuting zone baseline controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 period. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The other baseline controls include population counts, employment-to-population ratios, and region controls for nine regional census divisions. All models include a time trend. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 10: Geographical Correlates of the Intergenerational Effects of Manufacturing Decline.

	2SLS	
Interaction term	Main effect	Interaction
Segregation and Race		
Fraction Black	-0.213* (0.112)	-0.820** (0.363)
Income Segregation	-0.311** (0.131)	-2.239* (1.177)
Segregation of Affluence (>p75)	-0.308** (0.131)	-2.154** (1.063)
Fraction with Commute < 15 Mins	-0.596*** (0.143)	0.851*** (0.243)
Income Inequality		
Household Income per Capita	-0.190 (0.242)	-0.000 (0.000)
Gini coefficient	-0.281 (0.192)	-0.214 (0.331)
Fraction Middle Class (between p25 and p75)	-0.573** (0.280)	0.472 (0.427)
K-12 Education		
School Expenditure per Student	-0.465** (0.200)	0.021 (0.034)
Student Teacher Ratio	-0.053 (0.251)	-0.021 (0.015)
Test Score Percentile (Income adjusted)	-0.300*** (0.110)	-0.002 (0.004)
College		
Number of Colleges per Capita	-0.577*** (0.156)	5.757*** (1.823)
College Tuition	-0.458*** (0.164)	-0.000 (0.000)
College Graduation Rate (Income Adjusted)	-0.483*** (0.161)	-0.000 (0.000)
Social Capital		
Social Capital Index	-0.362*** (0.126)	0.038 (0.029)
Fraction Religious	-0.427** (0.167)	0.107 (0.292)
Violent Crime Rate	-0.317** (0.139)	-42.456 (36.070)
Local Labor Market		
Teenage (14-16) Labor Force Participation	-0.475*** (0.162)	39.034* (23.495)

Notes: Each column reports results from stacking the logarithms of changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011, and including an interaction term and main effect for the indicated variable (N = 1,444 = 722 commuting zones × 2 periods). The baseline controls include manufacturing share of employment, population counts, employment-to-population ratios, and region controls for nine regional census divisions. All models include a time trend. Standard errors are clustered by commuting zone. The variables are detailed in Tables 14 and 15. Further details are provided in text.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

C Appendix

Table 11: Alternative IV: 2SLS Estimates of Manufacturing Decline Effects on High-School Dropout Rate 1991–2011 and College Mobility 1999–2011.

Alternative 2SLS Estimates	(1)	(2)	(3)
	A. High-School Dropout Rate		
Commuting zone manufacturing decline	-.270***	-.166***	-.441*
	(.038)	(.053)	(.235)
Other baseline controls	–	Yes	Yes
Baseline manufacturing emp. share	–	–	Yes
	B. College Mobility		
Commuting zone manufacturing decline	.562***	.438***	.587
	(.125)	(.151)	(.393)
Other baseline controls	–	Yes	Yes
Baseline manufacturing emp. share	–	–	Yes
	C. 2SLS First Stage Estimates†		
Commuting zone import exposure	-2.29***	-1.87***	-.63***
	(.23)	(.13)	(.016)
F-statistic	155.0	48.4	71.7
Adjusted R^2	0.22	0.27	0.39

Notes: Alternative IV specification. In Panel A, each column reports results from stacking changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 1,444 = 722 commuting zones $\times$ 2 periods). High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. In Panel B, each column reports results from regressing changes in commuting zone measures of absolute college mobility on declines in manufacturing-to-total employment ratios over the period 1999–2011. The dependent variable is the annual change in college mobility between cohorts born in 1984 and 1993 (N = 616 commuting zones). College mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records. In Panels A and B, manufacturing decline is instrumented with an alternative measure of the commuting zone import exposure, constructed from Chinese imports to eight other high-income countries, excluding the US, as in Autor et al. (2013) and detailed in the text. The commuting zone baseline manufacturing controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 period. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. Other baseline controls include population counts, employment-to-population ratios, and region controls for nine regional census divisions. All models in Panel A include a time trend. Standard errors are clustered by commuting zone.

† For manufacturing share over 1991–2011.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 12: The Reduced Form: OLS and 2SLS Estimates of Trade Exposure Effects on High-School Dropout Rate 1991–2011 and College Mobility 1999–2011.

Reduced Form Estimates	OLS		Combined 2SLS	
	(1)	(2)	(3)	(4)
	A. High-School Dropout Rate			
Commuting zone import exposure	-.357***	-.338***	-.543***	-.656**
	(.082)	(.106)	(.171)	(.295)
Baseline manufacturing emp. share	–	Yes	–	Yes
Other baseline controls	Yes	Yes	Yes	Yes
	B. College Mobility			
Commuting zone import exposure	.674*	.016	1.41***	1.23
	(.392)	(.474)	(.493)	(.815)
Baseline manufacturing emp. share	–	Yes	–	Yes
Other baseline controls	Yes	Yes	Yes	Yes

Notes: Reduced form regression. In Panel A, each column reports results from stacking changes in commuting zone high-school dropout rates and changes in exposure to Chinese imports within local industries over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 1,444 = 722 commuting zones × 2 periods). High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. In Panel B, each column reports results from regressing changes in commuting zone measures of absolute college mobility on changes in exposure to Chinese imports within local industries over the period 1999– 2011. The dependent variable is the annual change in college mobility between cohorts born in 1984 and 1993 (N = 616 commuting zones). College mobility is CZ-level average of college attendance of children with parents at the 25th percentile in the national distribution. The college mobility measure comes from Chetty et al. (2014) and is based on the US tax records. In Panels A and B, the explanatory variable is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. In Columns (3) and (4), the import exposure is instrumented with the alternative instrument constructed from Chinese imports to eight other high-income countries, as in Autor et al. (2013). The commuting zone baseline manufacturing controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 period. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. Other baseline controls include population counts, employment-to-population ratios, and region controls for nine regional census divisions. All models include a time trend. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 13: Log-Log Specification and Baseline Control for Outcome: 2SLS Estimates of Trade Exposure Effects on High-School Dropout Rate 1991–2011.

High-School Dropout Rate	2SLS	
	(1)	(2)
	A. Log-Log Specification	
Commuting zone manufacturing decline	-.865***	-.498***
	(.392)	(.111)
Baseline manufacturing emp. share	–	Yes
Other baseline controls	–	Yes
	B. Baseline Control for High-School Dropout Rate	
Commuting zone import exposure	-.120***	-.397***
	(.029)	(.117)
Baseline Control for High-School Dropout Rate	.039***	.042***
	(.0023)	(.0030)
Baseline manufacturing emp. share	–	Yes
Other baseline controls	–	Yes

Notes: Log-Log Specification and Baseline Control for Outcome. In Panel A, each column reports results from stacking the logarithms of changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011. The dependent variable is the annual change in the high-school dropout rate (N = 1,444 = 722 commuting zones $\times$ 2 periods). High-school dropout rate is computed from the US Census for 1990 and 2000, and from the ACS for 2011 as a five-year average. In Panel B, each column reports results from stacking the logarithms of changes in commuting zone high-school dropout rates and declines in manufacturing-to-total employment ratios over the periods 1991–99 and 1999–2011, including controls for the start-of-period high-school dropout rate. In Panels A and B, the manufacturing decline is instrumented with the commuting zone import exposure from China's imports. The instrument is an employment-weighted average of annualized changes in exposure to Chinese imports within local industries, as detailed in the text. The commuting zone baseline controls are computed in 1991 for the 1991–99 period and in 1999 for the 1999–2011 period. Manufacturing employment is computed from the CBP; population data come from the Census Population Estimates. The other baseline controls include population counts, employment-to-population ratios, and region controls for nine regional census divisions. All models include a time trend. Standard errors are clustered by commuting zone.

$*p < 0.10$

$**p < 0.05$

$***p < 0.01$

Table 14: Correlates of the Intergenerational Effects: Variable Definitions, Part I.

Interaction term	
Segregation and Race	
Fraction Black	Number of individuals who are black alone divided by total population. US Census 2000.
Income Segregation	Rank-Order index estimated at the census-tract level using equation (13) in Reardon (2011); the δ vector is given in Appendix A4 of Reardon's paper. H(pk) is computed for each of the income brackets given in the 2000 census. See Appendix D for further details. US Census 2000.
Segregation of Affluence ($>$p75)	H(p275) estimated following Reardon (2011); we compute H(p) for 16 income groups defined by the 2000 census. We estimate H(p75) using a fourth-order polynomial of the weighted linear regression in equation (12) of Reardon (2011). US Census 2000.
Fraction with Commute $<$ 15 Mins	Number of workers that commute less than 15 minutes to work divided by total number of workers. Sample restricts to workers that are 16 or older and not working at home. US Census 2000.
Income Inequality	
Household Income per Capita	Aggregate household income in the 2000 census divided by the number of people aged 16-64. US Census 2000.
Gini coefficient	Gini coefficient computed using parents of children in the core sample, with income topcoded at $100 million in 2012 dollars. Tax Records, Core Sample.
Fraction Middle Class	Fraction of parents (in the core sample) whose income falls between the 25th and 75th percentile of the national parent income distribution. Tax Records, Core Sample.
K-12 Education	
School Expenditure per Student.	Average expenditures per student in public schools. NCES CCD 1996-1997 Financial Survey.
Student Teacher Ratio	Average student-teacher ratio in public schools. NCES CCD 1996-1997 Universe Survey
Test Score Percentile	Residual from a regression of mean math and English standardized test scores on household income per capita in 2000. George Bush Global Report Card.

Notes: These covariates are compiled by Chetty et al. (2014). The descriptions come from that source. See the reference for further details.

Table 15: Correlates of the Intergenerational Effects: Variable Definitions, Part II.

Interaction term	
College	
Number of Colleges per Cap.	Number of Title IV, degree offering institutions per capita. IPEDS 2000
College Tuition	Mean in-state tuition and fees for first-time, full-time undergraduates. IPEDS 2000.
College Graduation Rate	Residual from a regression of graduation rate (the share of undergraduate students that complete their degree in 150% of normal time) on household income per capita in 2000. IPEDS 2009.
Social Capital	
Social Capital Index	Standardized index combining measures of voter turnout rates, the fraction of people who return their census forms, and measures of participation in community organizations. Rupasingha and Goetz (2008).
Fraction Religious	Share of religious adherents. Association of Religion Data Archives
Violent Crime Rate	Number of arrests for serious violent crimes per capita. Uniform Crime Reports.
Local Labor Market	
Teenage (14-16) LFP	Fraction of children in birth cohorts 1985-1987 who received a W2 (i.e. had positive wage earnings) in any of the tax years when they were age 14-16. Tax Records, Extended Sample.

Notes: These covariates are compiled by Chetty et al. (2014). The descriptions come from that source. See the reference for further details.