Market competition and employment in construction sector in the USA: evidence from trade liberalisation

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Abstract: This paper investigates the effect of US-China trade liberalisation on the employment outcomes of workers in construction industries. Using individual-level data from the US decennial census, American Community Survey, and Current Population Survey data (over 70 million observations) and applying a difference-in-difference methodology that compares the outcomes of individuals in high versus low exposure to tariff reductions after the reform to before, we find negative and significant effects of trade liberalisation for employment in construction industries. The effects hold for both extensive and intensive margins, across a wide range of specifications, and various outcomes. A heterogeneity analysis reveals higher effects among males and non-Hispanic whites. The results call for compensatory policies for workers in industries that are negatively affected by trade policy changes.

Keywords: trade liberalisation; unemployment; construction; manufacturing; competition; income; trade relations; USA.

JEL codes: E24, F13, F14, L74, L78, O24.

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1 Introduction

Principle economic theories suggest that while international trade improves the aggregate welfare of participating countries it has a differential impact across subpopulations and industries (Casacuberta et al., 2004; Frankel and Romer, 1999). From a policymaker's point of view, it is essential to recognise and design policies that compensate for the adversely affected population and specifically for workers of those industries who lose their job as a result of international trade. However, it is challenging to establish a causal link as trade comes with other potential determinants of job loss such as economic development, productivity, and other growth-enhancing economic policies.

This paper attempts to solve these endogeneity issues using a large trade policy change that initiated a revolution between US and China trade relations. In October 2000, Congress passed a law that granted China the Permanent Normal Trade Relations (PNTR) status based on which China could permanently benefit from lower tariff rates defined by normal trade relations (NTR) rates. Nonetheless, for some industries, the differences between NTR rates and non-NTR rates (NTR-gap) were large which meant higher exposure to the trade policy change and for some industries, the difference was small with lower exposure to the resulting import competition. On the other hand, states also vary in terms of their industry composition. The cross-industry variation in trade exposure combined with across-state industry composition and over time change in trade policy generates a plausibly exogenous shock to examine the effect of an increase in product market competition on employment outcomes.

We investigate the effect of US-China trade liberalisation and its subsequent increases in product market competition on employment in the construction sector. We find negative, statistically significant, and economically large effects of trade on employment in construction. The findings suggest that a 30% reduction in tariff rates (a standard deviation of NTR-gap) is associated with a roughly 10 basis points reduction in the probability of employment in the construction sector, a roughly 2.3% decrease relative to the mean. The effects appear to be robust across a wide range of specification checks, subsamples, outcomes, and at the intensive margin. The negative employment effects of trade shock are larger among males compared to females, and among blacks and non-Hispanic whites compared to Hispanics. The cohorts exposed to the trade shocks have, on average, lower wealth as measured by the average house value. A placebo test shows that the trade shocks did not have any effect for those people employed in industries with low NTR-gap which further confirms the validity of the findings and the empirical method.

International trade has been and will be a highly controversial topic in the political atmosphere and social media. The main reason is that it affects people in various ways and not all people are better off after the trade. The results of this paper add to these debates and contribute to those conversations around trade liberalisation and specifically US-China trade relations by providing novel and compelling evidence of its negative consequences on a sector that is usually ignored in this literature, the construction industry.

This paper makes three contributions to the existing literature. First, this is the first study to establish a causal link between international trade liberalisation and employment outcomes in the construction industry. Second, while previous studies show the aggregate effects at the national, state, or county level, this paper implements two large data sets to explore the effects at the individual level. Third, using the individual level also enable the research design to explore the heterogeneity of the effects by gender and race within the manufacturing sector. This aspect of the analysis has been ignored in the previous literature.

The rest of the paper is organised as follows. Section 2 reviews the background of US-China trade liberalisation. Section 3 provides a brief literature review. Section 4 discusses the data sources and sample construction. Section 5 introduces the empirical strategy. Section 6 reviews the main results, robustness checks, and heterogeneity across subsamples. Section 7 discusses the potential endogeneity issues. Section 8 departs some concluding remarks.

2 Background on US-China trade relations

The US tariff schedule consists of two sets of tariff rates each one applicable to a specific set of countries. The first is tariff rates for market economies and those members of the World Trade Organization (WTO). These tariff rates, so-called Normal Trade Relation (NTR) rates, are low with the main purpose of improving trade volume. Second, are a set of tariff rates that are usually high and are set for non-market economies such as Cuba and China. These higher rates, so-called non-NTR rates, were set by the Smoot-Hawley Tariff Act of 1930, roughly 70 years before the period of this study. This fact is important in the identification strategy as it rules out the possibility of reverse causality, that the higher rates are set to protect industries with differential trends in productivity or other unobserved features almost 70 years in the future.

US presidents with the support of Congress could waive non-NTR rates and grant the NTR status to specific countries on an annual basis. Starting from 1980, president-granted Congress-approved NTR rates were given to China each year. However, it could not trigger a revolution in the US-China trade relations for some political reasons including the Chinese government's controversial actions during the 1990s such as Tiananmen Square Massacre (1989), the China-Pakistan missile deal (1992), and Third Taiwan Strait Crisis (1995–1996). The short-term non-NTR grants as well as US-China political issues and US sanctions against China generated uncertainties and pushed back free trade. These uncertainties were left in October 2000 when Congress passed a bill granting the Permanent Normal Trade Relations (PNTR) to China. The passage became effective as of 2001 as China entered WTO and became a member of market economies. The PNTR grant and entering WTO revolutionised China's export market and generated an exogenous shock to the US import market with differential impact across industries based on how large was their respective non-NTR rates (the old rates) relative to the according to NTR rates (the new rates).

3 Literature review

A relatively large body of literature examines the labour market consequences of international trade for both importing and exporting countries. For example, Noghani and Noghanibehambari (2019) investigate the effect of trade liberalisation on measures of managerial slack. They posit that the trade shock increases import competition in some industries more than others and managers in affected industries encounter a tighter market with lower chances of survival. In this environment, they reduce their wasteful corporate practices, excess expenditure, lax management, and overinvestment. They test this hypothesis using longitudinal panel data of US firms between the years 1990–2010 and implementing a difference-in-difference identification strategy. They find significant and robust evidence to support their hypothesis. Flammer (2015) uses the tariff changes in US manufacturing industries between the years 1992–2005 and show that firms respond to the tariff changes by increasing their Corporate Social Responsibility. He argues that firms try to differentiate themselves from their foreign rivals by choosing corporate social responsibility as their competitive strategy.

Navaei and Farnoud (2021) explore the environmental impact of trade liberalisation and its subsequent effect on the health of infants. They show that trade liberalisation reduced employment and total production in the manufacturing sector, a sector that is highly pollutant. The trade-induced reduction in counties' air pollution resulted in positive effects on infants' birth outcomes. The trade-induced health effects are not uniformly distributed across the population. Cherniwchan (2017) show that trade liberalisation led to sharp reductions in manufacturing employment which in turn resulted in substantial decreases in toxic pollutants including particulate matters (PM10) and sulphur dioxide (SO_2). Other studies show that trade could be detrimental for some subpopulations. For example, Pierce and Schott (2020) explore the effect of US-China trade policy change and show that areas with higher exposure to trade policy change exhibited relative rises in fatal drug overdoses with larger effects among whites.

Other studies point to the positive effects of international trade for developing and emerging countries. For instance, Olper et al. (2018) use a panel of developing countries over the years 1960–2010 and apply a synthetic control method to account for the heterogeneity of effects in order to explore the effect of trade liberalisation on child mortality. They find that trade liberalisation could reduce the child mortality rate by, on average, 10%. They show that such reductions are more significant in democratic countries, countries with higher income, and in cases that trade liberalisation was associated with reductions in taxation of farmers. Pierce and Schott (2018) show that the US-China trade liberalisation not only reduced production and employment in manufacturing industries, an industry with higher exposure to the trade policy change, but it also reduced net investments in this sector. The decline in investment is more concentrated in establishments with lower labour productivity.

Autor et al. (2019) explore the effect of international trade liberalisation on family formation and family structure in the US. They show that trade liberalisation differentially affected the employment and earnings of young males who worked in manufacturing industries, an industry that was documented to be highly exposed to trade. Consistent with economic theory, the reduction in employability and earnings led to reductions in marriage and fertility. Gaddis and Pieters (2017) explore the effect of trade liberalisation on gender differences in labour market outcomes in Brazil. They show that cohorts who worked in tradable sectors compared to other sectors after the trade liberalisation compared to before exhibited lower male-female differences in labour force participation and employment.

Brülhart et al. (2012) investigate the effect of the Iron Curtain fall of 1990, during which Central and Eastern European markets became open to trade with Austria. Using regional variation in proximity to the border combined with pre and post-1990 wage and employment effect, they generate a quasi-natural identification strategy. They find that regional access to new markets significantly affects wages and employment with larger and faster effects observed for wages.

An old and still important strand of this literature investigates the effect of trade liberalisation on aggregate economic growth and productivity (Casacuberta et al., 2004; Frankel and Romer, 1999; Salari et al., 2021; Sohn and Lee, 2010; Winters et al., 2004). For instance, Perla et al. (2021) construct a general equilibrium model with heterogeneous firms and show that after trade liberalisation and exposure to foreign competition countries experience faster technology adoption and economic growth. Some studies focus on the impact of trade on various measures of inequality. Kucera and Roncolato (2011) implements a social accounting matrix in a Leontief multiplier model and evaluates the winning and losing industries as well as the subsequent effects on household income inequality in India and South Africa. In a similar work, Galiani and Sanguinetti (2003) showed that trade liberalisation during the 1990s in Argentina was followed by increases in wage inequality and that the rise in inequality was higher in sectors with deepened import penetration. Revenga (1997) documents that workers in manufacturing industries in Mexico used to benefit from trade protection while the trade reform led to not only a decrease in their employability but also negative effects on their wages and rises in their measures of inequality. Other studies also document the differential impact of trade reforms on occupations, industries, wages, and growth (Beladi and Oladi, 2011; Bosch et al., 2012; Casacuberta et al., 2004; Chand and Sen, 2002; Choi, 2012; Davidson and Matusz, 2006; Dix-Carneiro and Kovak, 2017; Feinberg and Keane, 2001; Feler and Senses, 2017; Galiani and Sanguinetti, 2003; Kien and Heo, 2009; Lopez, 1994; Sohn and Lee, 2010; Winters et al., 2004).

4 Data sources and sample selection

This study uses various sources of data. the individual-level data are taken from two sources. First, US decennial census data for the years 1980, 1990, and 2000 are combined with American Community Survey data (hereafter census-ACS data) for the years 2001-2017. This data is extracted from Ruggles et al. (2020). The data is chosen in a time window that covers many years before and after the reform. We exclude years after 2017 as the new US-China trade policies during president Trump may confound the earlier effects. Second, we use monthly Current Population Survey (hereafter CPS data) data files over the years 1980–2017. The CPS data are extracted from Flood et al. (2020). The advantages of these datasets are their large sample size and the fact that they contain all required information for industry, employment status, labour force status, and other demographic and geographic characteristics.

To prepare the data for the purpose of this study, we apply some sample restrictions. First, we restrict the sample to individuals aged at least 25, the usual age of finishing college/university, and at most age 65, the usual age of retirement. We drop observations for those residing in US territories. Moreover, we drop observations for whom the industry classification is unknown, out of the default ranges, or missing.

Other state-level controls and industry-level data sources are as follows. The information on industry-specific NTR and non-NTR tariff rates are taken from Noghani and Noghanibehambari (2019). State-by-year population and race-age-composition of the population are taken from SEER (2019). Minimum wage data is extracted from Vaghul and Zipperer (2016). State-by-year indicators of occurrences of welfare reform are extracted from Noghanibehambari et al. (2020). The indicators to capture the passage of the Affordable Care Act are taken from Frean et al. (2017). Medicaid coverage rate and labour union coverage rate are extracted from Noghanibehambari and Salari (2020).

5 Empirical strategy

In order to construct a measure of import penetration, we follow the recent literature and take advantage of the spread between non-NTR rates and NTR rates which shows how much each industry could have been affected by granting China a PNTR status (Navaei and Farnoud, 2021; Noghani and Noghanibehambari, 2019; Pierce and Schott, 2018, 2020). The formal definition is as follows:

$$NTR\text{-}gap_j = Non\text{-}NTR_j - NTR_j \tag{1}$$

We calculate the gap for each industry j at the year 1999 (one year before the trade policy change) using *ad valorem equivalent* tariff rate (Feenstra et al., 2002). We calculate each state's exposure to the reform as the employment-share weighted mean of the gap as follows:

$$NTR\text{-}gap_s = \sum_j \frac{E_{js}^{1980}}{E_s^{1980}} NTR\text{-}gap_j \tag{2}$$

Where for each state, we calculate the average of NTR-gap across all industries using the share of state-industry-specific employment in the initial year 1980 relative to the total state employment as the weight.[1] The final outcome is the primary measure of import penetration for each state. We combine this measure with a pre- and post-reform indicator to capture the effect of trade liberalisation on employment outcomes in the industry sector using the following ordinary-least-square regressions:

(3)

Where i indexes individual, s the state, and t the year of observation. The parameter *Post* is a dummy that equals one for the years after the reform ($t > 2000$) and zero otherwise. The variable *NTR-gap* is calculated using equation (2). In Z, we include a series of individual-level controls including a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls (represented by matrix X) include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. The parameters λ and η represent state and year fixed effect. Note that the main effects of *Post* and *NTR-gap* are excluded from equation (3) as they are absorbed by state and year fixed effects. Finally, ε is a disturbance term. While we use Huber-White robust standard errors, we also show the results for clustering the standard errors at the state level in Appendix Table a2.

We use person weights provided by each data source (census, ACS, and CPS) to weight the regressions. To capture the real values, we deflate all monetary variables into 2017 real dollars using consumer price index data.

Technically, equation (3) is a difference-in-difference identification strategy. In this specification, the coefficient of interest is β which compares the employment outcomes of individuals in high versus low NTR-gap industries (first difference) after the trade policy change to before (second difference).

Table 1 shows the summary statistics of the final sample. Across all states and industries, the average NTR-gap is 36% with a standard deviation of 31%. On average, 4.7% and 5% of the samples' population are employed in construction industries for census-ACS sample and CPS sample, respectively. Figure 1 shows the geographic distribution of NTR-gap and construction employment across US states between the years 1980–2017.

Figure 1 Geographic distribution of NTR-gap and changes in construction employment (see online version for colours)

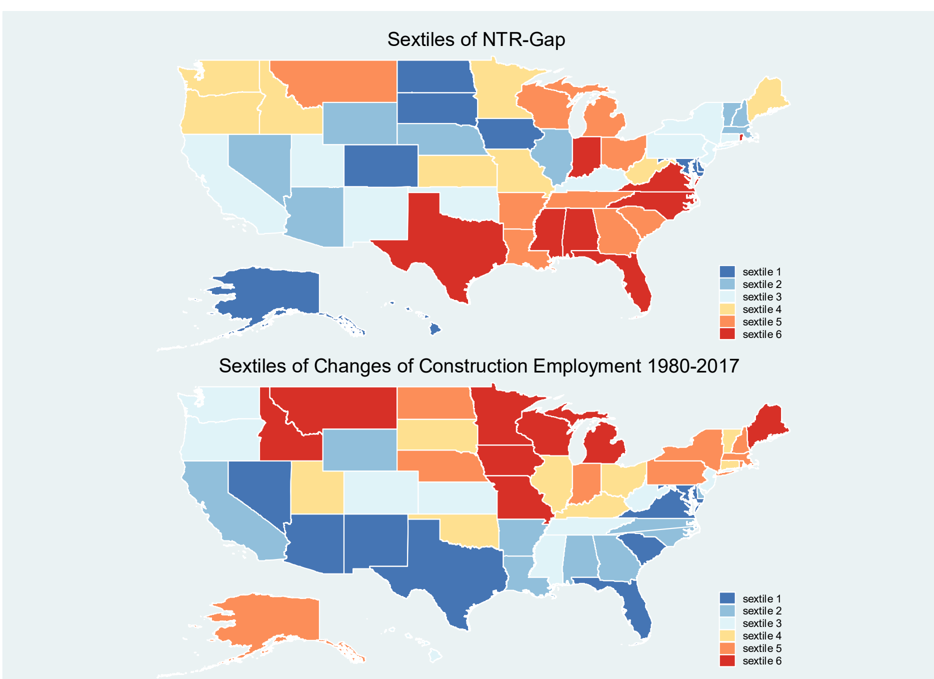


Table 1 Summary statistics

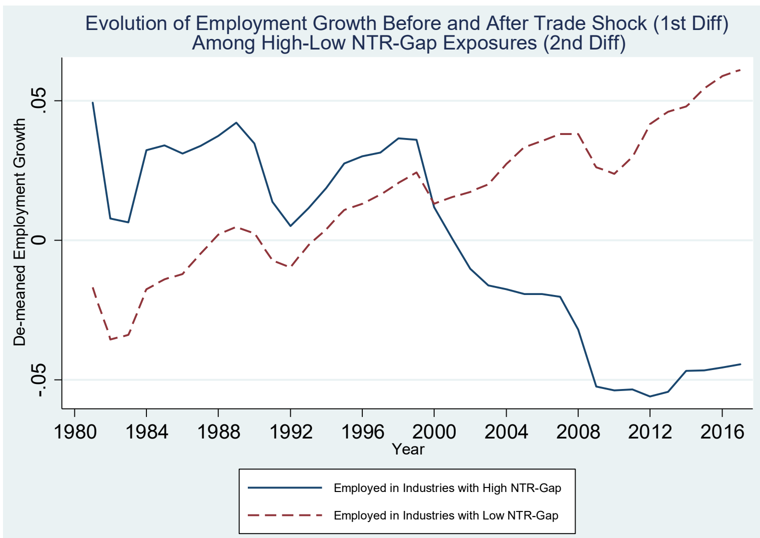
Variable	*Observations*	*Mean*	*Std. dev.*	*Min*	*Max*
Census (1980, 1990, 2000) and American Community Survey (2001–2017):					
Employed at mining industries	37,346,491	0.00515	0.07159	0	1
Employed at construction industries	37,346,491	0.0473	0.21229	0	1
Employed at manufacturing industries	37,346,491	0.10756	0.30983	0	1
Employed at agriculture industries	37,346,491	0.0147	0.12033	0	1
Employed at other industries	37,346,491	0.53792	0.49856	0	1
NTR-Gap	37,346,491	0.35996	0.31363	0.00141	1.95687
Race: White	37,346,491	0.85164	0.35545	0	1
Race: Black	37,346,491	0.10526	0.30689	0	1
Sex (Female=1)	37,346,491	0.51257	0.49984	0	1
Age	37,346,491	44.43966	11.60936	25	65
Number of own children	37,346,491	0.91992	1.17582	0	9
Education: high school graduate	37,346,491	0.88692	0.31669	0	1
Education: some college	37,346,491	0.50694	0.49995	0	1
Education: Bachelor and above	37,346,491	0.27009	0.44400	0	1
Is married	37,346,491	0.65426	0.47561	0	1
Current Population Survey (1980–2017):					
Employed at mining industries	33,145,745	0.00608	0.07772	0	1
Employed at construction industries	33,145,745	0.05027	0.21851	0	1
Employed at manufacturing industries	33,145,745	0.11279	0.31634	0	1
Employed at agriculture industries	33,145,745	0.01893	0.13629	0	1
Employed at other industries	33,145,745	0.54493	0.49798	0	1
NTR-gap	33,145,745	0.30938	0.31455	0.00141	1.95687
Race: White	33,145,745	0.84451	0.36237	0	1
Race: Black	33,145,745	0.09764	0.29683	0	1
Sex (Female = 1)	33,145,745	0.52026	0.49959	0	1
Age	33,145,745	43.34409	11.45512	25	65
Number of own children	33,145,745	1.01045	1.21829	0	9
Education: high school graduate	33,145,745	0.35158	0.47746	0	1
Education: some college	33,145,745	0.28292	0.45042	0	1
Education: Bachelor and above	33,145,745	0.23076	0.42132	0	1
Is married	33,145,745	0.67649	0.46781	0	1

Note: All dollar figures are converted into 2017 dollars to reflect real values.

6 Results

Over the years 1980–2017, the (de-meaned) employment growth of industries with low NTR-gap experienced an upward trend (red lines of Figure 2). While the employment growth of industries with high NTR-gap followed the same path for the years prior to the trade liberalisation it started to fall and diverge for the years after the reform (blue lines of Figure 2). This fact suggests negative employment effects of trade liberalisation for industries with high NTR-gap. However, this figure only reveals a correlational link. In order to establish the causality, we apply regressions introduced in equation (3).

Figure 2 The changes in employment growth across industries with high/low NTR-gap over the years before and after the trade policy change (see online version for colours)



We also implement an event-study analysis in which we assume a time event at year 2000, and calculate the coefficients for each group of year before and after the policy change. The results are reported in Figure 3. Not only that there is no pre-trend in the outcome but also the coefficients start to rise (in magnitude) after the trade liberalisation.

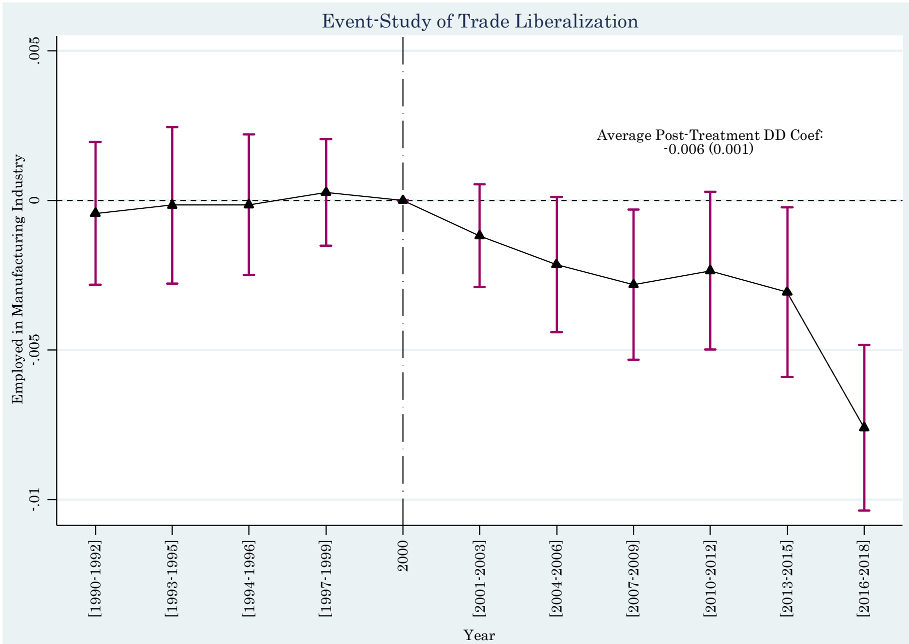
The main results are reported in Table 2 for two data sources and for different specifications across columns. The primary outcomes in these regressions are a dummy that equals one if the person is employed in the construction sector and zero otherwise. The main coefficient of interest is β of equation (3), the coefficient of interaction between Post and NTRGap. Focusing on the most parametrised specifications shown in columns 3 (for census-ACS data) and 6 (for CPS data), exposure to a one-unit higher NTR-gap after the trade liberalisation compared to before is associated with 36 and 16 basis points reduction in the probability of being employed in the construction sector, respectively. To put these numbers into perspective, one can compare the marginal effects with the mean of the dependent variable to capture the percentage effects of the implied coefficients. As reported in row 5, the effects are equivalent to a 7.6% and 3.1% reduction from the mean of the outcome. The effects are economically large and statistically significant at 1% level.

Table 2 The effect of US-China trade liberalisation on employment in construction industry

	Outcome: Employed in construction industries					
	Census-American Community Survey			*Current Population Survey*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	–0.0053***	–0.00505***	–0.00364***	–0.00123***	–0.00227***	–0.00156***
	(0.00067)	(0.00065)	(0.00069)	(0.0003)	(0.0003)	(0.0003)
Observations	37,346,491	37,346,491	37,346,491	33,145,745	33,145,745	33,145,745
R-squared	0.00107	0.04706	0.04713	0.00157	0.0545	0.05453
Mean dependent variable	0.04730	0.04730	0.04730	0.05027	0.05027	0.05027
Percentage effect	–11.19758	–10.67012	–7.68632	–2.45418	–4.50992	–3.09457
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	No	Yes	Yes
State-by-year controls	No	No	Yes	No	No	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25-65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

Figure 3 Event-study analysis of trade liberalisation on employment in manufacturing (see online version for colours)



One concern is that we are comparing construction industries with all other industries and that not all industries could be a good counterfactual for construction employment. To check for this issue, we restrict the sample to those in the labour force and whose primary industry of occupation is the construction sector and change the outcome to a dummy that equals one if the person is employed and zero otherwise. In this way, we am looking at the intensive margin effects, that is, taking advantage of pre and post-reform variations and variation in states' employment dependency on the construction industry rather than across industry NTR-gap variations. The results are reported in Table 3. In the full specifications, a one-unit higher NTR-gap is associated with 184 and 66 basis points reduction in the likelihood of being employed among people in the construction labour market for the census-ACs and CPS sample, respectively.

Previous studies suggest that trade could not only have differential effects by industry but also contain differential effects by gender and race (Fuller and Vosko, 2008; Gaddis and Pieters, 2017; Munro, 2001). To explore whether such heterogeneity exists in our identification strategy and whether the results are driven by a specific subsample, we apply equation (3) across subsamples by gender (reported in Table 4) and race-ethnicity (reported in Table 5). The census-ACS sample suggests that the negative employment effects of trade for construction employment are concentrated among males while the CPS data suggests roughly similar effects among males and females. However, for both subpopulations, the effects are negative and economically similar to the main results. The race-ethnicity analysis of Table 5 suggests that the effects are more pronounced for non-Hispanic whites and blacks and become statistically insignificant and economically small for the Hispanic population.

Table 3 The intensive-margin effect of US-China trade liberalisation on employment among those in construction industry

	Outcome: Is employed					
	Census-American Community Survey			*Current Population Survey*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	–0.01496***	–0.01605***	–0.01844***	–0.00055	–0.0016	–0.00666***
	(0.00325)	(0.00327)	(0.00351)	(0.00153)	(0.00151)	(0.00165)
Observations	1,954,023	1,954,023	1,954,023	1,838,572	1,838,572	1,838,572
R-squared	0.01768	0.04566	0.04612	0.01768	0.03914	0.03956
Mean dependent variable	0.90409	0.90409	0.90409	0.90582	0.90582	0.90582
Percentage effect	–1.65419	–1.77491	–2.03991	–0.06024	–0.17706	–0.73518
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	No	Yes	Yes
State-by-year controls	No	No	Yes	No	No	Yes

Note: The sample is restricted to those whose industry of occupation is construction. Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

Table 4 Heterogeneity of the effects by gender

	Outcome: Employed in Construction Industries			
	Census-American Community Survey		*Current Population Survey*	
	Females	*Males*	*Females*	*Males*
	(1)	*(2)*	*(3)*	*(4)*
Pos t ×NTR-gap	–0.00016	–0.00739***	–0.00149***	–0.00151**
	(0.00042)	(0.00135)	(0.00019)	(0.00062)
Observations	19,142,672	18,203,819	17,247,491	15,898,254
R-squared	0.00249	0.0302	0.00248	0.03464
Mean dependent variable	0.01007	0.08645	0.00948	0.09450
Percentage effect	–1.62283	–8.55345	–15.70488	–1.59798
State fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
State-by-year controls	Yes	Yes	Yes	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

To check for the robustness of the effects, we also explore the effects of trade on other individual outcomes. The results are reported in Table 6. These additional outcomes include the number of weeks worked last year (column 1, based on census-ACS sample), the estimated market value of the house if the person is an owner (column 2, based on census-ACS sample), the usual number of hours the person works in a week (column 3, based on CPS sample), and the number of hours that the person worked last week (column 4, based on CPS sample). Note that these regressions capture the net effect of trade liberalisation measures across all working-age persons. The idea is that trade has negative effects for those employed in industries with high NTR-gap and should have zero effect for those in low NTR-gap industries. Therefore, the net reduced-form effects should still be negative. To capture this effect for those in the construction labour market, we show the results by restricting the sample to those in the labour force and whose primary industry of occupation is in the construction sector. These estimates are reported in Table 7. The estimated effects of both Table 6 and Table 7 suggest negative and significant effects of trade liberalisation for other labour market and socioeconomic measures. For instance, among those people in the construction industry after the trade policy change compared to before, a 31 percentage points difference in NTR-gap (standard deviation of NTR-gap) is associated with \$11,371 reduction in house value (column 2, Table 7).

Table 5 Heterogeneity of the effects by race and ethnicity

	Outcome: Employed in construction industries					
	Census-American Community Survey			*Current Population Survey*		
	Non-Hispanic Whites	*Hispanics*	*Blacks*	*Non-Hispanic Whites*	*Hispanics*	*Blacks*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	–0.00367***	0.00147	–0.00282*	–0.00441***	0.00108	–0.00516***
	(0.00082)	(0.00335)	(0.00146)	(0.00038)	(0.00126)	(0.00068)
Observations	30,608,839	1,848,573	3,930,917	25,098,262	3,144,313	3,215,445
R-squared	0.04940	0.04633	0.02427	0.05126	0.09249	0.03277
Mean dependent variable	0.05057	0.04669	0.02364	0.05241	0.06857	0.02643
Percentage effect	–7.25090	3.15814	–11.92143	–8.42155	1.57127	–19.51554
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

Table 6 The effect of US-China trade liberalisation on other labour market and socioeconomic outcomes

	Census-American Community Survey		*Current Population Survey*	
	Outcome: weeks worked last year	*Outcome: house value ($1,000)*	*Outcome: usual hours works per week*	*Outcome: hours worked last week*
	(1)	*(2)*	*(3)*	*(4)*
Post × NTR-gap	−0.69874***	−37.32454***	−0.27311***	−0.27592***
	(0.05823)	(0.57022)	(0.02551)	(0.02456)
Observations	37,346,491	27,034,015	13,888,288	17,747,703
R-squared	0.13016	0.22963	0.07903	0.07118
Mean dependent variable	36.16933	248.10883	40.87540	40.52005
Percentage effect	−1.93187	−15.04361	−0.66814	−0.68093
State fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
State-by-year controls	Yes	Yes	Yes	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***p < 0.01, **p < 0.05 and *p < 0.1.

Table 7 The intensive-margin effect of US-China trade liberalisation on other labour market and socioeconomic outcomes

	Census-American Community Survey		*Current Population Survey*	
	Outcome: weeks worked last year	*Outcome: house value ($1,000)*	*Outcome: usual hours works per week*	*Outcome: hours worked last week*
	(1)	*(2)*	*(3)*	*(4)*
Post × NTR-gap	–0.18336	–36.68691***	–0.3295***	–0.24512***
	(0.16883)	(2.44005)	(0.08843)	(0.0907)
Observations	1,954,023	1,457,216	943,102	1,237,591
R-squared	0.06289	0.227	0.06105	0.05083
Mean dependent variable	44.46752	232.14184	42.08399	40.77563
Percentage effect	–0.41237	–15.80360	–0.78296	–0.60113
State fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
State-by-year controls	Yes	Yes	Yes	Yes

Notes: The sample is restricted to those whose industry of occupation is construction. Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The Results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

Table 8 The effect of US-China trade liberalisation on employment in manufacturing industry

	Outcome: Employed in manufacturing industries					
	Census-American Community Survey			*Current Population Survey*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	–0.00713***	–0.0079***	–0.00857***	–0.00197***	–0.00200***	–0.00964***
	(0.00089)	(0.00088)	(0.00094)	(0.00041)	(0.00041)	(0.00044)
Observations	37,346,491	37,346,491	37,346,491	33,145,745	33,145,745	33,145,745
R-squared	0.01605	0.04119	0.04124	0.01904	0.04646	0.04658
Mean dependent variable	0.10756	0.10756	0.10756	0.11279	0.11279	0.11279
Percentage effect	–6.62727	–7.34134	–7.97115	–1.74898	–1.77457	–8.54717
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	No	Yes	Yes
State-by-year controls	No	No	Yes	No	No	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.1$.

Table 9 The intensive-margin effect of US-China trade liberalisation on employment among those in manufacturing industry

	Outcome: Is employed					
	Census-American Community Survey			*Current Population Survey*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	–0.00715***	–0.0064***	–0.00698***	–0.00179**	–0.00226**	–0.00636***
	(0.00173)	(0.0017)	(0.0019)	(0.00091)	(0.0009)	(0.00098)
Observations	4,234,187	4,234,187	4,234,187	3,979,668	3,979,668	3,979,668
R-squared	0.00843	0.02745	0.02755	0.00934	0.02583	0.02617
Mean dependent variable	0.94870	0.94870	0.94870	0.94214	0.94214	0.94214
Percentage effect	–0.75413	–.67412	–0.73606	–0.19023	–0.24036	–0.67460
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	No	Yes	Yes
State-by-year controls	No	No	Yes	No	No	Yes

Notes: The sample is restricted to those whose industry of occupation is manufacturing. Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25–65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

Table 10 Placebo test: the effects of trade policy change across unaffected industries

	Census-American Community Survey			*Current Population Survey*		
	Outcome: employed in agriculture industry	*Outcome: employed in mining industry*	*Outcome: employed in other sectors*	*Outcome: employed in agriculture industry*	*Outcome: employed in mining industry*	*Outcome: employed in other sectors*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	0.00086	0.00054	0.00544	0.00074	–0.00034	0.00411
	(0.0006)	(0.00036)	(0.00353)	(0.00155)	(0.00098)	(0.00522)
Observations	37,346,491	37,346,491	37,346,491	33,145,745	33,145,745	33,145,745
R-squared	0.01291	0.01357	0.09877	0.01785	0.01545	0.09483
Mean dependent variable	0.01469	0.00515	0.53792	0.01893	0.00607	0.54493
Percentage effect	5.86884	10.43426	1.01075	3.88748	–5.64857	0.75375
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25-65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.1$.

An additional check for the robustness of the results is to explore the effects for another industry with high NTR-gap. The identification assumption stipulates that one should observe the same effects if we look into each industry with high exposure to trade policy change, that is, an industry with high NTR-gap. The manufacturing sector is another industry with this feature and has been shown to be highly affected by trade liberalisation (Galiani and Sanguinetti, 2003; Noghani and Noghanibehambari, 2019; Pierce and Schott, 2018, 2020). We re-evaluate this literature with the current data and identification strategy. The results are shown in Table 8 and Table 9 for the full sample and the sample restricted to workers in the manufacturing sector only, respectively. All the interaction terms are negative, economically large, and statistically significant, confirming that, in line with previous literature, trade liberalisation caused a negative effect on manufacturing employment.

Overall, the results of this section suggest that there are negative effects from trade liberalisation for the labour market and socioeconomic outcomes of workers in construction industries.[2]

7 Concerns over endogeneity

One potential concern over the endogeneity of the results is that the effects are only capturing the general trends of aggregate employment and that regardless of NTR-gap and the trade reform, one could observe the same reductions in employment. If that is the case, one may observe the same negative effects for industries with low and zero NTR-gaps. To explore this endogeneity issue, we run some placebo tests where the outcome is whether or not a person is employed in agriculture, mining, and all other industries. The results are reported in Table 10. All the coefficients of interaction terms are statistically insignificant and economically meaningless. Therefore, we can rule out the possibility of aggregate employment trends driving the main results.

8 Conclusions

Economic theory predicts that international trade has the potential to improve total production and welfare by applying the principles of comparative advantage and exploiting the resources with lower opportunity costs. However, the theory lacks to explain the heterogeneous effects of trade across subpopulations. As the quite large empirical evidence suggests, trade has winning and losing parties within each country. From a policymaker's perspective, it is essential to detect the losing parties and provide welfare programs or training programs to help them in their job transitions.

This paper aimed to provide empirical evidence of the effects of US-China trade liberalisation on employment in the construction industry. We use two large data sources that together combined information of more than 70 million individuals over the years 1980–2017 and applied a difference-in-difference identification strategy. We find negative effects of trade liberalisation on employment outcomes of construction workers. The results are statistically significant and economically large. The effects are robust both at the extensive and intensive margin, across a wide range of specifications, and for various measures of the labour market and socioeconomic outcomes. The effects are heterogeneous by gender and race and are more pronounced for males and whites. This heterogeneity is in line with previous literature which explored the effects of trade liberalisation on other measures and find that the negative effects of trade are concentrated among white males (Pierce and Schott, 2020). A placebo test showed that the effects could not have been driven by aggregate employment decline and that the trade shock did not have an effect on employment in sectors with low exposure to tariff reductions.

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Notes

1 Appendix Table a1 shows the average NTR-Gap across aggregated industry groups.

2 As an additional heterogeneity check, we also show the results for states with high/low construction employment in Appendix Table a3. While the effects are large for states with higher share of construction employment, they are robust for both subsamples.

Appendix

Table a1 NTR-gap across industries

Industry	*Mean*
Agriculture, forestry, fishing and hunting	0.001
Mining	0.021
Construction	0.354
Manufacturing	0.474
Trade	0.002
Transportation	0.000
Real Estate	0.000
Services	0.000
All other	0.000

Table a2 Clustering the standard errors at the state-level

	Outcome: Employed in construction industries					
	Census-American Community Survey			*Current Population Survey*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Post × NTR-gap	–0.0053***	–0.00505***	–0.00364***	–0.00123***	–0.00227***	–0.00156***
	(0.00077)	(0.00055)	(0.00087)	(0.0045)	(0.0047)	(0.0051)
Observations	37,346,491	37,346,491	37,346,491	33,145,745	33,145,745	33,145,745
R-squared	0.00107	0.04706	0.04713	0.00157	0.0545	0.05453
Mean dependent variable	0.04730	0.04730	0.04730	0.05027	0.05027	0.05027
Percentage effect	–11.19758	–10.67012	–7.68632	–2.45418	–4.50992	–3.09457
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	No	Yes	Yes
State-by-year controls	No	No	Yes	No	No	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25-65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

Table a3 Heterogeneity of the effects by high/low employment in construction

	Outcome: Employed in construction industries					
	Census-American Community Survey			*Current Population Survey*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
States with above median share of employment in construction:						
Post × NTR-gap	–0.0087***	–0.00687***	–0.00478***	–0.00124***	–0.00243***	–0.00169***
	(0.0047)	(0.00105)	(0.00102)	(0.0052)	(0.0053)	(0.0082)
States with below median share of employment in construction:						
Post × NTR-gap	–0.0021	–0.00457***	–0.00394***	–0.00098*	–0.00142***	–0.00187**
	(0.0047)	(0.00098)	(0.00104)	(0.0052)	(0.0057)	(0.0095)
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	No	Yes	Yes
State-by-year controls	No	No	Yes	No	No	Yes

Notes: Robust standard errors are reported in parentheses. All regressions are weighted using IPUMS-provided personal weights. The results of OLS regressions are reported. Individual-level controls include a quadratic function of age, dummies for the race (whites, blacks, and other races), dummies for education (high school graduates, some college degree, bachelor and above), a dummy for being married, and the number of children. State-by-year controls include the share of blacks, whites, males, people aged 25-65, labour union coverage rate, Medicaid coverage rate, minimum wage, dummies for welfare reforms, and dummies to capture the passage of the Affordable Care Act. Percentage effects are calculated using the coefficient divided by the mean of the dependent variable. All dollar figures are converted into 2017 dollars to reflect real values. *** $p < 0.01$, ** $p < 0.05$ and *$p < 0.1$.