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Minimum wages and the China Syndrome: Causal evidence from US local labor markets

Luke Milsom* Isabelle Roland†

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1 Introduction

The opening of the Chinese economy and its subsequent dominance in world trade has been one of the most important economic developments in the late 20th and early 21st centuries. Despite the well-known benefits of product market integration, a large literature has documented that rising Chinese import penetration has had adverse effects on labor markets. In their seminal contribution, Autor, Dorn, and Hanson (2013) find that exposure to Chinese import competition led to significant manufacturing job losses in the United States. Local labor markets (defined as commuting zones), however, differ significantly in how they fared with respect to manufacturing employment (see Section 3.2, Figure 1). An important question is whether labor market institutions have an impact on the dynamic response of manufacturing employment to rising import penetration. We contribute to this debate by studying whether minimum wages dampened or amplified the negative effect of Chinese import penetration on manufacturing employment in US local labor markets between 2000 and 2007.

We follow a rigorous double-edged identification strategy to tackle the potential endogeneity of both import penetration and minimum wage policies. Specifically, we combine instrumental variables with a border identification strategy. First, we adapt the instrumental variables of Autor, Dorn, and Hanson (2013) (henceforth ADH, 2013) and Pierce and Schott (2016) to address the endogeneity of import penetration. ADH (2013) develop a shift-share instrument that combines industry-specific changes in Chinese import penetration with local exposure shares given by the lagged industrial composition of US commuting zones. To remove potential demand-side endogeneity in import penetration, they use average changes in imports from China across eight non-US high-income countries. Pierce and Schott (2016) exploit a change in US trade policy. In 2000, Congress passed a law granting China "permanent normal trade relations" with the US, thereby eliminating the possibility of tariffs on Chinese imports increasing overnight. The authors use the concept of variable uncertainty reduction (as different industries faced larger potential tariff increases) and variable local employment composition to construct an instrument for import penetration. Using two instruments lends robustness to our analysis, and allows us to test overidentifying restrictions.

Second, we use a border identification strategy to distinguish the effects of minimum wage policies from the effects of other local labor market characteristics that are unrelated to policy[1]. Specifically, we rely on comparing commuting zones that are contiguous to each other but located in different states with different minimum wage policies. The approach essentially considers what happens to the response of manufacturing employment to import penetration when one crosses a policy border. Suppose that a state with a minimum wage above the Federal level is adjacent to a state with the Federal minimum wage. If minimum wage policies are an important determinant of how manufacturing employment responds to increased Chinese import competition, one should find an abrupt change in this response when one crosses the state border, because local labor market characteristics unrelated to policy are

[1]See e.g. Holmes (1998), Huang (2008), and Dube et al. (2010) for the application of border identification strategies in other settings.

arguably the same on both sides of the border. This strategy amounts to a matched difference-in-differences approach, and combines the power and generalisability of large scale empirical work with the internal validity of convincingly causal local case studies. Nesting instrumental variables within a border-identification strategy with demanding fixed effects enables us to provide convincingly causal evidence on the interaction between rising Chinese import penetration and minimum wage policies.

Using an annual panel of 234 unique pairs of commuting zones for the period 2000-2007, we find that minimum wages amplified the negative response of manufacturing employment to import competition. The additional percentage point decrease in the manufacturing employment share resulting from a one-unit increase in import penetration when the log deviation between the state's minimum wage and the Federal level is at its mean amounts to 25% of the direct effect of import competition on manufacturing employment. When our policy variable is a dummy for states with a minimum wage above the Federal level, we find that the additional percentage point change in the manufacturing employment share resulting from a one-unit increase in import penetration amounts to about 86% of the direct effect of import competition. We perform a comprehensive series of robustness checks. First, we add a second instrument based on Pierce and Schott (2016), which enables us to test overidentifying restrictions. Second, we assess the validity of our border identification strategy, including placebo tests and tests that insure that our results are robust to spillovers across commuting zones. Finally, we also apply insights from Borusyak et al. (2021) to assess the consistency of our estimates and further explore the validity of the ADH (2013) shift-share instrument.

This paper relates to several strands of the literature. First, it relates to the seminal literature on the "China Syndrome", which uncovers substantial and long-lasting adjustment costs in response to rising Chinese import penetration, among others large manufacturing job losses in US local labor markets (see e.g. ADH, 2013; Pierce and Schott, 2016; Autor et al., 2016). We contribute to this literature by examining whether the magnitude of these manufacturing job losses is affected by minimum wage policies. Second, our paper relates to the growing theoretical and empirical literature on the joint effects of labor market frictions and trade reforms. This literature studies the long-run impact of globalization and labor market rigidities on a variety of outcomes such as unemployment, job volatility, and the distribution of wages[2]. Most of these papers are theoretical and structural in nature, while ours is a purely reduced-form analysis. Another important distinction between this literature and our paper is that we focus on the consequences of labor market policies for the transitional dynamics of manufacturing employment following a rise in import penetration (as opposed to the long-run). Itskhoki and Helpman (2015) and Bellon (2016) show that falling trade costs can generate a short-run increase in unemployment. Kambourov (2009) and Ruggieri (2019) link the response of unemployment to labor market regulations. In particular, Ruggieri (2019) finds that the unemployment response to trade liberalization is larger the higher the minimum wage. Again, these papers are very distinct from ours in that they are theoretical and structural in nature. We are not aware of any existing reduced-

[2]See e.g. Helpman and Itskhoki, 2010; Helpman et al., 2010; Amiti and Cameron, 2012; Felbermayr et al., 2016; Dix-Carneiro, 2014; Fajgelbaum, 2016; Cosar et al., 2016; Helpman et al., 2017.

form causal evidence on the link between minimum wage policies and the response of manufacturing employment to the China trade shock.

Finally, our paper speaks to the literature on the effects of labor market institutions on labor market performance. Among others, Bentolila and Bertola (1990), Hopenhayn and Rogerson (1993), and Alvarez and Veracierto (2000) explore to which extent differences in labor market policies, including minimum wages, can generate differences in labor-market performance and aggregate efficiency. Our paper is methodologically related to Dube et al. (2010) who use policy discontinuities at state borders to identify the effects of minimum wages on earnings and employment in restaurants and other low-wage sectors. Finally, our paper also relates in spirit to contributions that study the impact of labor market institutions on the labor market performance of an economy subject to a large structural shock. For example, Veracierto (2008) studies the effect of firing costs on an economy that is subject to business cycle technological shocks, and Anderton et al. (2015) and di Mauro and Ronchi (2015) analyze the effects of labor market institutions on firms' adjustment to the Great Recession. Like Ruggieri (2019), we focus on the impact of labor market institutions on the labor market performance of an economy subject to a trade shock.

The rest of the paper proceeds as follows. Section 2 describes our double-edged identification strategy. Section 3 presents our data sources and some descriptive statistics. Section 4 presents our empirical findings. Section 5 presents a series of robustness checks and Section 6 concludes.

2 Identification strategy

As opposed to ADH (2013) who look at the impact of import competition on manufacturing employment using long differences for the periods 1990-2000 and 2000-2007, we perform our analysis on an annual panel of commuting zones for the period 2000-2007. We are interested in the interaction between policy and trade shocks in affecting the dynamics of manufacturing employment. Therefore, we require an identification strategy that deals with sources of endogeneity stemming from both angles. We address the endogeneity of import penetration by adapting the instrumental variables of ADH (2013) and Pierce and Schott (2016) to fit our setting. We address unobserved heterogeneity in local labor markets with a border identification strategy and demanding fixed effects. Each component of our strategy is described in detail below.

2.1 Border identification strategy

Our border identification strategy is designed to distinguish the effects of (state-level) minimum wage policies from the effects of other local labor market characteristics that are unrelated to policy (see e.g. Dube et al., 2010). Specifically, we rely on comparing commuting zones (CZs) that are contiguous to each other but located in different states with different minimum wage policies (henceforth, CZ-pairs). The approach essentially considers what happens to the response of manufacturing employment to import penetration when one crosses a state border (i.e. policy border). If minimum wage policies are an important determinant of how manufacturing employment responds to Chinese import penetration, one should find an abrupt change in this response when one crosses a border at which policy changes, because local labor market characteristics unrelated to policy are arguably the same on both sides of the border. By considering pairs, this method generalizes the case study approach by using all local differences in minimum wages in the US over the period 2000-2007. Thus, we combine the causal internal validity of a local case study approach with the statistical power and external generalisability of aggregation across the whole country. As shown in Section 3.2, we rely on substantial differences in treatment intensity within CZ-pairs.

Our empirical model regresses the manufacturing employment share in CZ i, which belongs to CZ-pair p at time t (denoted y_{ipt}) on import penetration in CZ i at time t (IP_{it}), a variable capturing minimum wage regulations in state s at time t (MW_{st}), and an interaction term between import penetration and the policy variable. Specifically, we estimate regressions of the form:

$$y_{ipt} = \beta_0 + \beta_1 MW_{st} + \beta_2 IP_{it} + \beta_3 IP_{it} \cdot MW_{st} + \beta_4 X_{st} + \tau_i + \rho_{pt} + \varepsilon_{ipt} \tag{1}$$

where τ_i are CZ fixed effects and ρ_{pt} are pair-time fixed effects. Each observation is weighted by its CZ's population share in total US population in 1990. IP_{it} measures the level of Chinese import penetration in CZ i in year t. As in ADH (2013), CZs differ in terms of their exposure to Chinese import competition because of the varying degree of importance of different manufacturing industries for local employment. In other words, we transform industry-level variation in import exposure into geographic variation by using differences in industry employment composition. As opposed to ADH (2013), we work with annual data on the level of import penetration for the period 2000-2007, rather than long differences for the periods 1990-2000 and 2000-2007. Our measure of import competition is given by Chinese import exposure per worker in a CZ, where total US imports per worker in industry j are apportioned to CZ i according to the share of manufacturing industry j in total CZ employment:

$$IP_{it} = \sum_j \frac{L_{ijt}}{L_{it}} \frac{M_{jt}}{L_{jt}} \tag{2}$$

where L_{ijt} is employment in manufacturing industry j in CZ i at time t; L_{it} is total employment in CZ i at time t; L_{jt} is total US employment in industry j at time t; M_{jt} is imports from China to the US in industry j at time t. A well-known concern is that realized US imports from China, M_{jt}, may be correlated with industry import demand shocks, in which case the OLS estimates may be biased, as both US manufacturing employment and imports may be positively correlated with unobserved shocks to US product demand. Therefore, IP_{it} is instrumented as detailed in Section 2.2.

The policy variable MW_{st} is the difference between the natural logarithm of the state-level minimum wage and the natural logarithm of the Federal minimum wage. Alternatively, it is defined as a dummy variable equal to one when a state has a higher minimum wage than the Federal level, and zero otherwise. Our fixed effects imply that we only use variation in minimum wages within each CZ-pair. The identifying assumption is $E[MW_{st} \cdot \varepsilon_{ipt}] = 0$, i.e. minimum wage differences within the CZ-pairs are uncorrelated with the differences in the residual dependent variable in either CZ.

X_{st} is a vector of further state-level controls. First, we include two state-level policies that might affect manufacturing employment, namely the state corporation tax and a measure of the business environment, namely an indicator variable for whether the state has "Right-to-Work" (RTW) laws. RTW laws are laws prohibiting agreements between employers and labor unions that require employees' union membership or payment of union fees as a condition of employment, either before or after hiring. RTW laws have been used as a proxy for pro-business policy (e.g. Holmes, 1998). Second, we control for manufacturing union membership (lagged to address potential simultaneity bias). Finally, we include state-level demographics controls, namely the share of a state's working-age population with college education, the share of the working-age population that is foreign born, and female labor force participation.[3]

The standard errors are clustered at the CZ-pair level. We face the additional issue that some CZs will be included multiple times in the data set. This is because a particular CZ will be in the sample as many times as it can be paired with a contiguous CZ in another state. This leads to a mechanical correlation across CZ-pairs and potentially along entire border segments. In estimating Equation (1), we therefore cluster standard errors both at the CZ-pair level and border-segment level.

2.2 Instrumental variables

A well-known concern is that realized US imports from China, M_{jt}, may be correlated with industry import demand shocks, in which case the OLS estimates may be biased, as both US manufacturing employment and imports may be positively correlated with unobserved shocks to US product demand. To identify the causal effect of rising Chinese import exposure on US manufacturing employment and the interaction between import exposure and minimum wage policies, we employ the instrumental variable strategy of ADH (2013). To identify the supply-driven component of Chinese imports, they instrument for growth in Chinese imports to the US using the growth of Chinese imports in eight other high-income countries[4]. Analogously, we instrument for the level of Chinese imports to the US using the level of Chinese imports in these eight other high-income countries. Specifically, our instrument is given by:

$$IP_{it}^{AIV} = \sum_j \frac{L_{ijt-10}}{L_{it-10}} \frac{M_{jt}^{other}}{L_{jt-10}} \tag{3}$$

[3]These demographics controls are included in the preferred specification in ADH (2013) (specification (6) in Table 3) at the CZ level. However, we are unable to find annual data at the CZ level, and therefore use state-level equivalents. For the same reason, we cannot include the percentage of CZ employment in routine occupations and the average offshorability index of occupations. Further, we are unable to obtain annual state-level equivalents for these two variables.

[4]These are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

This expression differs from the expression in Equation (2) in two respects. First, in place of realized US imports by industry (M_{jt}), it uses realized imports from China to other high-income countries (M_{jt}^{other}). Second, in place of contemporaneous employment levels by industry and CZ, Equation (3) uses employment levels from the prior decade ($t-10$). We use lagged employment levels to apportion predicted Chinese imports to CZs in order to mitigate the simultaneity bias that might result from the fact that contemporaneous CZ employment is affected by anticipated trade with China. This is a shift-share instrumental variable (SSIV) where industry-level shocks $\left(\frac{M_{jt}^{other}}{L_{jt-10}}\right)$ are apportioned to geographies (CZs) using exposure shares $\left(\frac{L_{ijt-10}}{L_{it-10}}\right)$. By adding pair-year fixed effects, we weaken the identifying assumption of ADH (2013) by only requiring *within-pair* quasi-random industry-level import shocks.

In robustness tests, we also use a shift-share version of the instrument developed by Pierce and Schott (2016). Pierce and Schott (2016) find a causal link between the sharp decline in US manufacturing employment between 2000 and 2007 and the US granting Permanent Normal Trade Relations (PNTR) to China. The change was passed by Congress in October 2000 and became effective upon China's accession to the World Trade Organization (WTO) at the end of 2001. Conferral of PNTR did not change the actual import tariff rates applied to Chinese goods since US imports from China had been subject to the NTR tariff rates reserved for WTO members since 1980. The change, however, greatly reduced uncertainty regarding the NTR rates because they required annual renewals by Congress. Failing renewal, US import tariffs on Chinese goods would have jumped to the higher non-NTR tariff rates assigned to non-market economies. PNTR removed the uncertainty associated with these annual renewals by permanently setting US duties on Chinese imports at NTR levels. Pierce and Schott (2016) quantify the transition from annual to permanent normal trade relations via the "NTR gap", defined as the difference between the non-NTR rates to which tariffs would have risen if annual renewal had failed (which averaged 37 percent in 1999) and the NTR tariff rates that were locked in by PNTR (which averaged 4 percent in 1999). Importantly, the NTR gap exhibits substantial variation across industries - in 1999, its mean and standard deviation were 33 and 14 percentage points. We construct a shift-share instrument for a CZ's exposure to Chinese import competition by weighting the industry-specific NTR gaps with the shares of the corresponding industries in total CZ employment.

$$IP_{it}^{PSIV} = \sum_{j} \frac{L_{ijt-10}}{L_{it-10}} Gap_{jt} \tag{4}$$

where Gap_{jt} is the NTR gap from Pierce and Schott (2016). Again, we use employment levels from the prior decade ($t-10$) in the exposure weights to mitigate the potential simultaneity bias.

In both our instruments, the sum of exposure shares at the CZ level $\left(S_{it-10} = \sum_j \frac{L_{ijt-10}}{L_{it-10}}\right)$ is not equal to one since non-manufacturing industries are excluded, and it varies across CZs. The shares are said to be "incomplete" (Borusyak et al., 2021). The sum is equal to the 10-year lagged share of the manufacturing sector in CZ i's total employment. Borusyak et al. (2021) note that SSIV coefficients will be biased in the presence of incomplete shares, unless one controls for the sum of exposure shares itself (S_{it-10}) in the regression. This is because the SSIV estimator will leverage non-experimental variation in the sum of exposure shares in addition to quasi-experimental variation in the shocks, $\frac{M_{jt}^{other}}{L_{jt-10}}$ or Gap_{jt} depending on the instrument we use. The solution is to control for the 10-year lagged share of the manufacturing sector in CZ i's total employment. This addresses the concern that the China exposure variable may in part be picking up an overall trend decline in US manufacturing rather than the component that is due to differences across manufacturing industries in their exposure to rising Chinese competition.

3 Data and descriptive statistics

3.1 Data sources and data set construction

We use commuting zones (CZs) as our unit of analysis. CZs have been used extensively as geographical units for defining local labor markets (see e.g. Autor and Dorn, 2013; ADH, 2013). The concept of commuting zones was developed by Tolbert and Sizer (1996), who used county-level commuting data from the 1990 Census to create 741 clusters of counties that are characterized by strong commuting ties within CZs and weak commuting ties across CZs. Our analysis includes the 722 CZs that cover the mainland of the United States. CZs have the advantage of covering both metropolitan and rural areas, and providing a time-consistent definition of local labor markets based on economic geography.

Our dependent variable is the manufacturing employment share defined as the percentage of employed individuals who work in manufacturing. We retrieve annual county-level employment by SIC industry for the period 2000-2007 (as well as for 1990) from the County Business Patterns (CBP) of the US Census Bureau. Two issues have to be dealt with. First, CBP employment figures are given within a band when a county has too few observations within an industry code to maintain privacy. In order to overcome this issue, we use the fixed point algorithm developed by ADH (2013) to impute employment levels for each year of our sample period. Second, the CBP data series reports industry employment by the then-prevalent industry coding system which is not consistent over the entire sample period. We therefore use the cross-walk algorithm developed by ADH (2013) to map industry codes to one consistent coding system, covering almost 400 different manufacturing industries. We aggregate the county-level data to the CZ level. The manufacturing employment share varies by CZ and year. The CBP employment data is also used in the construction of the import penetration variables. We control for the 10-year lagged share of the manufacturing sector to address the "incomplete shares" problem. The share of each CZ in total US population in 1990 is taken from ADH (2013).

Our trade data on imports from China is from U.N. Comtrade. Specifically, we obtain annual data by HS industry code for the period 2000-2007 for the US and the eight high-income countries used in the construction of our instrumental variable (Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland). We combine Comtrade and CBP data to construct the import penetration variables in Equations (2), (3), and (4). In Equation (4), the data on the difference between the NTR (normal trade relations) rates and non-NTR rates is taken from Pierce and Schott (2016).

The policy variable of interest is the difference between the natural logarithm of the state-level minimum wage and the natural logarithm of the Federal minimum wage. Alternatively, it is defined as a dummy variable equal to one when a state has a higher minimum wage than the Federal level, and zero otherwise. The data on state-level minimum wages is taken from Vaghul and Zipperer (2016).[5] The policy variable varies by state and year. The Federal minimum wage was $5.15 from 1997 (September) to 2007 (July). This means that the Federal minimum wage is constant during our sample period. The other two policy controls also vary by state and year. The indicator variable for Right-to-Work states is equal to one if a state has Right-to-Work laws in a given year and zero otherwise. The state corporation tax is the headline corporation tax faced by firms in a given state. It is taken from Wilson (2009). Data on manufacturing union membership at the state level is taken from the Union Membership and Coverage Database (see Hirsch and Macpherson, 2003). Our annual state-level demographics control variables are built using IPUMS. The proportion of the population with college education is defined as the proportion of the working age population (16-60) with at least one year of college. The proportion of foreign born is defined as the proportion of the working age population who are born outside the US, not including those born in American Samoa, Guam, Puerto Rico or the US Virgin Islands. Female labor force participation is the proportion of working-age females who are in the labor force (employed or unemployed). Finally, we construct an identifier variable for each cross-state pair of contiguous CZs using ArcGIS.

3.2 Descriptive statistics

Local labor markets (commuting zones) differ significantly in terms of changes in manufacturing employment and Chinese import penetration between 2000 and 2007. Figure 1 shows the wide geographical variation present in the data set. Panel (a) shows the absolute change in import penetration and Panel (b) shows the absolute change in the manufacturing employment share between 2000 and 2007.

[5]The state-level minimum wage is the "adult non-exempt minimum wage" in Vaghul and Zipperer (2016).

Figure 1: Change in import penetration and manufacturing employment share by commuting zone for 2000-2007

(a) Δ Import penetration **(b)** Δ Manufacturing employment share

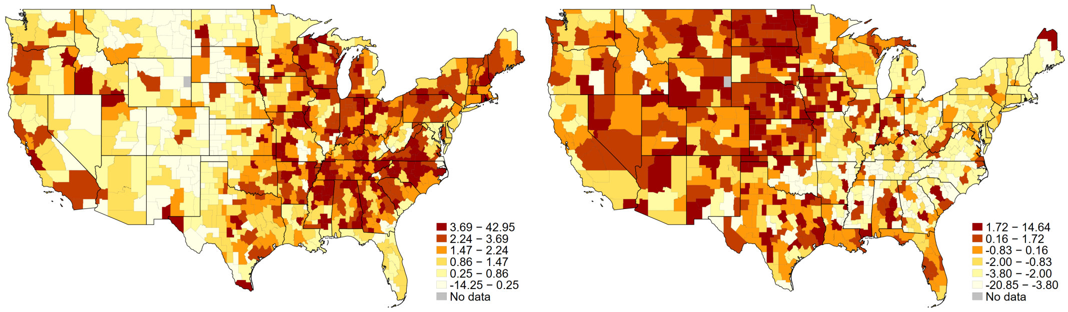


Figure 2 displays the location of the CZ-pairs in 2000 and 2007. Among the 722 CZs in the mainland US, 261 lie along a state border. This yields 234 pairs of contiguous commuting zones that lie on either side of a state border. Among these, the number of CZ-pairs with a minimum wage differential in 2000 was 20. By 2007, this number had risen to 134. Since we consider all contiguous CZ-pairs, an individual CZ will be replicated p times in our data set if it is part of p cross-state pairs. This is addressed by the way we construct our standard errors.

Figure 2: Contiguous border CZ pairs with minimum wage differentials

(a) 2000 **(b)** 2007

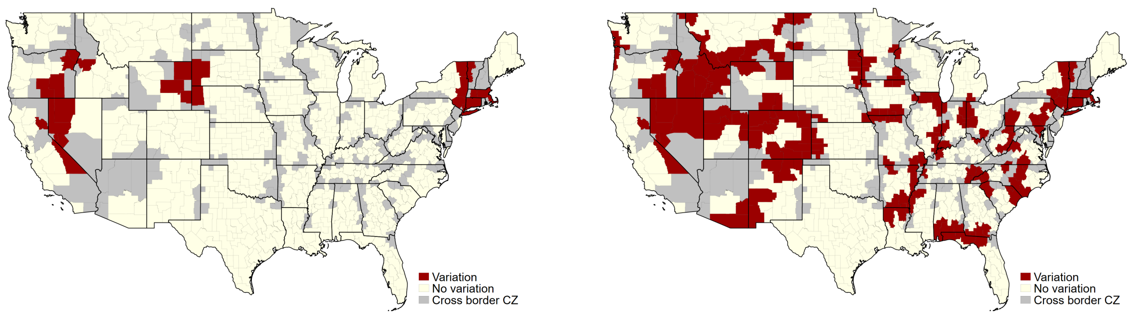


Figure 3 shows the yearly number of CZs that are part of a contiguous border pair that exhibits a minimum wage differential, as well as the yearly average minimum wage gap. The number of CZs that provide the variation to identify the interaction between minimum wages and import penetration is sizable. Moreover, there is a substantial average minimum wage gap among these CZs. In other words, contiguous CZs display substantial variation in minimum wages over the period of study, which enables us to identify minimum wage effects within CZ-pairs.

Figure 3: Number of contiguous border CZ pairs and average within-pair minimum wage differential by year

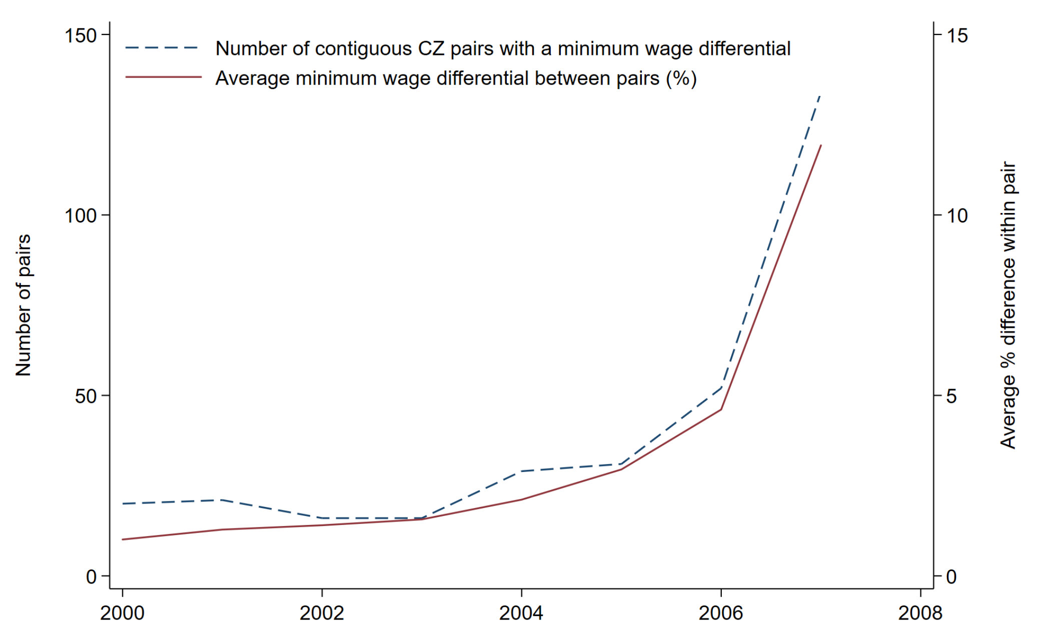


Table 1 presents descriptive statistics on manufacturing employment, hourly minimum wages, import penetration, the share of manufacturing employment in total CZ employment, the log deviation between the state-level minimum wage and the Federal level, the number of CZs, the number of CZ-pairs, and the number of states in our data set.

Table 1: Descriptive statistics

	Mean	Standard deviation
Manufacturing employment	19881.66	50929.72
Minimum wage ($ph)	5.3	0.46
Import penetration ($1000 per worker)	2.12	3.39
Share of CZ employment in manufacturing	17.35	10.72
Log min wage - Log Federal min wage	0.02	0.08
Unique commuting zones	265	
Unique border CZ-pairs	234	
Unique states	47	

4 Empirical analysis

Table 2 estimates Equation (1) using our sample of CZ-pairs. The policy variable is the difference between the logarithm of the state-level minimum wage and the logarithm of the Federal minimum wage. Column (1) has no controls except the ones of interest and the lagged manufacturing share to deal with the problem of incomplete shares. A coefficient of -0.369 on import penetration in column (1) can be interpreted as the causal percentage point change in the manufacturing employment share resulting from a one-unit increase in import penetration, i.e. an increase in import penetration of $1,000 per worker. It is significant at the 10% level. A coefficient of 2.056 on the log deviation between the state and Federal minimum wages means that 0.02056 can be interpreted as the causal percentage point change in the manufacturing employment share resulting from a 1% increase in the deviation of the state minimum wage from the Federal level. The coefficient is, however, not significantly different from zero. The coefficient on the interaction term between minimum wages and import competition is negative (-1.151) and significant at the 1% level, suggesting that minimum wage regulations amplify the negative impact of import competition on manufacturing employment. The magnitude of the coefficient suggests that the additional causal percentage point decrease in the manufacturing employment share resulting from a one-unit increase in import penetration when the log deviation in the minimum wage is at its mean of 0.08[6] amounts to -0.092. This additional impact amounts to 25% of the direct effect of import competition on manufacturing employment.

Column (2) augments the model with two state-level policy variables and manufacturing union membership. The only significant variable is the RtW dummy. A coefficient of -0.383 (significant at the 5% level) can be interpreted as the percentage point change in the manufacturing employment share associated with the introduction of RtW laws. Column (3) adds the demographics controls. Female labor force participation has a positive relationship with the manufacturing employment share (significant at the 10% level). Importantly, the coefficients on import penetration and its interaction with minimum wages are robust to the addition of policy and demographics controls.

In all the specifications, the Kleibergen-Paap LM statistics and associated p-values lead us to reject the null hypothesis of no correlation between the instrument and the endogenous regressor (null of no relevance). With one endogenous regressor and one instrument, the Cragg-Donald F statistic boils down to the F statistic of the first stage regression. Since it is close to 10 (in all columns), we reject the null of a weak correlation between the instrument and the endogenous regressor (null of weak instrument).

[6] This is the average log deviation between the state and Federal minimum wages in the sample of all CZs, where each observation is weighted by its CZ's share of population in 1990.

Table 2: Interaction between import penetration and minimum wages (log difference)

	(1)	(2)	(3)
IP	-0.369*	-0.377*	-0.363*
	(0.217)	(0.221)	(0.216)
(Log min wage - Log Federal min wage) # IP	-1.151***	-1.171***	-1.155***
	(0.424)	(0.446)	(0.441)
Log min wage - Log Federal min wage	2.056	1.707	1.455
	(1.377)	(1.237)	(1.276)
Lagged manufacturing share	-0.0452	-0.0457	-0.0465
	(0.0422)	(0.0430)	(0.0432)
RtW dummy		-0.383**	-0.556**
		(0.189)	(0.224)
Effective State tax rate		-14.58	-13.69
		(10.47)	(10.64)
% Manufacturing union membership (t-5)		-0.0203	-0.0191
		(0.0196)	(0.0186)
% Foreign born			0.0373
			(7.412)
% College educated			2.583
			(4.898)
Female labor force participation			10.20*
			(5.235)
Observations	3672	3672	3672
Cragg-Donald F stat	86.44	86.29	85.81
Kleibergen-Paap LM stat	9.869	9.895	9.365
Kleibergen-Paap p-value	0.00168	0.00166	0.00221

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3 below replicates the analysis of Table 2 using as policy variable a dummy equal to one when the state-level minimum wage is above the Federal level. A coefficient of -0.361 on import penetration in column (1) can be interpreted as the causal percentage point change in the manufacturing employment share resulting from a one-unit increase in import penetration, i.e. an increase in import penetration of $1,000 per worker. It is no longer significant. A coefficient of 0.674 on the indicator variable for the state minimum wage being above the Federal level can be interpreted as the causal percentage point change in the manufacturing employment share resulting from the state minimum wage being above the Federal level. It is significant at the 1% level. The coefficient on the interaction term between the indicator variable for the state minimum wage being above the Federal level and import competition is negative (-0.310) and significant at the 1% level, suggesting again that minimum wage regulations amplify the negative impact of import competition on manufacturing employment. It measures the additional causal percentage point change in the manufacturing employment share resulting from a one-unit increase in import penetration when the CZ is in a state with a minimum wage above the Federal level. This additional impact amounts to about 86% of the direct effect of import competition.

Column (2) augments the model with two state-level policy variables and manufacturing union membership. The only significant variable is again the RtW dummy. A coefficient of -0.388 (significant at the 5% level) can be interpreted as the percentage point change in the manufacturing employment share associated with the introduction of RtW laws. Column (3) adds the demographics controls. Female labor force participation again has a positive relationship with the manufacturing employment share (significant at the 5% level). Importantly, the coefficients on import penetration and its interaction with minimum wages are robust to the addition of policy and demographics controls.

Again, the Kleibergen-Paap LM statistics and associated p-values lead us to reject the null hypothesis of no correlation between the instrument and the endogenous regressor (null of no relevance). Since the Cragg-Donald F statistic is close to 10 (in all columns), we reject the null of a weak correlation between the instrument and the endogenous regressor (null of weak instrument).

Table 3: **Interaction between import penetration and minimum wages (dummy)**

	(1)	(2)	(3)
IP	-0.361	-0.377*	-0.362
	(0.219)	(0.225)	(0.222)
(Dummy for min wage above Federal level) # IP	-0.310***	-0.314***	-0.313***
	(0.105)	(0.108)	(0.105)
Dummy for min wage above Federal level	0.674***	0.595**	0.550**
	(0.247)	(0.236)	(0.247)
Lagged manufacturing share	-0.0437	-0.0444	-0.0455
	(0.0421)	(0.0427)	(0.0433)
RtW dummy		-0.388**	-0.578***
		(0.190)	(0.217)
Effective State tax rate		-12.96	-11.85
		(10.49)	(10.52)
% Manufacturing union membership (t-5)		-0.0224	-0.0213
		(0.0190)	(0.0181)
% Foreign born			0.199
			(7.499)
% College educated			3.517
			(5.183)
Female labor force participation			10.22**
			(5.106)
Observations	3672	3672	3672
Cragg-Donald F stat	81.43	80.34	79.29
Kleibergen-Paap LM stat	9.750	9.761	9.236
Kleibergen-Paap p-value	0.00179	0.00178	0.00237

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5 Robustness checks

In this section, we perform a wide range of robustness tests. First, we add a second instrument based on Pierce and Schott (2016), which enables us to test overidentifying restrictions. Second, we assess the validity of our border identification strategy, including placebo tests and tests that insure that our results are robust to spillovers across commuting zones. Finally, we also apply insights from Borusyak et al. (2021) to assess the consistency of our estimates and further explore the validity of the ADH (2013) shift-share instrument.

5.1 Pierce and Schott (2016) instrument

We explore the robustness of our results to the inclusion of the instrumental variable defined in Equation (4), which uses variation in China's legal trading status with the US. Including a second instrument allows us to test overidentifying restrictions. Tables 4 and 5 below replicate the results of Table 2 and Table 3 with a set of two instruments for import penetration. The coefficients on the interaction terms are very close to those of Tables 2 and 3, although slightly lower in magnitude. The coefficients retain the same level of significance (1%). Based on the Hansen J-statistics and associated p-values, we cannot reject the null hypothesis that the overidentifying restriction is valid.

Table 4: Interaction between import penetration and minimum wages (log difference)

	(1)	(2)	(3)
IP	-0.301*	-0.304*	-0.289*
	(0.178)	(0.181)	(0.169)
(Log min wage - Log Federal min wage) # IP	-1.147***	-1.161***	-1.146***
	(0.398)	(0.416)	(0.411)
Log min wage - Log Federal min wage	2.242*	1.915	1.676
	(1.311)	(1.165)	(1.192)
Lagged manufacturing share	-0.0521	-0.0532	-0.0546
	(0.0401)	(0.0407)	(0.0405)
RtW dummy		-0.420**	-0.582***
		(0.173)	(0.211)
Effective State tax rate		-14.03	-13.22
		(10.19)	(10.36)
% Manufacturing union membership (t-5)		-0.0200	-0.0190
		(0.0199)	(0.0190)
% Foreign born			0.536
			(7.344)
% College educated			2.207
			(4.662)
Famale labour force participation			10.30**
			(5.149)
Observations	3672	3672	3672
Cragg-Donald F stat	57.30	57.39	59.09
Kleibergen-Paap LM stat	11.26	11.21	11.19
Kleibergen-Paap p-value	0.0104	0.0107	0.0107
Hansen J stat	0.654	0.800	0.838
Hansen J p-value	0.721	0.670	0.658

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Interaction between import penetration and minimum wages (dummy)

	(1)	(2)	(3)
IP	-0.267	-0.288	-0.274
	(0.181)	(0.188)	(0.178)
(Dummy for min wage above Federal level) # IP	-0.303***	-0.306***	-0.306***
	(0.0923)	(0.0964)	(0.0937)
Dummy for min wage above Federal level	0.728***	0.649***	0.610***
	(0.228)	(0.222)	(0.227)
Lagged manufacturing share	-0.0529	-0.0531	-0.0545
	(0.0397)	(0.0403)	(0.0407)
RtW dummy		-0.433**	-0.604***
		(0.174)	(0.206)
Effective State tax rate		-12.19	-11.21
		(10.19)	(10.22)
% Manufacturing union membership (t-5)		-0.0209	-0.0204
		(0.0193)	(0.0185)
% Foreign born			0.723
			(7.409)
% College educated			2.769
			(4.912)
Famale labour force participation			10.39**
			(5.002)
Observations	3672	3672	3672
Cragg-Donald F stat	54.61	53.68	53.41
Kleibergen-Paap LM stat	10.86	10.83	10.69
Kleibergen-Paap p-value	0.0125	0.0127	0.0135
Hansen J stat	0.694	0.708	0.647
Hansen J p-value	0.707	0.702	0.724

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

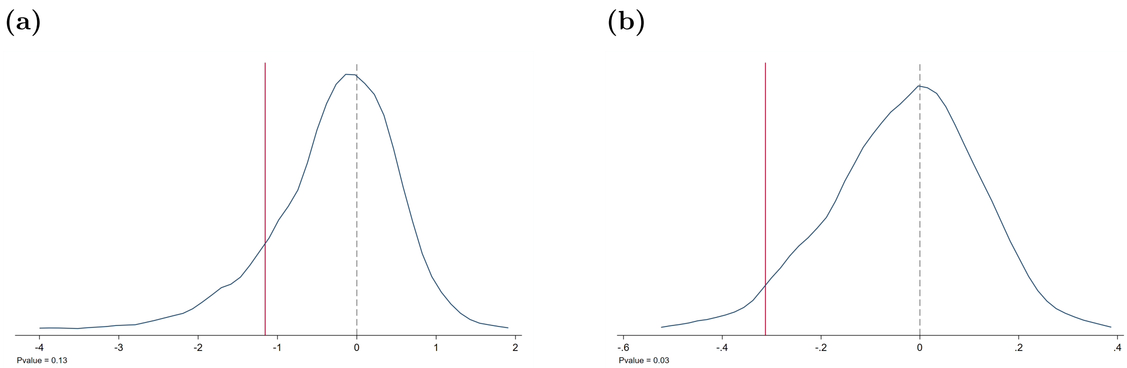
5.2 Validity of the cross-border identification strategy

Our border identification strategy builds on previous work, e.g. Holmes (1998), Huang (2008), and Dube et al. (2010). The addition of an instrumental variable strategy to address the endogeneity of import penetration does not affect the validity of the border identification strategy. In this section, we perform two kinds of tests for its validity, namely placebo tests across space, and tests for the presence of spillovers across commuting zones.

5.2.1 Placebo tests across space

To perform placebo tests across space, we randomly permute minimum wage laws across all states and estimate Equation (1) on this randomly generated data set. In this case, the true coefficient should be zero. We perform 1000 permutations of minimum wage laws and report the distribution of the permuted coefficients on the interaction term in Figure 4. Our actual coefficient estimates are also displayed for comparison (vertical line). Panel (a) shows the placebo distribution of the coefficient on the interaction between import penetration and the log difference between the state and Federal minimum wages, and Panel (b) shows the placebo distribution of the coefficient on the interaction between import penetration and the dummy variable. The actual coefficients are highly significant as they appear in the tails of their respective placebo distributions.

Figure 4: Placebo tests across space (1000 permutations)



5.2.2 Testing for spillover effects

A possible threat to identification is the possibility that spillovers across treatment and control commuting zones bias our coefficient estimates. Spillovers occur if employment in border CZs responds to minimum wage hikes across the state border (Dube et al, 2010). In other words, spillovers could take place if labor markets within a CZ-pair are linked. For example, in the case of labor markets with search costs, an increase in the minimum wage on one side of the border may pressure firms on the other side to increase wages, thereby reducing manufacturing employment. Therefore, an increase in the minimum wage on one side of the border could lead to a decrease in manufacturing employment on both sides of the border (treatment and control CZs) - leading us to underestimate the true effect of minimum wages on manufacturing employment (attenuation) when comparing the treatment and control CZs. If by contrast, an increase in the minimum wage on one side of the border leads to lower wages and higher employment on the other side of the border, we would overestimate the true effect of minimum wages (amplification). Spillover effects would not only bias the coefficient on the minimum wage policy variable, it would also affect the estimation of its interaction with import penetration. It is therefore crucial to exclude such spillovers.

To check for the presence of spillovers from minimum wage policies, we use county-level data and distinguish between border counties and interior counties within our CZ-pairs. Border counties are defined as those that are contiguous to a state border, and interior counties are defined as counties within a CZ not adjacent to a state border. We compare the effects of minimum wages on manufacturing employment in border counties to the effects on interior counties, the latter being less likely to be affected by spillovers. Specifically, we estimate the following spatially differenced equation[7]:

$$\tilde{y}_{ipt} = \gamma_0 + \gamma_1 MW_{st} + \tau_i + \rho_{pt} + \varepsilon_{ipt} \tag{5}$$

In Equation (5), $\tilde{y}_{ipt}$ denotes the average manufacturing employment share in border counties within CZ i relative to counties on the interior of the CZ: $\tilde{y}_{ipt} = \bar{y}_{ipt}^{bor} - \bar{y}_{ipt}^{int}$ where $\bar{y}_{ipt}^{bor}$ ($\bar{y}_{ipt}^{int}$) is the average manufacturing employment share among counties on the state border within CZ i (in the interior of CZ i) at time t. As before, τ_i are CZ fixed effects and ρ_{pt} are pair-year fixed effects. Standard errors are clustered at the border-segment and CZ-pair level.

The coefficient γ_1 captures the effect of a change in the minimum wage on one side of the border on the average manufacturing employment share of border counties within a CZ relative to the CZ interior, in relation to the relative manufacturing employment share of border counties on the other side of the border. A significantly negative (positive) coefficient would indicate an amplification (attenuation) effect. In the absence of spillover effects, γ_1 should not be significantly different from zero. The results are in Table 6. Column (1) uses the log deviation between the state-level and the Federal minimum wages as the policy variable. Column (2) uses the dummy variable for states with a minimum wage above the Federal level. The coefficient γ_1 is not significantly different from zero, independently of which policy variable we use. Therefore, we conclude that spillover effects are unlikely to bias our estimates.

[7]If instead we had considered the spatial difference in the manufacturing employment share between CZs on the border of a state and the average share of the CZs in the interior of the state, we would not be comparing similar areas and so could confound spillover effects with biases arising from endogeneity.

Table 6: Testing for spillovers

	(1)	(2)
Dummy for min wage above Federal level	-0.0355	
	(0.602)	
Log min wage - Log Federal min wage		-1.029
		(2.495)
Constant	0.953***	1.033***
	(0.204)	(0.223)
Observations	2112	2112

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.3 Validity of the shift-share IV

In this section, we perform further analysis with a focus on assessing the validity of the shift-share instrument of ADH (2013). Recent work by Borusyak et al. (2021), Borusyak and Hull (2020), Adao et al. (2019), and Goldsmith-Pinkham et al. (2020) has greatly increased our understanding of shift-share instruments and presented a new set of tools for assessing their validity and making correct inference. We take the approach of Borusyak et al. (2021) where the validity of the shift-share instrument rests on the exogenous nature of the industry-level shocks, as opposed to the exogenous nature of the exposure shares as in Goldsmith-Pinkham et al. (2020). Unfortunately, our setting with an interaction term between a state-level variable and the CZ-level shift-share instrument does not lend itself to the application of the framework of Borusyak et al. (2021) for asymptotically valid exposure robust inference[8]. However, we can check conditions for consistency and perform falsification and sensitivity tests.

5.3.1 Conditions for consistency

In our shift-share IV approach, we assume exogeneity of the industry-level *shocks* $\left(\frac{M_{jt}^{other}}{L_{j,t-10}}\right)$ and allow the exposure shares to be endogenous. As formalized in Borusyak et al. (2021), this set up implies that our regressions can be re-cast in equivalent terms as shock-level regressions. Therefore, conditions for consistency can be articulated in terms of the *shocks* themselves. Borusyak et al. (2021) show

[8]Building on work by Adao et al. (2019), Borusyak et al. (2021) show that traditional inference in a shift-share design may understate standard errors as it does not take into account inherent dependencies across geographies for locations that exhibit similar shock exposure. To overcome this, they perform exposure-robust inference on equivalent shock-level regressions.

that consistency follows from a law of large numbers when there are many uncorrelated shocks. For the law of large numbers to apply at the shock level, the number of observed independent shocks must grow with the sample. Furthermore, even though the approach of Borusyak et al. (2021) allows each observation to be mostly exposed to only a small number of shocks, shock exposure must be sufficiently dispersed on average (as measured by the Herfindahl index) such that no finite set of shocks asymptotically drives variation in the shift-share instrument. Under these conditions, Borusyak et al. (2021) show that the shift-share instrument is valid, even when shock exposure is endogenous.

Variability of the shocks and Herfindahl index of exposure shares Define s_{ij} to be the share of employment in CZ i in industry j and $s_j = \sum_i s_{ij}$. The many uncorrelated shocks assumption requires that the Herfindahl index (HHI) of expected exposure converges to zero, i.e. $\sum_j s_j^2 \to 0$. The Herfindahl condition implies that the number of observed industries grows with the sample (since $\sum_j s_j^2 \geq \frac{1}{N}$), and the average exposure is sufficiently dispersed across industries. In Table 7, we calculate the concentration of industry exposure with the inverse of the Herfindahl index $\left(\frac{1}{\sum_j s_j^2}\right)$, which corresponds to the effective sample size of the equivalent shock-level regressions. An equivalent condition is that the largest single exposure share is sufficiently small, so we also report the largest share in the sample. To compute the effective sample size across SIC3 groups, we further impose clustering by SIC3 industry classification. HHI is computed at the level of three-digit industry codes $\sum_c s_c^2$ where s_c aggregates exposure across industries within the same 3-digit group c. In column (1) of Table 7, we report these numbers for the raw shocks $\left(\frac{M_{jt}^{other}}{L_{j,t-10}}\right)$, and in column (2) we report the corresponding numbers for the residuals from regressing the shocks on year fixed effects with s_{jt}-weights to look at within-period shock variation. We also report the mean, standard deviation, and interquartile range of the shocks, calculated with s_{jt}-weights.

The distribution of shocks is similar to that reported by Borusyak et al. (2021) for the ADH (2013) sample, with a mean of 6.12, a standard deviation of 15.15, and an interquartile range of 4.7 in column (1). Column (2) confirms that even conditional on year fixed effects, there is sizable residual shock variation. The standard deviation and interquartile range of shock residuals are only mildly smaller than in column (1). The inverse HHI of the s_{jt} is much higher than reported in Borusyak et al. (2021) because we use annual data: 805 across industry-by-period cells and 421 when exposure is aggregated by SIC3 group. This suggests less industry-level variation is available when shocks are allowed to be serially correlated or clustered by SIC groups. The largest shock shares are also much smaller than in the ADH (2013) sample: 0.54% across industry-by-periods and 0.89% across SIC3 groups in column (1). The results are similar when we look at within-period shock variation in column (2). This suggests a sizable degree of variation at the industry level, as required for consistency.

Table 7: Assessing the variability of shocks

	Non-residualized shocks	Shocks residualized on year FE
	Descriptive statistics	
Mean	6.12	0.00
Standard deviation	15.15	15.05
Interquartile range	4.70	4.41
	Effective sample size (Inverse HHI of weights)	
Effective sample size	805	1,092
Effective sample size across SIC3 groups	421	705
	Largest share	
Largest share	0.0054	0.0051
Largest share across SIC3 groups	0.0089	0.0079
	Observation counts	
# of industry-period shocks	3,096	3,096
# of industries	394	394
# of SIC3 groups	134	134

Correlation across shocks The second part of the assumption of "many uncorrelated shocks" requires the shocks to be sufficiently mutually uncorrelated. To assess the plausibility of this assumption, we follow Borusyak et al. (2021) to analyze the correlation patterns of shocks across manufacturing industries using available industry classifications in our annual data. In particular, we compute intra-class correlation coefficients (ICCs) of shocks within different industry groups. The intra-class correlation coefficients are found by estimating a random effects model which provides a hierarchical decomposition of residual within-period shock variation.

$$\frac{M_{jt}^{other}}{L_{j,t-10}} = \mu_t + a_{ten(j),t} + b_{sic2(j),t} + c_{sic3(j),t} + d_j + e_{jt} \tag{6}$$

where $\left(\frac{M_{jt}^{other}}{L_{j,t-10}}\right)$ denotes the shocks, j denotes industry and t denotes time. μ_t are year fixed effects. $a_{ten(j),t}, b_{sic2(j),t}, c_{sic3(j),t}$ denote random effects respectively generated by the ten industry classifications of Acemoglu et al. (2016), 20 industry groups identified by SIC2 codes and 136 groups corresponding to SIC3 codes. d_j is a time-invariant industry random effect. We estimate this equation as a hierarchical linear model by maximum likelihood assuming Gaussian residual components. Specifically we estimate an unweighted mixed-effects regression, allowing time-varying ten-sector, SIC2 and SIC3 random effects and imposing an exchangeable variance matrix for these. Table 8 reports estimated ICCs from Equation 6, which summarize the residual shock variation due to each random effect. This reveals moderate clustering at the SIC3 level (with ICC of 0.157). There is less evidence for clustering of shocks at a higher SIC2 level (with ICC of 0.049) and particularly by ten cluster groups (with ICC of 0.023). This supports the assumption that shocks are mean-independent across SIC3 clusters. Based on this, we cluster at the SIC3 level when calculating the effective sample size in Table 7. The inverse HHI estimates in Table 7 indicate that at this level of shock clustering there is still an adequate effective sample size.

Table 8: Assessing the uncorrelatedness of shocks

	Estimate	**SE**
10 sectors	0.023	0.005
SIC2	0.049	0.024
SIC3	0.157	0.063
Industry	0.585	0.07
Number of industry periods	3,099	3,099

5.3.2 Falsification tests

In this section, we test the plausibility of the shock orthogonality assumption by regressing the ADH (2013) instrument on a set of controls – orthogonality should imply no significance. Table 9 shows the regression coefficient from a regression of the ADH (2013) instrument on each potential confounder, namely the share of the working-age population that is foreign born, the share of the working-age population with college education, and female labor force participation. These regressions are estimated with CZ and pair-year fixed effects, but no covariates. These are tests of quasi-exogeneity rather than conditional exogeneity. Standard errors are clustered at the CZ-pair and border-segment level. None of the coefficients are significant.

Table 9: Falsification tests

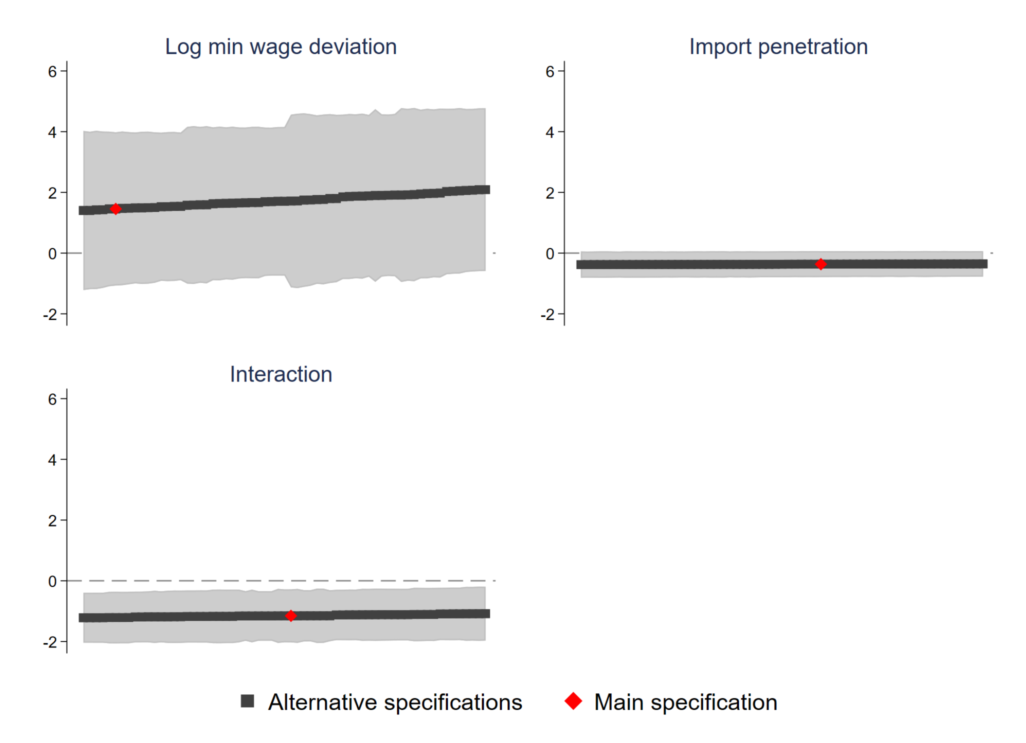
	(1)	(2)	(3)
% Foreign born	-0.340		
	(-0.65)		
% College educated		-0.154	
		(-0.61)	
Female labour force participation			-0.214
			(-1.70)
Constant	1.924***	1.784***	1.724***
	(7.54)	(42.87)	(84.37)
Observations	3672	3672	3672

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.3.3 Sensitivity analysis

Figure 5 shows, for each of the main three coefficients of interest, how they vary across different specifications of Equation 1. We construct each possible combination of the following controls and estimate one regression for each combination: RtW dummy, effective state tax rate, 5-year lagged manufacturing union membership, percentage of foreign born, percentage of college educated, and female labor force participation. We include the lagged manufacturing share in each regression. Each dot represents a coefficient from a different regression, with the corresponding 95% confidence interval. The red dot is the coefficient from our baseline regression. Regressions are estimated with CZ and pair-year fixed effects. We cluster standard errors at the CZ-pair and border-segment level. There are 64 possible unique combinations, and therefore 64 regressions reported in the figure. Figure 5 reports the coefficients for the log difference minimum wage policy variable. The coefficients are very robust across specifications. We come to a similar conclusion when using the dummy policy variable (not reported).

Figure 5: Sensitivity analysis



6 Conclusions

The opening of the Chinese economy and its subsequent dominance in world trade has been one of the most important economic developments in the late 20th and early 21st centuries. The backlash against globalization and international trade in the US (and elsewhere) finds some of its origins in the manufacturing job losses that followed China's entry into the WTO in 2001. An important question is whether labor market institutions have an impact on the dynamic response of manufacturing employment to rising import penetration. What kind of regulations worsen or mitigate the impact?

We contribute to the literature by developing a rigorous identification strategy which enables us to provide convincingly causal evidence on the interaction between rising Chinese import penetration and minimum wage policies in US local labor markets between 2000 and 2007. We develop a rigorous double-edged identification strategy to tackle the potential endogeneity of both import penetration and minimum wage policies. Specifically, we combine instrumental variables with a border identification strategy. First, we construct shift-share instrumental variables based on the contributions of Autor, Dorn, and Hanson (2013) and Pierce and Schott (2016) to address the endogeneity of import penetration. Second, we use a border identification strategy to distinguish the effects of minimum wage policies from the effects of other local labour market characteristics that are unrelated to policy. Specifically, we rely on comparing commuting zones that are contiguous to each other but located in different states with different minimum wage policies. The approach essentially considers what happens to the response of manufacturing employment to import penetration when one crosses a state border (i.e. policy border).

Our results are in line with theoretical predictions and findings from structural models. Commuting zones with minimum wages above the Federal level experienced larger losses in manufacturing employment in response to Chinese import penetration. The magnitude of the coefficient suggests that the additional causal percentage point decrease in the manufacturing employment share resulting from a one-unit increase in import penetration when the log deviation in the minimum wage is at its mean amounts to 25% of the direct effect of import competition on manufacturing employment. When our policy variable is a dummy for states with a minimum wage above the Federal level, we find that the additional causal percentage point change in the manufacturing employment share resulting from a one-unit increase in import penetration amounts to about 86% of the direct effect of import competition.

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