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The effects of import competition on worker health☆

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ABSTRACT

Occupational health is an important determinant of workers' welfare. Existing mechanisms and evidence from the international trade and occupational safety literatures combine to predict that import competition impacts work place injuries, especially at small firms that are most affected by foreign imports. We examine this prediction with novel data on injuries at US manufacturers using Chinese import growth in 1996–2007 as a shock to competition. The data show that injury rates in the competing US industries increase over the short to medium run, particularly at smaller establishments. Back-of-the-envelope calculations show that injury risk increases by 13% at the smallest establishments, the equivalent of a 1% to 2% reduction in workers' wages.

1. Introduction

Health and injury outcomes are important to workers and firms. Estimates reveal that in 2007 US firms and workers saw as many as 9 M occupational injuries and illnesses, 60,000 of which were fatal, that resulted in about $250B in costs to workers, firms and taxpayers (Leigh, 2011). Injury rates at US manufacturers are among the highest of any industry.[1] These same firms and workers also continue to see significant import competition from low cost markets, China in particular, which has important wage and employment effects. Labor standards, health and safety conditions are an important part of the employment contract, but have not been examined in the face of trade liberalizations or import competition. In this paper we ask if import competition from foreign markets affects injuries and worker health in US firms.

The link between import competition and worker injuries is supported by the intersection of evidence from the respective literatures. Import competition impacts firm survival (Pierce and Schott, forthcoming; Bloom et al., 2016; Bernard et al., 2006a, 2006b; Pavcnik, 2002), labor markets (Autor et al., 2013), and firm investments in new technology (Bustos, 2011; Ederington and McCalman, 2008). Literature on occupational safety and health (OSH) shows that injuries are determined by the relative priority the firm places on safety aside other goals like output (Zohar, 2000, 2002), technology upgrading and investments (Ruser and Butler, 2009), and labor market conditions (Probst and Brubaker, 2001). Together, the bodies of literature suggest that foreign competition will impact occupational injuries and worker welfare by affecting the firms' incentives related to output and safety. Welfare evaluations based on wages alone miss this effect of trade on workers' welfare.[2]

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[1] According to the Bureau of Labor Statistics estimates, there were 4.6 injury and illness cases per 100 workers in manufacturing in 2012, compared to 3.8 in natural resources and mining and 3.7 in construction.

[2] We do not estimate or compute general equilibrium welfare effects due to injuries. These computations will depend on whether firms face the true costs of their actions. Pouliakas and Theodossiou (2013) discuss information asymmetry, transaction costs, ineffective collective bargaining, and several other reasons why the social cost of injuries exceeds the private costs to firms and workers.

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We combine plant-level panel data on injuries and illnesses at US manufacturers from the Occupational Safety and Health Administration (OSHA) with an industry and time varying measure of Chinese import competition, adapted from recent work by Autor et al. (2013). We apply differencing, fixed-effect and instrumental variable strategies to identify the causal effect of trade liberalization and supply-driven import shocks on injury and illness rates at competing domestic plants.

The estimates show that import competition has significant consequences for worker injuries. Import competition raises injury rates for all but the largest plants. The effect is greatest for the smallest plants. Looking at 5-year log differences, the estimated elasticity of injury rates with respect to Chinese supply shocks is about 0.107 at the smallest decile of plants ($p < 0.01$) and 0.085 at the median ($p < 0.05$). Moving an industry from the 25th to the 75th percentile of Chinese import growth increases injury rates by about 12% at the smallest decile of plants in the industry and 10% at the median.

Estimates from the value of statistical life and injury literature show that the increases in injury risk resulting from Chinese import shocks are important in magnitude and are equivalent to wage decreases of approximately 0.4–1.6%.[3] For comparison, Arkolakis et al. (2012) discuss gains in real income due to trade liberalization of 1.4%. If variable trade costs are eliminated, Melitz and Redding (2015) find a welfare effect of 17%. We estimate that Chinese supply shocks in the US were responsible for between 62,000 and 90,000 injuries and illnesses annually during 2001–2007, about 7% of all cases in manufacturing, implying an annual cost to worker welfare between $2.2 and $9 billion each year.

Differencing and fixed-effect strategies mitigate the effect of unobserved plant, industry and geography specific characteristics. In addition, we tackle several identification problems. First, we consider long and short time differences to distinguish between short- and long-run effects of import competition and injuries. Second, underreporting is a recognized concern with self-reported injury data (Boone et al., 2011; Boone and van Ours, 2006). We estimate the model separately on the rates of injuries by severity and therefore susceptibility to misreporting. Third, it is difficult to identify exogenous trade shocks. In addition to instrumental variable techniques based on Autor et al., we identify import competition by adopting a liberalization in US trade policy towards China as a natural policy experiment according to Pierce and Schott (forthcoming) and examine the implications of global value chains using information in value added and intermediate input trade (Koopman et al., 2014). Finally, an assumption we maintain throughout is that small plants, those with fewer employees, are less productive and supply lower quality products (Melitz, 2003; Antoniades, 2015). Therefore, smaller firms face a greater threat of insolvency from import competition. Holmes and Stevens (2014) provide the alternative. Small plants – and especially those located close to metropolitan areas – produce specialty goods and are therefore shielded from import competition. We show that specialized plants are not driving our results. These robustness exercises also speak to regulatory differences across firms.

Our background section explains why import competition affects injuries in the short and long run and why the effect is heterogeneous across plants. For the short run, we combine results from the literatures on judgment proof firms and international trade to derive a prediction. In short, standard trade models and empirical evidence show that small, less productive firms are negatively affected by import competition and are more likely to drop out of the market (Pavcnik, 2002; Melitz, 2003; Bernard et al., 2006a, b; Pierce and Schott, forthcoming). Because firms are judgment proof, a higher shut-down probability implies that firms are less likely to be responsible for future costs like higher insurance premiums, demand penalties, and productivity losses associated with injuries that happen today. Therefore, at small firms an increase in import competition lowers the expected cost of an injury and leads these firms to operate with greater injury rates. In the long run, regulatory differences and existing equilibrium channels in the trade literature such as worker heterogeneity, technology upgrading, labor market institutions and quality differentiation are potentially associated with work place safety and affect firms across the entire size distribution. We do not have plant-level data to quantify coexisting mechanisms, but taking the measures of import competition as given we examine the effect of quality differentiation, technology upgrading, and worker heterogeneity across broad sectors and industries.

Our empirical exercise is closely related to recent studies on the labor market consequences of Chinese import competition in the US. China's exports to the US increased six-fold between 1996 and 2007, largely as a result of productivity gains associated with China's transition to a market economy and falling trade costs associated with its accession to the WTO in 2001 (Handley and Limão, 2016). This growth in foreign competition has been found to lower firm survival and overall employment in US manufacturing industries (Pierce and Schott, forthcoming) and lower income, employment, and labor force participation in local labor markets that house affected industries (Autor et al., 2013). Our empirical findings add to this literature evidence of non-pecuniary labor market effects on safety, injuries, and health. The results show that the effects of import competition on injuries are an important channel to consider for welfare overall and among workers at small plants in particular.

The rise in Chinese import exposure we study coincides with a period of decline in workplace injuries and illnesses in US manufacturing, which fell from 10.6 cases per 100 full-time workers in 1996 to 5.6 in 2007 (Bureau of Labor Statistics). This trend is due to factors such as workers demanding safer environments, firms investing more in safety, changes in technology,[4] and improvements in safety equipment (Ruser and Butler, 2009). Hummels et al. (2014) present theory that additional hours worked due to greater export opportunities leads to an increase in injuries, consistent with literature on health and safety. They find empirical support for this effect using Danish firm-level data. In contrast, we explain why firms facing import competition tradeoff safety for productivity and how this affects injury rates. This implies that total injuries may increase even if number of workers and number of hours remain fixed.

We also contribute to the literature on the interrelation between international trade and labor standards (see Brown et al., 1996; Brown, 2007). Most studies focus on the developing world.[5] Our theory and evidence show that occupational safety is also an important determinant of welfare in developed economies exposed to shocks in import competition.

The remainder of the paper is organized as follows: in Section 2 we discuss theory and regulatory background relating worker injury rates to firm survival and import competition. We describe our empirical strategy, data, and measurement in Section 3. In Section 4 we present our primary empirical results, and then we discuss the robustness of our findings and alternative explanations. Section 5 concludes.

2. Theory and regulatory background

This section motivates the empirical prediction that import competition leads especially small firms to sacrifice workplace safety. To this end, we also discuss several mechanisms and regulatory differences.

[3] Estimates for the value of nonfatal injuries vary across studies but generally range from 75% to 200% of yearly income. See Viscusi (1993) for a survey, and Hersch (1998) and Leeth and Ruser (2003) for later work.

[4] Between 1996 and 2007 US manufacturing employment fell 18.1% while real value added rose 60% as capital intensity increased 46% and output per production-worker hour increased 91% (NBER-CES Manufacturing Industry Database).

[5] In addition to wealth effects, long-run improvements in OSH can also be attributable to an increase in the relative price of labor and technological progress, both in production technology that alters workers exposure to risk and in safety technology (Ruser and Butler, 2009). Trade exposure may accordingly affect standards also through technology diffusion/spillovers from exporters and changes in factor prices, but neither channel seems a strong explanation of the effects among US workers studied here.

Workers face varying risks of being hurt on the job. Firms bear long-run costs for these occupational injuries, including workers' compensation payments, medical expenses, fines, and associated legal fees. Firms with workers' compensation insurance still face higher premiums if their plans are experience-rated. Substantial indirect costs not covered by insurance include disruptions in production, damage to capital, hiring and training costs for replacement workers, bad publicity, and productivity losses due to lower employee morale.[6]

Many of these costs are long-run in nature or persistent over time, which explains why firms at greater risk of shutting down will operate with more injuries. To illustrate this, let injury costs depreciate exponentially over time at a rate of d such that injury costs in period t are $c(t) = cd^t$, where c is a scalar. The expected total cost of an injury today is then $\mathrm{E}[C] = \int_{t=0}^{\infty} \exp(-\delta t) \exp(-rt) c(t) dt = \frac{c}{(\delta + r) d}$, where r is the firm's discount rate and δ is the exogenous probability of it shutting down in a given period. Future costs are only incurred if the firm is still operating, such that the firm's decision maker is "judgment proof".[7] An increase in the shut-down rate δ therefore lowers the expected cost of an injury because the firm is less likely to be active and responsible for its future costs. The Online Theory Appendix provides a model for firms which face a tradeoff between worker effort and workplace safety in the short run.[8] The model shows that the judgment proof firm responds to an increase in its shut-down rate by increasing worker effort at the expense of higher injury rates.[9]

How is this linked to import competition? Trade models predict that small, less productive firms are hit the hardest by import competition and potentially exit the market (Melitz, 2003). Empirically, import competition raises shut-down rates (Bernard et al., 2006a; Pierce and Schott, forthcoming; Bloom et al., 2016), especially at the least productive plants (Pavcnik, 2002; Bernard et al., 2006b).[10] In light of this evidence, our model of judgment proof firms predicts that an increase in import competition leads smaller firms to operate with greater worker injury rates.

Several features of this theory have empirical support in the existing literature. Bloom and van Reenen (2007) find that product market competition induces management adjustments to improve productivity and increase firm survival.[11] Lazear et al. (2013) find that firm's productivity gains during a recession are the result of getting more effort from their workers.[12] The judgment proof mechanism explains why firms take on excess risk because their losses are limited in the event the firm goes bankrupt (Shavell, 1984, 1986; Golbe, 1988; Gollier et al., 1997). Evidence shows that workplace safety is inversely related to the firm's operating margin and cash flow (Filer and Golbe, 2003; Cohn and Wardlaw, forthcoming).

Still, the notion that the smallest firms are most affected by import competition and that this competitive effect will raise injuries is not self-evident. Even if import competition raises shut-down rates, Beard (1990) and Larson (1996) argue that bankruptcy risk effectively subsidizes safety costs. Like realized injury costs, they are inconsequential if the firm goes bankrupt. Under this alternative hypothesis, exposure to import competition raises safety expenses and reduces injuries.

Existing mechanisms in the trade literature provide additional intuition for our empirical prediction based on long-run equilibrium adjustments to trade liberalizations. Industries that are less quality differentiated see greater output and employment declines in the face of low-wage competition (Khandelwal, 2010). Within industries, small firms are affected by import competition not only because they are less productive, but also because they produce lower quality products (Kugler and Verhoogen, 2012). Highly productive firms can escape market toughness by quality and technology upgrading (Antoniades, 2015; Bustos, 2011).[13] While imports of intermediate inputs are competition for domestic input producers, large firms benefit from sourcing foreign inputs (Kasahara and Rodrigue, 2008; Amiti and Konings, 2007; De Loecker et al., forthcoming; Goldberg et al., 2010).[14] If firms share rents with their employees in the form of better safety, then trade liberalization will improve working conditions at large firms where profits rise and deteriorate safety conditions at smaller firms (e.g. Egger and Kreickemeier, 2009; Amiti and Davis, 2012; Helpman et al., 2013). Yeaple (2005) shows that trade liberalizations affect worker compositions. If worker skill is correlated with injuries, then injury rates increase at small firms if they lose their best workers, but decrease if they fire their low-skill workers. Lastly, institutional and regulatory factors explain cross-sectional differences in injury rates across firm size and might also explain differential responses to import competition. Small firms are less likely to have experience-rated workers' compensation insurance and instead pay insurance and wage premia based on an industry average. Also, from a regulatory standpoint, small firms are less likely to be inspected and generally face lower penalties for safety violations (Mendeloff et al., 2006). We do not have plant-level data to quantify these coexisting mechanisms, but use publicly available industry-level data and estimate various alternative models to perform robustness checks and to provide intuition regarding their impact.

3. Empirical strategy

In this section we describe our microdata on worker injuries and develop an empirical model. We then combine our empirical model with existing measures of import competition and discuss the identification strategy.

3.1. Data

We employ plant-level data from the Occupational Safety and Health Administration Data Initiative (ODI) on worker injuries and illnesses at US manufacturers during 1996–2007. Each year, OSHA and participating state regulators select approximately 50,000 manufacturing establishments, or plants, from the universe of private sector plants with at least 40 employees per BLS records.[15] These data form an

[6] In the fair-wage literature, worker effort depends on how they are treated by the firm in the form of the wages they are paid. It seems reasonable that worker attitudes depend also on the safety conditions afforded by the firm.

[7] In practice, this is implicitly true for many indirect costs – e.g. higher insurance premiums are inconsequential if the firm shuts down. For direct costs, incorporation laws limit many decision-makers from financial liability following a shut down.

[8] One explanation for the tradeoff between output and safety is that the firm has constrained resources – e.g. manager's time – which it can choose to allocate to either motivating worker effort (as in Shapiro and Stiglitz (1984)) or increasing productivity or to improving safety and reducing worker injuries.

[9] The model also explains productivity gains – consistent with empirical evidence – due to an increase in worker effort, a testable hypothesis given appropriate firm-level data.

[10] Empirical evidence shows layoffs in response to import competition consistent with these models (e.g. Uysal et al., 2015).

[11] See Syverson (2011) for a review of empirical studies on intra-firm productivity gains following trade liberalizations.

[12] Their model differs from ours in that the adjustment in their model is driven by workers who exert more effort to reduce the probability of job loss when unemployment increases as job loss is more costly. To the extent that import competition similarly deteriorates the value of being currently unemployed for a manufacturing worker – who stands to remain unemployed for longer or be forced to switch to a lower-wage occupation (as found in Ebenstein et al., 2014) – workers might increase their effort and accept worse safety outcomes following import competition.

[13] Several other papers show that more productive firms may produce higher quality products, including Johnson (2012); Verhoogen (2008), and Baldwin and Harrigan (2011). Additional empirical evidence is provided in Aghion et al. (1997, 2001, 2005); Amiti and Khandelwal (2013); Bloom et al. (2016), and Iacovone (2012).

[14] Amiti and Konings (2007) give the example that a fall in the input tariff on compressors may force more competition in the compressor industry.

[15] Per the Quarterly Census of Employment and Wages, less than 0.1% of manufacturing plants in the US are government-owned and they employ less than 0.5% of all manufacturing workers.

unbalanced panel of 473,014 plant-year observations that cover 53% of all US manufacturing workers during this time.[16]

Each plant self-reports the total number of job-related injury and illness cases recorded in the year, the number of cases that required time away or restricted work or transfer to other duties, its primary industry at the SIC4 level, the average number of workers during the year, and a measure of equivalent full-time employment for the year imputed from total hours worked. We construct the injury rate P as the OSHA-defined Total Case Rate (TCR). For each establishment, TCR is computed as the number of reported injuries in the year divided by the imputed measure of employment, scaled up by 100. An increase in TCR therefore measures an increase in the rate of injuries normalized to 100 workers.[17] The data documentation notes that submission errors contribute to mismeasurement of TCR, especially for the very highest values in the data.[18] We drop outliers with an imputed TCR greater than 60, twice the highest industry average and eight times the overall average.[19] Summary statistics for these data are given in Panel A of Data Appendix Table A1.

While ODI is novel in that it contains detailed information on plant-level injuries and the yearly samples cover half of all US manufacturing workers, it does have some limitations. OSHA uses this dataset to monitor safety conditions at individual establishments, and the yearly samples are partly selected for these purposes. In addition to censuses within industry and establishment-size groups, some plants in the universe are automatically selected based on their injury record or other targeting strategies. These selection rules are not fully disclosed and vary over time. As a result, our data sample is partly biased towards high-injury plants, although we find that the correlation coefficient between average industry-year TCRs in our sample and the population estimates reported by the Bureau of Labor Statistics is 0.90.[20] We discuss the implications of the non-random sample in the context of our identification and results below. More detailed information on the injury data is provided in the Online Data Appendix.

We use trade data from two sources to construct measures of import competition. First, we use data on yearly imports from China at the SIC4 industry level provided by Autor et al. (2013) and describe in detail there.[21] Briefly, they concord product-level import data from the UN Comtrade Database to the industries that manufacture like products. We make use of their data on Chinese exports to the US and to an aggregate of eight other high-income nations.[22]

For our policy experiment based on Pierce and Schott (forthcoming) we employ US tariff data from Feenstra et al. (2002).[23] These data have both the Normal Trade Relations (NTR) tariff rates and the higher Column 2 tariff rates for eight-digit Harmonized Tariff System products. Following Pierce and Schott, we define the difference in the two rates as the NTR gap and construct a SIC4 industry-level measure by averaging the tariff differentials for all goods produced by the industry.[24] The mean NTR gap for our sample is about 0.31 and the standard deviation is 0.16. Summary statistics for the trade data are provided in Panel B of Data Appendix Table A1.

3.2. Specification

Let i denote plants, j denote industries, and t denote years. Our reduced form model, derived in the Online Theory Appendix, relates changes in injury rates P_{ij} over s-years to the change in import competition during the period, base-year employment L_{ijt}, and industry and year-specific effects:

$$\ln\left(P_{ij}\right)^{t:t+s} = \beta_0 + \beta_1 \ln\left(M_{uc,j}\right)^{t:t+s} + \beta_2 \ln\left(M_{uc,j}\right)^{t:t+s} \times \ln\left(L_{ijt}\right) + \beta_3 \ln\left(L_{ijt}\right) + \tau_j + \theta_t + \varepsilon_{ijt}, \quad (1)$$

where the change in import competition is identified as the log growth in US imports from China in industry j, $\ln(M_{uc,j})^{t:t+s}$.[25] The coefficients of interest β_1 and β_2 capture the elasticities of the shut-down rate with respect to import competition and import competition interacted with plant employment. Employment in the base year is assumed to be exogenous of the ensuing growth in import competition.

Based on the discussion in the background section we expect $\beta_1 > 0$, $\beta_2 < 0$, and $\beta_1 + \beta_2 \ln(L_{ijt}) > 0$ for the smallest plants. As small plants are hit by import competition their work place safety deteriorates. For large plants injury rates may decrease as they invest in new technology and increased profits from international markets translate into better working conditions.

Measuring import competition using the growth in Chinese imports by industry provides rich identifying variation across industries and time, but this measure is correlated with demand shocks. The coefficient estimates from an OLS regression of Eq. (1) will be biased if demand shocks also affect injury rates, and there are reasons to believe they do. Hummels et al. (2014) show that a demand shock will increase total injuries by making output more valuable. Elsewhere, studies link worker injuries to the business cycle, with mixed findings on the direction of the effects.[26] To control for this potential bias, we employ an instrumental variables strategy to exogenously identify import growth driven by supply shocks. Notably, the OLS estimates for $\beta_1 + \beta_2 \ln(L)$ in Eq. (1) are generally smaller than the IV estimates, suggesting the endogeneity would bias against our results.

[16] The full dataset covers all manufacturing industries and a select number of high-risk non-manufacturing industries (which we omit from our analysis). The scope of the data limits its viability for some studies, but makes it well-suited for our purposes. Other studies that have used the data include Mendeloff et al. (2012); Castle et al. (2009), and Neff et al. (2008).

[17] It is calculated with the imputed employment measure so that P is a measure of injury risk per unit of time worked, and therefore it is not directly affected by lengthening or shortening work schedules.

[18] We also drop the eight observations with employment above 25,000 as apparent reporting errors – none report employment above 10,000 in any preceding or following year. Our results are robust to their inclusion.

[19] In support of this, the correlation coefficient between industry-year TCRs in our sample and the BLS' population estimates, weighted by employment, is 0.06 when these observations are included and 0.90 when they are not. The results are similar when we include these 0.5% of observations and available upon request.

[20] The BLS Survey of Occupational Injury and Illness (SOII) also contains information on plant-level injuries, but the data is not publicly available. SOII is a nationally representative survey used to generate population estimates across all sectors of the economy, but it contains a much smaller sample of manufacturers than the ODI. The ODI data covers an average of 57% of all manufacturing establishments with 50 or more workers in a given year. For one, this allows us to use a within estimator to control for unobserved plant characteristics related to injury rates. Also, the size of the estimating sample is particularly advantageous given the stochastic nature of worker injuries.

[21] This bilateral industry-level trade data is publicly provided on David Dorn's website, for which we are grateful.

[22] They are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

[23] We are grateful to Feenstra et al. (2002) for making the US tariff data publicly available through John Romalis' website.

[24] We use the matching procedure in Pierce and Schott (2012) to concord HTS8 product tariffs to SIC4 industries. We are grateful to them for making their concordance available through Peter Schott's website. We first use the concordance from the Bureau of Economic Analysis and match 63% of the products. Next, we concord an HTS8 product to an SIC4 if all other products in its seven-digit "family" match to the same SIC4. We repeat the process using six-digit families and so forth, and this matches an additional 29% of products. Last, in the ordered set of HTS8 codes, we assign a product to an SIC4 if the industry preceding the gap is the same as the industry following the gap, which matches an additional 3% of products.

[25] Specifying firm outcomes as a function of industry-level import competition is consistent with related studies using firm data (e.g. Pierce and Schott, forthcoming; Bernard et al., 2006a, 2006b). We could alternatively conduct the analysis at the regional level as in Autor et al. (2013) by aggregating to a geographic measure of injuries, but this would eliminate a main source of variation – across firms – which is a part of our identification strategy. We could also specify firm-level injuries as a function of average import competition in the region. In our specification this implies that all firms of the same size within a region are affected equally. This seems unlikely because exposure to import competition and average firm size varies within regions across industries. Including geographic fixed effects (commuting zone and state-by-year) to allow for local determinants of injury rates does not significantly affect our results. This is also true when we allow time trends to vary across SIC2 industries.

[26] For example, Ruhm (2000) finds evidence that worker injuries are procyclical, while Brooker et al. (1997) and Boone et al. (2011) find the opposite.

Following Autor et al. (2013), we instrument for Chinese import growth in the US with Chinese import growth in a set of other OECD countries and estimate Eq. (1) using two-stage least squares (2SLS). In the first stage, we regress the two endogenous variables $\Delta\ln(M_{uc,j})^{t:t+s}$ and $\Delta\ln(M_{uc,j})^{t:t+s}\times\ln(L_{ijt})$ on the instruments $\Delta\ln(M_{oc,j})^{t:t+s}$ and $\Delta\ln(M_{oc,j})^{t:t+s}\times\ln(L_{ijt})$, along with base-year employment and any fixed effects included in Eq. (1), using OLS.

3.3. Identification

The main identifying assumption for the IVs is that the correlated growth across the markets is driven by changes in trade costs and China's productivity. Under these assumptions, the estimates for β_1 and β_2 from 2SLS in Eq. (1) are consistently identified.

We estimate in differences and include a rigorous set of fixed effects to ensure that no systematic information is moved into the error term that is correlated with our instrumental variables or results in endogeneity concerns for the variables that we treat as exogenous in our model. Differencing accounts for unobserved heterogeneity in plant characteristics that potentially affect injury rates, such as injury costs and regulatory differences. Industry and base-year fixed effects allow for industry-specific trends in injury rates across the full panel and for shocks that affect all establishments in a given period.

We estimate the model for $s=[1,6]$. We regard 5-year differences as our preferred specification, consistent with related work (e.g. Bloom et al., 2016; Amiti and Davis, 2012; Bernard et al., 2006a, 2006b), but we compare the estimates across intervals to speak to how these effects evolve over time. For the shortest intervals, the instruments will be weakened to the degree supply shocks are asynchronous across high-income countries. The instrument for longer differences is more forgiving of smaller delays in the shocks across markets, but we are left with less intra-industry variation and fewer observations, due to both the longitudinal limits of the panel and the increasing likelihood of plants shutting down or switching primary industries over longer time horizons.

Autor et al. (2013) list three threats to the IV validity that are also relevant for our analysis. One concern is that product demand shocks may be correlated across the US and other high-income countries, and therefore the first-stage IV results are not capturing the supply-side effect only. To check this, they apply a gravity strategy that isolates the import growth attributable to changes in trade costs and China's productivity. They find similar results and conclude that correlated demand shocks are not driving their estimates for employment and wage effects using the IV. We proceed with the same assumption for our identification of injury effects. Second, the correlation may be driven by adverse productivity shocks to US producers that are being supplanted by Chinese imports in US and other OECD markets, but China's explosive productivity growth – about 8% over this period (Brandt et al., 2012), more than double that in the US – makes it the more likely driver of its export growth across the markets. Lastly, Chinese imports may rise in both the US and other OECD countries following common technology shocks that adversely affect labor-intensive industries. However, Chinese export growth to the US during this time greatly outpaces that of other low and middle-income nations which may similarly exploit an adverse shock, suggesting that Chinese productivity shocks and falling trade costs for Chinese imports instead drive the growth.

To alleviate remaining endogeneity and measurement concerns associated with the instrumental variable technique outlined above, we also employ an alternative identification strategy based on a natural policy experiment. The US permanently reduced import tariffs on Chinese goods in 2001 when it acceded to the WTO. Following Pierce and Schott (forthcoming), we use the industry-specific tariff reductions to proxy for the changes in import competition associated with the policy shock, and we identify the effects on injuries using a difference-in-difference estimator.

The second identification concern we consider is potential measurement error. If the measurement error is not random in our estimating equations, it will bias our coefficient estimates. For example, it may be the case that workers at dying plants are more like to report injuries. In that case, part of β_2 captures misreporting. It is not clear that this is true, however. Even at dying plants, workers want to preserve a good reputation as they likely have to hunt for new jobs in the future. Nevertheless we perform a robustness check using the rate of fatalities and more serious nonfatal injuries and illnesses only – those that required time away from work, restricted work or transfer – under the premise that these incidences are difficult to hide and less susceptible to strategic misreporting. We also consider measurement issues in the independent variable. Supply-driven growth in SIC4-level imports may not perfectly capture competition shocks for firms given that foreign value added in imports is less than 100% (Hummels et al., 2001; Johnson and Noguera, 2012); further, some producers may source intermediate inputs within the same SIC4 industry such that import shocks for these firms actually represent access to cheaper inputs rather than competition for their output. We employ sector-level information on value added and intermediate input shares from Koopman et al. (2014) to examine these respective identification concerns.[27] For completeness, we also examine the effects of import competition from sources other than China.

Third, in our interpretation plant size proxies for differences in productivity and quality, which we do not observe in the data, and this explains why small plants experience the most adverse shocks from import competition (Melitz, 2003; Antoniades, 2015).[28] Findings in Holmes and Stevens (2014) suggest an alternative explanation. They show that the very smallest plants and plants located in urban areas are more specialized and therefore shielded from import competition. We perform several robustness checks that show that this is not driving our results.

Fourth, our identification is potentially biased because the yearly samples only target plants with 40 or more workers. Our sample of differences is therefore biased towards excluding plants that shrank during the period as some become ineligible for selection. Optimally, we would observe a balanced sample of all surviving plants, but that is not possible with our data source. This threatens our identification if the injury effects of import competition are different at growing plants such that our estimates are not representative of the full population. Further, this explains heterogeneity in the estimates as the selection bias is greatest for smaller plants already near the margin. This will bias our estimates towards zero if plants that grow tend to be those less affected by import competition, but we still address the issue with robustness checks that limit the sample to larger plants in the base year that are unlikely to be excluded with moderate falls in employment. This robustness exercise also helps examine the effects of regulatory differences that may advantage smaller firms.

We identify the coefficients using data on over half of the population of manufacturers,[29] so in the most limiting sense our estimates apply to this majority of plants. Nevertheless, we also limit the estimating samples in a pair of robustness checks to ensure our results are not driven solely by the subset of selected plants in the sample. First, we use only observations in industry-year cells for which the mean injury rate in the sample is within 20% of the population mean, based on BLS estimates. Second, we keep only industry-year cells for which observations in the sample account for at least half of total employment in the population per the NBER-CES Manufacturing Database. The correlation

[27] Shen and Silva (2015) also make use of this data to study the employment effects of value added trade in intermediate and final goods from China.

[28] Notably, using labor as a proxy is advantageous for regulators and policymakers who are looking to identify the plants most affected by import competition because plant size is directly observed and more widely available than is factor productivity.

[29] Annual US Census County Business Patterns data report an average of 59,489 manufacturing establishments with 50 or more employees during 1996–2007, and the ODI yearly samples cover an average of 33,842 establishments of this size.

coefficients between industry-year injury rates in the two subsamples and the population estimates according to the BLS are 0.97 and 0.91, respectively.

Next, we consider the potential bias in our main specification arising from heterogeneity in establishment discount rates. This will bias our estimates towards zero. The intuition is that plants with high discount rates already place a low value on the future and changes in shut-down rates are less relevant. Notably, this bias would seem to also work against our prediction that the effects are greatest at small plants given evidence in the literature that they are the more fiscally constrained and have higher discount rates (Beck et al., 2005). Nevertheless, we show in the Online Theory Appendix that specifying the model in levels allows us to separate discount rates from shut-down rates such that they difference out. Therefore as an alternative specification we estimate

$$P_{ij}^{t:t+s} = \beta_0 + \beta_1 \ln\left(M_{uc,j}\right)^{t:t+s} + \beta_2 \ln\left(M_{uc,j}\right)^{t:t+s} \times \ln L_{ijt} + \beta_3 \ln\left(L_{ijt}\right) + \tau_j + \theta_t + \varepsilon_{ijt}. \quad (2)$$

Estimating this model in levels has the advantage that we can include zero injury observations which we have to drop in the log-linear model. The tradeoff – as we show in Appendix – is that in this case the estimates are biased by heterogeneity in plant-specific injury costs. Therefore, we prefer model (1) over model (2).

4. Results

4.1. Baseline specification

Table 1 reports our main results for the effect of Chinese import shocks on injury rates at competing US manufacturers. The six columns give the second-stage estimates from the 2SLS regression of Eq. (1) for 1- to 6-year differences.[30] We provide the first-stage results in Table A2 of the Online Data Appendix. The IVs fail a weak instrument test in the first-stage regressions for 1-year differences, but are significantly correlated for all longer intervals.[31] For all intervals, we find that Chinese import shocks increased injury rates at the smallest US plants in the industry ($\beta_1 > 0$), and the effects were decreasing across plants in size ($\beta_2 < 0$). Under the identifying assumptions for our 2SLS strategy, these effects are the result of Chinese productivity gains and falling trade costs. The estimates for both coefficients of interest are statistically significant for all specifications save that for 1-year differences.

We estimate the marginal effect $\beta_1 + \beta_2 \times \ln(L)$ at approximately the smallest decile ($L = 40$), median ($L = 100$), and largest decile ($L = 400$) of plant employment size in the sample. These estimates are reported in Table 1 for each difference specification. For 5-year differences, we estimate that the elasticity of injury growth with respect to import shocks is about 0.107 at the smallest decile of plants ($p < 0.01$) and 0.085 at the median ($p < 0.05$). The effects are positive and significant at plants with less than 250 employees and weakly positive at all but the very largest plants in the sample.

These estimates imply that moving an industry from the first to third quartile of total Chinese import growth over 5 years increases injury rates by about 12% points at the smallest decile of plants and 10% points at the median. Alternatively, scaling the estimates by the interquartile range for the distribution of supply-driven import growth yields magnitudes half these sizes, based on estimates in Autor et al. (2013) that show supply shocks comprise 48% of total growth.

Looking across time intervals, we find that the estimated elasticities at the smallest decile of plants are small and statistically insignificant for 2 to 3 year intervals and become larger and significant after 4 to 5 years and appear to plateau there.[32] The estimates for the median plants follow a similar trend, while those for the very largest plants are not significantly different from zero for any specification. The results demonstrate that, at smaller plants in affected industries, the injury effects of import competition are persistent over 4 to 6 years.

We find that these results are robust when the estimating sample is limited to observations in industry-year cells that are the most representative of the population. These regressions exclude cells for which the sample does not cover at least 50% of total employment and for which the mean injury rate in the sample differs from the population estimate from the BLS by more than 20%. The results are robust to sensitivity checks and available upon request.

4.2. Magnitudes

Our estimates show that Chinese import shocks worsen injury rates at smaller plants in competing US industries. We use a back-of-the-envelope exercise to derive estimates for the magnitude of these effects, recognizing that they are only part of the story. Other channels like trade-induced labor reallocations and industry and local labor market spillovers likely contribute to the general equilibrium effect but lie beyond our analysis. Nonetheless, our estimates give an idea of the scale of these non-wage effects and their relative importance for worker welfare, providing a point of comparison with existing studies on the wage effects of trade.

We derive these estimates using the 5-year difference model to predict log growth in plant injury rates under both the realized growth in Chinese imports and the counterfactual of no import growth over the period, holding the other parameters of the model constant. From the employment-weighted average of the estimates, we back out the predicted yearly injury rates for all US manufacturing workers under the two scenarios.[33] The difference in a given year, scaled by US manufacturing employment, gives our estimate for the number of injuries attributable to Chinese import competition through these within-plant effects. For this exercise, we extend our estimates to the full population of manufacturers with 40 or more workers. The total effect will be smaller than these estimates to some degree if the relative effects tend to be greater at more dangerous plants – which are disproportionately represented in our data – and vice versa. In the limiting case that there are no changes at plants outside our sample, the net effects are roughly half these amounts.

These back-of-the-envelope estimates are presented in Table 2. Our estimates attribute 7.4% of all US manufacturing worker injuries and illnesses to Chinese import shocks, which increased annual morbidity by 0.51 case per 100 workers on average. Fig. 1 shows the predicted injury rates for 2007 across plant size bins and illustrates the heterogeneity in these effects. The increased morbidity at plants with 50 or fewer workers was 0.98 case per 100 workers (13% increase), while at plants with 1000 or more workers it was only 0.12 (2% increase). In aggregate, import shocks were responsible for between

[30] The R^2 statistics for the regressions are relatively small and indicate that our model explains 1–2% of the variation in injury rates within firms. This is due to our estimating a model in differences and using industry level variables to explain plant level injury rates, and it reflects that injury rates are difficult to predict.

[31] For differences of 2 or more years, the robust F-statistics for joint significance of the instruments considerably exceed 10, the benchmark for weak instruments suggested by Staiger and Stock (1997). That the F-statistics for 1-year differences fall below this value suggests the instruments in that specification are weak and the second-stage estimates may be inconsistent as a result. The weak instrument test statistics are suppressed in the remaining tables as they are quite similar to those reported in Table 1 — the first-stage equations are the same.

[32] It seems unlikely that the trends across time intervals are driven by changes in the composition of the estimating sample related to plants dropping out. The results look similar when limiting the estimating sample for each specification to only plants that survive at least 5 years past the base year (the criterion for inclusion in the 5-year difference sample). These results are available upon request.

[33] In aggregating, we first create an industry average using establishment-level effects and weighting each by plant employment, reported in the ODI data, and then we aggregate across industries and weight each by the manufacturing employment shares reported by the BLS.

Table 1
Injury effects of realized Chinese import growth (second-stage IV results).

Dependent variable: log growth in injury rates at plant $\ln(TCR)^{t:t+s}$
Import competition measure: log growth in Chinese imports in industry $\ln(M_{uc})^{t:t+s}$

	Interval duration (s)					
	1 Year	2 Years	3 Years	4 Years	5 Years	6 Years
Import competition	0.357	0.192**	0.114*	0.267***	0.193***	0.164***
	(0.234)	(0.086)	(0.061)	(0.063)	(0.064)	(0.061)
× Employment	−0.031	−0.037**	−0.023**	−0.043***	−0.024**	−0.022**
	(0.024)	(0.015)	(0.010)	(0.009)	(0.009)	(0.010)
Employment	0.026***	0.017**	−0.004	0.018*	−0.010	−0.015
	(0.007)	(0.008)	(0.009)	(0.010)	(0.012)	(0.016)
Estimated marginal effects by plant employment size						
40 Employees	0.245	0.057	0.031	0.107***	0.107***	0.083**
	(0.163)	(0.042)	(0.035)	(0.037)	(0.039)	(0.036)
100 Employees	0.217	0.024	0.010	0.067**	0.085**	0.063*
	(0.148)	(0.036)	(0.031)	(0.033)	(0.035)	(0.033)
400 Employees	0.174	−0.027	−0.021	0.007	0.052	0.033
	(0.130)	(0.035)	(0.031)	(0.031)	(0.033)	(0.033)
F-statistics for joint significance of instruments in first-stage regressions						
$\ln(M_{uc,j})^{t:t+s}$	3.88	42.62	70.91	110.55	58.64	58.94
$\ln(M_{uc,j})^{t:t+s} \times \ln L_{ijt}$	9.51	47.97	90.45	206.19	126.54	135.33
Base years (t)	1996–2006	1996–2005	1996–2004	1996–2003	1996–2002	1996–2001
Number of plant clusters	62,780	49,981	46,523	44,224	38,787	32,204
Observations	185,416	142,312	126,210	107,639	84,552	64,939
R^2	-	0.008	0.014	0.014	0.011	0.015

Notes: All regressions include a constant and year and industry (SIC4) fixed effects. Robust standard errors are clustered by plant and reported in parentheses. Employment is the natural log of the number of employees in the base year. Regressions include all plants with observations that span the respective interval, including those in states that drop out of the sample in later years and in "catch-all" SIC4 industry categories, xxx9. Omitting either or both groups does not significantly affect the results. The employment levels 40, 100, and 400 are approximately the 10th percentile, the median, and the 90th percentile for the distribution in the 5-year difference estimating sample. The distributions for the samples in the other regressions are similar. *F*-statistics for weak instrument tests are adjusted for robust standard errors.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2
Predicted injury effects for import growth specification under alternative trade environments.

Year	Manufacturing emp. (000s)	Sample injury rate	Predictions | Realized Chinese import growth		Predictions | No Chinese import growth		Difference in predictions		Share cases attributable to import growth (ppts)
			Injury rate	No. Injuries	Injury rate	No. Injuries	Injury rate	No. Injuries	
2001	16,641	6.74	6.51	1,083,329	6.07	1,010,109	0.43	73,220	6.6
2002	15,259	7.31	7.36	1,123,062	6.87	1,048,293	0.49	74,769	6.7
2003	14,509	7.41	8.57	1,243,421	7.90	1,146,211	0.67	97,210	7.8
2004	14,315	6.21	8.62	1,233,953	7.96	1,139,474	0.66	94,479	7.7
2005	14,227	5.55	6.15	874,961	5.75	818,053	0.40	56,908	6.5
2006	14,155	5.23	5.47	774,279	5.03	711,997	0.45	62,282	8.2
2007	13,879	5.46	5.70	791,103	5.22	724,484	0.48	66,619	8.4
Mean	*14,712*	*6.31*	*6.92*	*1,017,730*	*6.41*	*942,660*	*0.51*	*75,070*	*7.4*

Notes: Injury rates are calculated as the total case rate (TCR), the number of reported injuries and illnesses in a year per 100 equivalent full-time workers. Estimates are from the specification of Eq. 1 for 5-year intervals. Average rates across years are weighted by yearly manufacturing employment. Injury rates are estimated using a data sample targeting plants with at least 40 employees, but the estimates here are applied to all manufacturing employment. About 17% of manufacturing workers over this period are employed at plants of less than 40 workers, which lie outside our sample. The estimates for the number of injuries should therefore be multiplied by 0.83 to obtain the estimated effects for workers at plants covered by our sample only and the number of injuries attributable to Chinese import growth under the assumption that injury rates at out-of-sample plants are unaffected.

62,000 and 90,000 injuries, where the lower bound assumes injury rates were unchanged at the small plants that lie outside our sample and the upper bound applies estimates for the smallest plants in our sample to out-of-sample plants.

The value of statistical life literature estimates that nonfatal occupational injury and illness cost worker welfare between \$35,000 and \$100,000 in 2004 USD (Viscusi, 1993; Hersch, 1998; Leeth and Ruser, 2003).[34] Based on those values, the estimated within-plant injury effects of Chinese imports cost US manufacturing workers the equivalent of a 0.4%–1.6% reduction in wages and between \$2.2 billion and \$9 billion in aggregate each year.[35] This is in addition to direct and indirect costs borne by firms and the government.

4.3. Robustness checks

The main threat to identification is that we are not exogenously identifying supply shocks with our IV and our results are instead driven by unobserved factors related to both the import measure and the

[34] These values do not account for the documented heterogeneity in VSL related to worker income, age, gender, and other characteristics (Evans and Smith, 2006, 2010; Kniesner et al., 2010). We do not observe these in the data. The total welfare effects will differ if these injuries are disproportionately concentrated among some groups.

[35] These estimates exclude fatalities, which we do not identify. Supposing import competition explained 7% of the 392 worker fatalities in the US manufacturing industry (Census of Fatal Occupational Injuries) would imply an additional \$190 million in welfare loss based on a benchmark VSL estimates of \$7 million (Kniesner et al., 2010).

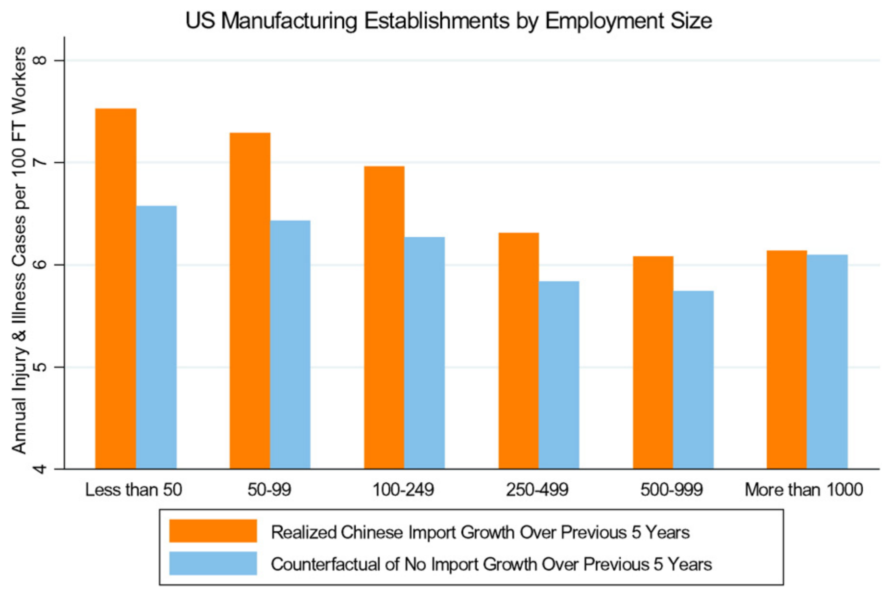

Fig. 1. Predicted injury rates in 2007 under alternative trade environments.

number of injuries, such as demand shocks. We address this concern by testing the model's predictions using a second strategy for identifying import competition based on the natural policy experiment applied by Pierce and Schott (forthcoming). The measure is grounded in trade policy and relies on a different set of exogeneity assumptions.

The US granted China Permanent Normal Trade Relations (PNTR) in 2001 when it acceded to the WTO. Previously, the US had granted Chinese imports the lower NTR tariff rates annually based on a vote of Congress.[36] The policy change eliminated uncertainty over future trade costs and encouraged Chinese firms to incur the fixed cost to export to the US (Handley and Limão, 2016). The ensuing import growth is especially large for products where the NTR gap, the difference in the NTR and non-NTR tariffs, is large. Letting $\overline{\tau}_j$ denote the (time invariant) NTR gap for industry j, we estimate the model

$$\ln(P_{ij})^{t:t+s} = \beta_0 + \beta_1 \overline{\tau}_j \times 1_{t \geq 2001} + \beta_2 \overline{\tau}_j \times 1_{t \geq 2001} \times \ln(L_{ijt}) + \gamma \ln(L_{ijt}) + \theta_t + \theta_j + \varepsilon_{ijt.} \quad (3)$$

for changes over $s = [1,5]$ years using for each the sample of observed changes on either side of PNTR, $t = \{2001 - s, 2001\}$. Industry fixed effects θ_j account for industry-specific trends in injury rates that span both pre- and post-PNTR periods, and base-year fixed effects θ_t control for the common trend across industries within each time period. If $\beta_1 > 0$, then industries with large PNTR gaps see greater increases in injury rates after 2001 on average. If $\beta_2 < 0$, then these increases are concentrated among small plants.

The results for Eq. (3) are given in Table 3. We find that permanently reducing Chinese import tariffs significantly affected injury rates at competing US manufacturers. The results support our main findings: the import shock increases injury rates at the smallest plants in the industry ($\beta_1 > 0$) and the effects are diminishing in plant size ($\beta_2 < 0$). The coefficient estimates are statistically significant for intervals of 3 or more years. We calculate the marginal effect of the NTR gap on injury rate growth at the smallest decile, median, and largest decile of plant employment and report those estimates for each specification in Table 3. The trend and the timing are consistent with outside evidence on labor adjustments following a trade shock. Pierce and Schott (forthcoming) estimate that liberalizing US tariffs on Chinese goods in 2001 caused a cumulative decline in US manufacturing employment growth over the following $s = [1,6]$ years of (in percentage points): 3, 6, 11, 13, 15, and 16.

For interpretation, we scale the marginal effects by the mean NTR gap for the sample, about 0.28. In the mean-affected industry, we estimate that the liberalization increases injury rates over the 5 years that follow by 5.8% points at the smallest decile of plants ($p < 0.01$). The effect was weakly positive at the median plant and weakly negative at the largest decile. These results provide direct evidence of changes in tariff policy affecting worker health at competing domestic plants, and are, to our knowledge, the first results to do so.

Several identification concerns related to these estimates are worth discussing. Our main identifying assumption here is that the NTR gap is exogenous to market conditions near PNTR in 2001 that may also correlate with injury rates. This assumption is grounded in the fact that the tariff rates were set long before this period and there were few changes in the decade leading up to PNTR and none to non-NTR rates, which explain 89% of the variation in the measure. In further support, Pierce and Schott show that their results are robust to using lagged tariff rates to construct the NTR gap. We follow their main specification and use 1999 tariff rates.

A related threat to identification arises because PNTR coincides with the business cycle peak in 2001. Pierce and Schott deal with the issue by comparing post-2001 changes to those that followed the previous peak in 1990, but we cannot do this because our data does not begin until 1996. It seems unlikely that our results are biased by correlation between the NTR gap and the injury effects of the business cycle across industries for a few reasons. First, industry NTR gaps are plausibly exogenous to changes in the macroeconomy during this period for reasons discussed above. Second, the literature finds that reported injury rates are procyclical, so any bias would seem to work against our finding an increase in injury rates following PNTR. Further, it is not evident that this bias explains intra-industry heterogeneity across plants.

As compared to our import growth measure, we have less intertemporal variation when identifying the model using the NTR gap. It only explains within-industry changes in injury growth before and after PNTR in 2001, and not changes within either period (e.g. 1996:1998 vs. 1998:2000) or in periods spanning the change

[36] The votes were often heavily contested. House of Representatives voted against temporary NTR status for China in 1990, 1991 and 1992, following the Tiananmen Square incident, but the Senate failed to act on those votes and the status was granted in each year.

Table 3
Injury effects of permanent import tariff reductions granted China in 2001.

Dependent variable: log growth in injury rates at plant $\ln(TCR)^{t:t+s}$.
Import competition measure: mean tariff reduction for industry's goods permanently granted China in 2001 $1(\text{post-NTR})^t \times$ NTR gap

	Interval duration (s)				
	1 Year	2 Years	3 Years	4 Years	5 Years
Import competition	−0.094	0.148	0.414**	0.634***	0.705***
	(0.141)	(0.165)	(0.173)	(0.179)	(0.190)
× Employment	0.022	−0.017	−0.067**	−0.116***	−0.122***
	(0.026)	(0.031)	(0.032)	(0.034)	(0.036)
Employment	0.000	−0.007	−0.005	0.001	−0.023***
	(0.005)	(0.007)	(0.007)	(0.007)	(0.008)
Estimated marginal effects by plant employment size					
40 Employees	−0.015	0.084	0.167**	0.205**	0.255***
	(0.066)	(0.077)	(0.079)	(0.082)	(0.084)
100 Employees	0.005	0.068	0.106	0.098	0.143**
	(0.057)	(0.068)	(0.069)	(0.072)	(0.073)
400 Employees	0.035	0.044	0.013	−0.063	−0.027
	(0.062)	(0.076)	(0.076)	(0.080)	(0.082)
Base years (t)	2000,2001	1999,2001	1998,2001	1997,2001	1996,2001
Number of plant clusters	21,468	17,953	20,601	20,672	21,553
Observations	31,781	25,205	27,194	26,114	27,496
R^2	0.010	0.017	0.017	0.019	0.018

Notes: All regressions include a constant and year and industry (SIC4) fixed effects. Robust standard errors are clustered by plant and reported in parentheses. Employment is the natural log of the number of employees in the base year. Regressions include all plants with observations that span the respective interval, including those in states that drop out of the sample in later years and in "catch-all" SIC4 industry categories, xxx9. Omitting either or both groups does not significantly affect the results. The employment levels 40, 100, and 400 are approximately the 10th percentile, the median, and the 90th percentile for the distribution in the 5-year difference estimating sample. The distributions for the samples in the other regressions are similar.
$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

(e.g. 1999:2003), which limits our estimating sample. Identification with the NTR gap holds appeal in that it directly links trade policy to worker health effects.

We next address measurement issues. First, is the concern for potential bias in our primary dependent variable due to misreporting in the injury data. We estimate Eq. (1) using the rate of fatal and serious nonfatal injuries and illnesses only. These injuries are more difficult to hide and less susceptible to strategic reporting by workers, and therefore the results are more robust to this potential bias in the left-hand variable of the equations. The results, shown in Table 4, are consistent with those from our main specification. Import competition affects worker injuries and not reporting behavior only. Second, our trade data do not distinguish inputs from final products and do not report value added. We distinguish broad sectors by value added and intermediate input information from Koopman et al. (2014) and limit the sample to observations above the 25th percentile of the final product and value added shares.[37] The results are reported in Table A3 in the Online Data Appendix and consistent with the baseline estimates. The greater coefficient magnitudes suggest that measurement errors due to value added and intermediate inputs dampen the estimates.

Third, we assume that size captures productivity and quality differences across plants in an industry. Holmes and Stevens (2014) provide an alternative explanation for why import competition affects small plants differently: producers of specialty and custom goods are likely smaller and better insulated from Chinese import competition. Holmes and Stevens discuss plants with less than 20 employees, which suggests we are largely shielded from this issue as our sample is intended to cover only plants with at least 40 employees. Less than 4% of observations in our data report 20 or fewer employees. We also limit issues relating to heterogeneity within industries by defining them according to SIC codes. Holmes and Stevens point out that the newer North American Industrial Classification System (NAICS) redefines several manufacturing industries in a way that increases the presence of custom and specialty plants relative SIC-defined industries.[38]

Still, we run several robustness checks that are presumed to limit the presence of specialty plants in the estimating sample based on Holmes and Stevens' findings. These results are presented in Table 5. In the first column we estimate our model only for plants with less than 50 employees. The results show that specialization within this size bin may be an issue as the very smallest plants are less affected by import competition than those that are slightly larger. In the second column we exclude plants with less than 100 workers in the base year to drop potentially specialized plants that are shielded from import competition. Next, Holmes and Stevens find that specialty plants tend to be located in urban areas, and so we exclude plants in more densely populated five-digit ZIP codes using data from the Census 2000 gazetteer files and present the results in the third column. In the fourth column, we exclude plants in industries with the highest shares of specialty plants based on estimates from Holmes and Stevens. For each check, the results are consistent with our main specification. Importantly, the heterogeneity across plant size persists within each of these subsamples, supporting that it is driven by productivity and quality differences and not specialization.

The estimates in column 2 of Table 5 also demonstrate that our results are not driven by bias due to the sample not covering plants where employment shrinks below the selection minimum. Less than 2% of plants with at least 100 employees in the base year report below 50 employees 5 years later, suggesting few among this group were operating but ineligible for selection because their employment was below 40.

[37] We also drop the Food, Beverage, and Tobacco sector (NACE 15 and 16), because the first stage estimates suggest that Chinese exports to the US are negatively correlated with exports to the other high-income nations in the instrument, which seems inconsistent with the underlying productivity improvements and falling trade costs that raise exports to all destinations. The results using any one or two of these stratifications bear coefficient patterns similar to those in our main specification. The only exception is the stratification using final goods only, where the results for the 5-year specification are much larger but statistically insignificant from zero, while the results for other intervals are comparable in magnitude and significance to the main results.

[38] For example, some small custom cabinet-makers are classified in the wood kitchen cabinet industry under NAICS, but these are classified as retail under SIC and excluded from our sample.

Table 4
Robustness to misreporting: severe injury effects of realized Chinese import growth (second-stage IV results).

Dependent variable: log growth in severe injury rates at plant $\ln(DART)^{t:t+s}$
Import competition measure: log growth in Chinese imports in industry $\ln(M_{uc})^{t:t+s}$

	Interval duration (s)					
	1 Year	2 Years	3 Years	4 Years	5 Years	6 Years
Import competition	0.249	0.104	0.047	0.170**	0.153**	0.166**
	(0.283)	(0.097)	(0.073)	(0.070)	(0.068)	(0.071)
× Employment	−0.008	−0.016	−0.005	−0.027***	−0.024**	−0.022*
	(0.030)	(0.017)	(0.012)	(0.010)	(0.010)	(0.011)
Employment	0.033***	0.004	−0.028***	−0.016	−0.029**	−0.039**
	(0.008)	(0.009)	(0.010)	(0.012)	(0.014)	(0.018)
Estimated marginal effects by plant employment size						
40 Employees	0.220	0.044	0.028	0.071*	0.065	0.084**
	(0.191)	(0.048)	(0.042)	(0.043)	(0.041)	(0.043)
100 Employees	0.212	0.029	0.023	0.047	0.043	0.064
	(0.172)	(0.042)	(0.039)	(0.039)	(0.037)	(0.039)
400 Employees	0.201	0.006	0.016	0.009	0.009	0.033
	(0.147)	(0.041)	(0.038)	(0.037)	(0.036)	(0.039)
Base years (t)	1996–2006	1996–2005	1996–2004	1996–2003	1996–2002	1996–2001
Number of plant clusters	57,339	45,526	41,872	39,733	34,575	28,680
Observations	170,419	130,346	113,785	96,890	75,895	58,266
R^2	–	0.009	0.011	0.014	0.013	0.018

Notes: All regressions include a constant and year and industry (SIC4) fixed effects. Robust standard errors are clustered by plant and reported in parentheses. Employment is the natural log of the number of employees in the base year. Regressions include all plants with observations that span the respective interval, including those in states that drop out of the sample in later years and in "catch-all" SIC4 industry categories, xxx9. The employment levels 40, 100, and 400 are approximately the 10th percentile, the median, and the 90th percentile for the distribution in the 5-year difference estimating sample. The distributions for the samples in the other regressions are similar.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5
Specialization robustness checks.

Dependent variable: 5-year log growth in injury rates at plant $\ln(TCR)^{t:t+5}$.
Import competition measure: 5-year log growth in Chinese imports in industry $\ln(M_{uc})^{t:t+5}$.

Dimension of selection (base-year value)	Plant employment ≤50	Plant employment ≥100	Population density (ZIP5) ≤75th percentiile	Share specialized plants in industry ≤75th percentile
Import competition	−1.035*	0.248***	0.207***	0.164***
	(0.566)	(0.088)	(0.076)	(0.058)
× Employment	0.336**	−0.031**	−0.023**	−0.021**
	(0.163)	(0.013)	(0.011)	(0.009)
Employment	−0.487**	0.002	−0.008	−0.017
	(0.224)	(0.017)	(0.014)	(0.013)
Estimated marginal effects by plant employment size				
40 Employees	0.204		0.121***	0.088***
	(0.141)		(0.047)	(0.033)
100 Employees		0.106**	0.100**	0.069**
		(0.045)	(0.042)	(0.029)
400 Employees		0.064	0.067*	0.041
		(0.040)	(0.039)	(0.029)
Base years (t)	1996–2002	1996–2002	1996–2002	1996–2002
Number of plant clusters	6527	22,659	29,546	28,531
Observations	9231	48,975	65,684	63,063
R^2	0.033	0.016	0.012	0.013

Notes: All regressions include a constant and year and industry (SIC4) fixed effects. Robust standard errors are clustered by plant and reported in parentheses. Employment is the natural log of the number of employees in the base year. Results shown are for differences over 5 years using the supply growth measure of import competition. The results for other intervals and for the tariff gap measure resemble the trends for the main specifications and are available on request. The sample for the third column excludes plants in ZIP codes with population densities above 2530 people/sq. mile. The sample for the fourth column excludes plants in industries (SIC4) with shares of specialized firms above 78%, based on estimates from Holmes and Stevens (2014). Sensitivity checks demonstrate the results are robust to the respective cutoffs used, and are available on request as well.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

We next estimate the model using the level-difference specification for injury rates given in Eq. (2). Presented in Table 6, the results support our main finding that injury rates went up at small plants hit with import competition. The trends across plant size and interval durations generally resemble those in our main results, with the exception that the estimated effects at larger plants are positive and statistically significant for longer differences in the levels specification.

For completeness we estimate the effect of import shocks from sources other than China. We follow Autor et al. (2013) and make use of their data to look at broad country groupings including low income countries, Mexico and CAFTA, and other exporters. The pattern of estimates is consistent with our baseline estimates. The one outlier, Mexico and CAFTA, is likely due to weak instruments.[39] The results are presented in Table A4 of the Data Appendix.

[39] Autor et al. (2013) document the relative weakness of the instrument for Mexico, which does not experience the great productivity gains which help identify the Chinese supply shock (Hanson, 2010). Further, the growth in Mexican exports to the US is largely explained by NAFTA (Romalis, 2007), which did not spur similar export growth to other OECD countries, in contrast to China's accession to the WTO.

Table 6
Levels specification for injury effects of realized Chinese import growth (second-stage IV results).

Dependent variable: level growth in injury rates at plant $(P)^{t:t+s}$.
Import competition measure: log growth in Chinese imports in industry $(M_{uc})^{t:t+s}$.

	Interval duration (s)					
	1 Year	2 Years	3 Years	4 Years	5 Years	6 Years
Import competition	4.950**	2.293**	1.177	3.182***	3.283***	1.968**
	(2.223)	(1.053)	(0.725)	(0.736)	(0.786)	(0.765)
× Employment	−0.606**	−0.383**	−0.159	−0.399***	−0.332***	−0.169
	(0.236)	(0.175)	(0.124)	(0.114)	(0.113)	(0.124)
Employment	0.393***	0.224**	−0.008	0.260**	0.195	−0.083
	(0.065)	(0.096)	(0.105)	(0.126)	(0.149)	(0.197)
Estimated marginal effects by plant employment size						
40 Employees	2.713*	0.881	0.590	1.712***	2.057***	1.345***
	(1.633)	(0.544)	(0.407)	(0.430)	(0.486)	(0.453)
100 Employees	2.157	0.530	0.444	1.347***	1.752***	1.190***
	(1.528)	(0.472)	(0.376)	(0.389)	(0.442)	(0.420)
400 Employees	1.317	−0.001	0.223	0.794**	1.292***	0.956**
	(1.418)	(0.459)	(0.392)	(0.375)	(0.419)	(0.425)
Base years (t)	1996–2006	1996–2005	1996–2004	1996–2003	1996–2002	1996–2001
Number of plant clusters	70,740	56,197	52,910	49,941	43,737	36,414
Observations	202,096	155,358	140,036	119,008	93,507	71,854
R^2	–	0.008	0.018	0.019	0.016	0.020

Notes: All regressions include a constant and year and industry (SIC4) fixed effects. Robust standard errors are clustered by plant and reported in parentheses. Employment is the natural log of the number of employees in the base year. Regressions include all plants with observations that span the respective interval, including those in states that drop out of the sample in later years and in "catch-all" SIC4 industry categories, xxx9. Omitting either or both groups does not significantly affect the results. The employment levels 40, 100, and 400 are approximately the 10th percentile, the median, and the 90th percentile for the distribution in the 5-year difference estimating sample. The distributions for the samples in the other regressions are similar.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.4. Coexisting mechanisms

Firms respond to import competition by upgrading product quality and investing in new technology. If quality upgrading or investments into new technology are associated with workplace safety, then these equilibrium adjustments may affect our estimates. To examine this we first split the sample by industry quality ladders (Khandelwal, 2010). Table 7 columns 1 and 2 show the results. Khandelwal argues that the quality differentiated industries are less affected by import competition. Consistent with this intuition, the results show that if we drop the most quality differentiated industries the direct effect of import competition increases and the effect is more homogenous across plants. Meanwhile, among industries with the greatest scope for quality differentiation, the marginal effects are smaller overall and statistically different from zero only for the smallest plants in the industries. This suggests that quality differentiation is not just important across industries to determine

Table 7
Specialization and quality differentiation mechanisms.

Dependent variable: 5-year log growth in injury rates at plant $\ln(TCR)^{t:t+5}$
Import competition measure: 5-year log growth in Chinese imports in industry $\ln(M_{uc})^{t:t+5}$

Sample	Quality ladder ≤75th percentile	Quality ladder >75th percentile	Quality ladder ≤75th percentile + Emp. ≥ 100	Δ K- invest./worker ≤avg.	Δ K-invest./worker ≥ avg.
Import competition	0.265**	0.198**	0.472***	0.334***	0.051
	(0.111)	(0.085)	(0.173)	(0.090)	(0.115)
× Employment	−0.018	−0.034**	−0.043**	−0.033***	−0.004
	(0.012)	(0.014)	(0.020)	(0.010)	(0.021)
Employment	−0.018	−0.007	0.018	0.003	0.022
	(0.016)	(0.020)	(0.025)	(0.014)	(0.022)
Estimated marginal effects by plant employment size					
40 Employees	0.199**	0.072*		0.212***	0.035
	(0.084)	(0.042)		(0.068)	(0.051)
100 Employees	0.183**	0.040	0.276**	0.181***	0.031
	(0.080)	(0.035)	(0.115)	(0.065)	(0.041)
400 Employees	0.158**	−0.008	0.217**	0.135**	0.025
	(0.077)	(0.033)	(0.107)	(0.062)	(0.040)
Base years (t)	1996–2002	1996–2002	1996–2002	1996–2002	1996–2002
Number of plant clusters	26,417	12,407	15,354	20,467	18,365
Observations	56,333	28,219	32,133	44,623	39,929
R^2	0.002	0.015	–	0.001	0.015

Notes: All regression samples cover base years (t) 1996–2002 and are stratified based on base-year values for the plants/industries. All regressions include a constant and year and industry (SIC4) fixed effects. Robust standard errors are clustered by plant and reported in parentheses. Results shown are for differences over 5 years. The results for other intervals generally resemble the trends for the main specifications and are available on request. The samples for the first and second columns include plants in SIC4 industries with the shortest 75% and longest 25% of quality ladders per Khandelwal (2010), respectively. The sample for the third column combines this condition with a minimum for employment size in the base year. The fourth and fifth columns split the sample into plants in industries with above and below average increases in yearly capital investments per worker between 1996 and 2007 based on data from the NBER-CES Manufacturing Industry Database. Sensitivity checks demonstrate the results are robust to the respective cutoffs used, and are available on request.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

injuries, but also seems to protect larger firms in quality differentiated industries. Column 3 drops the smallest plants, in addition to industries with the longest quartile of quality ladders, to account for potential specialization as suggested by Holmes and Stevens (2014). The effects grow in magnitude as compared to those results in Table 5 when we drop small plants but keep all industries. Specifically, the homogeneous effect is greater when we eliminate industries with high levels of quality differentiation, whereas the interaction between import competition and plant size is comparable.[40] We conclude that quality differentiation mitigates the effects across industries and likely explains part of the heterogeneous effects of import competition within industries.

Relatedly, we employ data on industry capital investments per worker from the NBER-CES Manufacturing Industry Database and split the sample into industries with below and above average growth in this measure over the relevant time period. The results are provided in columns 4 and 5 of Table 7. We find that the effect of import competition on injuries is concentrated among industries with below average growth in capital investments, where injury rates rise significantly even at the largest plants. In comparison, there are no significant effects within industries with large increases in capital investments, consistent with the intuition that firms can escape import competition by upgrading technology.

Next, we address the issue of skill heterogeneity. If firms simply lay off either high- or low-skill workers – as suggested by the evidence (Autor et al., 2013, 2014; Pierce and Schott, forthcoming) – then we would expect the total number of injuries at the plant to decrease in the face import competition unless there is a substantial increase in the injury rate.[41] To examine this we re-estimate our model with the number of injuries as the dependent variable. Appendix Table A5 shows the results. A greater increase in import competition leads to a greater number of injuries and small plants are affected the most. Second, while worker effort is notoriously difficult to observe, we can examine average work hours per employee. If firms push workers harder to improve productivity in the short run, then presumably this may also affect how long they work their labor. The results are reported in Appendix Table A6. At short time differences import competition is also associated with longer work hours especially at small plants.[42]

Finally, the robustness check on specialization where we drop the smallest plants also serves as an additional way to examine the potential effects of regulatory differences. When we drop the smallest plants the magnitudes of the coefficient estimates increase. This suggests that, if anything, regulatory differences for small firms work against our prediction.

Given our data it is impossible to quantify all coexisting mechanisms. However, through all robustness exercises, the results remain consistent with the intuition that import competition raises injury rates at small firms due to an increase in shut-down rates.

5. Conclusion

We find empirical evidence that the growth in Chinese imports in the US in the late 1990s and early 2000s significantly increased worker injury and illness rates in the competing industries in the short to medium run. We find that one significant contributor to these effects was the change in US trade policy in 2001, when import tariffs on Chinese goods were permanently reduced. The heterogeneous within-industry effects were greatest for small plants. Back-of-the-envelope estimates show that Chinese import shocks accounted for 7.4% of worker injuries and illnesses in US manufacturing during 2001–2007. Injury rates rose by 13% at the smallest plants, costing workers the equivalent of a 1–2% reduction in annual wages.

We predict that firms respond to greater shut-down risk by allocating resources towards productivity at the expense of safety and hypothesize that import competition will deteriorate worker health outcomes in the short run at marginal firms at risk of being pushed from the market during the transition to the new open economy equilibrium. Our theory and interpretation of the estimation results is consistent with recent evidence from Lazear et al. (2013) that find increases in worker effort during a recession drive firm productivity gains. However, in our data we cannot directly identify changes in effort or other firm-level factors that might affect injury rates, such as changes in technology or shifts towards employing more temporary or part-time workers to quantify coexisting mechanisms that are related to import competition and injuries. Future research matching more comprehensive firm-level data with detailed worker level data will be able to provide further evidence related to the underlying mechanisms for these effects.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jinteco.2016.06.003.

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[40] We also estimate the model for plants with less than 50 employees and excluding industries with long quality ladders. The coefficients are reversed in sign as in Table 5 Column 1 but are not statistically different from zero when we further restrict the sample. It is not clear that very small plants are insulated from import competition within less quality differentiated industries.

[41] Our data also shows that employment decreases as a result of import competition.

[42] At longer time differences the effects are not significant. This is not unexpected, because evidence shows that over longer time differences import competition actually results in layoffs (Pierce and Schott, forthcoming; Autor et al., 2013).

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