Centre for
Economic
and Financial
Research
at
New Economic
School

June 2018

The Impact of Real Exchange Rate Shocks on Manufacturing Workers: An Autopsy from the MORG

Douglas L. Campbell
Lester Lusher

Working Paper No 223

CEFIR / NES Working Paper series

The Impact of Real Exchange Rate Shocks on Manufacturing Workers: An Autopsy from the MORG†

Douglas L. Campbell
dcampbell@nes.ru
New Economic School

Lester Lusher
lrlusher@ucdavis.edu
UC Davis

June, 2018

Abstract

We study the impact of large real exchange rate shocks on workers in sectors initially more exposed to international trade using the Current Population Survey's (CPS) Merged Outgoing Rotation Group (MORG) from 1979 to 2010 combined with new annual measures of imported inputs, a proxy for offshoring. We find that in periods when US relative prices are high, and imports surge relative to exports, workers in sectors with greater initial exposure to international trade were more likely to be unemployed or exit the labor force a year later, but did not experience significant declines in wages conditional on being employed. Contrary to the usual narrative, we find negative wage effects for higher-wage, but not lower-wage workers, particularly for those who are less-educated.

JEL Classification: F10, F16, F41, N60, L60

Keywords: Real Exchange Rates, Labor Market Impact of Trade Shocks, Inequality, American Manufacturing

†. Special thanks are in order for the comments we have received from anonymous referees, at the YSI Inequality Workshop in New York, a brownbag a the New Economic School, seminars at the University of Illinois, Florida International, the Central European University in Budapest, the University of Cagliari, the Stockholm Institute of Transition Economics at the Stockholm School of Economics and the National University of Mongolia, and at the 1st MENA Trade Workshop in Tunis, the Industrial Organization and Spatial Economics Conference in St. Petersburg, the Midwest International Conference in Columbus, the FREIT conference in Slovenia, and the International Economic Conference on Economic and Social Development at the Higher School of Economics in Moscow. Thank you very much to Zalina Alborova, Nadezhda Kotova, Viacheslav Savitskiy, Yulia Zhestkova, Konstantin Butich, Elizaveta Savinova and Alexander Rubin for their excellent research assistance. We have benefitted enormously from extensive feedback from Chris Meissner, David Albouy and Peter Galbraith. All errors are our own.

As the US dollar has appreciated since the end of 2014, there is heightened interest in knowing what the impact will be on the sectors, and workers, most exposed.[1] A long line of literature, including Revenga (1992), Gourinchas (1999), Klein et al. (2003), Campa and Goldberg (2001), and Campbell (2016b) for the US has mostly found that employment in sectors more exposed to international trade is sensitive to real exchange rate (RER) appreciations.[2] Campbell (2016b) finds that value-added, investment, hours worked, and TFP also decline when the US RER appreciates, and that a temporary RER shock appears to have a surprisingly long-lasting impact. Internationally, Ekholm et al. (2012) find that oil price appreciations differentially affect firms more exposed to trade shocks in Norway.[3]

However, this literature, for the US, has focused on the sector-level impact using the Annual Survey of Manufactures. This means that questions about individual, worker-level outcomes could not be answered, particularly as the ASM only reports average wages for those employed in each sector. What happens to the wages of workers who are forced to leave the industry? Are *workers* in sectors with greater initial exposure to trade more likely to become unemployed or exit the labor force when the dollar appreciates? Do these RER-induced trade shocks impact wages and the income distribution? Are the poor and those with lower levels of education impacted relatively more?

In this paper, we seek to answer these questions, and to find out what can be learned about the impact of RER movements on individual workers using the Current Population Survey's (CPS) Merged Outgoing Rotation Group (MORG) data. We employ the same essential methodology as Klein et al. (2003) and Campbell (2016b), and test what

1. See, for example, this Econbrowser post by Menzie Chinn. Three periods of dollar appreciations as recorded by the Fed's broad trade-weighted index are visible in Figure 5. An alternative motivation is that there is considerable doubt and uncertainty over the efficacy of unconventional monetary policy. However, some of the strongest evidence for unconventional policy (and, arguably, even conventional policy) is that using high-frequency identification of announcement effects on asset prices, including exchange rates (see, for example, Rogers et al. (2014) and Glick, Leduc, et al. (2013)). Thus, the literature on the impact of exchange rates on the real economy can also help clarify the impact of unconventional monetary policy.

2. To be more precise, Revenga (1992) studies the impact of import price changes using RER changes as an instrument, and finds a significant impact, although it is sensitive to the inclusion of year FEs. Gourinchas (1999) finds a small effect, although he uses quarterly data up to just two lags. Campa and Goldberg (2001) find that "exchange rate movements do not have large effects on numbers of jobs or on hours worked", using 2-digit SIC data without controlling for a differential impact on more import-competing sectors, which thus become part of their control group. Klein et al. (2003), in our view the seminal paper in this line of research, adopts a difference-in-difference methodology for the 1980s dollar appreciation period we follow closely, and finds large employment effects confirmed by Campbell (2016b) on a longer panel with more robustness checks.

3. Other international studies include Berman et al. (2012) and Moser et al. (2010), who generally find modest impacts for Europe, while Dai and Xu (2015) argues for small effects for China (and none for import-competing sectors).

happens to workers in sectors initially more exposed to trade when US RERs appreciate, and imports surge relative to exports. Our focus on the US is convenient from a research design perspective, as there were two periods of sharp RER appreciations, which were also associated with large structural trade deficits. (Figure 1 shows the correlation between a measure of the RER, weighted average relative unit labor costs (WARULC), and the ratio of import penetration to the export share of shipments.) The most clear-cut case from a research design perspective for the US was the 1980s, when US unit labor costs increased 50% from 1979 to 1985 relative to trading partners, driven by an appreciation in the nominal value of the dollar. Large fiscal deficits are thought to be a major factor in this appreciation, which were the result of the election of Ronald Reagan and plausibly exogenous from the perspective of initially more open manufacturing sectors. The sector-level literature has demonstrated that these sectors experienced large contractions in terms of both output and employment. Our strategy is then to control for a multitude of third factors which may have caused these contractions, such as sectoral changes in demand and productivity, tariffs, real interest rates interacted with sectoral investment shares and openness, and several controls associated with the rise of China and China's WTO accession.

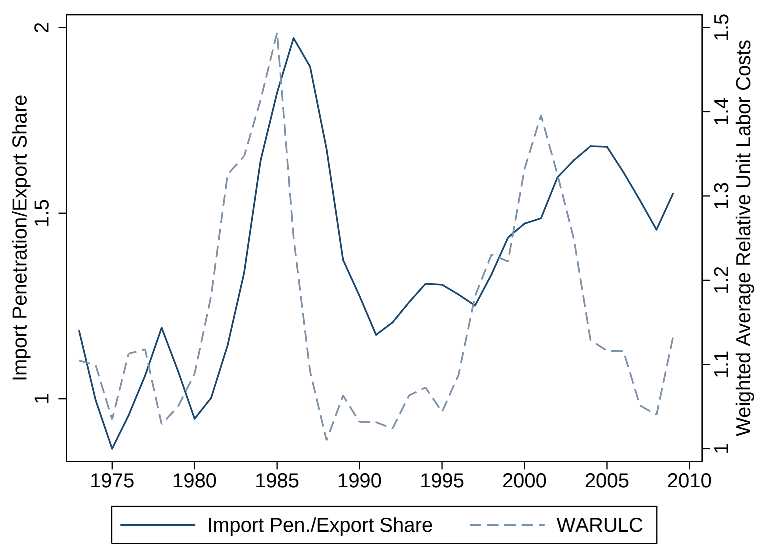


Figure 1: Two Adverse Trade/RER Shocks

Notes: WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate developed by Campbell (2016a). Import penetration = Imports/(Imports+Shipments-Exports), and Export Share = Exports/Shipments. Data come from the ASM from the Census Bureau, and trade data from WITS (World Bank).

We find that workers more exposed to trade shocks are less likely to be employed, and more likely to be unemployed or out of the labor force one year later. For workers overall, we do not find an impact on wages conditional on being employed, nor for more- or less-educated workers. Surprisingly, we find a negative effect on wages for higher-wage workers, but not for middle- or lower-wage workers. This finding poses a puzzle for those who believe that trade shocks are a major factor in the rise in measured US wage inequality. If so, then surely the two largest trade shocks in post-war US economic history should have had a large differential impact on lower-wage workers. We see scant evidence in support of this thesis in CPS MORG data.[4]

We then reconcile the ostensibly conflicting results that rich workers did worse even though college-educated workers did not by showing that poorly-educated, but high-wage manufacturing workers in exposed sectors appear to suffer very badly when relative prices are high. Lastly, following the lead of Feenstra and Hanson (1999), we create new annual measures of imported intermediate inputs – often used as a proxy for "offshoring" – for 517 SIC sectors from 1979 to 2002, and for 302 NAICS sectors from 1997 to 2010.[5] Similar to Campbell (2016b), we find that workers in sectors with larger initial shares of imported inputs do not appear to be adversely affected by RER shocks.[6]

Our results strengthen the findings in the line of literature running from Revenga, 1992 to Campbell (2016b) by showing that the apparent impact of RERs on outcomes is not limited to the ASM data, but is also readily apparent in CPS MORG data, collected using different methods, even if the results are more robust using ASM data. In addition, our findings here add to the continuing debate on the cause of the rise in inequality, and suggest that at least the direct impact of these two RER shocks appears not to have had a large impact on the distribution of wage income. This contradicts the findings of many, if not most, trade theory models from Heckscher-Ohlin onward, which tend to imply that the wage distribution should be sensitive to trade shocks or liberalization, even if the empirical literature on trade and inequality has been decidedly mixed.[7]

4. See Figure 6. This finding is confirmed by two other kinds of evidence presented in a previous version of this paper, Campbell and Lusher (2016). RER appreciations are not associated with changes in the ratio of non-production worker to production worker wages, or to labor's share of income, using ASM data. In addition, trade shocks (RERs or trade with China) are not associated with the top 1% share of income (or persistently with the bottom 90%).

5. We have made these new measures of offshoring, and the raw data, publicly available at http://dougcampbell.weebly.com/.

6. Note that neither Ekholm et al. (2012) nor Campa and Goldberg (2001) find that sectors with more intermediate inputs are differentially impacted by RER movements either.

7. Recent papers which have argued for a link between trade and inequality include Feenstra (2007), Kaplan and Rauh (2010), Lawrence (2008), Haskel et al. (2012), Goldberg and Pavcnik (2007), Jaumotte et al. (2013), and Helpman et al. (2012). On the other hand, the early trade literature, including papers by Krugman and Lawrence (1993), Leamer (1994), and Feenstra and Hanson (1999), mostly concluded that trade was not a primary cause of the rise of inequality since 1980. A comprehensive survey of

The rest of the paper proceeds as follows. First, we describe our data collection efforts for the MORG individual-level data and the new sector-level intermediate input data, and then describe our identification strategy and present our empirical results.

1 Data, Motivation, and Identification

1.1 Data

We use data on individual workers from the Bureau of Labor Statistic's Current Population Survey (CPS) Annual Earnings File, also known as the Merged Outgoing Rotation Group (MORG), following Ebenstein et al. (2014) and Ebenstein et al. (2015). The attractive feature of this data is that workers are interviewed in consecutive years, allowing one to follow the labor market outcomes of individual workers exposed to trade shocks. This is helpful, as the key shortcoming in an alternative (and not publicly available) dataset, the Master Earnings File from the Social Security Administration, is the omission of employment status, making it virtually impossible for studies, such as Autor et al. (2014), to determine whether the decline in total earnings for workers more exposed to trade shocks (in this case China) were due to spells of non-employment or to lower wages.

There are also several challenges with the MORG data. First, the sectoral classifications change over time, as the SIC classification is used until 2002 (and it, as well, changes in 1983), and the NAICS system from 2003. Recognizing that we only have a pseudo-panel in any case (with variables measured in year-to-year changes, each individual shows up once in our data), we matched various sectoral manufacturing data using SIC data for the period until 2002, and NAICS data for the period after.[8] In the regressions using this panel setup, we simply use separate sectoral fixed effects before and after the change in classification. Another challenge is that in several years, such as 1984, 1985, 1994 and 1995, not all of the workers can be matched. Since there happened to have been a RER shock in 1984 and 1985 and a corresponding increase in the trade

much of the early literature by Cline (1997) concluded that trade was responsible for 20% of the rise in wage inequality (which still sounds substantial to us). Note that Piketty et al. (2014) have shown that the rise in inequality in the US is mostly a story of the top 1%, and Piketty *et al.* 2014 have found that this is already well explained by top marginal tax rates, confirmed by both Roine et al. (2009) and also Campbell and Lusher (2016), the latter two which also show that various kinds of trade flows or imbalances are not correlated with top income shares.

8. One might then ask how we handled sectoral variables measured in changes from 2002 to 2003. The answer is that these variables come from the ASM, for which we have overlapping data using both classifications.

deficit, this likely has weakened our results. A third annoyance is that workers for some sectors are (inconsistently) top-coded. The good news is that this applies to very few workers in our sample (much less than 1%), but on the other hand it means that this data cannot really be used to study incomes at the very top of the distribution, which others have suggested may have been undersampled in any case. Lastly, we only have many sector-level variables (such as trade, productivity, and output) for manufacturing sectors, which are arguably the most comparable control group to manufacturing workers in sectors that compete internationally. However, we also include services workers as a control group in regressions when we divide the sample to look at different kinds of workers (*i.e.*, those without college educations).

The sectoral data we match includes data from the Annual Survey of Manufactures provided by the Census Bureau, and trade data from the World Bank (WITS). Sectoral tariff data come from Schott (2008) via Feenstra et al. (2002). In addition, we control for several other trade-related shocks. The first is the interaction between China's accession to permanent normal trade relations (PNTR) interacted with a control for the NTR gap by sector (the measure designed by Pierce and Schott (2016)). The second is a measure of exposure to the expiration of Chinese textile quotas. As many authors, including Brambilla et al. (2010) and Khandelwal et al. (2013) have highlighted, the end of Chinese textile quotas associated with the end of the Agreement on Textile and Clothing (ATC) (the successor to the Multifiber Arrangement, MFA), had a large impact on the sectors in which the quotas were binding. Thus, we have also included a control for the weighted average of the fill rate – the imports divided by the allowable quotas – just before the quotas were lifted (also following Pierce and Schott (2016)).[9]

We provide a data of summary statistics in Table 5. Interestingly, while the most open manufacturing sectors look like other sectors in terms of wages and education, this becomes less true by the end of the sample, when workers in more open sectors had higher wages and were actually better-educated. We also plot the evolution of wages for different percentiles of the income distribution relative to median income in our sample of manufacturing workers from the MORG in Figure 6.

We follow Feenstra and Hanson (1999) in creating new measures of imported intermediate inputs, often used as a proxy for offshoring, for both NAICS and SIC sectors. For NAICS, we use the imported input estimates provided by the BEA for the benchmark years 1997, 2002, and 2007, and then extrapolate for the intervening years based

9. *I.e.*, the sectors affected by the end of quotas interacted with a post-2005 dummy using data from Brambilla et al. (2010) on the pre-2005 fill rates of the Chinese quotas (converting the HS codes to SIC using the concordance provided by J. R. Pierce and P. K. Schott (2012).

on sectoral changes in materials usage for the using sectors, and imports for the commodity sectors. For the SIC, we worked with the IO Use table provided by the BEA for the benchmark years 1972, 1977, 1982, 1987, and 1992, and then followed Feenstra 1999 by employing a "proportionality" assumption, *i.e.*, that the share of intermediates which are imported is simply the ratio of imports to domestic consumption. We provide a detailed description of the construction of these indices in the online appendix.

The chief measure of the real exchange rate used in this paper is the Weighted-Average Relative Unit Labor Cost (WARULC) index, introduced by Campbell (2016a) to address an index numbers problem which afflicts the RULC indexes created by the IMF (which do not include China or other developing countries), and which also affects other commonly used RER indices such as those created by the Federal Reserve.[10] We use this as trade economists have generally considered RER measures based on relative unit labor costs as being the best measures of competitiveness, while both Thomas et al. (2008) and Campbell (2016a) show that the class of weighted average relative (WAR) indices outperform traditional indices in predicting trade balances, both in and out of sample. However, our results are robust to using other measures of the RER which also address the index numbers problem, such as Penn-adjusted Weighted Average Relative Prices, also provided by Campbell (2016a), or even the Federal Reserve's Broad Trade-Weighted RER Index. As the results appear to be robust to the choice of index, we refer readers interested in the details of these indices to Section 2.1 of our unpublished appendix, which includes a direct comparison of the different indices (Figure 2), and to Campbell 2016a.

1.2 Theoretical Motivation

The theoretical rationale for this paper is intuitive: that RER movements will disproportionately affect workers in more open sectors, who may then be more likely to become

10. According to Campbell 2016a (previously circulated as Campbell 2014), the four key problems with the IMF's index are that it (1) is computed as an index-of-indices, and thus does not reflect compositional changes in trade toward countries that have lower unit labor costs, (2) does not include China, (3) uses fixed trade weights, which have become outdated, and (4) uses country-specific deflators, which can become biased over time without the benefit of multiple benchmarks (this is the same problem that afflicted previous versions of the Penn World Tables). WARULC addresses all four of these problems explicitly, and so it is the key measure of the RER used in this paper. The WARULC index is computed as $I_{US,t}^{WARULC} = \prod_{i=1} \left(\frac{ULC_{US,t}}{ULC_{i,t}}\right)^{\Omega_{i,t}}$, where $ULC_{i,t} = \frac{w_{i,t}}{e_{i,t}} / \frac{Y_{i,t}}{PPP_{i,t}}$, $\Omega_{i,t}$ are time-varying trade weights (a weighted average of import, export, and third-country competition weights, the same as used by the BIS and very similar to the Fed's weights), and where $w_{i,t}$ are manufacturing wages of country i at time t, $e_{i,t}$ is the local currency price of a dollar, and $Y_{i,t}$ is manufacturing production, converted to dollars at PPP (which equals one for the US). One of the key differences with the IMF's index is that for this index the ULCs are actual unit labor costs rather than indices of unit labor costs.

unemployed or experience relative declines in their earnings. Thus, we follow Autor et al. (2014) in taking a reduced form approach with just a brief verbal discussion of our (intuitive) theoretical motivation.

The theoretical model we have in mind is a Specific Factors model in which capital is non-mobile across industries and labor is mobile in the long run, but imperfectly mobile in the short run. Further assume that there are two sectors, one tradable and one non-tradable. With sticky wages, arising from, for example, workers displeasure at taking pay cuts, and labor search costs, an appreciation of the nominal exchange rate will result in a reduction in demand for workers in the more tradable sector. Instead of reducing wages, firms will react to this reduction in competitiveness by laying off workers rather than by cutting salaries (or will go bankrupt). Workers in the tradable sector will then look for jobs, including in the non-tradable sector, but due to search frictions and their sector-specific capital, may spend time unemployed or accept lower wages. Of course, whether they become unemployed or experience declines in wages is an empirical question.

1.3 Identification Approach

The basic difference-in-difference approach in this paper is to compare the plight of manufacturing workers in sectors which are more exposed to international trade vs. those who work in sectors which are less exposed when US relative prices (the real exchange rate) appreciates vs. times when US relative prices are close to fundamentals. In the US, both large RER shocks were accompanied by correspondingly large structural trade deficits. To some extent, our identification method does not necessarily rely on the source of these deficits being RER shocks – clearly there was a trade shock of some kind in these periods (see Figure 1). In any case, as argued by Campbell (2016b), theory and intuition effectively rule out reverse-causality as a major concern in this case. It is simply implausible that a decline in manufacturing employment or a trade deficit would cause a currency to *appreciate* – on the contrary, we should expect it to cause a currency to depreciate. In addition, RERs have been observed to impact both trade and employment with a lag. This lag is a second factor which mitigates against reverse causality, although it certainly does not prevent third-factor causality. Thus, as in Campbell (2016b), our strategy is to implement a "repeated" difference-in-difference approach, and ask what happens to workers in sectors initially more exposed to trade when relative prices appreciate, while controlling for other potential third factors.

To determine which sectors are more exposed, we compute a measure of openness which is a weighted average of import penetration and export share, lagged over a number of years. Thus, our measure of openness is:

$$Openness_t \equiv \frac{M_t}{M_t + X_t} * \frac{M_t}{M_t + S_t - X_t} + \frac{X_t}{M_t + X_t} * \frac{X_t}{S_t}, \tag{1.1}$$

where S_t are shipments at time t, M_t are imports, X_t are exports, and openness was computed for each manufacturing sector separately (there are a maximum of 87 manufacturing sectors in the MORG).

2 Empirical Results

2.1 Annual Cross-Sectional Results

Our first exercise is simply to regress a dummy variable indicating that a worker is employed one year later on this measure of openness, while controlling for sectoral demand and productivity growth, running the following regression on repeated cross-sections of the data:

$$E_{ih,t+1} = \alpha_t + \beta_0 Openess_{ht} + \beta_1 \Delta ln D_{h,t+1} + \beta_3 \Delta ln Prod._{h,t+1} + \epsilon_{h,t}, \tag{2.1}$$

$$\forall h = 1, ..., 87, \ t = 1979, ..., 2010,$$

where $E_{ih,t+1}$ is a dummy for employment status one year later, $D_{h,t+1}$ is "demand", or domestic consumption, defined as total shipments (from the ASM) plus exports minus imports (using WITS data), and *Prod.* is labor productivity, defined here as value-added divided by the number of production workers (also from the ASM). We plot the coefficients on openness each year with two standard deviation error bounds vs. weighted average relative unit labor costs (WARULC), a measure of the RER, in Figure 2. The magnitude of -.2 around 1985 suggests that as you move from the 25th percentile of openess (.055) to the 90th percentile (.272) you increase the probability of non-employment by 4.3% (=-.2*(.272-.055)). In 2001, the coefficient was just -.12, although openness had increased by that point to .067 for sectors in the 25th percentile, and to .44 for sectors in the 90th percentile, meaning that the increase in probability of non-employment as you move from the 25th to 90th percentile actually increased in magnitude to 4.6%.

While workers in more open sectors were generally no more likely to be unemployed than workers in other sectors (Figure 2), two exceptions were the mid 1980s and the early 2000s, when US relative unit labor costs were much higher than that of US trading

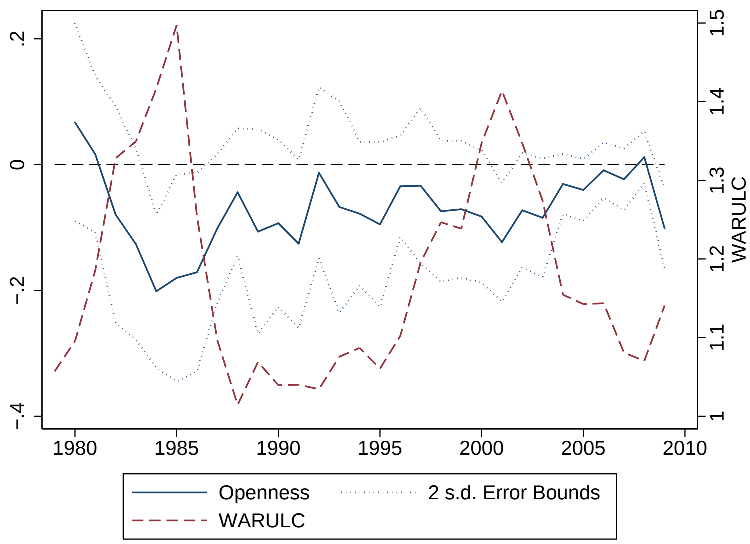


Figure 2: RER Shocks, Openness, and Employment

We have plotted the annual coefficients on "openness" from annual regressions of an employment indicator (one year later) at the individual level on openness (measured at the sector level), with controls for log changes in demand and productivity, with standard errors clustered at the sector level. Notes: WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate developed by Campbell (2016a).

partners (note that the value of 1 indicates parity, and a value of 1.5 indicates that RULCs are 50% higher in the US than in a weighted average of trading partners). When we use unemployment or not-in-the-labor-force (NILF) instead (Figure 7), we get roughly similar results. In Figure 7 Panel (c) and (d), we show that the results appear to be a bit stronger using the ASM (reprinted with permission from Campbell (2016b)) than from the MORG, particularly for the late 1990s and early 2000s period. One of the big differences is that workers in more open sectors seemed a bit more likely not to be employed one year later even during the early 1990s, although the impact was never statistically significant.

2.2 Description of Panel Approach

Next we adopt a panel difference-in-difference approach, using all of our data, to see if the impact in more open sectors in high RER years is really statistically different from the lower RER years. Our benchmark regression is:

$$\Delta lnW_{ih,t+1} = \alpha_{t+1} + \beta_0 L.3\text{-}7yr.Open._{ht} + \beta_1 ln(RER_t) * L.3\text{-}7yr.Open._{ht} + \tag{2.2}$$

$$\beta_2 \Delta lnD_{h,t+1} + \beta_3 \Delta lnTFP_{h,t+1} + \textstyle\sum_{i=4}^{n} \beta_i C_{i,t+1} + \alpha_h + \nu_t + \epsilon_{ht},$$

$$\forall h = 1, ..., 87, \ t = 1979, ..., 2010,$$

where $\Delta lnW_{ih,t+1}$ is the log change in wages for individual i in sector h from time t to t+1 (or replaced by another dependent variable, such as indicators for employment, unemployment, not-in-the-labor force, or for overtime work one year later), $ln(RER_t))$ is a measure of the real exchange rate, $D_{h,t}$ is domestic sectoral demand (defined as shipments plus imports minus exports), $TFP_{h,t}$ is a measure of sectoral productivity, $C_{i,t}$ are various other controls, while α_h and ν_t are sectoral and year fixed effects.

$L.3\text{-}7yr.Open._{ht}$ is an average of openness (defined as in equation 1.1) at 3, 4, 5, 6 and 7 year lags:

$$L.3\text{-}7yr.Openness_t \equiv (1/5) \sum_{k=3}^{7} Openness_{t-k}. \tag{2.3}$$

This ensures that the time series change in the interaction term will be driven primarily by movements in the exchange rate, with minimal feedback from the RER back into openness. While Campbell (2016b) showed that the results of a similar exercise using the ASM are robust to using a fixed measure of openness, the rationale for using a (slowly) evolving measure is that, given our long panel, many things changed over the period and some sectors became substantially more open. Any sector's exposure to RER movements should depend on its current exposure rather than what its exposure happened to have been in the 1970s.

Note that when we have the log change in wages on the left-hand side of equation 2.7, we also conservatively control for the initial level of wages on the right-hand side. Our results tend to get stronger without this control. The errors are clustered by sector and year. Since this is a pseudo-panel, in which individuals appear in the sample only once (as our interest is in changes in variables over consecutive years), including individual-level fixed effects is not possible. To create a 31-year panel, we combined data from the 1979-1982 period using the IND70 SIC classification, with data from the 1983-2002 period using the IND80 SIC classification, and data for the 2003-2010 period which uses NAICS. We then include industry fixed effects for each of the NAICS and SIC industries separately. Arguably, when running a panel this long, including sectoral*decade interactive fixed effects may be advisable, although we include such effects out of necessity. However, our results are similar if we limit our sample to the 1979 to 2002 period for which the classification changes are more minor (see Online Appendix). The downside of including sectoral FEs for the 2003-2010 period separately is that the rise of China happened in large part after its December 2001 accession to the WTO, so controlling for sectoral FEs will largely soak up the China shock. This is not a problem for us, as our focus is instead on the impact of large RER movements, but it poses a challenge for those who might like to use the MORG to study the impact of the rise of China.

One of the nice features of this dataset is we can also test the effects on different subsamples of individuals. Were the impacts larger for less-educated workers? Or for low-wage workers? When we run the panel regression on subsamples, we also include services workers in the control group, and ask, relative to the same category of workers in services or manufacturing, what is the impact of being in a more open sector when the RER shock hits? The results are similar whether or not services workers are included in the control group (see the Appendix for additional results).

2.3 Panel Regression Results

Our main results for the labor market impact of RER movements on workers in the most-exposed sectors are presented in Table 1. We find that when US relative unit labor costs appreciate relative to trading partners, workers who began the period working in sectors which were initially more exposed to trade do not experience any change in their wages conditional on being employed, measured hourly in column 1 of Panel A, or measured weekly in column 2. They do, however, experience a decline in the probability of being employed in the subsequent period (column 3), an increase in the probability of being unemployed (column 4), and in the probability of leaving the labor force (column 5), although this effect is only marginally significant and suspicious given the opposite sign and significance on lagged openness.[11] There is also a decline in the number of workers in these sectors who work overtime (column 6), although, once again, the opposite sign on lagged openness makes interpretation of this result less than straightforward.[12] We have found that the results for overtime are not consistent across all other specifications we have tried (some of which are in the appendix). Employment and unemployment are generally the most robust, followed by NILF.

In the employment regression, the coefficient of -.2 on the interactive variable L.3-7yr.Avg.Openness*ln(RER) implies that in 2001, when US RULCs were 40% higher than those of trading partners, a worker in a sector with average lagged openness at the 90th

11. Note that it is not feasible to do a Heckman selection model instead in this case, as we do not have any variables which predict employment but not log changes in wages, which is a necessary condition for using the Heckman method.

12. Note that there is no way to balance the sample across dependent variables, since the log change in wages can only be computed for workers who were still working a year later, so that the "Employed", "Unemployed", and "Not-in-the-Labor-Force" will naturally have more observations. The overtime indicator variable is also only computed for those who are employed.

percentile of .33, roughly .3 higher than a sector in the 10th percentile, would have been 2% less likely to have a job a year later (=-.2*.3*ln(1.4)) relative to the sector in the 10th percentile compared to years in which RULCs were at parity. Over the period 1997 to 2004, a worker who began in this sector would have had a cumulative probability of becoming non-employed of 11%. A worker in a sector with an openness of .53 – thus one of the most open sectors in the economy – by contrast, would have been about 3.6% more likely to not have been employed when surveyed again in 2002. To put these numbers in perspective, note that a worker in a sector with labor productivity growth of 40% in 2001, the fastest growth that year, would have resulted in a reduction in the likelihood of being employed the following year of 2.4% (=.4*-.06) vs. a sector with no productivity growth (roughly the 5th percentile).

Breaking down the impact by college education (Panels B and C), and wages (D, E, and F), which include workers in services sectors as part of the control group, we do find slightly stronger results on employment for those with no college education than for workers with at least some college, although the difference is not statistically significant. For workers with no college education in Panel B, we do find a significant negative impact on weekly wages, in addition to a slightly larger impact on on employment (although the difference with those workers with some college is not quite significant).

While the impact by levels of education is suggestive of differential impacts for less-educated workers, when we test the impact on workers sorted by levels of wages, the picture appears more complicated. Workers in the top third actually do experience significant declines in both hourly and weekly wages, while workers in the bottom two-thirds do not. However, the coefficient on employment is similar for all three groups, albeit only borderline significant. Notably, if we exclude services workers, the results on employment are similar, but the wage impacts are slightly different. For example, workers without college education experience a significant decline in weekly wages when we don't include service workers. That our results for wages seem to depend a bit on the exact specification is probably a reason to read these results with caution.

However, interestingly, we do not see similar results when we look instead at workers with high vs. low wages. Here, we find that workers in the top third of wages experience significant declines in both hourly and weekly wages, while the coefficients on employment are similar. Workers in the bottom third experience an increase in hourly wages, significant at 95%, although this result is not robust across specifications (such as when we drop services workers).

Table 1: The Impact of Real Exchange Rate Shocks on the Labor Market

	$\Delta\, ln$ HW	$\Delta\, ln$ WW	Employed	Unem.	NILF	Δ Over.
A. Manufacturing Only						
L.3-7yr.Open.*ln(RER)	0.013	-0.030	-0.20***	0.088**	0.12**	-0.24**
	(0.054)	(0.075)	(0.061)	(0.035)	(0.046)	(0.11)
L.3-7yr.Avg.Openness	-0.024	-0.0060	0.040	0.031	-0.071***	0.12**
	(0.038)	(0.050)	(0.039)	(0.027)	(0.024)	(0.051)
$\Delta\, ln$ Demand	0.012	0.046**	0.077***	-0.071***	-0.0066	0.023
	(0.018)	(0.021)	(0.022)	(0.018)	(0.011)	(0.028)
$\Delta\, ln$ VA/Prod. Worker	-0.0043	-0.0018	-0.060***	0.027***	0.033**	0.047**
	(0.013)	(0.015)	(0.019)	(0.0093)	(0.014)	(0.023)
Observations	252653	256337	355608	355608	355608	154531
B. No College Education						
L.3-7yr.Open.*ln(RER)	-0.094	-0.14	-0.25***	0.15**	0.099*	-0.46**
	(0.059)	(0.095)	(0.075)	(0.062)	(0.057)	(0.20)
Observations	621450	630621	1020665	1020665	1020665	426688
C. At least some College						
L.3-7yr.Open.*ln(RER)	-0.0023	0.0057	-0.14**	0.10***	0.041	-0.55
	(0.10)	(0.17)	(0.057)	(0.037)	(0.054)	(0.38)
Observations	730130	753920	1084615	1084615	1084615	272032
D. Top Third of Wages						
L.3-7yr.Open.*ln(RER)	-0.24***	-0.29***	-0.13*	0.11***	0.018	-0.62*
	(0.080)	(0.082)	(0.075)	(0.041)	(0.037)	(0.33)
Observations	467229	473557	519800	519800	519800	147153
E. Middle Third of Wages						
L.3-7yr.Open.*ln(RER)	0.028	0.015	-0.14**	0.14**	-0.0040	-0.49**
	(0.089)	(0.10)	(0.063)	(0.061)	(0.041)	(0.23)
Observations	463491	467767	521468	521468	521468	236952
F. Bottom Third of Wages						
L.3-7yr.Open.*ln(RER)	0.26**	0.26	-0.16**	0.091**	0.069	-0.13
	(0.13)	(0.21)	(0.071)	(0.044)	(0.057)	(0.17)
Observations	420860	424040	518529	518529	518529	300355

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. Each regression includes industry and year FEs over the period 1979-2010. Panel A is manufacturing only, the other panels include workers in services as part of the control group. The dependent variables are: (1) the Hourly Wage, (2) the Weekly Wage, (3) Employment (binary), (4) Unemployment (binary), (5) Not in the Labor Force (binary), (6) Change in overtime. The key variable of interest is the interaction between lagged 3-7 year average openness (L.3-7yr.Avg.Open.) and the log of the RER. WARULC = Weighted Average Relative Unit Labor Costs is the measure of the real exchange rate used here.

We reconcile these seemingly conflicting results in Figure 3, when we break the "No College" sample further by those with the highest, middling, and lowest wages, and plot the coefficient on the key interactive term with two standard deviation error bounds. Here we see that poorly-educated workers who had nevertheless managed to get high-wage manufacturing jobs (those in the top third of wages are in light blue) did very poorly after being hit by trade shocks. Conditional on being employed, they experienced very large declines in wages, with a coefficient of -.44 on hourly wages. This translates to a differential decline in wages of 4.6% for workers in the 90th vs. 10th percentile of openness in 2002, which admittedly sounds suspiciously large. Note that if we exclude services workers, the coefficient falls to -.37, a bit smaller if still large and significant. The impact on employment and other variables does not seem to differ significantly between workers in different wage classes.

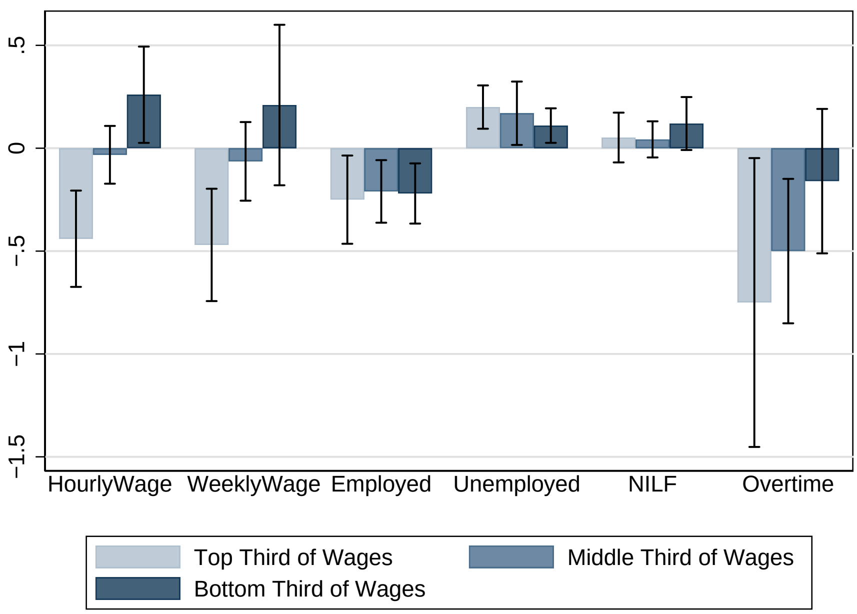


Figure 3: Impact on Workers Without College Education, by Wage Levels

This figure is a plot of the impact of RER Shocks on non-college educated workers with different initial wage levels (those in the top third of wages in light blue, those in the middle third in blue, and those in the bottom third in dark blue), with two standard error bounds. The coefficients are from the interactive term lagged log RER*lagged sectoral openness from Appendix Table 6.

2.4 Robustness: Impact of Various other Trade Shocks

Next, in Table 2, we control for various other trade shocks which are often thought to impact labor markets as well as other shocks including real interest rate movements. In column (1), we control for Chinese import penetration. In column (2), we include the Pierce/Schott Post-PNTR*NTR gap measure. Interestingly, this does render our key interaction term insignificant, although the coefficient is not significantly different from that of the main specification. The significance is restored with the inclusion of sectoral FEs. We think that, on balance the weight of evidence, in both the panel regression and in the annual cross-sections, is consistent with a negative employment impact, although the impact is not statistically sigificant in every specification. In column (3), we control for tariffs (we also try the change in tariffs in the Appendix), and in column (4) we include a control for the MFA agreement (as described in the Data section). In column (5), we control for the share of sectoral intermediate inputs (defined narrowly, and broadly) interacted with a measure of the real exchange rate (5, 6), and the capital-labor ratio as well as this variable interacted with the real interest rate (defined here as the interest rate on 30-year mortgages minus the core CPI, both from FRED). Reasoning that more tradable sectors might also be more sensitive to interest rates, we also interacted the RIR with our measure of openness as a control, and found that it was not influential. With the exception of Chinese import penetration, which has the expected sign, we do not find significant impacts of these other variables on employment (or on the other variables, so we have suppressed those results for space).[13]

In columns (1) and (2) of Table 2, we did not include sectoral FEs given the likely collinearity with the China shock. When we include these in the Online Appendix, we find that the impact of lagged Chinese import penetration is not significant. Note that this is likely driven because of the special nature of our sectoral FEs, which includes a post-2002*sectoral interactive effect. The coefficient of -.18 on Chinese Import Penetration would imply that a worker in a sector in the 90th percentile of Chinese penetration in 2002 would be 3.2% less likely to be employed a year later (=-.18*.18).

While some of these non-results may seem counterintuitive, with the exception of the Post-PNTR*NTR Gap variable, which is significant when using ASM data, the other results are in line with what Campbell (2016b) found using the ASM. The reason that duties or shipping costs may not have had an impact could be that neither actually changed little over this period (relative to movements in RERs), while the MFA exposure variable is likely largely soaked up by the post-2003*industry interaction. And neither Campbell (2016b), nor others such as Ekholm et al. (2012) who have looked into the matter have found consistent evidence that sectors with more (or less) intermediate

13. We also tested whether workers in *occupations* exposed to exchange rate shocks suffered declines in wages and employment during periods of RER shocks. We found that they did not (see the Online Appendix).

inputs have done worse when RERs are elevated. Note that theoretically, it is not clear what should happen to sectors with more intermediate inputs. On one hand, these sectors should be helped by cheaper prices for intermediate inputs. On the other, sectors with more intermediate inputs might be sectors in which substituting imports for domestic production may be facilitated.

Our results also seem to be relatively insensitive to the choice of RER index. For example, if we use the Fed's Broad Trade-Weighted RER index instead (Online Appendix Table 11), the results are little-changed.

While our primary focus is on the impact of trade shocks induced by movements in relative prices, in fact it is not necessarily an integral part of the story that movements in relative prices were the cause of the trade shocks. Thus, these results hold up when we instrument for sectoral changes in import penetration and the export share using movements in real exchange rates (Table 7), or if we instrument using the overall manufacturing trade balance (Table 8 of the Additional Appendix). When relative prices appreciate, the manufacturing trade deficit worsens, and the sectors that trade the most suffer the most. Even if you believe the trade deficit worsened for other reasons beside relative prices, the point remains that this trade shock – whatever the cause – appeared to be highly correlated with adverse labor market outcomes for workers most exposed. And yet it was not correlated with a relative decline in wages for low income workers. Thus, this lack of correlation, we believe, is interesting irrespective of whether we have completely solved the identification problem as it creates a puzzle for those who believe that trade shocks have been a major driver of inequality.

Table 2: Robustness: The Impact of Various Trade Shocks on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
L.3-7yr.Avg.Openness	0.0039	-0.042	0.038	0.035	0.029	0.044
	(0.038)	(0.044)	(0.038)	(0.038)	(0.036)	(0.039)
L.3-7yr.Avg.Open.*ln(WARULC)	-0.18**	-0.11	-0.23***	-0.22***	-0.19**	-0.19**
	(0.076)	(0.078)	(0.072)	(0.073)	(0.087)	(0.086)
$\Delta\, ln$ Demand	0.10***	0.099***	0.078***	0.079***	0.081***	0.077***
	(0.019)	(0.020)	(0.017)	(0.017)	(0.018)	(0.017)
$\Delta\, ln$ VA/Prod. Worker	-0.066***	-0.078***	-0.059***	-0.059***	-0.056***	-0.058***
	(0.021)	(0.026)	(0.018)	(0.018)	(0.018)	(0.019)
Chinese Import Penetration	-0.18**					
	(0.080)					
Post-PNTR x NTR Gap		-0.0076				
		(0.028)				
Duties			0.0070			
			(0.046)			
C.I.F.				-0.052		
				(0.040)		
MFA Exposure				0.014		
				(0.034)		
MP Int.Share*ln(RER)					-0.0098	
					(0.0082)	
MP Int.Share					-0.0026	
					(0.0037)	
MP Int.Sh.(Narrow)*ln(RER)						-0.0026
						(0.0030)
MP Int.Sh. (Narrow)						0.35
						(0.40)
Observations	322139	355608	304742	304742	296748	296748
Sector FEs	No	No	Yes	Yes	Yes	Yes

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. The dependent variable is a dummy variable for employment one year later. Each regression includes year FEs, and the last 4 columns include sector FEs, over the period 1979-2010. ln(RER) is the log of WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate. L.3-7yr.Avg.Openness is the average of openness lagged 3, 4, 5, 6, and 7 years. Thus, the interaction term on lagged openness and the log of WARULC is the key variable of interest in this regression.

2.5 Alternative IV Approach

As an additional robustness check, next we pursue what is essentially an instrumental variables strategy. First we regress the log change in sectoral import penetration and the export share of shipments on the log of WARULC, while controlling for log changes in sectoral demand and sectoral FEs.

$$\Delta lnMPPen_{it} = \alpha_i + \beta_1 ln(WARULC)_{t-1} + \beta_2 \Delta ln(Demand)_{it} + \epsilon_{it} \tag{2.4}$$

where $MPPen$ is import penetration (defined as imports divided by domestic demand), and we run this exact equation also on the export share of shipments. We include sectoral fixed effects, although when we omit them, the results are little-changed. The results in Table 3 suggest that the level of the RER tends to have the predicted impact on changes in import penetration and export share, even if the magnitude is not quite symmetric. One partial explanation for this result could be that the level of import penetration is generally 50% higher than the export share of shipments in our sample, and a second potential explanation is that imports enter in both the numerator and denominator of import penetration, which could mechanically dampen its elasticity with respect to the RER. In any case, since RERs presumably impact the labor market predominantly via its impact on trade, it is reassuring to find that RERs seem to have a large impact on both import penetration and the export share of shipments.

Table 3: First Stage Regresison of Trade Flows on the RER

	Δ ln(Import Pen.)	Δ ln(Export Share)
L.ln(WARULC)	0.22***	-0.38***
	(0.0056)	(0.0032)
L.$ln\,\Delta$ Demand	0.068***	-0.31***
	(0.0062)	(0.0035)
Observations	295828	294940

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. The dependent variable in column (1) is the log change in Import Penetration, and in column (2) it is the log change in the export share of shipments. Each regression includes industry FEs over the period 1979-2010. ln(WARULC) is the log of Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate.

We then use the predictions in the log change of these caused by changes in the exchange rate, and then scale by the size of sectoral import penetration/export demand.

$$Impact_{MPPen} = \beta_1 * ln(WARULC)_{t-1} * L.MPPen_{it} \tag{2.5}$$

The intuition for this equation is that it will provide us with the expected increase in import penetration (export share) from a shock to the RER. It is the level of the increase rather than the log change which would have an impact on labor market outcomes. This is because, for example, a 100% increase from an initial import penetration of just .1%

to .2% would not be expected to have a measurable impact on employment or wages. However, a 50% increase from 10% to 15% likely would. We also do the analagous estimation using the export share of shipments. To get the total impact of a RER shock, we have to sum the impact on import penetration and the export share of shipments.

$$TotalImpact_{it} = Impact_{it}^{MPPen} - Impact_{it}^{ExpShare} \tag{2.6}$$

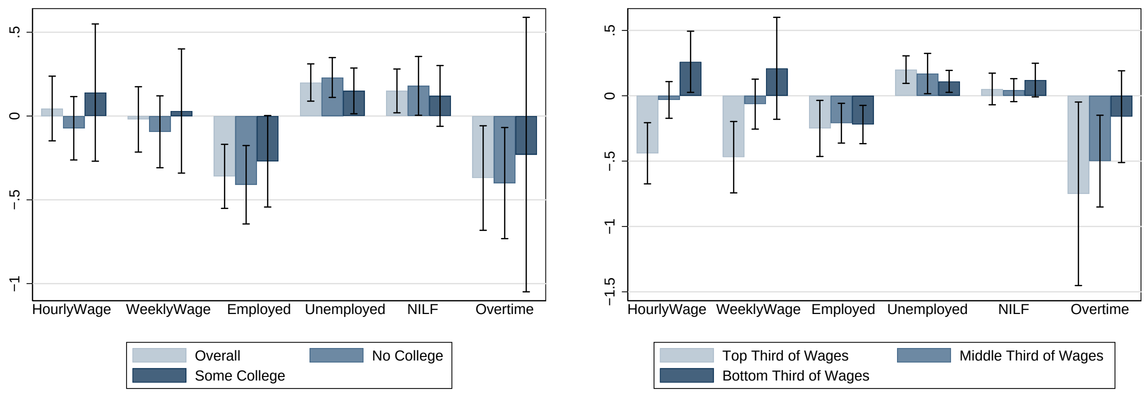
We then use the $TotalImpact_{it}$ as a regressor in our equation. In this case, we will only pick up the impact of RER changes on labor market outcomes that operate via trade flows. This may, if anything, underestimate the impact, as some businesses may respond to rising labor costs caused by changes in RERs by moving away from labor, even if there are no changes in trade costs otherwise. This reasoning is why we do not use this as our main specification.

Thus, we now run the following regression:

$$ln\Delta W_{iht} = \alpha_t + \beta_0 Impact_{it} + \beta_1 L.3\text{-}7yr.Open._{ht} + \sum_{i=3}^{n} \beta_i C_{i,t} + \alpha_h + \nu_t + \epsilon_{ht}, \tag{2.7}$$

$$\forall h = 1, ..., 87, \ t = 1979, ..., 2010,$$

The results, in Figure 4, are for the most part strikingly similar to Table 1. The main differences are that the results on NILF and overtime are perhaps a bit stronger while none of the wage regressions are significant. Overall, this supports the view that these trade shocks had the largest impact via employment rather than wage adjustment. As an alternative, we can also control for import penetration interacted with an import-weighted RER index, and the export share of shipments interacted with an export-weighted RER index, and the results for each are not significantly different, and so we leave this table to the Online Appendix (Table 1).



(a) By Education Levels

(b) By Wage Levels

Figure 4: Impact of RER Shocks (Using an IV) on the Labor Market

Notes: In Panel (a), we plot the impact of an RER shock using our IV first on the full sample (light blue), then for workers with no college eduction (blue) and with college education (dark blue). In Panel (b), we plot the impact by different initial levels of wages (top third in light blue, vs. the middle-third in blue, and the bottom third in dark blue). The full regression results are reported in Appendix Table 7.

2.6 Impact on Changing Sectors or Occupations

We can also gauge the impact of RER shocks on the probability that a worker changes her/his sector or occupation. In the first column of Table 4, we show that RER appreciations are not significantly associated with sectoral switches for workers in more open sectors, as the positive coefficient of .11 is not statistically significant. In the second column, we consider "All Leaves" from the sector, including becoming unemployed and leaving the labor force (when one leaves employment, the sectoral status does not necessarily change). Not surprisingly, when we include these, the coefficient on the interaction terms become highly significant, with a coefficient a bit larger (.27) than the combination of the coefficients on unemployed and NILF (.088 and .12) from Table 1. In the third column, we find that RER appreciations do not appear to drive occupational changes in more open sectors, although when we include becoming unemployed or leaving the labor force, we do get significance albeit with a smaller magnitude (.16). Lastly, our CPS MORG data also includes relatively complete data on being part-time for economic reasons. We do not see a significant impact of RER movements on this variable. In Panel B, we find similar results when we restrict to workers without college educations. On the whole, we expected stronger results here and so this table could be seen as a caveat to the main results. However, also note that changes in sectoral demand also don't seem to predict sectoral changes unless changes in employment status are added in. This is not the case with changes in occupational status, however.

Table 4: Robustness: The Impact of RER Movements on Changing Sectors/Occupations

	Δ Sector	All Leaves	Δ Occ.	All Leaves(Occ.)	PT,Econ
A. Changing Sectors					
L.3-7yr.Open.*ln(RER)	0.11	0.27**	0.025	0.16***	0.048
	(0.13)	(0.11)	(0.060)	(0.063)	(0.042)
L.3-7yr. Average Openness	-0.0037	-0.051	0.087	0.013	-0.027
	(0.085)	(0.082)	(0.063)	(0.043)	(0.033)
$\Delta\, ln$ Demand	-0.029	-0.076**	-0.027**	-0.056***	-0.031***
	(0.025)	(0.036)	(0.012)	(0.017)	(0.0056)
$\Delta\, ln$ VA/Prod. Worker	0.016	0.055**	0.062***	0.077***	0.0061
	(0.017)	(0.022)	(0.015)	(0.017)	(0.0052)
Observations	282814	282814	282814	282814	246090
B. Changing Sectors, No College					
L.3-7yr.Open.*ln(RER)	0.050	0.27**	0.047	0.20**	0.057
	(0.12)	(0.12)	(0.096)	(0.083)	(0.052)
Observations	174305	174305	174305	174305	146656

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. Each regression includes year and industry FEs over the period 1979-2010. In Panel's A and B, the dependent variables are (1) A dummy for changing sector, (2) A dummy for changing sector, inclusive of not having a job the next year, (3) Changing one's occupation, (4) Changing one's occupation, inclusive of not having a job the following year, and (5) Part-time for economic reasons. ln(RER) is the log of WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate. L.3-7yr.Avg.Openness is the average of openness lagged 3, 4, 5, 6, and 7 years. Thus, the interaction term on lagged openness and the log of WARULC is the key variable of interest in this regression.

3 Conclusion

In this paper, we investigate the impact of the two largest trade shocks, caused by RER movements, in the US post-war period on workers in sectors more exposed to trade using the CPS MORG. Although the evidence on the impact of RER movements from the MORG is more robust when using the ASM, on the whole it appears to be supportive of the idea that RER movements have a large impact on workers in more-exposed sectors. When RERs are elevated, workers in more open sectors are less likely to be employed, and more likely to be unemployed or out of the labor force a year later. However, conditional on being employed, their wages are not significantly lower. Workers without any college education appear to be hurt worse, as do workers who initially had higher wages. We reconcile these findings by noting that workers with higher wages but no college education appeared to have suffered very badly in our sample. On the whole, our findings do not point to a large role for the two major trade shocks in the rise of inequality since 1980.

References

Autor, D., D. Dorn, G. H. Hanson, and J. Song. 2014. "Trade Adjustment: Worker-Level Evidence." *The Quarterly Journal of Economics* 129 (4): 1799–1860.

Berman, N., P. Martin, and T. Mayer. 2012. "How do Different Exporters React to Exchange Rate Changes?" *The Quarterly Journal of Economics* 127 (1): 437–492.

Brambilla, I., A. K. Khandelwal, and P. K. Schott. 2010. *China's Experience Under the Multi-fiber Arrangement (MFA) and the Agreement on Textiles and Clothing (ATC).* Technical report.

Campa, J. M., and L. S. Goldberg. 2001. "Employment Versus Wage Adjustment and the US Dollar." *Review of Economics and Statistics* 83 (3): 477–489.

Campbell, D. L. 2014. *Through the Looking Glass: A WARPed View of Real Exchange Rate History.* Working Paper.

Campbell, D. L. 2016. "Measurement Matters: Productivity-Adjusted Weighted Average Relative Price Indices." *Journal of International Money and Finance* 61 (C): 45–81.

Campbell, D. L. 2016. *Relative Prices, Hysteresis, and the Decline of American Manufacturing.* Working Papers w0212. Center for Economic and Financial Research (CEFIR).

Campbell, D. L., and L. Lusher. 2016. "Trade Shocks, Taxes, and Inequality."

Cline, W. R. 1997. *Trade and Income Distribution.* Peterson Institute.

Dai, M., and J. Xu. 2015. "Firm-specific Exchange Rate Shocks and Employment Adjustment: Theory and Evidence."

Ebenstein, A., A. Harrison, and M. McMillan. 2015. *Why are American Workers Getting Poorer? China, Trade and Offshoring.* Technical report. National Bureau of Economic Research.

Ebenstein, A., A. Harrison, M. McMillan, and S. Phillips. 2014. "Estimating the Impact of Trade and Offshoring on American Workers Using the Current Population Surveys." *Review of Economics and Statistics* 96 (4): 581–595.

Ekholm, K., A. Moxnes, and K. H. Ulltveit-Moe. 2012. "Manufacturing Restructuring and the Role of Real Exchange Rate Shocks." *Journal of International Economics* 86 (1): 101–117.

Feenstra, R. C. 2007. *Globalization and Its Impact on Labour.* Working Papers 44. The Vienna Institute for International Economic Studies, wiiw.

Feenstra, R. C., J. Romalis, and P. K. Schott. 2002. *US Imports, Exports, and Tariff Data, 1989-2001.* Technical report. National Bureau of Economic Research.

Feenstra, R., and G. Hanson. 1999. "The Impact of Outsourcing and High-Technology Capital on Wages: Estimates for the United States, 1979-1990." *Quarterly Journal of Economics:* 907–940.

Glick, R., S. Leduc, et al. 2013. "The effects of unconventional and conventional US monetary policy on the dollar." Citeseer.

Goldberg, P. K., and N. Pavcnik. 2007. "Distributional Effects of Globalization in Developing Countries." *Journal of Economic Literature* 45:39–82.

Gourinchas, P.-O. 1999. *Exchange Rates and Jobs: What Do We Learn From Job Flows?* Technical report.

Haskel, J., R. Z. Lawrence, E. E. Leamer, and M. J. Slaughter. 2012. "Globalization and US wages: Modifying classic theory to explain recent facts." *The Journal of Economic Perspectives* 26 (2): 119–139.

Helpman, E., O. Itskhoki, M.-A. Muendler, and S. J. Redding. 2012. *Trade and Inequality: From Theory to Estimation.* Working Paper, Working Paper Series 17991. National Bureau of Economic Research.

Jaumotte, F., S. Lall, and C. Papageorgiou. 2013. "Rising Income Inequality: Technology, or Trade and Financial Globalization." *IMF Economic Review* 61 (2): 271–309.

Kaplan, S. N., and J. Rauh. 2010. "Wall Street and Main Street: What Contributes to the Rise in the Highest Incomes?" *Review of Financial Studies* 23 (3): 1004–1050.

Khandelwal, A. K., P. K. Schott, and S.-J. Wei. 2013. "Trade Liberalization and Embedded Institutional Reform: Evidence from Chinese Exporters." *American Economic Review* 103 (6): 2169–2195.

Klein, S. Schuh, and R. K. Triest. 2003. "Job Creation, Job Destruction, and the Real Exchange Rate." *Journal of International Economics* 59 (2): 239–265.

Krugman, P., and R. Lawrence. 1993. *Trade, Jobs, and Wages.* Technical report. National Bureau of Economic Research.

Lawrence, R. Z. 2008. *Blue-Collar Blues: Is Trade to Blame for Rising US Income Inequality?* Peterson Institute.

Leamer, E. E. 1994. *Trade, Wages and Revolving Door Ideas.* Technical report. National Bureau of Economic Research.

Moser, C., D. Urban, and B. W. di Mauro. 2010. "International Competitiveness, Job Creation and Job Destruction–An Establishment-Level Study of German Job Flows." *Journal of International Economics* 80, no. 2 (): 302–317.

Pierce, J. R., and P. K. Schott. 2012. "Concording US Harmonized System Codes over Time." *Journal of Official Statistics* 28 (1): 53–68.

Pierce and Schott. 2016. "The Surprisingly Swift Decline of US Manufacturing Employment." *American Economic Review* 106 (7): 1632–62.

Piketty, T., E. Saez, and S. Stantcheva. 2014. "Optimal Taxation of Top Incomes: A Tale of Three Elasticities." *American Economic Journal: Economic Policy* 6 (1): 230–271.

Revenga, A. L. 1992. "Exporting Jobs?: The Impact of Import Competition on Employment and Wages in US Manufacturing." *The Quarterly Journal of Economics:* 255–284.

Rogers, J. H., C. Scotti, and J. H. Wright. 2014. "Evaluating asset-market effects of unconventional monetary policy: a multi-country review." *Economic Policy* 29 (80): 749–799.

Roine, J., J. Vlachos, and D. Waldenström. 2009. "The Long-Run Determinants of Inequality: What Can We Learn From Top Income Data?" *Journal of Public Economics* 93 (7): 974–988.

Schott, P. K. 2008. "The Relative Sophistication of Chinese Exports." *Economic policy* 23 (53): 6–49.

Thomas, C., S. Fahle, and J. Marquez. 2008. "Measuring US International Relative Prices: A WARP View of the World." *FRB International Finance Discussion Paper,* no. 917.

4 Appendix

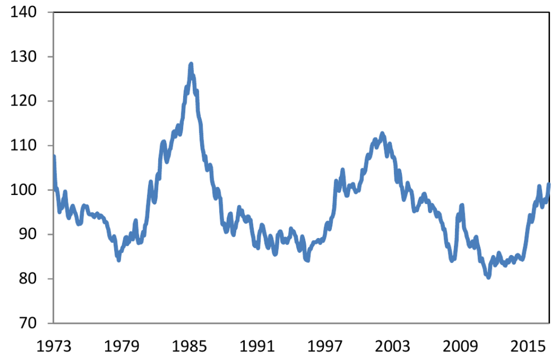


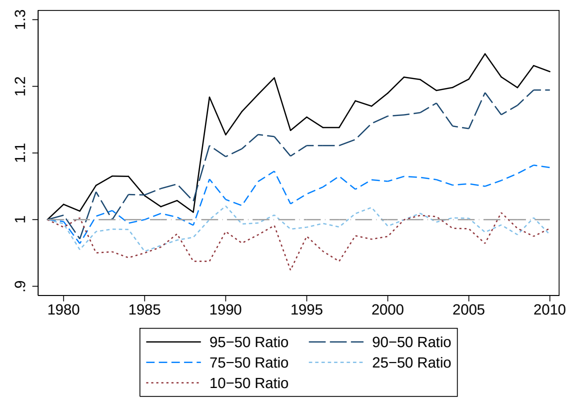
Figure 5: The Fed's Broad, Trade-Weighted RER Index

Source: Federal Reserve Board (via FRED).

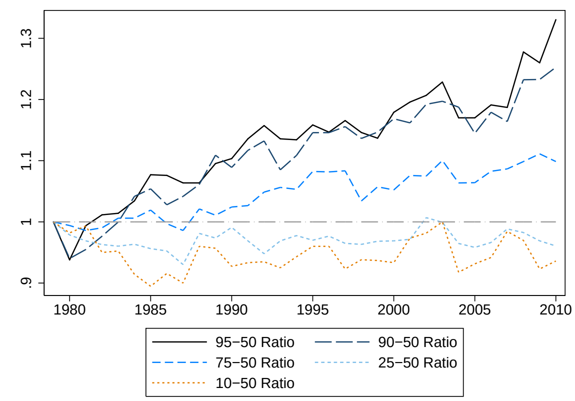
Table 5: Summary Statistics for MORG Variables, Select Years

	(1) 1979	(2) 1985	(3) 1998	(4) 2008	(5) All Years
A. Wages and Sectoral Openness					
L.3-7 year Average Openness	0.095	0.11	0.16	0.23	0.14
	(0.97)	(0.089)	(0.14)	(0.16)	(0.13)
$\Delta\, ln$ Hourly Wage	0.090	0.037	0.049	0.016	0.051
	(0.27)	(0.32)	(0.48)	(0.37)	(0.31)
Hourly wage	6.96	10.3	15.8	22.53	13.35
	(3.6)	(5.18)	(10.8)	(13.6)	(7.11)
Hourly wage, Top Quarter of Open Sectors	7.32	10.6	15.5	25.2	13.6
	(3.5)	(5.54)	(9.43)	(15.74)	(10.38)
$\Delta\, ln$ Weekly Pay	0.087	0.048	0.018	-0.016	0.048
	(0.38)	(0.36)	(0.44)	(0.46)	(0.39)
Weekly pay	285	424	659	949	555
	(157)	(231)	(438)	(611)	(414)
Δ Overtime	-0.0043	-0.013	-0.040	-0.075	-0.012
	(0.64)	(0.61)	(0.53)	(0.51)	(0.59)
B. Demographic Variables					
Age	40.3	41.5	42.1	44.3	41.7
	(14.4)	(13.8)	(11.8)	(12.1)	(13.0)
Female	0.35	0.35	0.34	0.30	0.34
	(0.48)	(0.48)	(0.47)	(0.46)	(0.48)
No College	0.74	0.66	0.56	0.50	0.62
	(0.44)	(0.47)	(0.50)	(0.50)	(0.48)
No College, Top Quarter of Open Sectors	0.70	0.66	0.53	0.41	0.61
	(0.46)	(0.47)	(0.50)	(0.49)	(0.49)
C. Employment Variables					
Employed One Year Later	.78	.79	.90	.84	.83
Unemployed One Year Later	.057	.051	.023	.083	.047
Not-in-the-Labor-Force One Year Later	.16	.15	.074	.082	.12

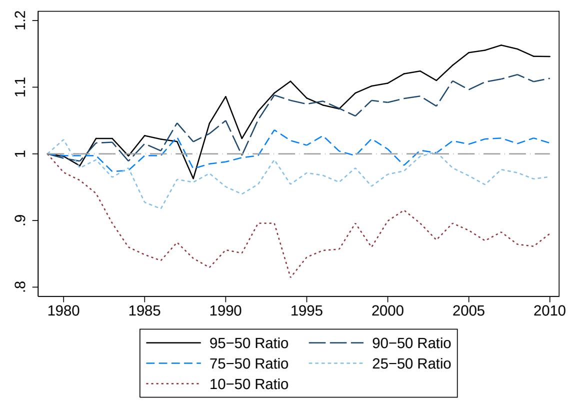
Standard Errors in parentheses. Manufacturing sectors only.



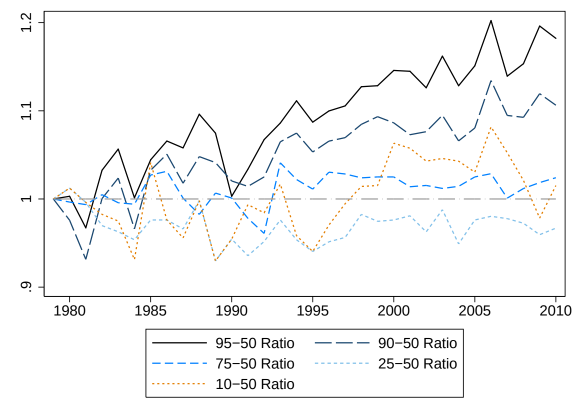
(a) Hourly Wages, Manufacturing



(b) Weekly Wages, Manufacturing



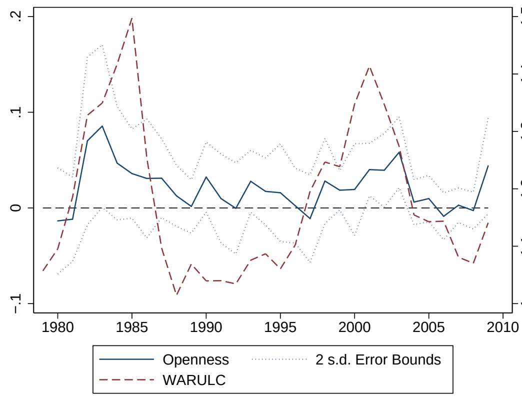
(c) Hourly Wages, All Sectors



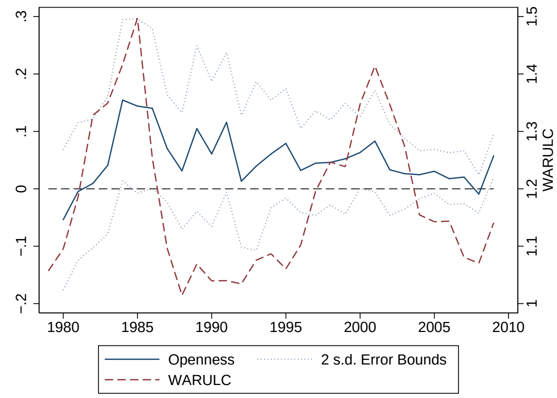
(d) Weekly Wages, All Sectors

Figure 6: The Evolution of Inequality in the MORG

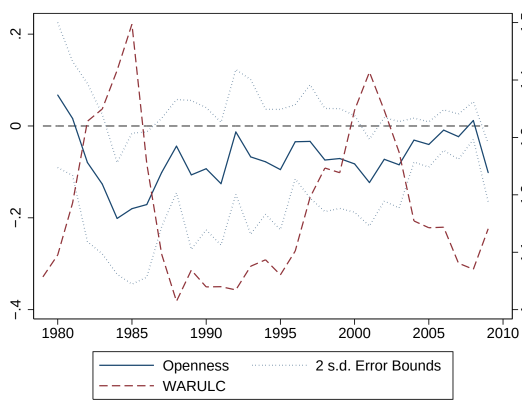
Panels (a) and (b) plot the evolution of the ratios of various percentiles of the income distribution for the manufacturing sector, and then Panels (c) and (d) do the same for all sectors. Thus panel (a) suggests that the ratio of wages between the 90th and 50th percentiles had increased a bit more than 20% from 1979 to 2010.



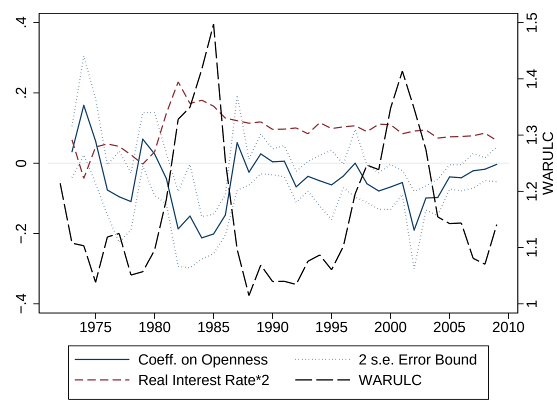
(a) Unemployment



(b) NILF



(c) Employed (MORG)



(d) Employment (ASM, Campbell, 2016b)

Figure 7: RERs, Openness, Unemployment, Labor Force Exits, and Employment

We have plotted the annual coefficients on "openness" from annual regressions of unemployment (Panel a), not-in-the-labor-force (NILF; Panel b), and the probability of being employed (panel c) one year later at the individual level on openness (measured at the sector level), with controls for log changes in demand and productivity, with standard errors clustered at the sector level. In Panel d, we reprinted Figure 6 from Campbell 2016b, which uses data from the Annual Survey of Manufactures (ASM) to plot the coefficient of openness on the log change in sectoral employment after controlling for other relevant variables. WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate developed by Campbell (2016a).

Table 6: The Impact of Real Exchange Rate Shocks on the Labor Market

	$\Delta\, ln$ HW	$\Delta\, ln$ WW	Employed	Unem.	NILF	Δ Over.
A. No College, Top Third of Wages						
L.3-7yr.Open.*ln(RER)	-0.44***	-0.47***	-0.25**	0.20***	0.052	-0.75**
	(0.12)	(0.14)	(0.11)	(0.054)	(0.062)	(0.36)
Observations	129508	130454	146830	146830	146830	70976
B. No College, Middle Third						
L.3-7yr.Open.*ln(RER)	-0.032	-0.064	-0.21***	0.17**	0.043	-0.50***
	(0.072)	(0.098)	(0.078)	(0.079)	(0.045)	(0.18)
Observations	223110	224433	253424	253424	253424	141655
C. No College, Bottom Third						
L.3-7yr.Open.*ln(RER)	0.26**	0.21	-0.22***	0.11**	0.12*	-0.16
	(0.12)	(0.20)	(0.075)	(0.043)	(0.066)	(0.18)
Observations	268832	270387	334481	334481	334481	204814

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. Each regression includes industry and year FEs over the period 1979-2010. The dependent variables are: (1) the Hourly Wage, (2) the Weekly Wage, (3) Employment (binary), (4) Unemployment (binary), (5) Not in the Labor Force (binary), (6) Change in overtime. The key variable of interest is the interaction between lagged 3-7 year average openness (L.3-7yr.Avg.Open.) and the log of the RER. WARULC = Weighted Average Relative Unit Labor Costs is the measure of the real exchange rate used here.

Table 7: Impact of IV for RER Shocks on the Labor Market

	$\Delta\, ln$ HW	$\Delta\, ln$ WW	Employed	Unem.	NILF	Δ Over.
A. Full Sample						
Implied Trade Impact	0.045	-0.020	-0.36***	0.20***	0.15**	-0.37**
	(0.099)	(0.10)	(0.098)	(0.057)	(0.067)	(0.16)
L.3-7yr.Avg.Openness	-0.031	-0.017	0.029	0.030	-0.059***	0.12***
	(0.037)	(0.044)	(0.034)	(0.024)	(0.022)	(0.043)
Observations	216517	219462	305992	305992	305992	134539
B. No College Education						
Implied Trade Impact	-0.073	-0.094	-0.41***	0.23***	0.18**	-0.40**
	(0.097)	(0.11)	(0.12)	(0.061)	(0.090)	(0.17)
Observations	129347	130208	189234	189234	189234	101088
C. At least some College						
Implied Trade Impact	0.14	0.030	-0.27*	0.15**	0.12	-0.23
	(0.21)	(0.19)	(0.14)	(0.070)	(0.093)	(0.42)
Observations	87170	89254	116758	116758	116758	33451
D. Top Third of Wages						
Implied Trade Impact	-0.028	-0.17	-0.22**	0.15***	0.065	-0.86
	(0.17)	(0.19)	(0.095)	(0.060)	(0.049)	(0.55)
Observations	71667	72389	78607	78607	78607	26867
E. Middle Third of Wages						
Implied Trade Impact	-0.0019	0.014	-0.062	0.12	-0.060	-0.22
	(0.16)	(0.18)	(0.11)	(0.091)	(0.055)	(0.35)
Observations	73085	73435	81196	81196	81196	48655
F. Bottom Third of Wages						
Implied Trade Impact	0.086	0.012	-0.35***	0.19***	0.16	-0.29
	(0.18)	(0.26)	(0.13)	(0.053)	(0.11)	(0.18)
Observations	71765	71997	84346	84346	84346	55687

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. The dependent variable is a dummy variable for employment one year later. Each regression includes industry and year FEs over the period 1979-2010. ln(RER) is the log of WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate. L.3-7yr.Avg.Openness is the average of openness lagged 3, 4, 5, 6, and 7 years. Thus, the interaction term on lagged openness and the log of WARULC is the key variable of interest in this regression.")