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

September, 2016

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 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, 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. (2003a), 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. (2003a), and test what happens to workers in

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 3. 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. (2003a), 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 sample.

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).

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 the US relative 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. Similar to Campbell (2016b), 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 the rise of China.

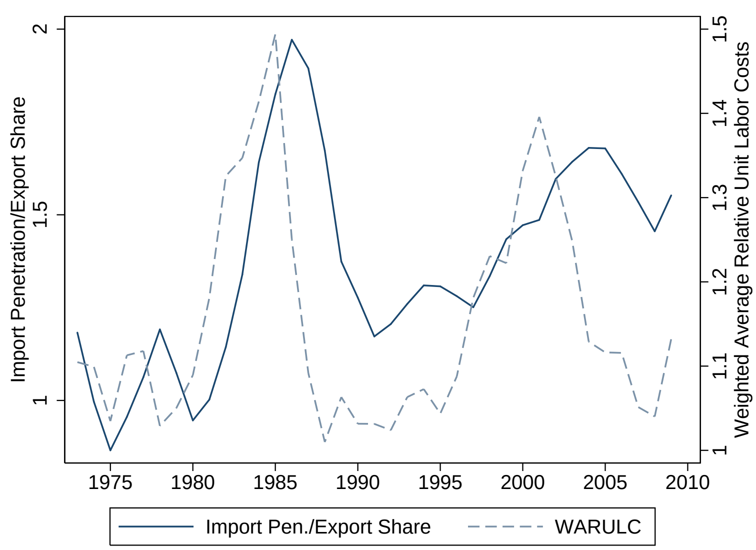


Figure 1: Two Adverse Trade/RER Shocks

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, but we do find a striking negative impact for those without any college education. Surprisingly, we also 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 and those without any college education both did worse 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. 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 4. 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, and nor are trade shocks (RERs or trade with China) 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

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 2014 have found that this is already well explained by top marginal tax rates, confirmed by both Roine et al. (2009) and also Campbell 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.

trade 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, since we only have many sector-level variables (such as trade, productivity, and output) for manufacturing sectors, we limit our analysis to manufacturing. In some ways this is a strength, since manufacturing sectors are disproportionately affected by trade relative to service sectors, and other manufacturing sectors which trade less are likely the most appropriate control group. Contrary to popular belief, there has been no increase in the services share of total trade according to BEA data in the past several decades (see Campbell (2016b), Figure 13).

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).

We provide a data of summary statistics in Table 8. 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 4.

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 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 an Additional Appendix, Section 6.2.

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.[9] 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 6.1 of our unpublished appendix, which includes a direct comparison of the different indices (Figure 7), 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 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

9. 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 $I^{WARULC}_{US,t} = \prod_{i=1} \left(\frac{ULC_{US,t}}{ULC_{i,t}}\right)^{\Omega_{i,t}}$, 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.

than by cutting salaries (or will go bankrupt). Workers in the tradable sector will then look for jobs 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 weaken. 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 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 5), we get roughly similar results. In Figure 5 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.

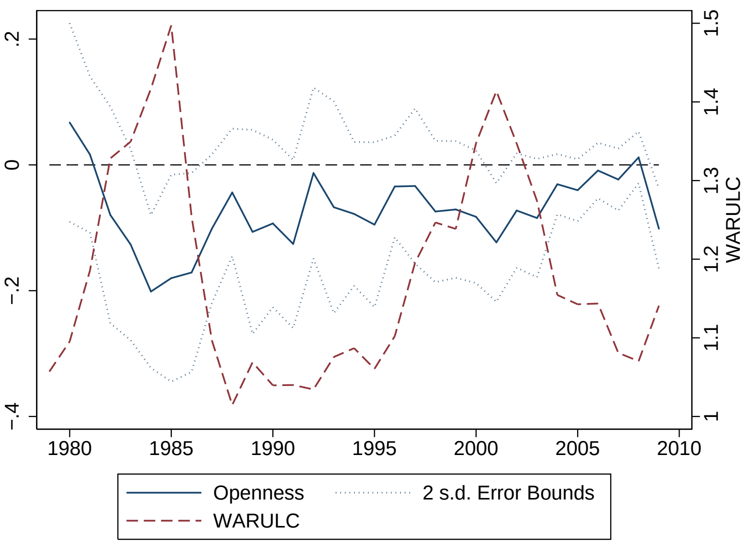


Figure 2: RER Shocks, Openness, and Employment

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).

2.2 Description of Panel Approach

The next step is to 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:

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

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

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

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 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. Note, that because 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 Appendix Table 11). The downside of including sectoral FEs for the 2003-2010 period separately is that factors associated with China's December 2001 accession to the WTO will not be identified. 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.

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 substantial 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.[10] There is also a decline in the number of workers in these sectors who work overtime (column 6), although the opposite sign on lagged openness makes interpretation of this result less than straightforward.[11] We have found that the results for NILF and overtime are not consistent across all other specifications we have tried (some of which are in the appendix; NILF appears more robust), while the results for employment and unemployment appear to be robust.

In the employment regression, the coefficient of -.23 on the interactive variable L.3-7yr.Avg.Openness*ln(WARULC) 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 percentile of .33, roughly .3 higher than a sector in the 10th percentile, would have been 2.1% less likely to have a job a year later (=-.23*.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 unemployed 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 have been unemployed when surveyed again in 2002.

Breaking down the impact by college education (Panels B and C), and wages (D, E, and F), we find that the most interesting differences come from a differential impact on those with differing levels of education. 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). The coefficient of -.18 on weekly wages in Column 2 of Panel D indicates that, from 2001 to 2002, wages for a worker employed in a sector with openness of 30% would have fallen by about 2.5% (=exp(.3*ln(1.4)*-.18)-1) relative to a worker in a sector with no openness.

However, interestingly, we do not see similar results when we look instead at workers

10. 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.

11. 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.

with high vs. low wages, complicating the picture one gets when we think about what impact this might have on overall inequality. Changes in hourly wages for high-wage and low wage workers are not statistically different from each other or zero when trade shocks hit, nor are the differences in the other variables significant. We reconcile these seemingly conflicting results in Table 2, when we break the "No College" sample further by those with the largest, middling, and lowest wages. Here we see that poorly-educated workers who had nevertheless managed to get high-wage manufacturing jobs did very poorly after being hit by trade shocks. They were less likely to be employed than those with middling incomes, and even conditional on being employed, saw their salaries fall by much more than workers with lower wages. In Panel's D and E, we also look at differences by sex, but do not see much of a differential effect.

Lastly, our results 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 (Table 13), the results are little-changed.

2.4 Robustness: Impact of Various other Trade Shocks

In Table 3, we control for various other trade shocks which are often thought to impact labor markets as well as other thing such as real interest rate movements. These include Chinese import penetration (column 1), tariffs (2), changes in tariffs (3), the cost of insurance and freight charges (4), 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).[12] Here, we did not include sectoral FEs given the likely collinearity with the China shock. When we include these in Table 4, we find that the impact of lagged Chinese import penetration is not significant, although the other variables are. Note that this is likely driven because of the special nature of our sectoral FEs, which includes a post-2002*sectoral interactive effect.

12. 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 (Table 12).

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. Full Sample						
L.3-7yr.Open.*ln(RER)	-0.0083	-0.084	-0.23***	0.12***	0.11**	-0.32**
	(0.055)	(0.071)	(0.067)	(0.033)	(0.047)	(0.14)
L.3-7yr.Avg.Openness	-0.025	-0.0022	0.037	0.027	-0.064***	0.14***
	(0.039)	(0.046)	(0.037)	(0.026)	(0.023)	(0.053)
$\Delta\, ln$ Demand	0.0093	0.044**	0.080***	-0.071***	-0.0086	0.029
	(0.018)	(0.019)	(0.022)	(0.018)	(0.012)	(0.030)
$\Delta\, ln$ VA/Prod. Worker	-0.0047	-0.0036	-0.060***	0.027***	0.033**	0.043*
	(0.015)	(0.013)	(0.019)	(0.0097)	(0.014)	(0.024)
Observations	216517	219462	305992	305992	305992	134539
B. No College Education						
L.3-7yr.Open.*ln(RER)	-0.098	-0.18**	-0.27***	0.14***	0.13*	-0.33**
	(0.069)	(0.089)	(0.097)	(0.047)	(0.071)	(0.14)
Observations	129347	130208	189234	189234	189234	101088
C. At least some College						
L.3-7yr.Open.*ln(RER)	0.087	0.044	-0.17**	0.093**	0.073	-0.24
	(0.15)	(0.16)	(0.084)	(0.037)	(0.062)	(0.33)
Observations	87170	89254	116758	116758	116758	33451
D. Top Third of Wages						
L.3-7yr.Open.*ln(RER)	-0.085	-0.19**	-0.13**	0.094**	0.041	-0.71*
	(0.085)	(0.086)	(0.058)	(0.045)	(0.030)	(0.40)
Observations	71667	72389	78607	78607	78607	26867
E. Middle Third of Wages						
L.3-7yr.Open.*ln(RER)	0.0011	-0.032	-0.012	0.11*	-0.095***	-0.33*
	(0.092)	(0.11)	(0.072)	(0.063)	(0.030)	(0.20)
Observations	73085	73435	81196	81196	81196	48655
F. Bottom Third of Wages						
L.3-7yr.Open.*ln(RER)	0.091	0.011	-0.21**	0.12***	0.083	-0.13
	(0.14)	(0.19)	(0.090)	(0.031)	(0.073)	(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. 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 2: The Impact of Real Exchange Rate Shocks on the Labor Market

	Δ *ln* HW	Δ *ln* WW	Employed	Unem.	NILF	Δ Over.
A. No College, Top Third of Wages						
L.3-7yr.Open.*ln(RER)	-0.37***	-0.41***	-0.21**	0.14*	0.072	-0.97**
	(0.13)	(0.15)	(0.096)	(0.077)	(0.068)	(0.44)
Observations	26796	26912	29844	29844	29844	17329
B. No College, Middle Third						
L.3-7yr.Open.*ln(RER)	-0.092	-0.18	-0.084	0.17*	-0.086***	-0.38*
	(0.11)	(0.11)	(0.10)	(0.093)	(0.026)	(0.23)
Observations	46782	46923	52238	52238	52238	35943
C. No College, Bottom Third						
L.3-7yr.Open.*ln(RER)	0.016	-0.10	-0.29***	0.17***	0.12	-0.084
	(0.10)	(0.15)	(0.10)	(0.026)	(0.096)	(0.18)
Observations	55769	55890	65707	65707	65707	45283
D. Female						
L.3-7yr.Open.*ln(RER)	0.054	0.11	-0.23**	0.093*	0.14*	-0.55**
	(0.076)	(0.095)	(0.10)	(0.056)	(0.083)	(0.27)
Observations	68635	69198	105088	105088	105088	44943
E. Male						
L.3-7yr.Open.*ln(RER)	-0.032	-0.17*	-0.22***	0.13**	0.085**	-0.23
	(0.082)	(0.093)	(0.081)	(0.059)	(0.042)	(0.17)
Observations	147882	150264	200904	200904	200904	89596

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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.

While some may find these results counterintuitive, they are in line with the results in Campbell (2016b), which used ASM data. The reason is that duties or shipping costs 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 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 the 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.

Table 3: Robustness: The Impact of Various Trade Shocks on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
L.3-7yr.Avg.Openness	0.0017	0.042	0.042	0.038	0.028	0.043
	(0.037)	(0.038)	(0.038)	(0.039)	(0.037)	(0.039)
L.3-7yr.Open.*ln(RER)	-0.18***	-0.23***	-0.23***	-0.22***	-0.19**	-0.20**
	(0.038)	(0.072)	(0.072)	(0.072)	(0.086)	(0.079)
$\Delta\, ln$ Demand	0.10***	0.079***	0.080***	0.081***	0.082***	0.077***
	(0.025)	(0.023)	(0.024)	(0.023)	(0.024)	(0.023)
$\Delta\, ln$ VA/Prod. Worker	-0.067***	-0.060***	-0.059***	-0.060***	-0.056***	-0.058***
	(0.021)	(0.020)	(0.020)	(0.020)	(0.019)	(0.020)
Chinese Import Penetration	-0.18**					
	(0.090)					
Duties		0.0045				
		(0.041)				
Change in Duties			-0.032			
			(0.029)			
C.I.F.				-0.054		
				(0.040)		
Δ C.I.F.				0.0071		
				(0.029)		
MFA Exposure				0.014		
				(0.027)		
MP Int.Share*ln(RER)					-0.0095	
					(0.0090)	
MP Int.Share					-0.0026	
					(0.0039)	
MP Int.Sh.(Narrow)*ln(RER)						-0.0026
						(0.0033)
MP Int.Sh. (Narrow)						0.36
						(0.40)
Observations	305992	295885	295885	295885	295320	295320

$*p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 (but not industry 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.

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 would hold up were we to instrument for sectoral changes in import penetration and the export share using movements in real exchange rates (Table 10), or if we instrument using the overall manufacturing trade balance. 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.

2.5 China in Detail

As the rise of China was, by itself, a major trade shock, and also a major source of the divergence between the Fed's RER index and WARP (see Figure 7 and Campbell (2016a)), we decide to examine it in more detail. In Panel A of Table 4, we report the results on the coefficient "Chinese Penetration", defined as Chinese imports divided by domestic demand (analagous to import penetration), with the other coefficients from equation 2.7 suppressed. Somewhat surprisingly, when we include sectoral FEs in Panel A, the impact of lagged Chinese import penetration is only significant at 90% for weekly wages, but not for any of the other variables. This is likely driven by the sectoral fixed effects, which, as mentioned previously, have separate sectoral dummies for each of the classification periods, one of which is 2003 to 2010 – which nearly corresponds to the period following Chinese entry into the WTO. When we exclude sectoral FEs, in panel B, we see that workers in sectors with more Chinese import penetration were less likely to employed and significantly more likely to have left the labor force a year later. Even so, it is a bit disconcerting that chinese penetration does not predict employment losses or wage declines in the regressions with fixed effects. In Panel C, once again, we see that the effects are stronger for those without a college education, who suffer actual declines in wages, and weaker for those with at least some college. When we break these up into those with the top, middle, and bottom third of wages, however, we do not find any significant impacts.

Table 4: Robustness: The Impact of China

	$\Delta\, ln$ HW	$\Delta\, ln$ WW	Employed	Unem.	NILF	Δ Over.
A. Full Sample w/ Sector FEs						
Chinese Penetration	-0.057	-0.097*	0.024	-0.0016	-0.022	-0.056
	(0.064)	(0.056)	(0.066)	(0.047)	(0.043)	(0.11)
Observations	216517	219462	305992	305992	305992	134539
B. Full Sample, No Sector FEs						
Chinese Penetration	-0.12	-0.11	-0.18**	0.0079	0.17**	0.074
	(0.099)	(0.11)	(0.090)	(0.017)	(0.081)	(0.049)
Observations	216517	219462	305992	305992	305992	134539
C. No College Education						
Chinese Penetration	-0.38***	-0.45***	-0.22*	-0.0018	0.22**	0.069
	(0.10)	(0.11)	(0.11)	(0.021)	(0.11)	(0.062)
Observations	129347	130208	189234	189234	189234	101088
D. At least some College						
Chinese Penetration	-0.020	0.0067	-0.13***	0.019	0.11***	0.093
	(0.079)	(0.089)	(0.041)	(0.016)	(0.040)	(0.073)
Observations	87170	89254	116758	116758	116758	33451
E. Top Third of Wages						
Chinese Penetration	0.033	0.062	0.033	-0.012	-0.021	0.061
	(0.12)	(0.11)	(0.036)	(0.017)	(0.025)	(0.16)
Observations	71667	72389	78607	78607	78607	26867
F. Middle Third of Wages						
Chinese Penetration	-0.078	-0.16*	-0.017	0.0052	0.011	-0.16
	(0.068)	(0.092)	(0.061)	(0.040)	(0.055)	(0.18)
Observations	73085	73435	81196	81196	81196	48655
G. Bottom Third of Wages						
Chinese Penetration	-0.064	-0.13	-0.042	0.023	0.019	0.0080
	(0.13)	(0.11)	(0.084)	(0.055)	(0.055)	(0.13)
Observations	71765	71997	84346	84346	84346	55687

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. Note that the sectoral FEs include separate sectoral dummies for before and after 2001, which is likely orthogonal to the China shock. Panel's B through G thus do not include sectoral FEs. The dependent variables are: (1) the log change in the Hourly Wage, (2) the log change in the Weekly Wage, (3) Employment (binary), (4) Unemployment (binary), (5) Not in the Labor Force (binary), (6) Change in overtime (the indicator variables measured 12 months later). The key variable of interest in this regression is Chinese import penetration.

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 5, 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 (.12 and .11) 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 isn't the case with changes in occupational status, however.

2.7 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 6 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

Table 5: 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.

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.

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

Table 6: 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.

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.

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

We then use the $TotalImpact_{it}$ as a regressor in our equation. In this case, we should 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.

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 Table 7, 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.

Table 7: Robustness: Impact of RER Shocks on the Labor Market, by Sex and Education

	$\Delta\, ln$ 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.")

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 a bit weaker than that 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.

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., and J. B. Jensen. 2012. "Evaluating estimates of materials offshoring from US manufacturing." *Economics Letters* 117 (1): 170–173.

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.

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.

Klein, S. Schuh, and R. K. Triest. 2003. "Job Creation, Job Destruction, and the Real Exchange Rate." *Journal of International Economics* 59, no. 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.

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

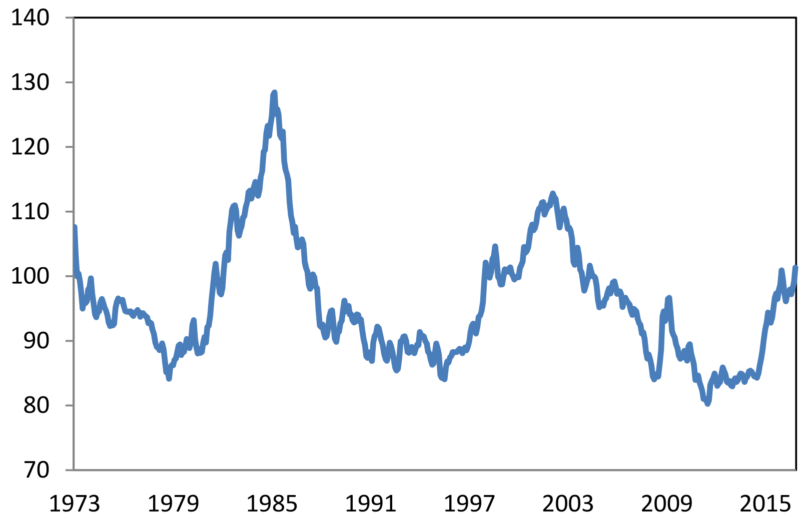


Figure 3: The Fed's Broad, Trade-Weighted RER Index

Source: Federal Reserve Board (via FRED).

Table 8: 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

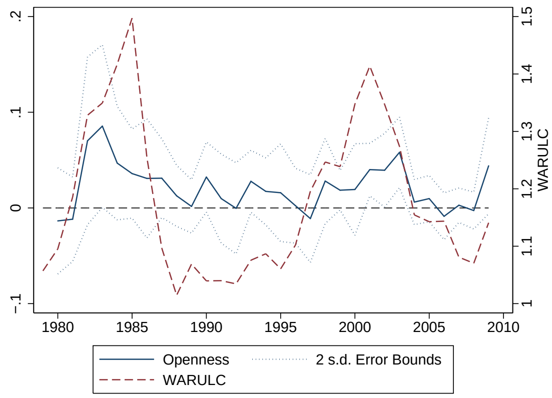
(b) Weekly Wages, Manufacturing

(c) Hourly Wages, All Sectors

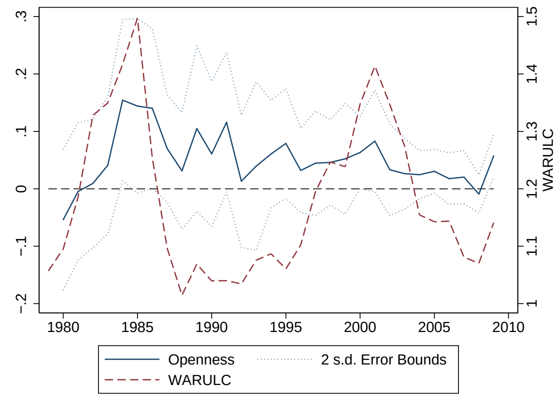
(d) Weekly Wages, All Sectors

Figure 4: The Evolution of Inequality in the MORG

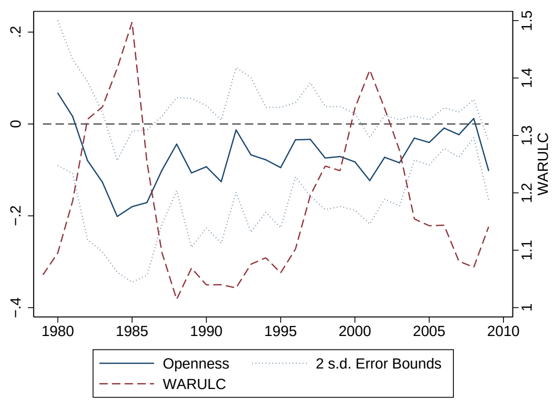
Panels (a) and (b) plot the evolution of the ratios of various percentiles of the income distribution for the manufacturing sector, and then Panels (c) and (d) do the same for all sectors. Thus panel (a) suggests that the ratio of wages between the 90th and 50th percentiles had increased a bit more than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



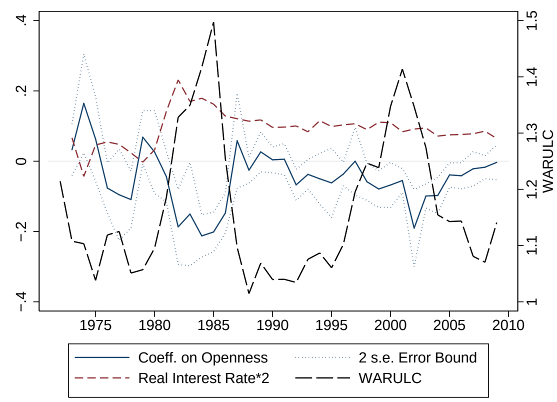
(a) Unemployment



(b) NILF



(c) Employed (MORG)



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

Figure 5: 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 9: Robustness of Table 1: No Sectors with N less than 30

	$ln\,\Delta$ HW	$ln\,\Delta$ WW	Employed	Unem.	NILF	Δ Over.
A. Full Sample						
L.3-7yr.Open.*ln(RER)	0.052	-0.025	-0.27**	0.16***	0.11	-0.24
	(0.090)	(0.11)	(0.11)	(0.062)	(0.088)	(0.20)
L.3-7yr.Avg.Openness	-0.055	-0.021	0.061	0.026	-0.087**	0.13
	(0.048)	(0.054)	(0.060)	(0.038)	(0.037)	(0.078)
$ln\,\Delta$ Demand	0.013	0.040*	0.10***	-0.079***	-0.023	0.040
	(0.021)	(0.022)	(0.027)	(0.022)	(0.016)	(0.037)
$ln\,\Delta$ VA/Prod. Worker	-0.013	-0.00046	-0.080***	0.032**	0.048***	0.028
	(0.018)	(0.016)	(0.026)	(0.014)	(0.018)	(0.030)
Observations	175334	177783	247537	247537	247537	108238
B. No College Education						
L.3-7yr.Open.*ln(RER)	-0.044	-0.086	-0.35**	0.19**	0.16	-0.16
	(0.11)	(0.15)	(0.17)	(0.080)	(0.12)	(0.24)
Observations	102982	103665	150793	150793	150793	80605
C. At least some College						
L.3-7yr.Open.*ln(RER)	0.10	-0.026	-0.18	0.12*	0.056	-0.42
	(0.19)	(0.16)	(0.14)	(0.070)	(0.096)	(0.43)
Observations	72352	74118	96744	96744	96744	27633
D. Top Third of Wages						
L.3-7yr.Open.*ln(RER)	-0.021	-0.11	-0.14*	0.13*	0.0091	-0.51
	(0.17)	(0.19)	(0.086)	(0.069)	(0.055)	(0.52)
Observations	58943	59549	64709	64709	64709	21840
E. Middle Third of Wages						
L.3-7yr.Open.*ln(RER)	0.23*	0.29*	-0.050	0.17	-0.12***	-0.32
	(0.14)	(0.17)	(0.12)	(0.11)	(0.040)	(0.35)
Observations	58690	58975	65262	65262	65262	38959
F. Bottom Third of Wages						
L.3-7yr.Open.*ln(RER)	0.044	-0.089	-0.25*	0.16**	0.095	-0.045
	(0.16)	(0.24)	(0.15)	(0.071)	(0.095)	(0.26)
Observations	57701	57890	67875	67875	67875	44797

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 10: Impact of Predicted Trade Shocks, by Wage Levels

	Δln HW	$\Delta\, ln$ WW	Employed	Unem.	NILF	Δ Over.
A. Full Sample						
Implied Trade Impact	-0.046	-0.10	-0.31***	0.24***	0.070	-0.21
	(0.080)	(0.088)	(0.088)	(0.051)	(0.063)	(0.17)
$\Delta\, ln$ Demand	0.028*	0.062***	0.078***	-0.071***	-0.0075	0.024
	(0.015)	(0.017)	(0.017)	(0.014)	(0.011)	(0.025)
$\Delta\, ln$ VA/Prod. Worker	-0.0086	-0.0076	-0.058***	0.026***	0.032**	0.045*
	(0.017)	(0.018)	(0.018)	(0.0100)	(0.013)	(0.026)
Observations	217271	220231	307070	307070	307070	135075
B. No College Education						
Implied Trade Impact	-0.14	-0.14	-0.39***	0.29***	0.11	-0.22
	(0.097)	(0.12)	(0.11)	(0.073)	(0.077)	(0.19)
Observations	129837	130704	189916	189916	189916	101494
C. At least some College						
Implied Trade Impact	0.057	-0.056	-0.24*	0.18***	0.068	-0.097
	(0.16)	(0.17)	(0.13)	(0.062)	(0.092)	(0.33)
Observations	87434	89527	117154	117154	117154	33581
D. Richest 3rd						
Implied Trade Impact	0.012	-0.16	-0.22**	0.15**	0.071	-0.41
	(0.18)	(0.21)	(0.090)	(0.074)	(0.048)	(0.44)
Observations	71800	72524	78752	78752	78752	26914
E. Middle 3rd						
Implied Trade Impact	-0.12	-0.11	-0.14	0.19**	-0.055	-0.13
	(0.13)	(0.15)	(0.10)	(0.080)	(0.065)	(0.26)
Observations	73302	73656	81435	81435	81435	48803
F. Bottom 3rd						
Implied Trade Impact	0.12	0.085	-0.49***	0.27***	0.22*	-0.12
	(0.18)	(0.24)	(0.17)	(0.080)	(0.11)	(0.21)
Observations	72169	72403	84826	84826	84826	56028

Notes: Errors clustered by sector and year in parentheses. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. There are 36 regressions total (each column in each panel is a separate regression), and each regression includes industry and year FEs over the period 1979-2010. The dependent variable in the first two columns are the log change in hourly and weekly wages, an in column (6) it is the change in whether one works overtime. In Panels B-F, the other controls are suppressed for space. The key variable, Implied Trade Impact, is the predicted impact on both import penetration and the export share based on RER movements. First, we ran a simple regression of log changes in import penetration (export share) on the log of WARULC and the log change in sectoral demand. We then took the predicted log change in sectoral import penetration (export share) by multiplying the coefficient on WARULC times WARULC and then multiplied by the previous level of import penetration (export share). Panel A includes the full sample, Panel B includes only those with no college education, C those with some college, D only those in the top third of wages, E only those in the middle third of wages, and Panel F is run only on those in the bottom third of wages.

5 Online Appendix

Table 11: The Impact of Real Exchange Rate Shocks on the Labor Market: Shortened Panel, 1979-2002

	$ln\,\Delta$ HW	$ln\,\Delta$ WW	Employed	Unem.	NILF	Δ Over.
A. Full Sample						
L.3-7yr.Open.*ln(RER)	-0.0083	-0.084	-0.23***	0.12***	0.11**	-0.32**
	(0.055)	(0.072)	(0.067)	(0.033)	(0.047)	(0.14)
L.3-7yr.Avg.Openness	-0.025	-0.0022	0.037	0.027	-0.064***	0.14***
	(0.040)	(0.048)	(0.037)	(0.026)	(0.023)	(0.053)
$ln\,\Delta$ Demand	0.0093	0.044**	0.080***	-0.071***	-0.0086	0.029
	(0.018)	(0.021)	(0.022)	(0.018)	(0.012)	(0.030)
$ln\,\Delta$ VA/Prod. Worker	-0.0047	-0.0036	-0.060***	0.027***	0.033**	0.043*
	(0.015)	(0.015)	(0.019)	(0.0097)	(0.014)	(0.024)
Observations	216517	219462	305992	305992	305992	134539
B. No College Education						
L.3-7yr.Open.*ln(RER)	-0.098	-0.18**	-0.27***	0.14***	0.13*	-0.33**
	(0.068)	(0.089)	(0.097)	(0.047)	(0.071)	(0.14)
Observations	129347	130208	189234	189234	189234	101088
C. At least some College						
L.3-7yr.Open.*ln(RER)	0.087	0.044	-0.17**	0.093**	0.073	-0.24
	(0.15)	(0.16)	(0.084)	(0.037)	(0.063)	(0.33)
Observations	87170	89254	116758	116758	116758	33451
D. Top Third of Wages						
L.3-7yr.Open.*ln(RER)	-0.085	-0.19**	-0.13**	0.094**	0.041	-0.71*
	(0.085)	(0.087)	(0.059)	(0.046)	(0.034)	(0.40)
Observations	71667	72389	78607	78607	78607	26867
E. Middle Third of Wages						
L.3-7yr.Open.*ln(RER)	0.0011	-0.032	-0.012	0.11*	-0.095***	-0.33
	(0.092)	(0.11)	(0.072)	(0.063)	(0.030)	(0.20)
Observations	73085	73435	81196	81196	81196	48655
F. Bottom Third of Wages						
L.3-7yr.Open.*ln(RER)	0.091	0.011	-0.21**	0.12***	0.083	-0.13
	(0.14)	(0.19)	(0.090)	(0.031)	(0.073)	(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. 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. This is a replication of 1 using a shorter sample with a consistent panel classification.

Table 12: Including Services, Impact of Occupational Exposure

	(1) Manuf. Only	(2) All Sectors	(3) All Sectors	(4) No College	(5) No College
L.Openness	0.00530	0.00530		0.0194	
	(0.0227)	(0.0227)		(0.0275)	
L.Openness*L.ln(WARULC)	-0.00685	-0.00685		-0.0925*	
	(0.0500)	(0.0500)		(0.0474)	
L.Occ.Openness			-0.0173		-0.00266
			(0.0191)		(0.0216)
L.Occ.Openness*L.ln(WARULC)			0.130		0.0582
			(0.106)		(0.112)
Observations	239850	239850	239850	142041	142041

Two-way clustered standard errors in parentheses. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include industry and year fixed effects over the period 1979-2010. The dependent variable is the log change in wages.

Table 13: The Impact of RER Shocks: Using the Fed's Broad Trade-Weighted RER Index

	$ln\,\Delta$ HW	$ln\,\Delta$ WW	Employed	Unem.	NILF	Δ Over.
A. Full Sample						
L.3-7yr.Open.*ln(Fed's RER)	0.032	-0.049	-0.22**	0.11***	0.12	-0.31**
	(0.065)	(0.080)	(0.097)	(0.040)	(0.072)	(0.14)
L.3-7yr.Avg.Openness	-0.029	-0.014	0.013	0.041*	-0.053**	0.11**
	(0.039)	(0.045)	(0.035)	(0.024)	(0.022)	(0.044)
$ln\,\Delta$ Demand	0.0092	0.044**	0.078***	-0.070***	-0.0081	0.027
	(0.018)	(0.021)	(0.023)	(0.018)	(0.012)	(0.034)
$ln\,\Delta$ VA/Prod. Worker	-0.0044	-0.0032	-0.059***	0.026***	0.033**	0.044*
	(0.015)	(0.015)	(0.019)	(0.0097)	(0.014)	(0.027)
Observations	222021	225111	313308	313308	313308	137618
B. No College Education						
L.3-7yr.Open.*ln(Fed's RER)	-0.042	-0.12	-0.28**	0.14**	0.14	-0.32**
	(0.097)	(0.086)	(0.13)	(0.059)	(0.10)	(0.15)
Observations	132028	132922	192854	192854	192854	103124
C. At least some College						
L.3-7yr.Open.*ln(Fed's RER)	0.12	0.060	-0.13	0.057	0.070	-0.26
	(0.17)	(0.16)	(0.11)	(0.046)	(0.077)	(0.36)
Observations	89993	92189	120454	120454	120454	34494
D. Top Third of Wages						
L.3-7yr.Open.*ln(Fed's RER)	-0.048	-0.10	-0.084	0.041	0.043	-0.51
	(0.098)	(0.10)	(0.075)	(0.054)	(0.049)	(0.43)
Observations	73535	74293	80635	80635	80635	27415
E. Middle Third of Wages						
L.3-7yr.Open.*ln(Fed's RER)	0.14	0.098	0.048	0.083	-0.13**	-0.31
	(0.10)	(0.12)	(0.10)	(0.076)	(0.053)	(0.24)
Observations	74877	75240	83144	83144	83144	49747
F. Bottom Third of Wages						
L.3-7yr.Open.*ln(Fed's RER)	0.092	-0.029	-0.17	0.13***	0.042	-0.21
	(0.11)	(0.19)	(0.13)	(0.044)	(0.10)	(0.21)
Observations	73609	73850	86455	86455	86455	57126

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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.

6 Online Data Appendix

6.1 Additional Notes on RER Indices (Online Appendix, Cont.)

Note: This section is reprinted, with permission, from the Online Appendix to 2016b.

The main measure of the real exchange rate used in this paper is the Weighted Average Relative Unit Labor Cost (WARULC) index designed by 2016a to address the shortcomings of the IMF's Relative Unit Labor Cost (RULC) index. 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 (Japan still held a 20% weight in the 2000s while China was excluded), and (4) uses country-specific deflators, which can become biased over time without the benefit of multiple benchmarks. (This last point is the same problem that afflicted older versions of the Penn World Tables predating version 8.0).

Note that most real exchange rate indices, such as those produces by the Federal Reserve, the OECD, the BIS, and many other central banks, also use time-varying trade weights, and that time-varying trade weight indices are often used in studies on the impact of RER movements on manufacturing, such as in Klein et al. (2003b).

Campbell (2016a) introduced WARULC, a simple weighted-average of RULCs which includes China, uses time-varying trade-weights, and also uses multiple-benchmarking of country-specific productivity series using PWT v8.0 methodology. This last point will be subtle for general readers, and so I would refer interested parties to Campbell 2016a.

The WARULC index is computed as

$$I_{US,t}^{WARULC} = \prod_{i=1} \Big(\frac{ULC_{US,t}}{ULC_{i,t}}\Big)^{\Omega_{i,t}}, \tag{6.1}$$

where

$$ULC_{i,t} = \frac{w_{i,t}}{e_{i,t}} / \frac{Y_{i,t}}{PPP_{i,t}}, \tag{6.2}$$

and where $\Omega_{i,t}$ 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 (equal to 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. Manufacturing PPP data were computed using ICP data for benchmark years, and then interpolated in between using manufacturing deflators from the OECD, or country-specific sources in the case of China.

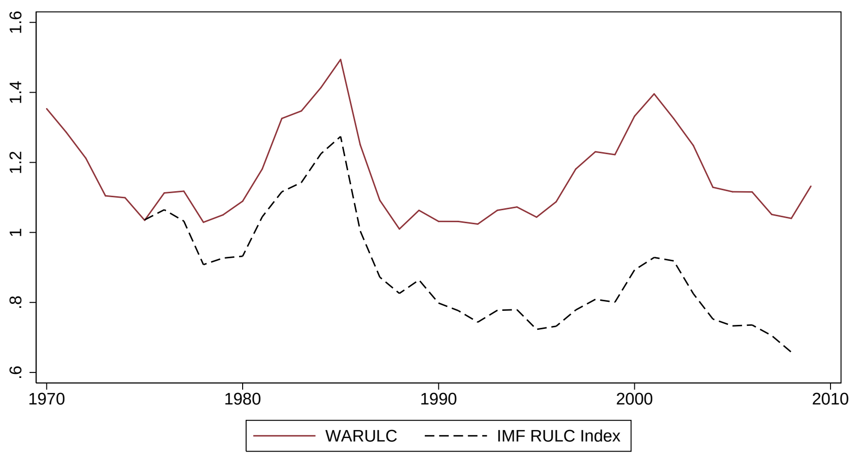


Figure 6: WARULC vs. IMF RULC Index

Sources: Campbell (2016) and the IMF

Both measures are plotted in Figure 6 vs. the IMF's RULC index. The IMF's index suggests a steady depreciation of US relative unit labor costs over the period, implying that US manufacturing has become steadily more competitive since the 1970s. WARULC, by contrast, implies that US manufacturing became less competitive in the early 2000s.

It turns out that all four of the adjustments from the IMF's RULC to WARULC are important. For example, changing the indexing method while using fixed trade-weights would yield an index almost identical to the IMF's index, even if China is included. Without the multiple benchmarking, WARULC would still have a more negative slope. I refer readers interested in the differences in these indices when some of these adjustments are left out to 2016a.

I also consider alternative measures of relative prices. Figure 7 compares several state-of-the-art measures of relative prices which use PWT v8.0 data and methodology to more commonly used measures provided by the Federal Reserve Board and IMF. Indexing the IMF's RULC series to begin at the same level as the WARULC index in 1975, the IMF's index implies that US ULCs were nearly 40% lower than trading partners by the 2000s, which is implausible. I have also plotted an updated version of Weighted Average Relative Prices (WARP) (from Thomas et al. (2008)) using PWT v8.1, and Penn-Adjusted Weighted Average Relative Prices (PWARP), introduced in Campbell

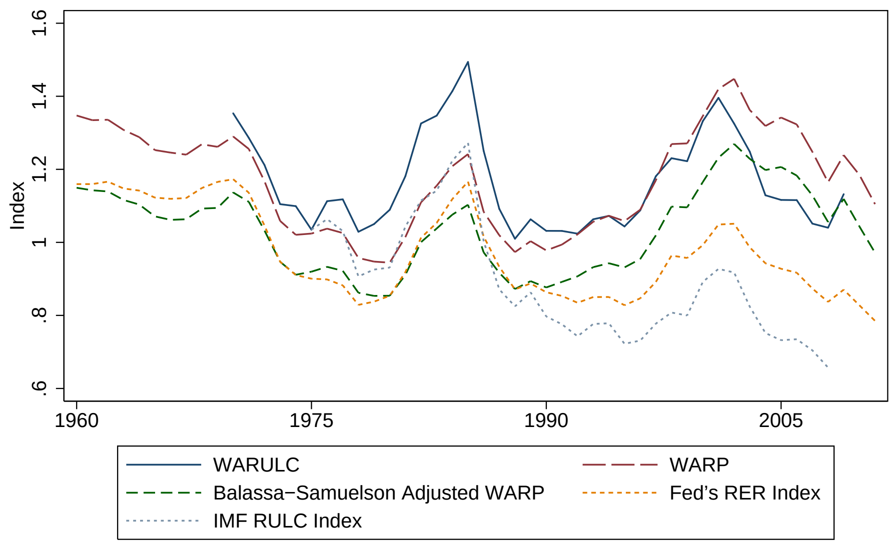


Figure 7: Comparing Various Exchange Rate Measures
Source: Campbell (2016) and the IMF

(2016a). The Federal Reserve's CPI-based Broad Trade-Weighted Real Exchange Index, plotted in yellow, also implies that the dollar tended to depreciate over the period. The three "Weighted Average Relative" (WAR) indices all yield broadly similar results, although there are certainly differences in the details and in the implied degree of overvaluation. One of the differences is that the other WAR measures show a slower dollar depreciation in the mid-2000s, which is consistent with the finding that relatively open manufacturing sectors continued to fair poorly in this period (**??**). Another difference is the slightly more negative overall slope of WARULC, which is due to the declining share of labor income in manufacturing in the US relative to many other developed countries, which appears to be a broad-based phenomenon in manufacturing not caused by outsized changes in a small number of sectors.

6.2 Creating New Measures of Imported Intermediate Inputs

Given the rise of China, which has been linked to the decline in American Manufacturing, it is understandable that there is considerable academic and public interest in "offshoring". It is surprising, then, that there are no publicly-available, annual measures of offshoring at the detailed sector level (generally proxied by manufactured imported intermediate goods) of which we are aware. Thus, we have filled this gap by providing new estimates of imported intermediate goods at the 4-digit SIC level from 1972 to 2009, and for NAICS from 1992 to 2010. In this portion of the online Appendix, we detail the construction of our indices, and provide a detailed user-guide.

6.2.1 Creation of SIC Series

- In the first step, we downloaded the raw Input-Output Use tables for the benchmark years, 1972, 1977, 1982, 1987, and 1992 from the BEA. Then we created crosswalks between the IO SIC codes and the SIC codes used by the Annual Survey of Manufactures (ASM). We then combined the IO data with sectoral data from the ASM on materials inputs and import data by sector from WITS. Often we needed to apportion data for one IO SIC sector to several ASM SIC sectors. When we did this for the using sectors, we apportioned it based on the relative size of materials usage based on the ASM. For commodity (providing) sectors, we apportioned intermediate imports based on the ratios of imports in each ASM SIC sector.

- We then make use of the "proportionality" assumption which is used by the BEA in their later estimates of imported intermediate inputs, and also by Feenstra and Hanson (1999) – hereafter FH. While this assumption is not perfect, Feenstra and Jensen (2012) showed that this assumption is mostly correct, as they find that at the 3-digit level, direct estimates of imported intermediate inputs at the three digit level from the Linked/Longitudinal Firm Trade Transaction Database (LFTTD), which do not rely on the proportionality assumption, have a correlation of .68 with data that does rely on this assumption, or a correlation of .87 when the shares are value-weighted. Thus we assume that a sector consumes the same fraction imports of a commodity as it does of domestic consumption. From FH, for each industry i, its sum of intermediate inputs from sector j is computed as:

$$\sum_j (input\, purchases\, of\; j\; by\, industry\; i) * \frac{imports\, of\, j}{consumption\, of\, j} \tag{6.3}$$

  where consumption of good j is measured by: $shipments + imports - exports$.

- There were also a small number of cases where the right-hand term, also known as import penetration, is either less than zero or greater than one. This could happen if, for example, imports and exports were equal and greater than shipments, or if exports were greater than shipments plus imports (perhaps indicating a an inconsistency with the data). One option would be to relax the implicit assumption that imports are not re-exported. While our import data is ostensibly imports for domestic production, and not for re-export, for some industries, this assumption is clearly not met. At the same time, for 1992, only 5 out of 462 sectors yield problematic figures for import penetration, and so for these sectors, we made a second "proportionality" assumption, assuming that the share of imports that are re-exported are equal to $\frac{imports}{imports+shipments}$. Thus, the imports for domestic use will be given by the following formulation:

$$M_{Domestic} = M\left(1 - \left(\frac{X}{M + Shipments}\right)\right) \tag{6.4}$$

Where M = Imports, X = Exports, and $M_{Domestic}$ are imports for domestic consumption. The intuition for this formula is that total imports are multiplied by the share of imports plus domestic shipments which are consumed at home, equal to one minus the share of imports plus shipments which are exported. Thus, in these cases, we recalculated equation (3) replacing "imports of j" with $M_{Domestic}$ from equation 4, and consumption using the formula:

$$shipments + M_{Domestic} - X_{Domestic}, \tag{6.5}$$

where $X_{Domestic}$ are exports produced domestically, equal to exports times shipments plus imports. This construction of import penetration has the benefit that it always varies between 0 and 1. However, it also has the downside that if, in reality, a larger share of exports come from domestic production than from imports, then it will underestimate intermediate imports. If we use this formulation for only the 5 "problem cases" then we will be changing the rank order of import penetration across sectors. (One solution might be to just assume an import penetration ratio of 1 in these cases, but given that domestic production was substantial in each of these 5 cases, this would appear to be counterfactual). Thus, the last step is to make a rank-order adjustment, assuming that, for each of these 5 sectors, their rank order in this alternative calculation, which yields generally lower estimates for import penetration, is their true rank, and then adjusting upwards by the ratio of the average import penetration using equation 6.3 with that based on equation 6.5, which in practice is a 20% upwards adjustment for these observations. In this way, their rank in terms of import penetration based on equation 6.5 is roughly preserved. The original and reformed series are compared below in Figure 8.

- We did find that, for a small handful of sectors, the imported intermediate inputs calculated this way are too large. For some commodity sector-using sector combi-

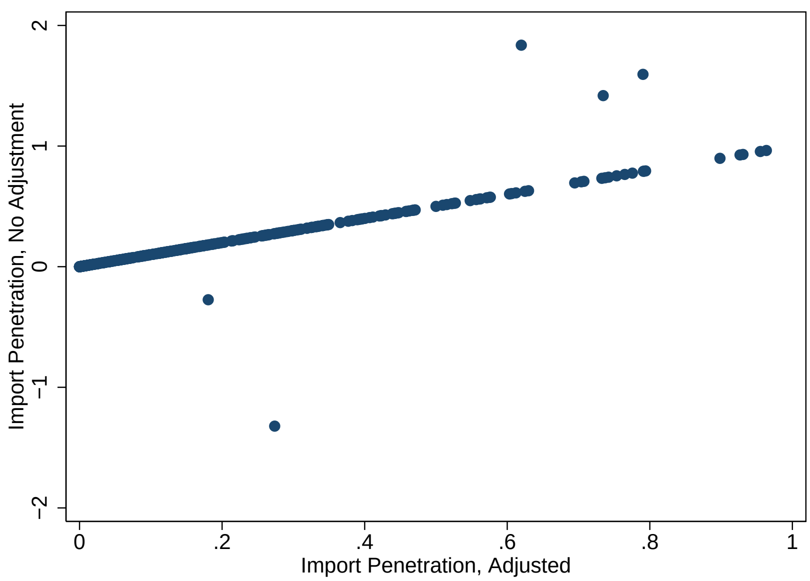


Figure 8: Import Penetration vs. Adjusted Import Penetration, 1992

Note: "Adjusted Import Penetration" adjusts sectors with implausible values for re-exports. With this adjustment, import penetration is forced to vary between zero and one.

nations, the amount of imported intermediate imports we record are larger than the total imports recorded in that particular commodity sector (and this does not seem to depend on the data source – as we tried USITC data, and also WITS data). In addition, when we sum intermediate import uses by commodity sectors (thus, for each commodity sector we add up all of the uses across using sectors), we find that for roughly 12% of sectors, the intermediate imports are again "too large", in that their value is greater than total recorded imports. This is true both for the imported intermediate inputs provided at the NAICS level by the BEA itself, and for our our projections for imported intermediate inputs which were taken from the raw data. While the IO tables are constructed using data from the ASM, there are a variety of reasons why this might be the case. The ASM data are meant to be annual, as are the IO data, but one can imagine that if a good is produced in January, then the intermediates purchased likely came in the previous year, while intermediates purchased at the end of the year will likely go towards production in subsequent years. Given that some volatility in manufacturing is to be expected, this could be one factor which generates implausibly large (or small) estimates for some years. It is also the case that the "Make" table is not entirely consistent with the variable "shipments" from the ASM, although the two variables are very highly correlated. We tried using materials shipments from the

ASM, and then multiplying this amount by the ratio of using sector i's make to each use of commodity j, but this led to even more inconsistent results. We also considered capping intermediate imports for any commodity sector at the total recorded imports, but eventually, we decided against this on the grounds that it is arbitrary, and for our purposes we are mostly interested in the degree of reliance on intermediate imports, and so we decided to preserve this feature of the data.

- Equation 6.3 can be used to derive the intermediate import matrix for the benchmark years. To extrapolate for the other years, we first modeled the evolution of intermediate inputs based on changes in materials usage of the using sectors and changes in import penetration of commodity sectors. Thus, we estimate:

$$ln(MPI)_{ijt} = \rho L5.ln(MPI)_{ij,t-1} + \beta_1 ln\Delta ImportPen._{jt} + \beta_2 ln\Delta Materials_{it} + \epsilon_{ijt} \tag{6.6}$$

where MPI_{ijt} = imported intermediate imports of commodity j used by sector i at time t, $ImportPen.$ is import penetration, "Materials" is materials used by sector i, and we have suppressed the constant. The results are displayed in Column 1 of Table 14, which show that lagged intermediate imports enter with a coefficient equal to one, and that changes in import penetration and materials inputs are highly predictive of change in imported intermediate inputs between the benchmark years. Given the lagged coefficient of 1, we now estimate the model in log changes:

$$ln\Delta MPI_{ijt} = \beta_1 ln\Delta ImportPen._{jt} + \beta_2 ln\Delta Materials_{it} + \epsilon_{ijt} \tag{6.7}$$

We run this model for the full period (column 2), and for each of the years individually in Table 14, and find that the coefficients are roughly the same, and have considerable predictive power in terms of r-squared, of .44 on the full sample.

To test this model out of sample, we ran the panel while excluding the year 1992, and then we generated out-of-sample predictions using realized changes in imports, shipments, and materials costs of the using sector. Below in Figure 9 we show the out-of-sample results. On the whole, it looks like our model validates fairly well. The mean absolute error is .73, while regressing our predictions on the actual data of log changes in imported intermediate inputs yield a coefficient of 1.02 (with an error of .07), and an R-squared of .54. Thus, these results appear to

Table 14: Modeling the Evolution of Intermediate Imports: SIC

	(1) ln(MP Inputs)	(2) ln5yr. Δ MPI	(3) 1992	(4) 1987	(5) 1982	(6) 1977
L5.ln(MP Inputs)	1.00***					
	(0.0049)					
ln 5yr. Δ Import Pen.	1.13***	1.14***	1.15***	1.16***	1.08***	1.05***
	(0.060)	(0.053)	(0.084)	(0.076)	(0.100)	(0.12)
ln 5yr. Δ Matcost	0.44***	0.46***	0.57***	0.72***	0.34***	0.45***
	(0.037)	(0.053)	(0.14)	(0.045)	(0.11)	(0.091)
Observations	110148	110148	19964	32965	30570	26649
r2	0.96	0.44	0.54	0.52	0.34	0.27

Notes: Standard errors clustered by commodity-using sector pair in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. The dependent variable is the log of imported intermediate inputs in column (1), and the log 5 year change of imported intermediates in columns (2)-(6). Column (2) is run on the full sample, while columns (3)-(6) are run on individual years. These regressions were run without a constant.

be a method we can use to extrapolate to years in which there is no benchmark data. While there is significant error in this method, note that when we do our actual extrapolation, since there are 5 years in-between benchmarks, we'll generally never have to extrapolate more than 3 years, and even then, we can form multiple estimates derived from "backward" and "forward" estimation.

- Using these regression coefficients, we first fill in missing data in the benchmark years simply by using the regression predictions from column (2). (Except for 1972, there we run the same regression backwards, where the dependent variable is now the change in imported inputs from the subsequent benchmark, and predict the missing values for the initial benchmark that way.)

- Then, we extrapolate forward and backward from the base years. For the years after 1992, we use the formula:

$$MPI_{ijt} = MPI_{ij,t-1} * exp(1.14 * ln\Delta ImportPen. + .46 * ln\Delta MaterialsCost) \quad (6.8)$$

  For years in between benchmark years, we use a weighted average of the forward extrapolation from the previous benchmark year (using formula 6.8, and the backwards extrapolation from the subsequent benchmark year (which also uses a formula similar to 6.8 to extrapolate backwards using the regression coefficients).

$$MPI_{ij,t+s} = \frac{k-s}{k} MPI^F_{ij,t+s} + \frac{s}{k} MPI^B_{ij,t+s} \quad (6.9)$$

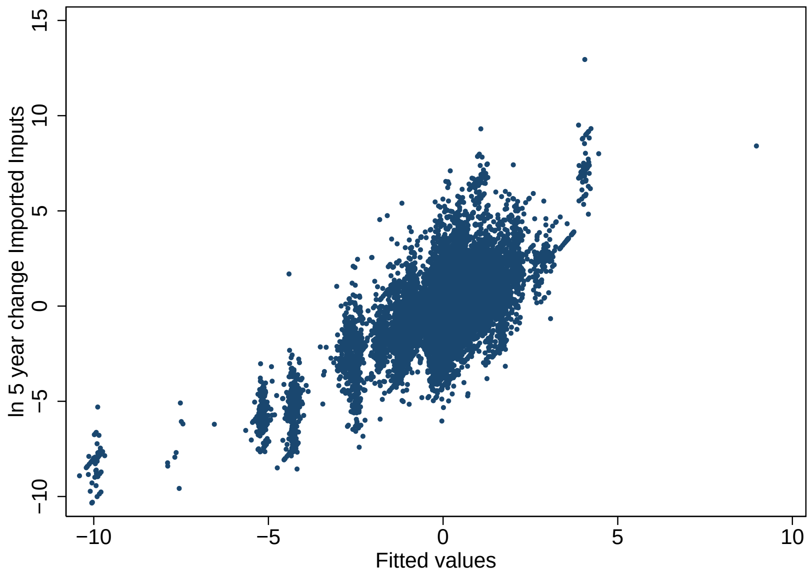


Figure 9: Out-of-Sample Test of Intermediate Imports, 1992

Notes: On the x-axis we have plotted the model predictions for intermediate imports (used by sector i of commodity j), which is based on changes in import penetration of the commodity and in total materials usage of the using sector.

where t is a benchmark year, k is the number of years between benchmarks, and s is the number of years after the last benchmark. Thus, for 1988, which is one year after the 1987 benchmark, this estimate gives the forward estimate a weight of .8, and the backwards estimate from the 1992 benchmark a weight of .2. However, in some cases, there may be 10 years between any two benchmarks, such as between the 1972 and the 1982 benchmark. In this case, in 1973, the forward estimate from 1972 will be given a weight of .9 and the backward estimate extrapolating from 1982 will be given a weight of .1.

- Comparing our results to the Feenstra and Hanson (1999) estimates for 1990 in Figure 10, the correlation between the two is pretty good, albeit not perfect. Regressing the log of the Feenstra estimates on our own, we get an R-squared of .82, while our estimates are bit smaller on average.

- We then summed across commodities to arrive at the total amount of imported intermediate inputs for each SIC using sectory by year. Following 1999, we also provide estimates of "narrow" offshoring, defined as total intermediate inputs within the same 2-digit SIC sector. Lastly, we converted these estimates to the MORG version of SIC for the 1979 to 2002 period.

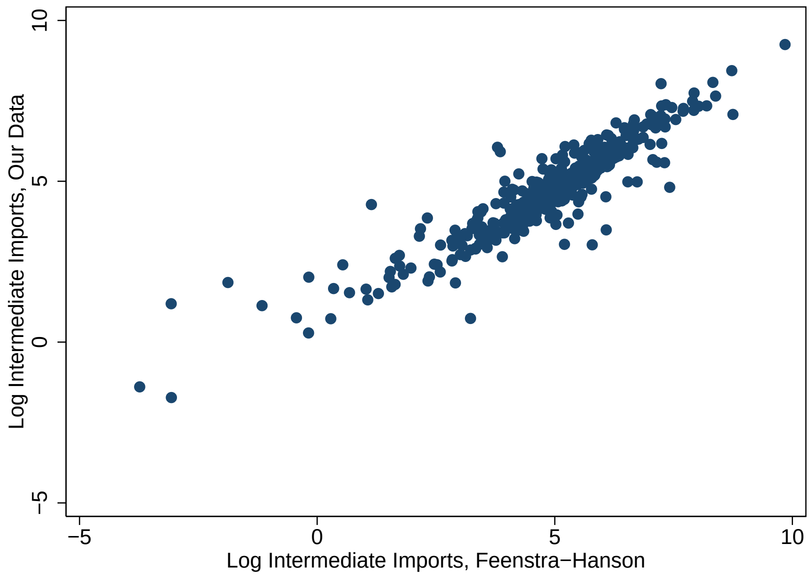


Figure 10: Feenstra-Hanson Comparison, 1990

6.2.2 Creation of NAICS Series

- Intermediate import data were downloaded from the BEA: (including from here: http://www.bea.gov/industry/iedguide.htm) for the years 1997, 2002, and 2007. These were the only years in which intermediate imports were computed directly by the BEA. The 2012 data will become available in 2017.[13]

- These data used NAICS codes which are specific to the IO database (we call it IONAICS), which differ slightly each year. Thus, we created a crosswalk between the IONAICS codes in each year and NAICS codes from the Annual Survey of Manufactures (in order to match this data to ASM data and use it as a panel). As, in many cases there is one IONAICS sector which matches to several ASM NAICs sectors. Thus, it is necessary to partition the intermediate inputs data to the multiple NAICS sectors based on (1) intermediate materials consumption for the using sectors, and (2) imports for the commodity sectors. To make a concrete example, in 2002, the IONAICS sector 315100 is matched to two ASM NAICs sectors: 314991 and 314999. Thus, if a given using sector used 10 million worth of inputs from this sector, then we divided those imports based on the relative share of imports of each of those sectors. Thus, if 314991 had total imports of 400 and sector 314999 had imports of 600, then we would do a 40/60 split. For the using

13. We thank Robert Correa, and Economist at the BEA, for providing this information.

sectors, we would do the same thing, only using materials input usage.

- Next, as we did with SIC, we tested to see if we could predict changes in intermediate inputs in the data provided by the BEA in order to extrapolate out of sample. Column (1) of Table 16 shows the simple model where we regress log imported inputs (commodity j used by sector i) on its 5 year lag and changes in import penetration measured by commodity and changes in materials cost measured by the using sector. Although the point estimate of the lag is not so close to one, the point of this exercise is to get coefficients which can help us predict the evolution of intermediate inputs solely based on changes in other variables so that we can extend the series to additional years. Thus, in column (2), we replace the left-hand-side variable with the log change in imported intermediate imports (MPI). Again, the coefficients look broadly similar to what we had previously in Table 14, although the coefficient on materials usage is a bit higher and the coefficient on import penetration is a bit lower. Unfortunately, when we run this regression on invdividual years, it falls apart. Perhaps this is due to the massive volatility experienced by the manufacturing sector in this period, which experienced a collapse, or due to the fact that, since we are now using the BEA's estimates for imported intermediates, we now have less control over the exact data generating process. In column (4), we try instead an alternate model, in which which swap out import penetration for simply the log change in imports. This tends to do better, as in this case, there is at least a small amount of out-of-sample predictive ability (see 11 below).

- Given the somewhat rough out-of-sample test results in Figure 11, one option would certainly just to do a linear extrapolation. If the period from 1997 to 2010 did not include any big events, this might be advisable. However, this period also includes a major financial crisis and recession, and we suspect that, in this case, the collapse in both materials used and in imports in the 2009-2010 period would have had to have been reflected in fewer intermediate inputs. In addition, our own out-of-sample tests do both have a lower mean absolute error as compared to either a random walk or an extrapolation of the previous trend, both for 2002 and for 2007. In addition, part of the reason the out-of-sample tests here may be worse is that the in-sample portion of the model is only one year in each case. When we actually do the extrapolation, we'll be using twice as much data, which means that the models performance should be improved.

Table 15: Modeling the Evolution of Intermediate Imports: NAICS

	(1) ln(MP Inputs)	(2) ln5yr. Δ MPI	(3) 2007	(4) Full	(5) 2007	(6) 2002
L5.ln(MP Inputs)	0.75***					
	(0.0047)					
ln 5yr. Δ Import Penetration	0.84***	0.83***	-0.035			
	(0.032)	(0.035)	(0.050)			
ln 5yr. Δ Matcost	1.40***	1.17***	1.39***	0.76***	0.99***	0.71***
	(0.034)	(0.037)	(0.038)	(0.037)	(0.040)	(0.077)
ln 5yr. Δ Imports				0.99***	0.64***	1.35***
				(0.024)	(0.029)	(0.038)
Observations	12452	12452	6419	12452	6419	6033
r2	0.71	0.11	0.18	0.19	0.23	0.18

Notes: Standard errors in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. The dependent variable in column is the log of imported intermediate inputs in column (1), and the log 5 year change of imported intermediates in columns (2)-(6). Columns (2) and (4) were run on the full sample, while columns (3) and (5) were run just on 2007 and column (6) was run only using data from 2002. The constant was suppressed in each of these regressions.

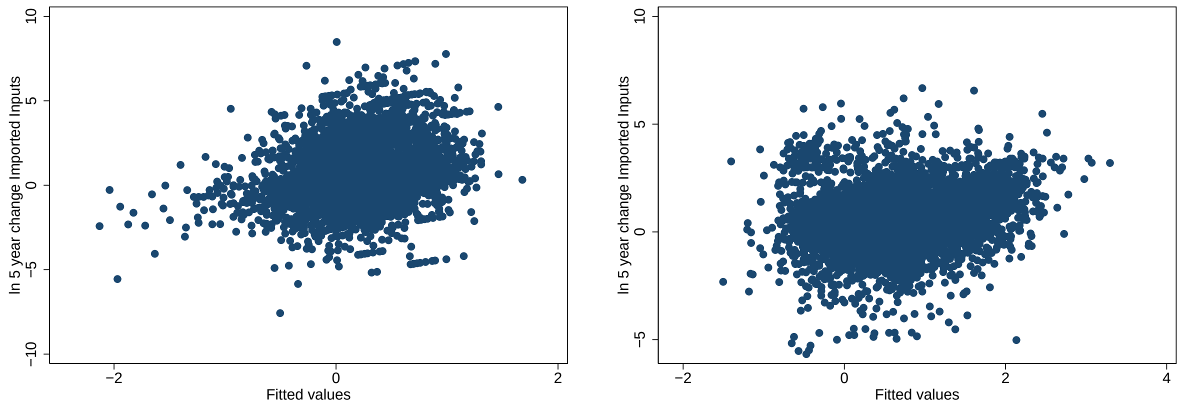
Table 16: Out-of-Sample Tests

	Mean Absolute Error	
	2002	2007
Random Walk	1.27	1.11
Random Walk with Drift		1.66
Our Model	1.17	.95
N	6419	6033

Notes: The Mean Absolute Error is compared for each model for 2002 and for 2007. The "Random Walk" simply uses the estimate for imported intermediate inputs from the previous benchmark. The "Random Walk with Drift" uses the time trend from 1997 to 2002 to predict imported intermediates in 2007. "Our model" uses our regression results to test out-of-sample.

- In the first part of the extrapolation, we once again fill in the missing observations for the benchmark years using the regression in column (4) of Table 16.
- Then, we fill in the remainder of the years in the exact same way as with the SIC indices. For the years after the 2007 benchmark, we use the formula:

$$MPI_{ijt} = MPI_{ij,t-1} * exp(.99 * ln\Delta Imports + .76 * ln\Delta MaterialsCost) \qquad (6.10)$$



(a) 2002

(b) 2007

Figure 11: Out-of-Sample Tests of Intermediate Input Growth

Notes: The fitted values are derived from a regression of log changes in imported inputs on log changes in imports and materials costs.

For years in between benchmark years, we use a weighted average of the forward extrapolation from the previous benchmark year (using formula 6.10, and the backwards extrapolation from the subsequent benchmark year (which also uses a formula similar to 6.10 to extrapolate backwards using the regression coefficients).

$$MPI_{ij,t+s} = \frac{k-s}{k} MPI^{F}_{ij,t+s} + \frac{s}{k} MPI^{B}_{ij,t+s} \tag{6.11}$$

where t is a benchmark year, k is the number of years between benchmarks, and s is the number of years after the last benchmark. Thus, for 1998, which is one year after the 1997 benchmark, this estimate gives the forward estimate a weight of .8, and the backwards estimate from the 1992 benchmark a weight of .2. However, in some cases the 2002 benchmark data is simply missing, in which case there are 10 years between benchmark observations. In this case, in 1998, the forward estimate from 1997 will be given a weight of .9 and the backward estimate extrapolating from 2007 will be given a weight of .1.

- We then summed across commodities to arrive at the total amount of imported intermediate inputs for each NAICS using sectory by year. Once again, we also included "narrow" estimates of offshoring by summing up imported inputs for each using sector for all the commodities within the same 3-digit NAICS classification. Lastly, we converted these estimates to the MORG version of NAICs for the 2003 to 2010 period.