Trade Shocks, Taxes, and Inequality†

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Abstract

We study the impact of trade shocks on inequality using newly constructed micro and macro data. First, we use 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 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. Second, sectors most exposed to trade shocks do not experience relative increases in inequality. Third, using aggregate international data for 31 countries, we find that various trade shocks, such as increases in trade with China, are not generally correlated with changes in the income distribution. Instead, using new historical data, we confirm a close connection between top marginal tax rates and top income shares, and find that the level of top marginal tax rates impacts *changes* in the top 1% share of income, implying that top income shares are a function of historical marginal tax rates.

JEL Classification: F10, F16, F41, N60, L60
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The US has experienced a dramatic increase in inequality since 1980, which roughly coincides with the advent of personal computing, the Reagan tax cuts, and a large increase in international trade. Figure 1 shows that since 1988 the percentile of the world income distribution that corresponds to working class incomes in rich countries has stagnated, while the percentile corresponding to the middle classes in countries such as China have prospered (from Lakner and Milanovic, 2013).[1]

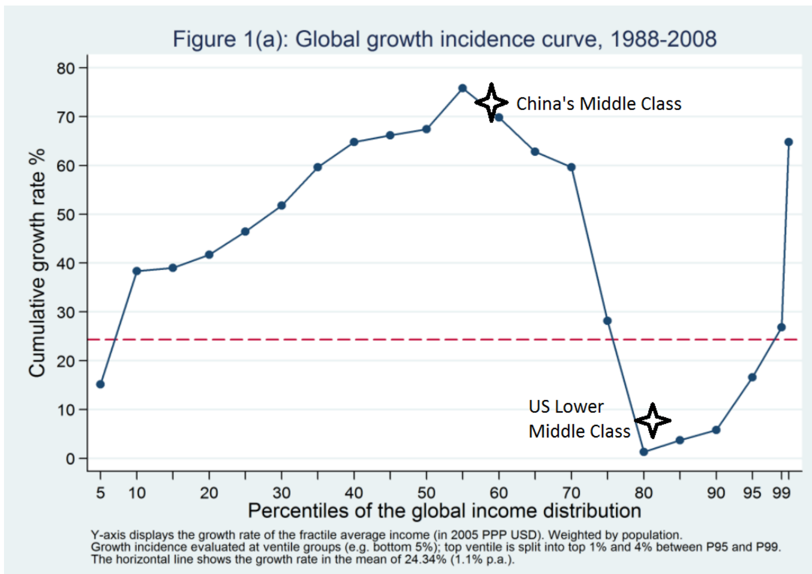


Figure 1: Changes in the Global Income Distribution

Notes: This chart is taken from Lakner and Milanovic 2013, with the stars for China's middle class and the US lower middle class added.

Meanwhile, recent research, including Autor et al. (2013), Pierce and Schott (2015), and Ebenstein et al. (2012) indicate that the rise of China had a surprisingly large negative impact on American manufacturing in the early 2000s. Campbell (2016b) finds that the dollar's sharp appreciation in the late 1990s and early 2000s also contributed to the sudden collapse in manufacturing employment, while Acemoglu et al. (2015) argue that the "sag" in *overall* U.S. employment in the 2000s was partly caused by the collateral damage from Chinese import competition.

In this paper, we investigate the extent to which trade shocks, such as that caused by the rise of China, have been responsible for the rise in inequality in the US and other major economies since 1980 using several distinct, complementary approaches using both micro and macro-level data. There is, of course, already a very large literature on the impact of *globalization* generally on wages and inequality. The most closely related papers to our first approach are those which use individual-level micro data. Recent

1. Via Paul Krugman, who has written about this several times on his blog, including on 12/1/2015.

examples include Ebenstein et al. (2014) and 2015, who also make use of the Current Population Survey's (CPS) Merged Outgoing Rotation Group (MORG) data, and find that workers in occupations more exposed to offshoring have suffered disproportionately. Autor et al. (2014) find that low-wage workers in sectors more exposed to Chinese competition had also been adversely affected using Social Security data, with significant effects from 1999.[2] We complement this literature by exploiting the fact that dramatic changes in real exchange rates in the US since 1980 imply that "globalization" reached US shores in two distinct waves (Figure 2), in the 1980s and in the late 1990s. When the US dollar appreciates, imports surge and exports stagnate. However, not all sectors of the economy, or even within manufacturing, are equally exposed. Cement manufacturing is inherently less exposed than automobile production. Thus, our first method is a repeated difference-in-difference approach, asking whether workers in manufacturing sectors which are more exposed to trade shocks suffer relative to other, less-exposed workers when the US dollar is overvalued.

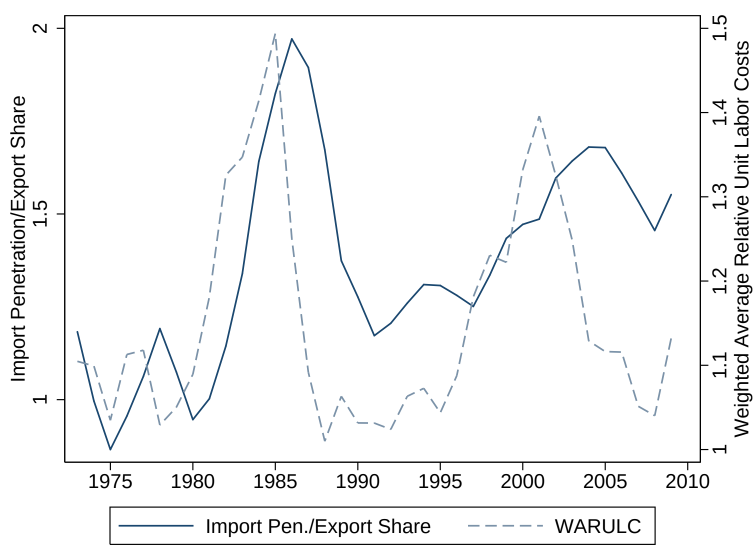


Figure 2: Adverse Trade Shocks: RER Movements

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).

In this approach, we use the CPS's MORG data matched to sectoral data from the

2. This study uses a very similar research design as Campbell (2016b), only we use individual, rather than sector-level data which allows us to ask different questions. Utar (2014) uses Danish worker-level data to study the impact of the relaxation of Chinese textile quotas, is also relevant. Roine et al. (2009) does exercises using international panel data we follow closely in the third section of this paper.

Annual Survey of Manufactures for 87 sectors, and, similar to the flavor of Ebenstein et al. (2014) and Autor et al. (2014), find that workers more exposed to trade shocks are less likely to be employed, and more likely to be unemployed 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 lower wage workers. We reconcile these ostensibly conflicting results by showing that poorly educated, but high-wage manufacturing workers in exposed sectors appear to suffer badly when relative prices are high. Lastly, 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.[3] We find that workers in sectors with larger initial shares of imported inputs do not appear to be adversely effected by trade shocks, but that, for longer periods of time, we do find some correlation between sectoral increases in intermediate inputs and increases in inequality. We conclude from this that more work is needed.

Our second approach is to use data from the Annual Survey of Manufactures (ASM), and ask what happens to inequality within sectors more exposed to international trade when the US real exchange rate is overvalued relative to trading partners. We find no correlation between increases in inequality (proxied by the ratio of non-production worker to production worker wages) for the most exposed sectors during periods of adverse trade shocks. We also find that increases in inequality were not concentrated in sectors which are most exposed to trade with China. In addition, productivity growth, capital intensity, changes in capital intensity, and IT's share of investment are also poor predictors of increases in sectoral inequality. Instead, the rise in inequality in manufacturing appears to have been a broad-based phenomenon across industries, and not limited to those sectors most exposed to international trade, nor to periods when severe trade shocks hit. On the whole, the sectoral data suggests at most a secondary role for trade, capital investment, offshoring, or technology in the sharp rise in inequality in US manufacturing since 1980, with no clear evidence in favor of any of these factors.

While the benefits of using sectoral data include a fairly compelling research design, there are also limitations. If jobs are lost due to trade, this may cause unemployment or increase inequality directly, but it might also lead the central bank to respond with more accomodative monetary policy, reversing the employment loss and causing other changes in the distribution of income. Thus, our third approach is to analyze data

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

internationally for 31 countries, including 16 with data from before 1960, 10 from the 1920s, and two as early as 1900, and ask whether globalization, trade imbalances, or trade with China and other developing countries is correlated with the rise of inequality generally. We again consistently find no correlation between inequality and various trade shocks. In addition, the thesis of skill-biased technological change performs particularly poorly when confronted with international data. Inequality increased in Anglo-Saxon countries starting around 1980, but inequality did not increase in other technologically advanced economies, including Germany, Japan, France, or in Scandinavia. What could explain the evolution of international inequality?

In the 1990s, the debate over rising inequality largely revolved around the question: Was it trade or skill-biased technological change?[4] 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.[5] A comprehensive survey of 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), with skill-biased technological change (SBTC) generally assumed to have accounted for the remainder.[6]

More recently, Levy and Temin (2007) argued that the weakening of organized labor in the US, the declining significance of the minimum wage after the inflation of the 1970s, and, most importantly, the large cuts in top marginal tax rates in the early 1980s were the main causes of the increase in inequality as measured by top income shares.[7] In what were seminal contributions, Roine et al. (2009), Alvaredo et al. (2013), and Piketty et al. (2014) showed a strong correlation internationally between the top 1% share of income and top marginal tax rates, both cross-sectionally and over time. Figure 9 reviews the key evidence from this literature, showing that while the top 1% share of income increased dramatically in the US and UK just after cuts in top marginal tax rates, there was no increase in several other technologically advanced countries, such as Germany and Japan.

Here we complement this literature in several ways. First, given that recent papers have highlighted the impact of the rise of trade with China (*e.g.* Autor et al. (2014))

4. Others suggested it was changes in the relative supply of low-skilled workers.

5. See 2003 for a survey of the literature to that point.

6. For example, see Acemoglu (1998) "Why do New Technologies Complement Skills?" Others, including Card and DiNardo (2002), remain skeptical about whether skill-biased technological change explains the rise in inequality.

7. This followed papers by Lindsey (1987), Feenberg and Poterba (1993), Feldstein (1995), Slemrod (1996), and Saez (2004) which all suggested that the tax changes in the 1980s were responsible for sizeable changes in the distribution of reported income.

and other low-income countries (Krugman (2008)) and of large impacts of real exchange rate shocks on the labor market (Campbell (2016b)), we test the impact of these various shocks on top income shares as well as other parts of the distribution (such as the bottom 90%).[8] Second, thanks to the growth of the World Wealth and Incomes database managed by Alvaredo, Atkison, Piketty and Saez, we can now study a larger sample of 31 countries, nearly twice as many countries as Roine et al. (2009) and twice as many data points as Piketty et al. (2014). Given that this is a seminal result, validating it out-of-sample is crucially important as spurious results have a tendency to fail out-of-sample.

Additionally, we highlight the economic significance of the the dynamic model advocated by Roine et al. (2009). A single change in top marginal rates tends to cause changes in top income shares that play out over decades. We believe that this is important for two reasons. First, it suggests that current inequality is a function of historical top rates, an interesting insight by itself. Secondly, the seemingly long adjustment process for top incomes suggests that the most likely mechanism for how top marginal tax rates impact top (pretax) income shares is via bargaining rather than changes in labor supply or tax avoidance, which would more plausibly impact top shares over a much shorter time horizon. This is because the starting point in any salary negotiation tends to be one's initial salary, which makes it plausible that big events such as the Reagan tax cut, which lowered top rates from 70% to 32% for top earners, would impact the trajectory of top income shares in a way which would take decades to play out. This dynamic model also answers the puzzle about why top income shares continued to increase even after the much more modest Clinton tax increase in 1993.

Also note that the answer appears to be top marginal tax rates may also answer the puzzle of why so many researchers had found correlations between inequality and trade liberalization, particularly for developing countries. Both cuts in top marginal tax rates and trade liberalization episodes tend to be adopted as a part of a variety of reforms. For example, former communist countries, who tend to have very low top marginal tax rates (Russia has a top rate of 13%, for example), liberalized trade at the same time as a multitude of other policies were changed which acted to compress the income distribution (these countries had been, after all, communist). For many Latin

8. Other recent papers which have argued for a link between trade and inequality (to varying degrees) include, Feenstra (2007), Kaplan and Rauh (2010), Lawrence (2008), Haskel et al. (2012), Goldberg and Pavcnik (2007), Ebenstein et al. (2014); 2015, Jaumotte et al. (2013), and Helpman et al. (2012). Although other research, such as Ottaviano et al. (2013), implies that offshoring has a positive effect on native employment. Chapter 4 of Feenstra (2015) also includes an interesting discussion of recent related research.

American countries and India, trade liberalization was also often joined by other, often market friendly reforms. For example, Mexico cut its top marginal tax rate from 55% to 35% just before NAFTA. Chile cut it's top marginal rate from 56% to 30% in the 1980s when it liberalized. India's top rate when from 66% in 1980 to .4 in the 1990s and then to .3 by 1999.

The rest of the paper proceeds as follows. First, we describe our data collection efforts for the MORG individual-level data and present our empirical results. Then we provide our sector-level analysis and end with the country-level, international analysis.

1 Trade and Inequality: An Autopsy from the MORG

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); 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. 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 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.[9] In the regressions using this panel setup, we simply use separate sectoral fixed effects for before and after the change in classification. Another challenge is that in several years, such as 1984, 1985, and 1994 and 1995, not all of the workers can be matched. Since there happened to have been a RER shock in 1984 and 1985 and a corresponding increase in the trade 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, but on the other hand it means that this data cannot really be used to study incomes at the top of the distribution.

The sectoral data we match includes data from the Annual Survey of Manufactures

9. 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.

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 also followed 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 1999 in employing a "proportionality" assumption that assumes that the share of intermediates which are imported is simply the ratio of imports to domestic consumption. We provide a detailed description of the construction of these indices in the Online Appendix, Section 7.1.

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 index numbers problems which afflict the RULC indexes created by the IMF, and which also afflict other commonly used RER indices such as those created by the Federal Reserve.[10]

1.2 Regression 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.

10. According to 2016a (previously circulated as 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. However, the 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 2016a.

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). To reduce the probability of endogeneity, we then take an average of this openness measure lagged 3, 4, 5, 6, and 7 years:

$$L.3\text{-}7yr.Openness_t \equiv (1/5) \sum_{k=3}^{7} Openness_{t-k}. \tag{1.2}$$

Our benchmark regression is:

$$ln\Delta W_{iht} = \alpha_t + \beta_0 L.3\text{-}7yr.Open._{ht} + \beta_1 ln(RER_{t-1}) * L.3\text{-}7yr.Open._{ht} + \tag{1.3}$$

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

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

where $ln\Delta W_{iht}$ is the log change in wages for individual i in sector h at time t (or replaced by another dependent variable, such as indicators for employment, unemployment, not-in-the-labor force, or for overtime work), $ln(RER_{t-1})$) 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. Note that when we have the log change in wages on the left-hand side, 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 classification, and data for the 2003-2010 period which uses the NAICS classification. 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 in our case we include such effects due to necessity. However, our results are similar if we limit our sample to the 1979 to 2002 period, for which the classification is consistent.

1.3 Regression Results (Using the MORG)

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.[11] 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 difficult.[12] We have also found that the results for NILF and overtime are not consistent across various other specifications we have tried (some of which are in the appendix), 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 (=-.21*.3*ln(1.4)). 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,

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

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

E, and F), we find 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 with high vs. low wages, complicating the picture one gets when we think about what impact this might have on overall inequality. Changes in 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 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.

In Table 3, we control for various other trade shocks which are often thought to impact labor markets. These include changes in Chinese Penetration, tariffs, changes in tariffs, the cost of insurance and freight charges, and the share of sectoral intermediate inputs (defined narrowly, and broadly), interacted with a measure of the real exchange rate. With the exception of changes in Chinese import penetration, we do not find significant impacts of these other variables on employment (or on the other variables, so we have suppressed those results for space).[13]

While we look at 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

13. We also tested whether workers in *occupations* exposed to exchange rate shocks suffered declines in wages and employment during periods of RER shocks. We found that they did not (Table 11).

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

	$ln\,\Delta$ 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.

Table 2: The Impact of Real Exchange Rate Shocks on the Labor Market

	$ln\,\Delta$ HW	$ln\,\Delta$ WW	Employed	Unem.	NILF	Δ Over.
A. No College, Richest Third						
L.3-7yr.Open.*ln(RER)	-0.37***	-0.41**	-0.21**	0.14*	0.072	-0.97**
	(0.13)	(0.18)	(0.096)	(0.077)	(0.068)	(0.43)
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, Poorest Third						
L.3-7yr.Open.*ln(RER)	0.016	-0.10	-0.29***	0.17***	0.12	-0.084
	(0.10)	(0.16)	(0.10)	(0.034)	(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)	(0.28)
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
	(0.081)	(0.094)	(0.077)	(0.060)	(0.040)	(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.

the trade deficit worsened for other reasons beside relative prices, the point remains that this trade shock – whatever the cause – appeared to be highly correlated with adverse labor market outcomes for workers most exposed. And yet it was not correlated with a relative decline in wages for low income workers. Thus, this lack of correlation, we believe, is interesting irrespective of whether we have completely solved the identification problem as it creates a puzzle for those who believe that trade shocks have been a major driver of inequality.

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

	(1)	(2)	(3)	(4)	(5)	(6)
L.3-7yr.Avg.Openness	0.034	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)	-0.23***	-0.23***	-0.23***	-0.22***	-0.19**	-0.20**
	(0.067)	(0.069)	(0.069)	(0.068)	(0.089)	(0.081)
$ln\,\Delta$ Demand	0.085***	0.079***	0.080***	0.081***	0.082***	0.077***
	(0.021)	(0.023)	(0.023)	(0.023)	(0.024)	(0.023)
$ln\,\Delta$ VA/Prod. Worker	-0.063***	-0.060***	-0.059***	-0.060***	-0.056***	-0.058***
	(0.018)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Δ Chinese Pen.	0.22***					
	(0.065)					
Duties		0.0045				
		(0.041)				
Change in Duties			-0.032			
			(0.025)			
C.I.F.				-0.054		
				(0.040)		
Δ C.I.F.				0.0071		
				(0.024)		
MFA Exposure				0.014		
				(0.027)		
MP Int.Share*ln(RER)					-0.0095	
					(0.0098)	
MP Int.Share					-0.0026	
					(0.0035)	
MP Int.Sh.(Narrow)*ln(RER)						-0.0026
						(0.0031)
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 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.")

1.4 Implications for Inequality

The implications of the regressions in Tables 1 and 2 for inequality are mixed. On one hand, there is a clear decline in employment, and concomitant rise in unemployment and in workers leaving the labor force. In addition, less-educated workers are impacted more severely, and see sizeable declines in their wages. On the other hand, when we separate workers by wage levels, workers at both the top and bottom seem to lose employment at a slightly higher rate (modestly higher for those at the bottom, although the difference isn't statistically significant) while higher-wage workers suffer larger income declines conditional on working. This would seemingly imply that these trade shocks likely had at best a modest direct impact on inequality in this period. Employment declines in the most open sectors would likely directly increase measured inequality. However, these job losses would have influenced central bank behavior, meaning that the net impact is not straightforward to intuit.

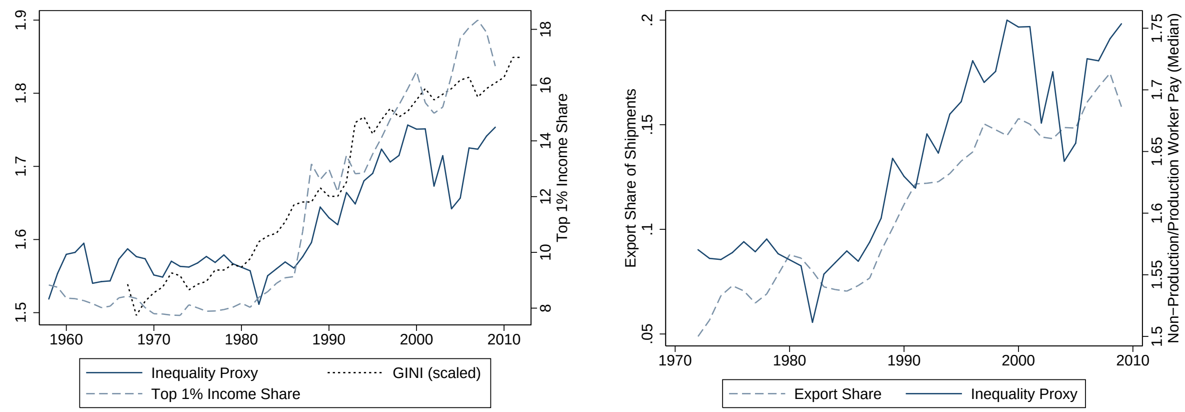
Thus, to understand what the impact is on inequality, another method we can use is to look at the impact of trade shocks on sectoral inequality. We have seen a steady rise in inequality in the 1980s, and thus we can test whether sectors more exposed to trade shocks experienced a rise in inequality in periods when US relative prices were appreciated relative to other periods. We do just this in the next section.

2 Sector-Level Evidence

2.1 Sector-Level Data

To try to identify the impact of trade shocks caused by (large) movements in relative prices on sectoral inequality, we use a panel difference-in-difference this time using data from the Annual Survey of Manufactures for 359 disaggregated manufacturing sectors with balanced data over the period 1973 to 2009. While we could also do these calculations using the MORG data, there are a few benefits of the ASM data relative to the MORG. The major advantage is that now that we have a proper panel, it is necessary to have sectors that span the length of the panel, and so it is convenient not to have a split in the middle of the panel as is the case with the MORG panel. Another nice feature is that the ASM contains 359 different manufacturing sectors with full data, versus a maximum of 87 for the MORG, which means that our estimates will be more precise.

The key problem with the ASM data is that we will use the ratio of non-production worker to production worker pay as a proxy for the ratio of skilled to non-skilled worker wages. Yet, while admittedly imperfect, it does seem to track the top 1% share of income produced by Piketty and Saez or a Gini index computed by the Census Bureau (Figure 3(a)). (Figure 3(b) shows that this proxy for inequality also seems to track the export share of shipments for the US manufacturing sector as a whole – a motivation for looking at trade and inequality.) Thus we will also use labor's share of output as an outcome variable, and we will confirm our results (to an extent) using an additional source – the BLS's Occupational Employment Statistics. However, there are also two problems with this data: (1) it only ranges from 2002 to 2010, so it is impossible to know what the "pre-treatment trend" is, and (2) it is top-coded at fairly low-levels, so that, for instance, in some years all CEOs in some industries are reported as having earned the top-coded amount. Thus we will use the ratio of the 75th/10th percentile earnings.



(a) Inequality in Manufacturing vs. Overall

(b) Inequality in Manufacturing vs. Export Share

Figure 3: Trade and Inequality

Notes: Inequality here is proxied by the ratio of non-production to production worker wages. The "Export Share" is for manufacturing shipments, defined as exports divided by shipments, where shipment data come from the Census Bureau's ASM and export data are from WITS, using the SIC classification. The income share of the 1% (for the economy as a whole) are from Piketty and Saez (2007), and the Gini is from the Census Bureau via FRED.

2.2 Sector-Level Identification Strategy

Our identification strategy will be exactly the same as before. The goal is to compare how inequality evolved in relatively more open sectors when US relative unit labor costs were high compared to when US unit labor costs were in line with US trading partners.

The definition of openness is the same as in equation 1.2. The estimating equation is:

$$ln\Delta I_{ht} = \alpha_t + \beta_0 L.3\text{-}7yr.Openness_{ht} + \beta_1 ln(RER_{t-1})) * L.3\text{-}7yr.Openness_{ht} + \quad (2.1)$$

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

$$\forall h = 1, ..., 359, \; t = 1973, ..., 2009,$$

where I_{ht} is a measure of inequality (or unit labor costs) of industry h at time t, $L.3\text{-}7yr.Openness_{ht}$ is an average of weighted openness lagged 3 to 7 years, in sector h (replaced with export share or import penetration in some regressions), RER is a measure of the real exchange rate, such as WARULC, $D_{h,t}$ is real sectoral demand, $TFP_{h,t}$ is a measure of TFP (we use 4 and 5-factor measures of productivity in addition to value-added and shipments divided by production worker or total employment), and the Cs are various other controls.[14] Our baseline regression also includes sectoral fixed effects α_h, year fixed effects ν_t, and two-way clustered errors, by both industry and year, and all regressions are weighted by initial period value-added. The results do not appear to be sensitive to the choice of weights, as qualitatively similar results can be attained when weighting by average value-added, employment, or shipments.[15]

2.3 Sector-Level Empirical Results

Our core estimation strategy is displayed graphically in Figures 4 and 5. In Figure 4(a), we plot the evolution of inequality in more open sectors vs. less open sectors over time with two standard deviation error bounds, and show that, if anything, inequality worsened in more closed sectors in the 2000s, although the difference was not statistically significant. There appears to have been scant difference in the 1980s. In Figure 4(b), we divide the sample between sectors with at least 5% of consumption coming from China in 1995 and those with less than 3%. We find, surprisingly, that those sectors with

14. These controls include sectoral input prices (labor, materials, energy, and investment), sectoral input prices interacted with sectoral input shares, capital-labor ratios, capital-labor ratios interacted with real interest rates, openness interacted with the real interest rate, and the Campa-Goldberg measure of markups by sector interacted with the real exchange rate.

15. These robustness checks, and others, are contained in the Additional Appendix. For instance, the results would not change significantly using a geometric rather than an arithmetic average of export share and import penetration as a measure of openness. Additionally, the results are robust to omitting defense, and computer-related sectors, given that the periods of dollar appreciation are associated with large increases in defense-spending and also since the official productivity data for the computer sector has been called into question by Houseman *et al.* (2010). We also omit the publishing sector as this is marginally a manufacturing sector and was dropped from manufacturing in the NAICs classification, but our results are robust to including publishing.

relatively higher initial exposure to Chinese imports actually experienced a decline in inequality in the 2000s, although the difference with the non-Chinese competing sectors was not significant.

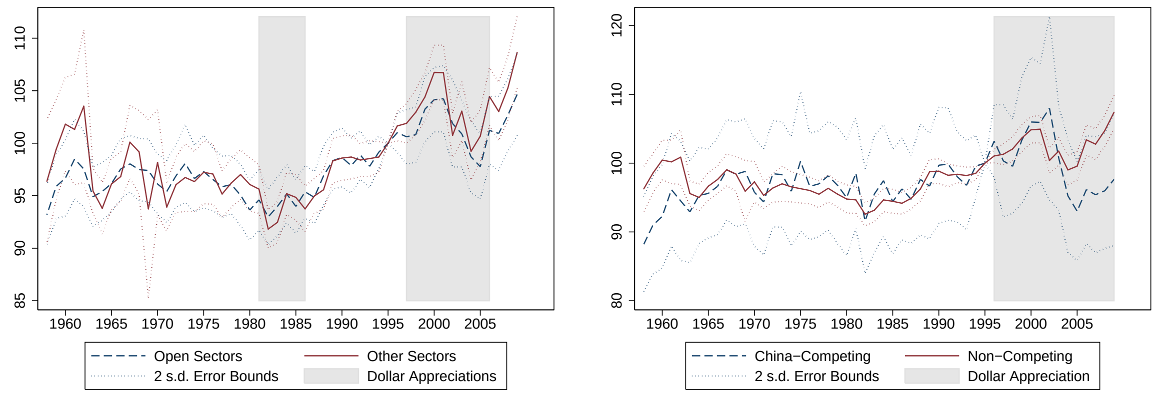
We also want to be sure that our results are not an artifact of the particular measure of inequality we use in this dataset, the ratio of non-production worker to production worker wages from the ASM. Thus, in Figure 5, we plot the evolution of inequality using the ratio of workers wages at the 75th percentile of the distribution to wages at the 10th percentile from the BLS's Occupational Employment Statistics using NAICs data for the years 2002 to 2010.[16] Here, China-competing sectors did exhibit increasing inequality relative to other sectors in this period, although the difference is, once again, not statistically significant.

Note that while research (*e.g.*, Campbell 2015b, and see Klein *et al.* 2002 for an overview of literature to that point) has generally found that the episodes of dollar appreciation were the cause of the ensuing trade deficits, the actual cause of these adverse trade shocks (whether it is relative prices or another factor) is not a necessary condition for the validity of the identification strategy used in this paper. The research design is simply to compare the evolution of inequality in more open sectors compared to less open sectors in periods when import penetration grew quickly relative to export shares versus other periods. The critical assumption is that there was no other third factor that we have neglected to control for which may have caused (or, in our case, prevented) a large movement in inequality in more tradable sectors and which also caused a large percentage increase in imports and a decline (or slow growth) in exports.

Estimating equation 2.1 in Table 4, column (1), we show that there appears to be no relation between appreciation in WARULCs and movements in the ratio between non-production worker wages and production worker wages in relatively more open sectors. This may seem to be a suprising result given the correlation in Figure (3), and given that 2016b found that employment, investment, and output in relatively more open manufacturing sectors are all quite sensitive to movements in relative prices. We also find that lagged Chinese import penetration does not predict increases in inequality – in fact the point estimate is negative, although insignificant.[17] Lastly, we find that labor productivity growth (value-added per production worker) is strongly associated with

16. Unfortunately, this data is top-coded at a fairly low value making it unsuitable for gauging trends in inequality at the 90th percentile or higher as would clearly be preferable, as the largest changes in inequality in the US come much further up the distribution. Since this data only begins in 2002, we also do not know what the pre-trend was in the period before Chinese competition, another reason why we stick to the ASM data in our panel regressions.

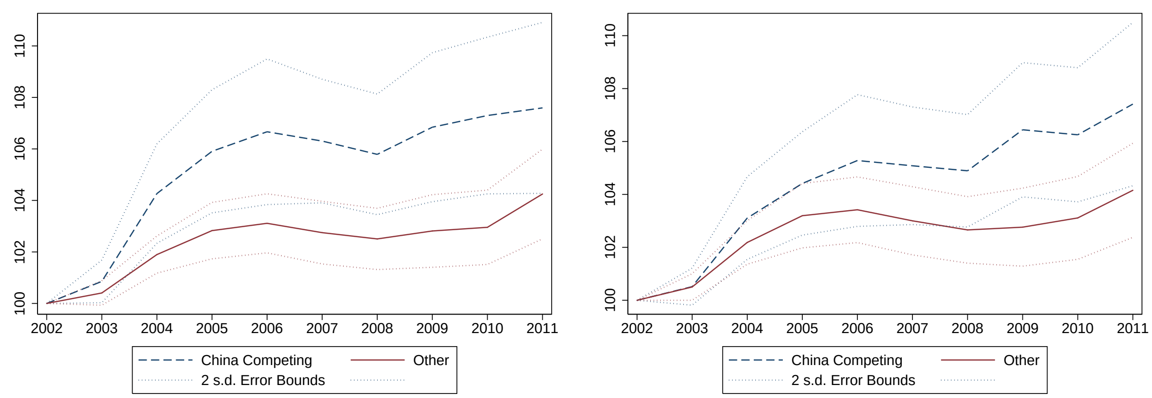
17. We also tried changes in Chinese import penetration, and again found no relationship.



(a) By Degrees of Overall Openness

(b) By Exposure to Chinese Competition

Figure 4: Evolution of Inequality, Disaggregated (SIC)



(a) Based on Initial Exposure to China

(b) Based on Actual Increase in Chinese Imports

Figure 5: Changes in Inequality, China-Competing Sectors vs. Others (NAICs)

Notes: Inequality in Figure 4 here is proxied by the ratio of non-production to production worker wages (per worker), whereas in Figure 5, inequality is proxied by the ratio of wages of workers at the 75th percentile to workers at the 10th percentile of earnings by 4-digit NAICs sectors from the BLS's Occupational Employment Stastistics. Open sectors in 4(a) are defined by those with a share of openess of at least .15 (openness = average of import penetration the export share of shipments), and non-open sectors are defined as those with openess of less than .1. In 4(b), China-competing sectors are defined as those with at least 5% of domestic consumption originating in China in 1995, and other sectors are those with less than 3%. In 5(a) a cutoff of 5% of domestic consumption originating in China in 2002 was used, and in 5(b), the "China Competing" sectors are those in the top quarter of the distribution of increases in Chinese import penetration from 2002 to 2010.

declining inequality, although we are suspicious about what may be driving this. We also control for various other factors which may affect output or employment, and thus inequality. These include demand growth by sector (which is not consistently significant), the share of imported intermediate inputs, lagged capital-labor ratios, lagged capital-labor ratios interacted with the real interest rate, and the costs of inputs, and the costs of these inputs interacted with the share of these inputs at the sectoral level. None of these controls other than productivity are consistently significant (many are suppressed for space). Thus, in the robustness table which follows, we redo the results with these insignificant regressors removed.

However, in column (2) when we use a multi-factor measure of productivity growth instead of value-added per production worker, we do get a marginally significant estimate for the interaction term of lagged average openness and the log of WARULC. However, the variable loses significance when we run a quantile regression in column (3), or in various other plausible specifications. In column (2) we also do see a positive correlation between TFP growth and sectoral inequality, significant at 95% confidence. This result is robust to estimation using a quantile regression in column (3), but not to other specifications in Table 5.

In column (4), we separate openness into import penetration (defined as imports divided by domestic consumption, where domestic consumption is shipments plus imports minus exports) and the export share of shipments. Additionally, we interact import penetration with an import Weighted Relative Unit Labor Cost index (iWARULC) and the export share of shipments with an export-WARULC index. Again, we see no tendency of sectors which are more exposed to trade to have any trends in sectoral inequality when domestic unit labor costs are high relative to trading partners. In column (5), we use the actual changes in the export share of shipments and in import penetration, but find that neither are significant. Thus, this column is a striking reversal of the apparent trend in Figure 3(b). This is essentially because the rise in inequality by sector was relatively broad-based, and there is no correlation between the sectors that experienced the largest rise in exports, and those that experienced the largest rise in inequality. In column (6), we interact the average of intermediate inputs lagged 3 to 7 years with the lagged log of WARULC. Again, we find that sectors which have more intermediate inputs also did not experience relative increases in inequality when the RER appreciated.[18]

In Table 5, we provide a number of robustness tests, by varying the inclusion of year and industry FEs, and other controls. We also test for the impact of trade shocks on

18. In addition, we found that when we controlled for log changes in intermediate inputs, this also does not predict changes in inequality.

sectoral ULCs, defined here simply as the ratio of wages to value-added, or labor's share of income. In this table, each cell represents a separate regression, for 36 regressions total, with the non-primary regressors suppressed for space. What we find is that no variable is a consistent predictor of inequality or of ULCs across specifications with the possible exception of changes in export share, which predict log changes in ULCs in four out of six specifications with a positive sign (indicating more exports imply more inequality). However, the preferred specification is in column (6), in which case the coefficient is positive but it is not significant. Here, note that multi-factor TFP-growth also loses significance in three out of six specifications, and that even the statistically signficant regression results imply that TFP growth was responsible for only 1-17% of the rise in inequality from 1980 to 2000.[19] Another check is to look at the overall change in inequality from 1980 to 2000 vs. the changes in TFP in Figure 6(a), in order to abstract from cylical concerns in favor of the big picture. However, here there is no correlation.[20]

Lastly, we check the relation between long-run changes in sectoral inequality and some of our other key variables of interest. There is no correlation between capital stock growth and inequality (Figure 6(b)), as might be expected if it was thought that investment in robots or heavy machinery were a primary cause of inequality. Despite the fact that at least a plurality of economists likely still believe in the thesis of skill-biased technological change, there is no correlation between increases in sectoral inequality and computer investment 6(c). (We also tried changes in computer investment from 1979 to 1990, and found that this is also uncorrelated with sectoral changes in inequality.) Lastly, we do find a slight correlation between increases in offshoring and changes in inequality. However, a number of caveats are in order: (1) a simple regression appears to be driven by outliers, as a quantile regression does not yield a significant relationship, (2) when we use a narrower measure of imported inputs, which may actually be a better reflection of offshoring, there is no relationship, (3) there only appears to be a relationship during this particular time period, but not before or since, (4) the relationship is non-random, and so it is hard to say what is driving this, (5) the R-squared of this regression is less than .02, and even if we take these results at face value, it only implies that offshoring lead to a relatively small share of the overall increase in sectoral inequality.

19. This estimate uses the OLS estimate minus two standard deviations for the lower bound, and the quantile estimate plus two standard deviations for the upper bound.

20. Nor was there a correlation in the period before 1980, and while we do not show the results, there also hasn't been a significant correlation since.

Table 4: Trade Shocks and Inequality: Sectoral Evidence

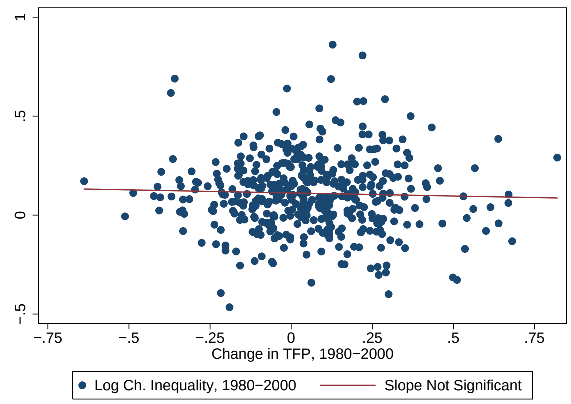
	(1)	(2)	(3)	(4)	(5)	(6)
	$ln\,\Delta$ I	$ln\,\Delta$ I	$ln\,\Delta$ I	$ln\,\Delta$ I	$ln\,\Delta$ I	$ln\,\Delta$ I
$ln\,\Delta$ VA-per-Prod. Worker	-9.56***			-9.59***	-9.82***	-9.58***
	(1.12)			(1.12)	(1.23)	(1.14)
$ln\,\Delta$ Demand	0.017	-0.041	-0.031**	0.017	0.031	0.017
	(0.023)	(0.028)	(0.015)	(0.023)	(0.022)	(0.023)
Post-PNTR x NTR *Gap_i*	-1.82	-3.43	0.98	-2.18	-1.85	-1.71
	(15.2)	(17.5)	(15.7)	(15.0)	(15.8)	(16.3)
L.3-7yr.Avg.Openness	-0.023	-0.026	-0.016			
	(0.018)	(0.016)	(0.013)			
L.3-7yr.Open.*L.ln(RER)	0.11	0.14**	0.085			
	(0.071)	(0.068)	(0.065)			
L1.Chinese Import Pen.	-0.0047					
	(0.016)					
$ln\,\Delta$ TFP (5-factor)		0.071**	0.095***			
		(0.034)	(0.026)			
L.3-7yr.MPP.*L.ln(iRER)				0.030		
				(0.051)		
L.3-7yr.XPSh.*L.ln(eRER)				0.054		
				(0.085)		
Δ Export Share					0.067	
					(0.055)	
Δ Import Penetration					-0.069	
					(0.050)	
L.3-7yr.MPInputs*L.ln(RER)						-0.043
						(0.11)
Observations	11994	11994		11994	12352	11994

*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered by sector and year in parentheses. The dependent variable is the log change in the ratio of non-production to production worker pay, a proxy for sectoral inequality. Each regression includes industry (359) and year FEs over the period 1972-2009 on a perfectly balanced sample. The key trade shock regressors are in bold. 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 the RER (WARULC) is the key variable of interest in the first three columns of this regression. Column (3) is a quantile regression, the others are OLS. "L.3-7yr.MPP.*L.ln(iRER)" is lagged avg. openness interacted with an import-weighted version of WARULC, and "RER" is an export weighted RER index (WARULC). "MPInputs" stands for imported inputs relative to value-added. In column (4), lagged average import penetration and export share are omitted for space. A number of other controls are suppressed for space, including lagged capital-per-worker, capital-per-worker interacted with real interest rates, Campa-Goldberg markups, output prices, sectoral input prices (materials, energy, investment goods), and those prices interacted with sectoral input usage.

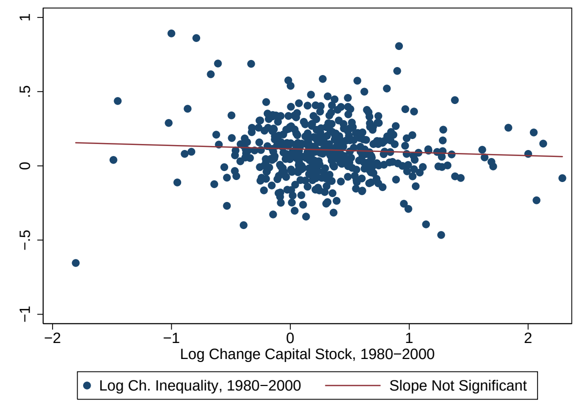
Table 5: Robustness Exercises: Impact of Trade on Inequality and ULCs

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var: Log Change in Inequality						
L.3-7yr.Avg.Openness	-0.023*	-0.023*	0.017	-0.017	0.025	-0.026
	(0.013)	(0.013)	(0.024)	(0.016)	(0.019)	(0.016)
L.3-7yr.Avg.Open.*L.ln(WARULC)	0.13**	0.13**	-0.085	0.083	-0.16	0.13*
	(0.060)	(0.060)	(0.085)	(0.057)	(0.11)	(0.068)
$ln\,\Delta$ TFP (5-factor)	0.076**	0.076**	0.045	0.025	0.00027	0.075**
	(0.030)	(0.030)	(0.037)	(0.024)	(0.024)	(0.034)
L.3-7yr.MPPen.*L.ln(iWARULC)	0.030	0.030	-0.11	0.0081	-0.18*	0.033
	(0.045)	(0.045)	(0.096)	(0.045)	(0.11)	(0.060)
L.3-7yr.XP Share*L.ln(eWARULC)	0.067	0.067	-0.064	0.047	-0.042	0.066
	(0.082)	(0.082)	(0.094)	(0.083)	(0.092)	(0.082)
Δ Export Share	0.041	0.041	0.054	0.064	0.092*	0.036
	(0.047)	(0.047)	(0.054)	(0.051)	(0.050)	(0.053)
Δ Import Penetration	-0.0076	-0.0076	-0.039	-0.035	-0.070	-0.0047
	(0.050)	(0.050)	(0.065)	(0.046)	(0.057)	(0.058)
Dep. Var: Log Change in ULCs						
L.3-7yr.Avg.Open.*L.ln(WARULC)	-0.10	-0.10	-0.16	0.021	-0.22	-0.12*
	(0.071)	(0.071)	(0.10)	(0.077)	(0.19)	(0.070)
L.3-7yr.MPPen.*L.ln(iWARULC)	-0.11	-0.11	-0.16	-0.089	-0.12	-0.17
	(0.11)	(0.11)	(0.11)	(0.12)	(0.20)	(0.11)
L.3-7yr.XP Share*L.ln(eWARULC)	-0.011	-0.011	-0.031	0.16	-0.12	0.048
	(0.083)	(0.083)	(0.090)	(0.10)	(0.20)	(0.080)
Δ Export Share	0.19**	0.19**	0.13	0.42***	0.41***	0.16
	(0.093)	(0.093)	(0.10)	(0.13)	(0.14)	(0.098)
Δ Import Penetration	-0.00079	-0.00079	-0.045	0.29**	0.23*	-0.038
	(0.074)	(0.074)	(0.084)	(0.12)	(0.13)	(0.068)
Year FE	No	Yes	No	Yes	No	Yes
Industry FE	No	No	Yes	Yes	No	Yes
Full Controls	Yes	Yes	Yes	No	No	Yes

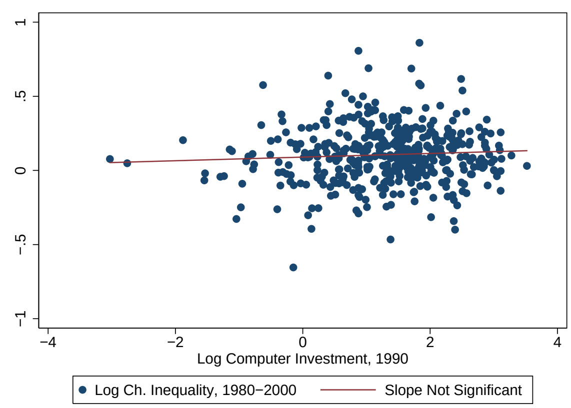
Two-way Clustered standard errors in parenthesis, clustered by year and 4-digit SIC sectors. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. There are six sets of six regressions, for 36 regressions total. The first three rows of six regressions use the log change in the ratio of non-production to production worker wages as a proxy for inequality. Rows 4-6 use sectoral unit labor costs as the dependent variable. Each column contains different combinations of controls and fixed effects as indicated. For example, all of the regressions in column one include a full set of controls, but no year or sectoral fixed effects, while column (6) includes year and sectoral fixed effects and a full set of controls. All regressions are weighted by initial period value-added.



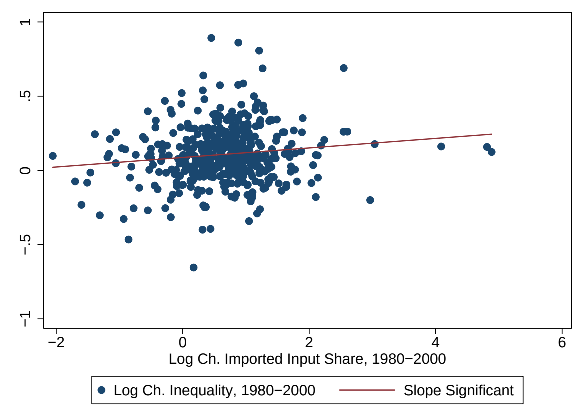
(a) TFP Growth



(b) Capital Stock Growth



(c) Computer Investment



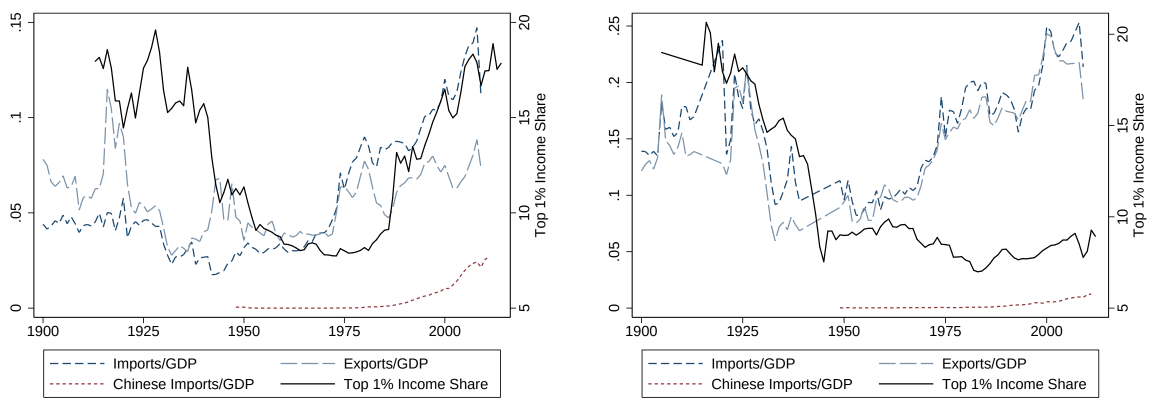
(d) Offshoring Growth

Figure 6: TFP, Capital, Computers, Offshoring and Inequality

Notes: Inequality here is proxied by the ratio of non-production to production worker wages in the manufacturing sector, from the ASM. Values above zero on the y-axis thus indicate an increase in inequality. Computer investment for the year 1990 comes from Feenstra and Hanson (1999). “Offshoring growth” is proxied here by imported intermediate inputs, from a data series we created based on BEA IO data.

3 International Evidence

A useful complement to using individual or sector-level data is to use aggregate, international data. If workers can move flexibly between sectors, then trade could have an impact on aggregate inequality even if there is no evidence of outsized gains in inequality in the sectors most exposed to trade. In this case, if trade shocks do impact inequality, then one would expect to see a correlation between trade shocks and aggregate inequality. Internationally, there appears to be little correlation between trade levels and the top 1% share of income (Roine et al. (2009)). Figure 7 demonstrates this lack of correlation comparing trade and the income share of the top 1% for the US and France. For the US, there is perhaps an imperfect correlation between levels of trade and inequality, but in France, even as trade increased after 1970, the income share of the top 1% actually fell. On the other hand, from the 1980s, the US trade deficit did increase around the same time, while the US has also been significantly more exposed than France to trade with China. As of 2011, US imports from China as a share of GDP were 2.7%, whereas French imports from China were only 1.2% – and this despite the fact that France trades more overall as a share of GDP. Of course, the trade deficits in the US were largely the product of currency appreciation caused, in turn, by a budget deficit which was the product of cuts in top marginal tax rates. Thus, it is an empirical question how much of the rise in inequality these trade shocks can explain vs. changes in top marginal tax rates.



(a) US

(b) France

Figure 7: Trade vs. 1% Share of Income

3.1 International Data

We use the top 1%, 90-99%, and bottom 90% shares of income from the World Wealth and Income Database (formerly called the World Top Incomes Database), managed by Alvaredo, Atkinson, Piketty, Saez, and Zucman, as our main data source.[21] We then collated data on top marginal tax rates from a number of sources, including Roine et al. (2009), listed in Appendix Section 5.1.[22] Trade data comes from IMF DOTS, and then oil-related trade via UN Comtrade was subtracted to compute trade flows ex-oil.

3.2 International Methodology

Piketty et al. (2014) – hereafter PSS – explain the dramatic rise in the income share of the top 1% in the US and other Anglo countries as a result of the sharp decline in top marginal tax rates that these countries had in the 1980s (see Figure 17). Other major economies, including France, Japan, and Germany, did not see either large reductions in top marginal tax rates or in increases in the income shares of the top 1% over this period. Using data for 18 countries over the period 1960 to 2010, they run the regression:

$$ln(1\%IncomeShare_{it}) = \alpha + \beta ln(1 - TopMTR_{it}) + c * t + \epsilon_{it}, \tag{3.1}$$

where t is a time trend, the dependent variable is the top 1% share of income, "Top MTR" is the top marginal tax rate, and where the errors are robust.

PSS find that top marginal tax rates are one of the key drivers of international inequality. As this is arguably one of the most important empirical findings in the past 20 years, we believe it is important to subject their results to robustness tests, including to changes in specification, to out-of-sample testing, to other parts of the income distribution, and to the inclusion of additional controls, such as various kinds of trade shocks. Thus, we will estimate:

$$ln(IncomeShare^k_{it}) = \alpha_i + \beta_1 ln(TopMTR_{it}) + \beta_2 TradeShock_{it} + \beta_3 ln\Delta GDP_{it} + c_i\alpha_i * t + \nu_t + \epsilon_{it}. \tag{3.2}$$

where $IncomeShare^k_{it}$ is the income share of group k (the top 1%, or bottom 90%, for example), $\alpha_i * t$ are country-specific trends, α_i are country fixed effects, and ν_t are year dummies.

We will also test this static specification vs. several intuitive dynamic specifications

21. Note that Waldenström and Roine (2014) find that top income shares are highly correlated with Gini coefficients both cross-sectionally and over time.

22. We have made this data available on our webpage at http://dougcampbell.weebly.com/.

(following Roine et al. (2009)):

$$ln\Delta(1\%IncomeShare_{it}) = \alpha_i + \beta_1 TopMTR_{it} + \beta_2 ln\Delta GDP_{it} + c_i\alpha_i * t + \nu_t + \epsilon_{it}, \quad (3.3)$$

where we now have the log change in the top income share on the left-hand side, and the level of top marginal tax rates on the right-hand side. We also control for log changes in GDP, as the top 1%'s share of income is observed to be strongly procyclical, and will include varying fixed effects and country-specific trends as before.

3.3 International Panel Regression Results

First, we benchmark the Piketty *et al.* (2014) results in Panel A of Table 6, with errors clustered at the country level. In columns 2-6, we include different combinations of year trends, year FE, country FEs, and country-specific year trends. We find that the results are mostly robust to these controls, although in column (6) the coefficient shrinks and significance is lost. However, this is quickly restored when we alter the functional form of the top marginal tax rate, using its level rather than one minus the MTR. In Panel C, we find that this result even tests out of sample on an additional sample including historical data for most of the countries in the original 18 country sample, plus data for an additional 12 countries. However, in Panel D, when we use the entire sample, significance is lost in the final two columns.[23] We suspect that significance on the subsamples is attained for the wrong reasons – the country-specific trends in the latter sample, for example, imply that inequality is increasing naturally for many countries, but the trend terms are less likely to be significant in the full panel.

However, we also do not believe this particular setup is justified by the dynamic nature of how people respond to tax changes in reality. What we actually observe is that when goverments cut their top marginal tax rates, in the US case from .72 to .53 in 1980, and then to .32 in 1988, the trajectory of the income share of the top 1% changes (see Figure 17). Although there does appear to be a large short-run change based on tax avoidance and reporting, the *growth rate* of top income shares clearly changes. We believe that this is consistent with the theoretical rationale provided by PSS, in which the top marginal tax rate plays a role in bargaining for CEO salaries and other executives. When income over a certain cutoff is taxed at a very high rate (such as 72%), one may be

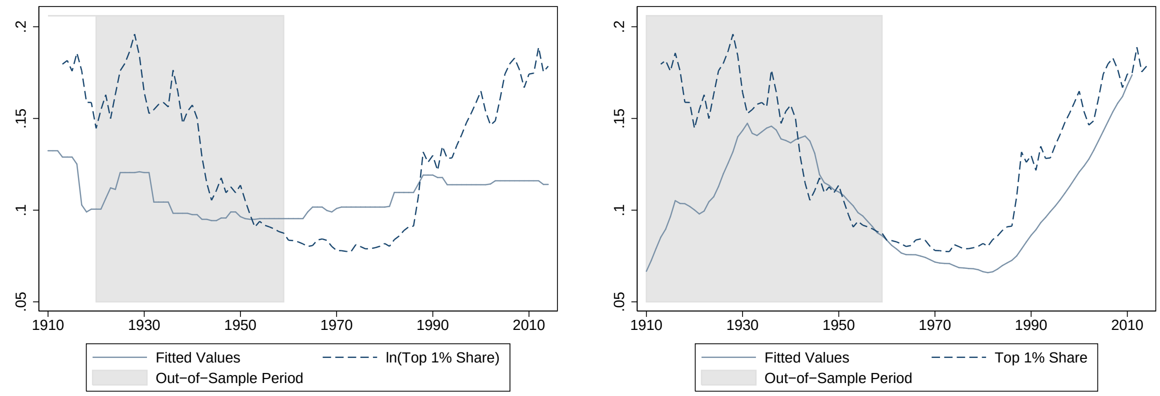
23. When we also include the log of the trade share of GDP, or the trade balance as a share of GDP (in the second and third rows of Table 6), we find that trade is generally not significantly correlated with income shares of the top 1%. We also tried the import and the export shares of trade, and also find that neither predict changes in inequality.

Table 6: Determinants of Top Income Shares: Testing Out of Sample

	(1)	(2)	(3)	(4)	(5)	(6)
A. PSS Sample						
ln(1-Top MTR)	0.32***	0.38***	0.33***	0.28***	0.31***	0.075
	(0.075)	(0.13)	(0.086)	(0.060)	(0.071)	(0.049)
Observations	814	814	814	814	814	814
B. PSS Sample						
Top Marginal Tax Rate	-1.11***	-1.46***	-1.31***	-1.09***	-1.18***	-0.45**
	(0.18)	(0.31)	(0.18)	(0.18)	(0.16)	(0.15)
Observations	814	814	814	814	814	814
C. Out-of-Sample						
Top Marginal Tax Rate	-1.50*	-1.31*	-0.85***	-0.85*	-0.72***	-0.54**
	(0.83)	(0.73)	(0.12)	(0.49)	(0.12)	(0.23)
Observations	766	766	766	766	766	766
D. Full Sample						
Top Marginal Tax Rate	-1.27***	-1.05***	-1.06***	-1.44**	-0.34	-0.0084
	(0.30)	(0.23)	(0.14)	(0.64)	(0.53)	(0.52)
Observations	1602	1602	1602	1602	1602	1602
Year Trend	No	Yes	Yes	No	Moot	Moot
Year FE	No	No	No	Yes	No	Yes
Country FE	No	No	Yes	Yes	Yes	Yes
Country-Specific Year Trend	No	No	No	No	Yes	Yes

Notes: Standard errors clustered by country in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. The dependent variable is the log of the top 1% share of income. There are four sets of six regressions, 24 total. Each column varies the fixed effects and year trends as noted. The first two panels use the original sample from Piketty, Saez, and Stantcheva (2014)–PSS–with 18 countries over the period 1960 to 2010. Panel B alters the functional form of the marginal tax rate. Panel C tests out-of-sample for a total of 31 countries either with data before 1960 or for countries not included in the PSS sample. Panel D includes the full sample.

naturally less likely to bargain hard for a salary increase, knowing most of it will be paid to the government in any case. Yet, the other factor in wage bargaining has always been the current level of wages (see, for example, Bewley (1999)), as workers are resentful of pay cuts. Thus, the natural outcome of an interaction in bargaining outcomes should be that CEOs and other executives would bid up their salaries relative to their own starting point. Of the other mechanisms for how the top 1% share could respond to marginal tax rates mentioned by PSS, through either labor supply or tax avoidance, both could also conceivably have a dynamic response, for example if an individual's labor supply decisions were persistent, or if learning were important for tax avoidance. However, intuitively, we would expect that the bulk of these adjustments should happen relatively quickly, so that if the dynamic specification is more accurate, this would seemingly imply



(a) Static Model

(b) Dynamic Model

Figure 8: Predicting Top Income Shares: Static vs. Dynamic Model

Here, we omit year dummies and trends. Thus, the static model is: $ln(1\%IncomeShare_{it}) = \alpha + \beta TopMTR_{it} + \epsilon_{it}$, and the dynamic model is: $ln\Delta(1\%IncomeShare_{it}) = \alpha + \beta_1 TopMTR_{it} + \epsilon_{it}$. Both are estimated on the period after 1960. The dynamic model was set to the actual in levels in 1960, and then updated based on the predicted log changes in the top 1% share of income from the model.

that the impact of top marginal tax rates in bargaining is the more critical mechanism over long periods.

In Figure 17 we compare the performance of a simple static vs. dynamic model estimated on the PSS sample of countries for 1960-2010, vs. data for the US. We see that in terms of both in-sample fit and out-of-sample performance, the dynamic model seems to dominate. The one discordant note is that the model predicts rising, rather than roughly flat, levels of inequality for the early 1900s.[24]

In the second set of regressions, beginning with Panel A of Table 7, we find that the top marginal tax rate impacts log changes in the Top 1%'s share of income significantly in all specifications, regardless of which combinations of year or country fixed effects, and country-specific trends are included as controls. We also believe this better reflects the reality that when top marginal tax rates were lowered for the US and several other Anglo countries in the 1980s, the top 1% share of income began to increase and, on a cyclically-adjusted basis, appears to have been increasing ever since. Alternatively, one could include the top 1% share as a lagged dependent variable, as in Panel B, with similar results, and with coefficients generally not far from one.

In Panel C, we add in controls for trade shocks in various forms. These include the

24. This raises the question of whether a lagged dependent variable model wouldn't be prefereed. Intuitively, it would be preferable, and we do just this in the subsequent table. However, we find a coefficient very close to one on the lagged variable, indicating that not much changes.

ratio of imports to nominal GDP, the log change in imports and exports (separately) as a share of GDP, Chinese imports, the trade balance as a share of GDP, and changes in the trade balance. None are significant in this specification, which includes year and country fixed effects, and none are significant if we were to add in country-specific trends either. This conclusion holds up when we include the level of the top 1% share of income on the left-hand side as well. Thus, trade shocks simply do not seem to be a driver of international trends in the top 1% share of income.[25]

3.4 Robustness and Impact on Other Parts of the Distribution

We also performed a number of robustness checks, tested various other trade shocks, and studied the impact on other parts of the distribution. While there is not enough space in the main paper to include all of these results, we summarize them briefly here. First, as OLS is often sensitive to outliers, we rerun all of the regressions in 7 using quantile regressions (Appendix Table 14), finding no substantive differences. Second, we tested whether various other trade shocks (Appendix Table 13), including the import share of GDP, the log change in imports from Low-Wage countries over GDP, the log change in Chinese Imports divided by GDP, and overall trade with low wage countries as a share of GDP are correlated with top income shares. We find, in each case, that they are not. Thirdly, in Appendix Table 15, we examine the impact of trade shocks on other parts of the income distribution, including the bottom 90% and the income share of the 90-99th percentile. Here we did occassionally find a role for trade shocks in shaping inequality, although the correlation was not necessarily always significant across all specifications and samples (see Tables 16 and 17). The finding, which we do not claim is either causal or necessarily very robust, is in fact that growth in exports as a share of GDP, or improvements in the trade balance, are associated with a shift in income from the 90 to 99th percentile to the bottom 90%. If true and causal, then it implies that positive trade shocks, or movements toward globalization, may in fact bolster incomes of the those at the bottom of the income distribution.

25. However, we do not necessarily believe that this implies that trade or globalization does not matter at all for the income distribution. It could be, for example, that for any tax system, there is a naturally steady-state level of income for the top 1%, and that this level is impacted by the degree of globalization or level and skill-biasedness of technology. For example, it seems that this natural steady-state level in a world with no income tax is likely higher today than it was in the pre-war period, judging by the fact that the US has similar levels of inequality now and in the period before WWI when income taxes were quite small.

Table 7: Taxes, Trade, and the Top 1%: Dynamic Models

	(1)	(2)	(3)	(4)	(5)	(6)
A. Dep.Var.: ln Δ Top 1%Sh.						
Top Marginal Tax Rate	-0.047***	-0.055***	-0.072***	-0.041***	-0.093***	-0.079***
	(0.011)	(0.0074)	(0.0094)	(0.013)	(0.0067)	(0.021)
ln Δ GDP	0.20**	0.22**	0.23**	0.15	0.25**	0.17*
	(0.097)	(0.097)	(0.10)	(0.10)	(0.10)	(0.10)
Observations	1532	1532	1532	1532	1532	1532
B. Dep.Var.: ln Top 1% Sh.						
L.ln(Top 1% Sh.)	0.98***	0.98***	0.97***	0.97***	0.93***	0.93***
	(0.0026)	(0.0014)	(0.0074)	(0.0062)	(0.0047)	(0.0074)
Top Marginal Tax Rate	-0.081***	-0.082***	-0.11***	-0.11**	-0.098*	-0.076
	(0.013)	(0.011)	(0.012)	(0.044)	(0.050)	(0.047)
Observations	1532	1532	1532	1532	1532	1532
Year Trend	No	Yes	Yes	No	No	No
Year FE	No	No	No	Yes	No	Yes
Country FE	No	No	Yes	Yes	Yes	Yes
Country-Specific Year Trend	No	No	No	No	Yes	Yes
C. Dep.Var.: ln Δ Top 1% Sh.						
Top Marginal Tax Rate	-0.069***	-0.045***	-0.045***	-0.075***	-0.050***	-0.043**
	(0.022)	(0.015)	(0.015)	(0.022)	(0.017)	(0.016)
Poor-country Imports/GDP	0.079					
	(0.090)					
ln Δ (Imports/GDP)		-0.0054				
		(0.0081)				
ln Δ (Exports/GDP)			-0.0082			
			(0.010)			
Chinese Imports, % of GDP				0.032		
				(0.16)		
Trade Balance, % of GDP					0.031	
					(0.041)	
Δ Trade Balance, % of GDP						-0.041
						(0.093)
Observations	1141	1380	1380	1119	1393	1386

Notes: Standard errors clustered by country in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. There are three sets of six regressions, for 18 regressions total. There are also 31 countries in this unbalanced panel, with data starting as early as 1886, 3 countries by 1903, 10 by 1922, 14 by 1960, and reaching a max of 29 by 2002 (with 31 countries included at one point or another). The dependent variable in Panel A is the log change in the top 1% share of income. The dependent variable in Panel B is the log of the top 1% share of income, and a lag of the dependent variable is included in this panel as a control. With FEs, of course, this setup could be subject to Nickell Bias, although with a 110 year sample for some countries, such as the US and Germany, this bias should be relatively small. The dependent variable in Panel C is once again the log change in the top 1% share of income, with various controls for trade shocks included.

4 Conclusion and Interpretation

We found that US workers in sectors most exposed to trade shocks are less likely to be employed, and are more likely to be unemployed or to have left the labor force when US relative prices are appreciated. However, we do not find that low-wage workers necessarily fare worse than high-wage workers, but rather that high-wage workers who happen to have low education levels tend to do very badly. We also do not find much evidence for various trade shocks being correlated with increases in inequality at the sectoral level, nor did inequality increase disproportionately in capital or technology-intensive sectors. In addition, the international evidence also points to the conclusion that the impact of trade shocks on overall inequality is at best subtle. Rising trade and trade deficits do not appear to be correlated with inequality. Both the disaggrated data for the US and the aggregate international evidence also does not provide any support for the thesis that skill-biased technological change is at the heart of the recent rise in inequality in many countries. Both of these conclusions are also consistent with the observation that much of the action in inequality in the US, UK, and other countries experiencing increases in inequality is confined to the top 1%, .1%, or even .01% of the population (see Piketty and Saez (2003)). While this fact could perhaps be consistent with a story in which technology or trade allows superstars to prosper, this is not the usual story told in which computers or non-routine skills are seen as being the key to rising inequality.

While little support is found for trade or technology, in this paper we find additional support for the thesis that it was changes in the top marginal tax rates that led to the large rise in inequality in the US and other countries since 1980, as we find strong effects on a new set of data. Our finding that the level of top marginal tax rates affects changes in top 1% share of income indicates that "history matters" for the top 1% share, and provides further evidence that taxes matter via bargaining.

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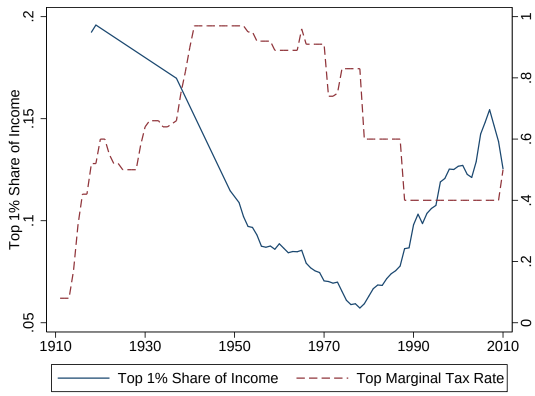
5 Appendix

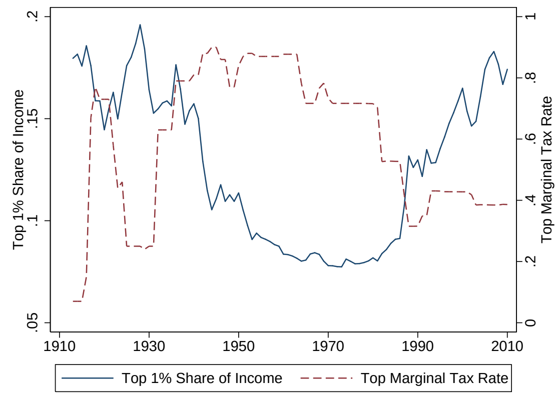
5.1 Top Marginal Tax Rate Data

Table 8: Data Sources for Top Marginal Tax Rates

	Source	Coverage
1	Piketty, Saez, Stantcheva (2014)	18 countries from 1960-2010
2	Piketty (2014)	Data from 1900 for the UK, Germany, the US, Japan, and France
3	KPMG	Many countries from 2006-2015 (used whenother data not available)
4	University of Michigan, World Tax Project	Many Countries from 1974-1999
5	OECD via The Tax Foundation	OECD countries from 2000 to 2012
6	Roine, Vaclos, Waldenstrom (2009)	Argentina, 1932-1973 (with missing), 2001
7	Roine, Vaclos, Waldenstrom (2009)	Australia, 1909-1939, 1954-1973, 1977-1979
8	Saez, Veall (2005)	Canada, 1920-1959
9	Peter, Buttrick, and Duncan (1999)	China, 2001
10	Atkinson and Sogaard (2008)	Denmark, 1915-2010
11	Roine, Vaclos, Waldenstrom (2009)	France, pre-1960
12	Roine, Vaclos, Waldenstrom (2009)	Germany, 1958-1959
13	Piketty (2014)	Germany, 1900-1957
14	Alvaredo, Bergeron, Cassan (2014)	India, 1886-1923
15	Leigh and Van der Eng (2007)	Indonesia, 1920-1939, 1944, 1956, 1964
16	Constructed from Finance Acts	Ireland, 1923-1959
17	Saez and Moriguchi (2008)	Japan, 1870-1899
18	Malaysian Institute of Accountants	Malaysia, 2000-2006
19	Reynolds	Mauritius, 1979
20	Atkison, Barnes, Piketty (2010), Chapter 10	Netherlands, 1914-1939, 1946, 1950
21	Atkinson and Leigh (2008)	New Zealand, 1911-1959
22	Atkison, Barnes, Piketty (2010), Chapter 11	Portugal
23	University of Michigan, World Tax Project	Singapore, 1974-1999
24	W.e.f. 2001, w.e.f. 2005	Singapore, 2001-2005
25	Finance Statistics of South Africa	South Africa, 1991-2014
26	Kim and Kim (2015)	South Korea, 1933-1940, 1963-2010
27	OECD via the Tax Foundation	South Korea, 2011-2012
28	Atkison, Barnes, Piketty (2010), Chapter 10	Spain
29	Du Rietz, Johansson, Stenkulaa (2013)	Sweden
30	Peter, Buttrick, and Duncan (1999)	Taiwan, 1997-2005
31	Atkinson, Anthony, and Leigh (2010)	United Kingdom
32	IRS, Statistics of Income Division	United States, 2011-2014

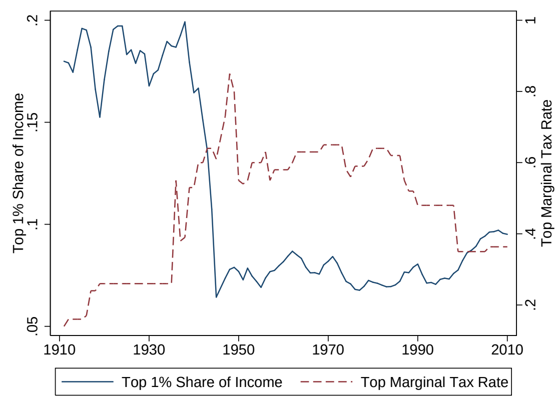


(a) UK

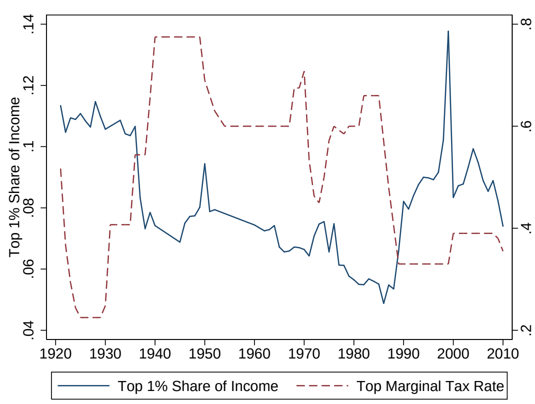


(b) USA

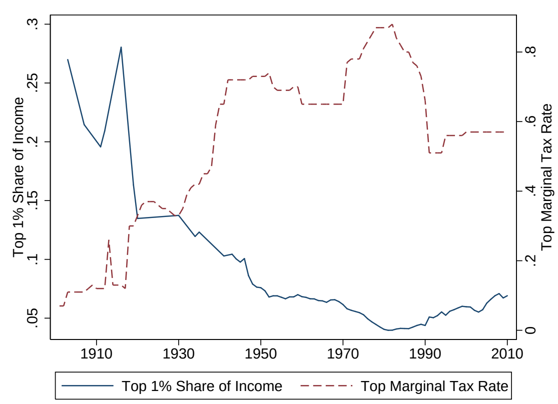
(c) Germany



(d) Japan



(e) New Zealand



(f) Sweden

Figure 9: Top Marginal Tax Rate vs. 1% Share of Income

Sources: World Wealth and Incomes Database

Table 9: Impact of RER Shocks on the Labor Market, by Sex and Education

	$ln\,\Delta$ HW	$ln\,\Delta$ 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)
$ln\,\Delta$ 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)
$ln\,\Delta$ 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. Women						
Implied Trade Impact	0.057	0.081	-0.45**	0.22**	0.23**	-0.45
	(0.13)	(0.13)	(0.19)	(0.096)	(0.12)	(0.29)
Observations	68924	69494	105519	105519	105519	45168
C. Men						
Implied Trade Impact	-0.073	-0.17	-0.43***	0.27***	0.16**	-0.026
	(0.11)	(0.12)	(0.10)	(0.067)	(0.071)	(0.24)
Observations	148347	150737	201551	201551	201551	89907
D. 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
E. 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
F. Top 25% of Wages						
Implied Trade Impact	-0.047	-0.20	-0.23**	0.15*	0.080	-0.84
	(0.24)	(0.23)	(0.11)	(0.084)	(0.062)	(0.66)
Observations	53729	54312	58802	58802	58802	16791
G. Bottom 50% of Wages						
Implied Trade Impact	0.050	0.010	-0.31**	0.22***	0.089	-0.075
	(0.073)	(0.098)	(0.12)	(0.059)	(0.082)	(0.16)
Observations	163542	164271	186325	186325	186325	114969

Errors clustered by sector and year in parentheses. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. There are 30 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-E, the other controls are suppressed for space. WARULC = Weighted Average Relative Unit Labor Costs, a measure of the real exchange rate. Panel A includes the full sample, Panel B includes only women, C men, D only those without any college education, and Panel D includes those who have at least some college.

Table 10: Impact of Realized Trade Shocks Market, by Wage Levels

	$ln\,\Delta$ HW	$ln\,\Delta$ 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)
$ln\,\Delta$ 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)
$ln\,\Delta$ 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 adn 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.

6 Online Appendix

Table 11: 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.

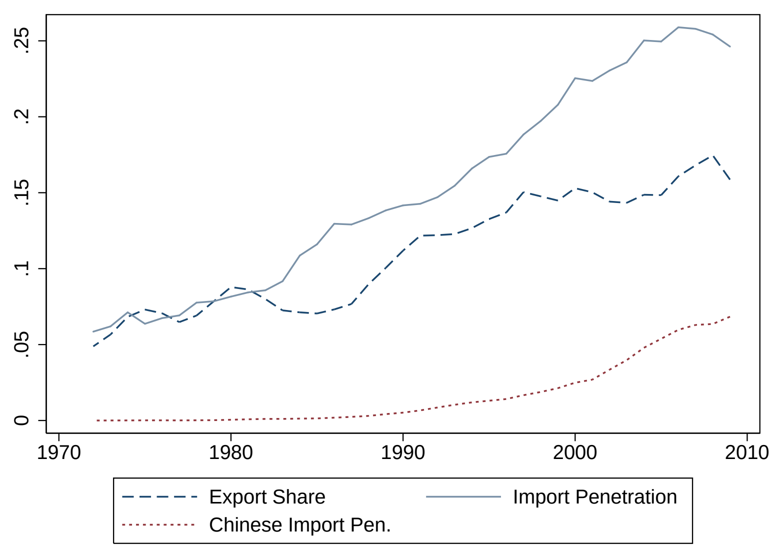


Figure 10: Export Share, Inequality, and Chinese Import Penetration

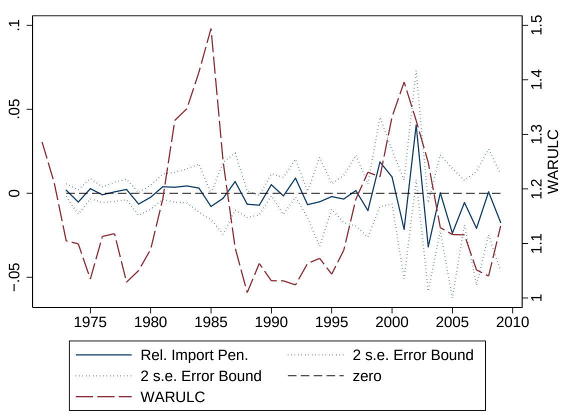
Table 12: Robustness Exercises: Non-Production Worker and Production Worker Wages

	(1)	(2)	(3)	(4)	(5)	(6)
Dep.Var: Log Δ Non-Prod. Wages						
L.ln(WARULC)*Rel.Openness	-0.0035	-0.0035	-0.011	0.00051	-0.027	-0.0052
	(0.0075)	(0.0075)	(0.012)	(0.0085)	(0.017)	(0.010)
L.ln(iWARULC)*R.Import Pen.	-0.0018	-0.0018	-0.011	-0.00040	-0.039**	-0.0034
	(0.0082)	(0.0082)	(0.011)	(0.0070)	(0.016)	(0.0091)
L.ln(eWARULC)*R.Export Sh.	-0.0029	-0.0029	-0.0039	0.0020	0.0032	-0.0021
	(0.011)	(0.011)	(0.014)	(0.010)	(0.015)	(0.011)
Δ Export Share	0.042	0.042	0.072	0.039	0.050	0.044
	(0.059)	(0.059)	(0.051)	(0.036)	(0.046)	(0.066)
Δ Import Penetration	-0.047	-0.047	-0.044	-0.039	-0.073	-0.044
	(0.089)	(0.089)	(0.054)	(0.043)	(0.053)	(0.13)
Dep.Var: Log Δ Prod. Wages						
L.ln(WARULC)*Rel.Openness	-0.022***	-0.022***	-0.0037	-0.011	-0.013	-0.024***
	(0.0079)	(0.0079)	(0.010)	(0.0067)	(0.016)	(0.0083)
L.ln(iWARULC)*R.Import Pen.	-0.014**	-0.014**	-0.014	-0.0087	-0.030*	-0.015*
	(0.0063)	(0.0063)	(0.010)	(0.0075)	(0.018)	(0.0082)
L.ln(eWARULC)*R.Export Sh.	-0.0085	-0.0085	0.0059	-0.0023	0.0074	-0.0090
	(0.0092)	(0.0092)	(0.012)	(0.0094)	(0.015)	(0.0100)
Δ Export Share	0.00085	0.00085	0.018	0.0051	-0.022	0.0087
	(0.034)	(0.034)	(0.035)	(0.037)	(0.045)	(0.036)
Δ Import Penetration	-0.039	-0.039	-0.0041	-0.036	-0.021	-0.039
	(0.046)	(0.046)	(0.059)	(0.052)	(0.055)	(0.051)
Year FE	No	Yes	No	Yes	No	Yes
Industry FE	No	No	Yes	Yes	No	Yes
Full Controls	Yes	Yes	Yes	No	No	Yes

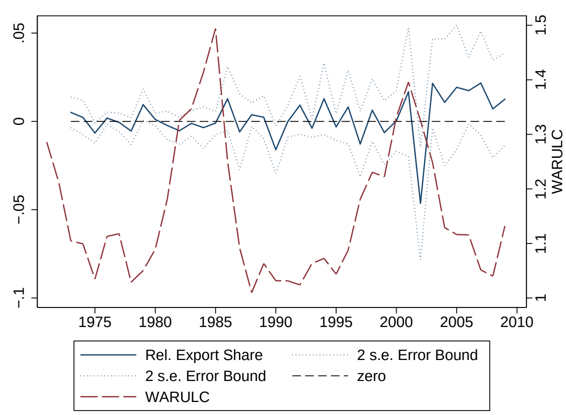
Two-way Clustered standard errors in parenthesis, clustered by year and 4-digit SIC sectors. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. There are six sets of six regressions, for 36 regressions total. The first three rows of six regressions use the log change in the ratio of non-production to production worker wages as a proxy for inequality. Rows 4-6 use sectoral unit labor costs as the dependent variable. Each column contains different combinations of controls and fixed effects as indicated. For example, all of the regressions in column one include a full set of controls, but no year or sectoral fixed effects, while column (6) includes year and sectoral fixed effects and a full set of controls. All regressions are weighted by initial period value-added.

Figure 11: Impact of Relative Openness on Inequality by Year

Notes: These are the results from yearly regressions of relative openess on inequality by sector with controls for demand and TFP growth, with two standard deviation error bounds plotted in dotted dark blue, compared to the WARULC index in maroon. Campbell (2016) plots a similar graph for employment, and shows that periods of RER appreciation are associated with steep declines in employment in relatively open sectors.



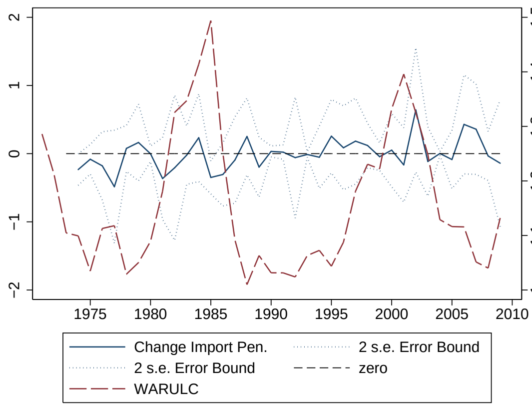
(a) Import Penetration



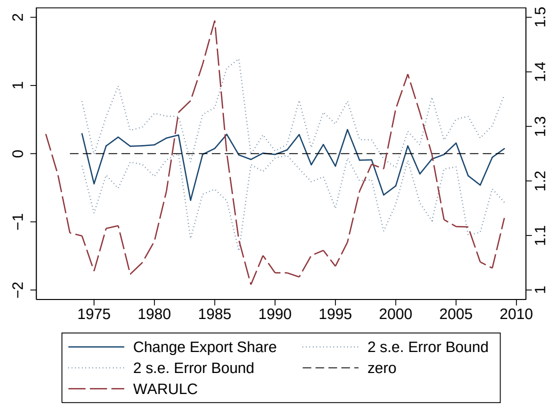
(b) Export Share

Figure 12: Trade Exposure and Changes in Inequality, 1973-2009

Notes: These are the results from yearly regressions of import penetration and export share on inequality by sector with controls for demand and TFP growth, with two standard deviation error bounds plotted in dotted dark blue, compared to the WARULC index in maroon.



(a) Import Penetration



(b) Export Share

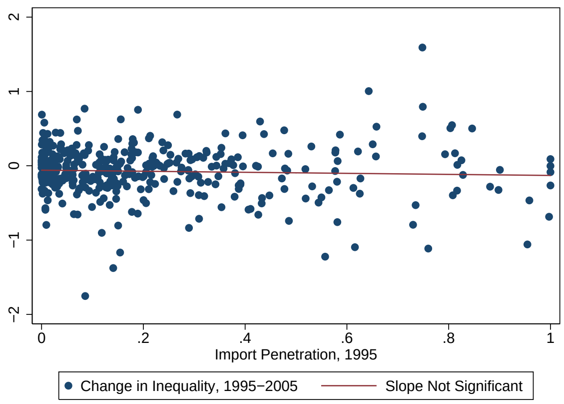
Figure 13: Change in Trade Exposure vs. Evolution in Inequality, 1973-2009

Notes: These are the results from yearly regressions of the change in import penetration and the change in export share on inequality by sector with controls for demand and TFP growth, with two standard deviation error bounds plotted in dotted dark blue, compared to the WARULC index in maroon. The main takeaway here is that neither import penetration or the export share of shipments seem to be correlated with increases in inequality during periods of trade shocks.

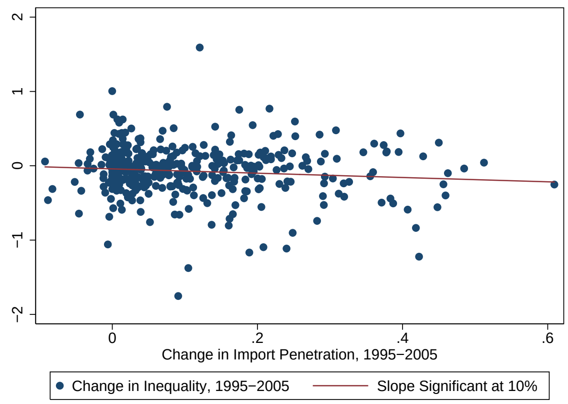
Table 13: Taxes, Trade, and the Top 1%: Other Trade Shocks

	(1)	(2)	(3)	(4)	(5)	(6)
Top Marginal Tax Rate	-0.047***	-0.083***	-0.069***	-0.083***	-0.091***	-0.069***
	(0.016)	(0.021)	(0.022)	(0.021)	(0.025)	(0.022)
Imports/GDP	-0.028					
	(0.038)					
ln Δ LW Imports/GDP		-0.0059				
		(0.017)				
Exports to LW/GDP			0.021			
			(0.055)			
ln Δ Exports to LW/GDP				-0.0053		
				(0.020)		
ln Δ Chinese Imports/GDP					-0.00041	
					(0.0071)	
Trade with LW/GDP						0.022
						(0.035)
Observations	1393	1126	1141	1126	1068	1141

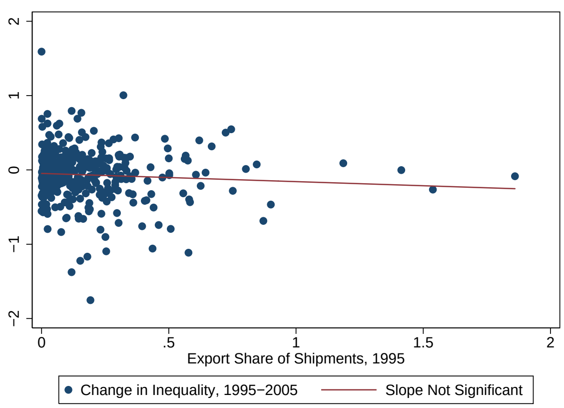
Notes: Standard errors clustered by country in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. The dependent variable is the log change in the top 1% share of income. There are also 30 countries in this unbalanced panel, with data starting as early as 1886, 3 countries by 1903, 10 by 1922, 14 by 1960, and reaching a max of 29 by 2002 (with 30 countries included at one point or another). The dependent variable in Panel A is the log change in the top 1% share of income. The dependent variable in Panel B is the log of the top 1% share of income, and a lag of the dependent variable is included in this panel as a control. With FEs, of course, this setup could be subject to Nickell Bias, although with a 110 year sample for some countries, such as the US and Germany, this bias should be relatively small. The dependent variable in Panel C is once again the log change in the top 1% share of income, with various controls for trade shocks included.



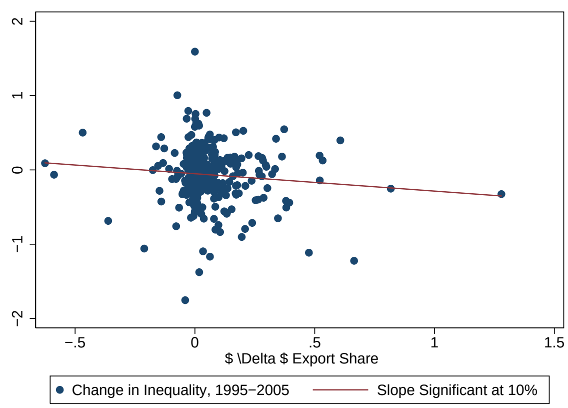
(a) Δ Inequality vs. Initial Import Penetration



(b) Δ Inequality vs. Δ Import Exposure



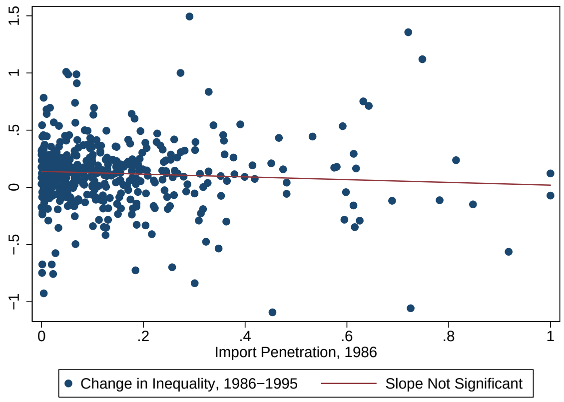
(c) Δ Inequality vs. Initial Export Share



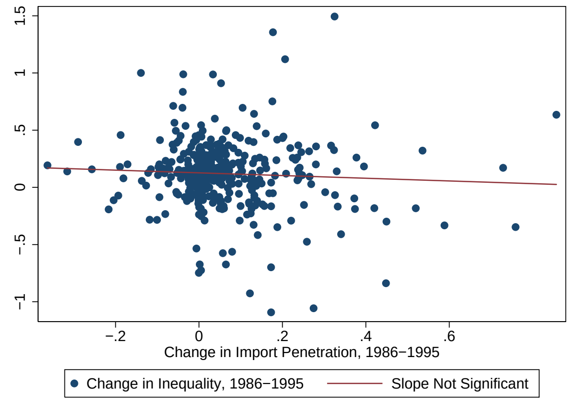
(d) Δ Inequality vs. Δ Export Share

Figure 14: Trade Exposure and Inequality, 1995-2005

Notes: Each dot is a 4-digit SIC manufacturing sector. Inequality here is proxied by the ratio of non-production to production worker wages in the manufacturing sector, from the ASM. Values above zero on the y-axis thus indicate an increase in inequality. Trade data are from WITS.



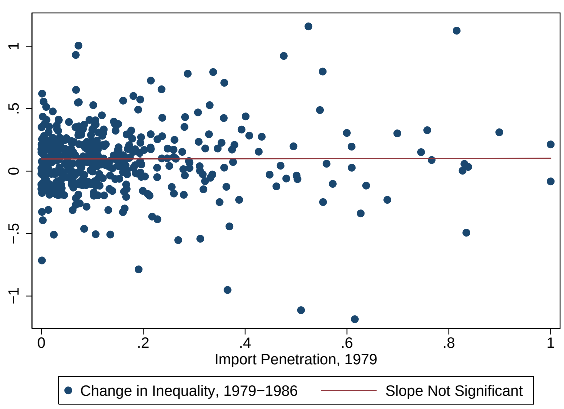
(a) Δ Inequality vs. Initial Import Penetration



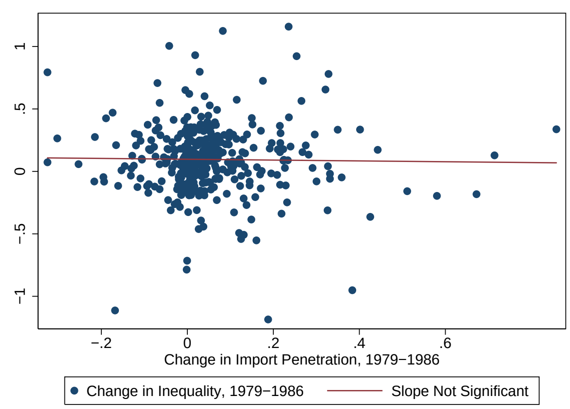
(b) Δ Import Exposure vs. Δ Inequality

Figure 15: Trade Exposure and Inequality, 1986-1995

Notes: Inequality here is proxied by the ratio of non-production to production worker wages in the manufacturing sector, from the ASM. Values above zero on the y-axis thus indicate an increase in inequality. Trade data come from WITS.



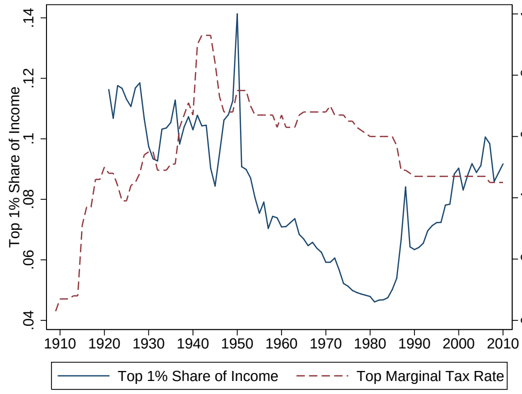
(a) Δ Inequality vs. Initial Import Penetration



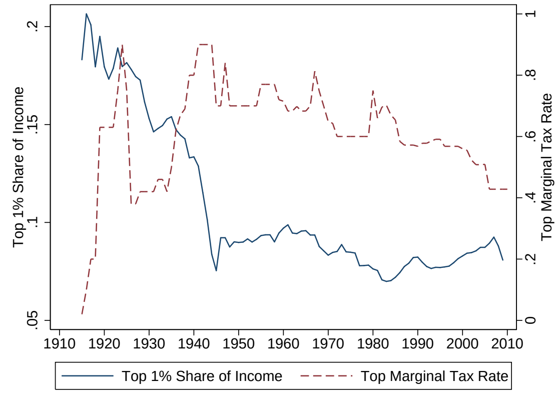
(b) Δ Import Exposure vs. Δ Inequality

Figure 16: Trade Exposure and Inequality, 1979-1986

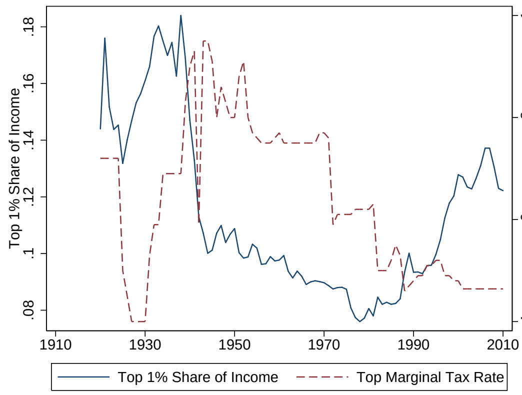
Notes: Inequality here is proxied by the ratio of non-production to production worker wages in the manufacturing sector, from the ASM. Values above zero on the y-axis thus indicate an increase in inequality. Trade data come from WITS.



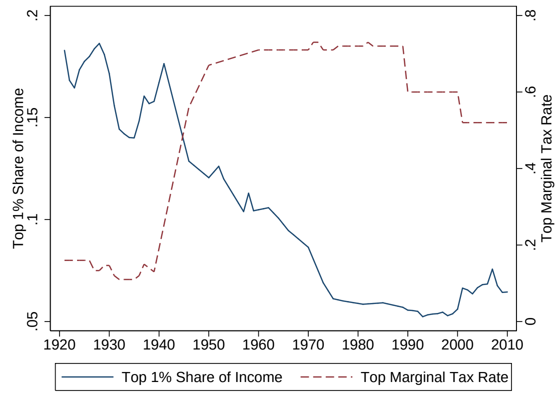
(a) Australia



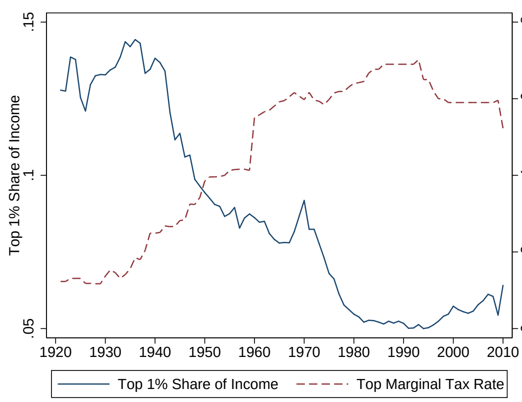
(b) France



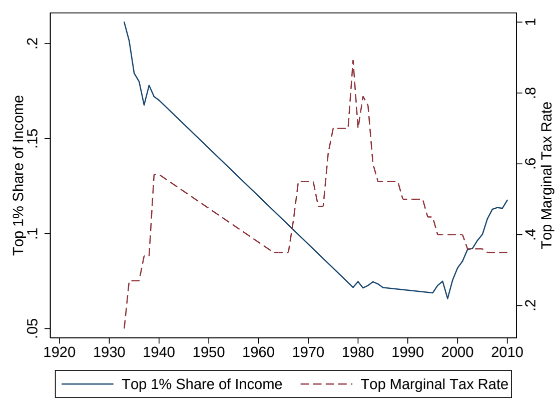
(c) Canada



(d) Netherlands



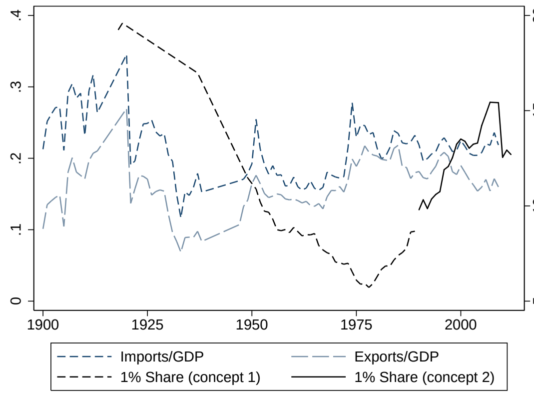
(e) Denmark



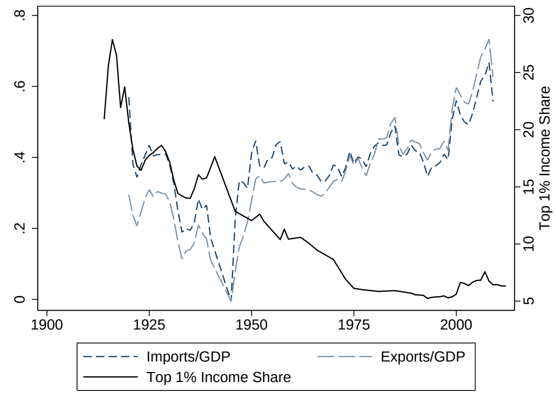
(f) Korea

Figure 17: Top Marginal Tax Rate vs. 1% Share of Income

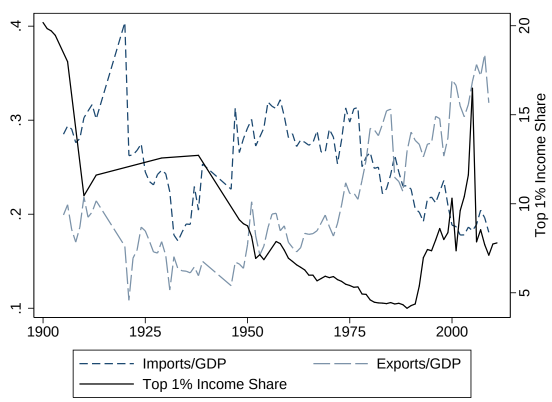
Sources: World Wealth and Incomes Database, and trade data from IMF DOTS and WITS (to subtract oil-related sectors).



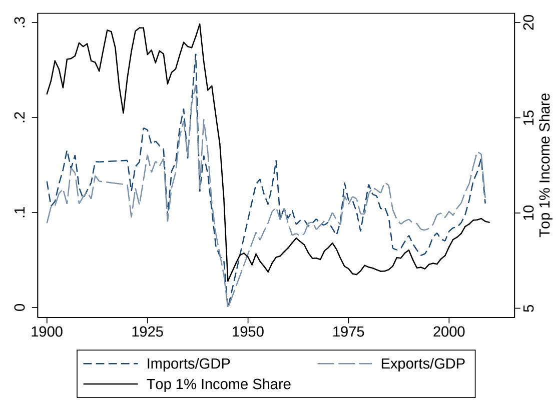
(a) UK



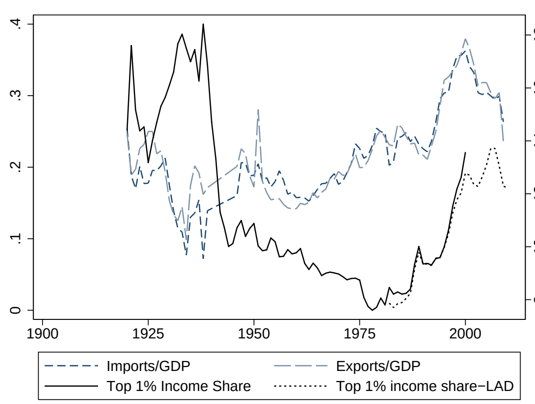
(b) Netherlands



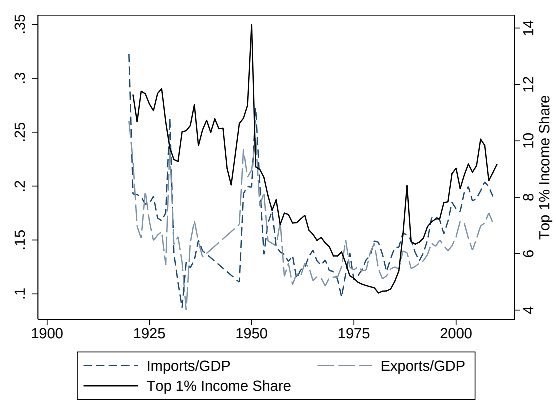
(c) Norway



(d) Japan



(e) Canada



(f) Australia

Figure 18: Trade vs. 1% Share of Income

Table 14: Taxes, Trade, and the Top 1%: Dynamic Models w/ Quantile Regs

	(1)	(2)	(3)	(4)	(5)	(6)
A. Dep.Var.: ln Δ Top 1%Sh.						
Top Marginal Tax Rate	-0.041***	-0.051***	-0.074***	-0.042***	-0.079***	-0.058***
	(0.0097)	(0.0064)	(0.0074)	(0.016)	(0.0089)	(0.021)
ln Δ GDP	0.17**	0.19*	0.21**	0.094	0.24*	0.11
	(0.084)	(0.099)	(0.10)	(0.11)	(0.13)	(0.097)
Observations	1532	1532	1532	1532	1532	1532
B. Dep.Var.: ln Top 1% Sh.						
L.ln(Top 1% Sh.)	1.00***	1.00***	1.00***	1.00***	1.00***	1.00***
	(0.0017)	(0.00057)	(0.0029)	(0.0024)	(0.0037)	(0.0022)
Top Marginal Tax Rate	-0.045***	-0.053***	-0.076***	-0.047***	-0.078***	-0.058**
	(0.011)	(0.0072)	(0.011)	(0.016)	(0.0089)	(0.023)
Observations	1532	1532	1532	1532	1532	1532
Year Trend	No	Yes	Yes	No	No	No
Year FE	No	No	No	Yes	No	Yes
Country FE	No	No	Yes	Yes	Yes	Yes
Country-Specific Year Trend	No	No	No	No	Yes	Yes
C. Dep.Var.: ln Δ Top 1% Sh.						
Top Marginal Tax Rate	-0.058***	-0.049***	-0.046***	-0.062***	-0.047***	-0.048**
	(0.015)	(0.018)	(0.018)	(0.020)	(0.015)	(0.019)
Poor-country Imports/GDP	-0.069					
	(0.081)					
ln Δ (Imports/GDP)		0.0015				
		(0.0025)				
ln Δ (Exports/GDP)			-0.0050			
			(0.058)			
Chinese Imports, % of GDP				-0.11		
				(0.18)		
Trade Balance, % of GDP					0.040	
					(0.036)	
Δ Trade Balance, % of GDP						-0.12
						(0.079)
Observations	1141	1380	1380	1119	1393	1386

Notes: Standard errors clustered by country in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. There are three sets of six quantile regressions, for 18 regressions total. There are also 31 countries in this unbalanced panel, with data starting as early as 1886, 3 countries by 1903, 10 by 1922, 14 by 1960, and reaching a max of 29 by 2002 (with 31 countries included at one point or another). The dependent variable in Panel A is the log change in the top 1% share of income. The dependent variable in Panel B is the log of the top 1% share of income, and a lag of the dependent variable is included in this panel as a control. With FEs, of course, this setup could be subject to Nickell Bias, although with a 110 year sample for some countries, such as the US and Germany, this bias should be relatively small. The dependent variable in Panel C is once again the log change in the top 1% share of income, with various controls for trade shocks included.

Table 15: Taxes vs. Trade, Impact on the Non-Top Income Shares

	(1)	(2)	(3)	(4)	(5)	(6)
A. Dep.Var.: ln Δ 90-99% Sh.						
Top Marginal Tax Rate	-0.043	-0.021	-0.021	-0.045	-0.016	-0.019
	(0.030)	(0.017)	(0.016)	(0.033)	(0.016)	(0.016)
Poor-country Imports/GDP	0.062					
	(0.16)					
ln Δ (Imports/GDP)		-0.011				
		(0.020)				
ln Δ (Exports/GDP)			-0.043**			
			(0.018)			
Chinese Imports, % of GDP				-0.13		
				(0.11)		
Trade Balance, % of GDP					-0.044***	
					(0.015)	
Δ Trade Balance, % of GDP						-0.11
						(0.099)
Observations	998	1110	1110	981	1119	1113
B. Dep.Var.: ln Δ Bottom 90% Sh.						
Top Marginal Tax Rate	0.019**	0.0070	0.0067	0.019**	0.0049	0.0048
	(0.0073)	(0.0058)	(0.0055)	(0.0074)	(0.0060)	(0.0058)
Poor-country Imports/GDP	0.0052					
	(0.016)					
ln Δ (Imports/GDP)		0.014				
		(0.012)				
ln Δ (Exports/GDP)			0.028***			
			(0.0070)			
Chinese Imports, % of GDP				0.019		
				(0.032)		
Trade Balance, % of GDP					0.0085	
					(0.0099)	
Δ Trade Balance, % of GDP						0.094**
						(0.040)
Observations	1001	1111	1111	984	1121	1114

Notes: Standard errors clustered by country in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. There are six regressions in each panel, for 12 regressions total. There are also 25 countries in this unbalanced panel. The dependent variables in Panel's A and B are the log change in the income share of the bottom 90% and the 90th to 999th percentile, respectively.

Table 16: Taxes vs. Trade, Impact on the Non-Top Income Shares

	(1)	(2)	(3)	(4)	(5)	(6)
A. Dep.Var.: ln Δ 90-99% Sh.						
Top Marginal Tax Rate	-0.0067	-0.0021	-0.0000072	-0.0056	-0.0032	-0.0014
	(0.010)	(0.0071)	(0.0053)	(0.011)	(0.0097)	(0.0059)
Poor-country Imports/GDP	-0.11					
	(0.083)					
ln Δ (Imports/GDP)		-0.0014				
		(0.021)				
ln Δ (Exports/GDP)			-0.016**			
			(0.0084)			
Chinese Imports, % of GDP				-0.14		
				(0.22)		
Trade Balance, % of GDP					0.0049	
					(0.035)	
Δ Trade Balance, % of GDP						-0.11
						(0.089)
Observations	998	1110	1110	981	1119	1113
B. Dep.Var.: ln Δ Bottom 90% Sh.						
Top Marginal Tax Rate	0.012**	0.0088**	0.0097***	0.013***	0.0080	0.0071*
	(0.0046)	(0.0035)	(0.0031)	(0.0045)	(0.0056)	(0.0037)
Poor-country Imports/GDP	0.046					
	(0.076)					
ln Δ (Imports/GDP)		0.0015				
		(0.0084)				
ln Δ (Exports/GDP)			0.014			
			(0.012)			
Chinese Imports, % of GDP				0.058		
				(0.11)		
Trade Balance, % of GDP					0.0011	
					(0.025)	
Δ Trade Balance, % of GDP						0.066**
						(0.027)
Observations	1001	1111	1111	984	1121	1114

Notes: Standard errors clustered by country in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. There are six regressions in each panel, for 12 regressions total. There are also 25 countries in this unbalanced panel. The dependent variables in Panel's A and B are the log change in the income share of the bottom 90% and the 90th to 999th percentile, respectively. Each regression is a quantile regression on the median.

Table 17: Impact of Trade Shocks on Non-Top Income Shares: Robustness

	ln Δ <90%	ln Δ <90%	ln Δ 90-99%	ln Δ 90-99%
Δ Trade Balance, % of GDP	0.094**	0.022		
	(0.040)	(0.023)		
ln Δ (Exports/GDP)			-0.043**	-0.018
			(0.018)	(0.023)
Observations	1114	778	1110	778
Sample	Full	PSS Sample	Full	PSS Sample

Notes: Standard errors clustered by country in parenthesis. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The dependent variable in the first two columns is log changes in income share of the bottom 90% of earners, and in colums (3) and (4) it is the log change in the income share of those between the 90th and 99th percentiles. There are 25 countries in the full sample, and 18 in the "PSS" – Piketty, Saez, and Stantcheva sample.

7 Online Data Appendix

7.1 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.

7.1.1 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:

$$\sum_j (input\, purchases\, of\, j\, by\, industry\, i) * \frac{imports\, of\, j}{consumption\, of\, j} \tag{7.1}$$

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:

$$M_{Domestic} = M\left(1 - \left(\frac{X}{M + Shipments}\right)\right) \tag{7.2}$$

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:

$$shipments + M_{Domestic} - X_{Domestic}, \tag{7.3}$$

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 7.1 with that based on equation 7.3, which in practice is a 20% upwards adjustment for these observations. In this way, their rank in terms of import penetration based on equation 7.3 is roughly preserved. The original and reformed series are compared below in Figure 19.

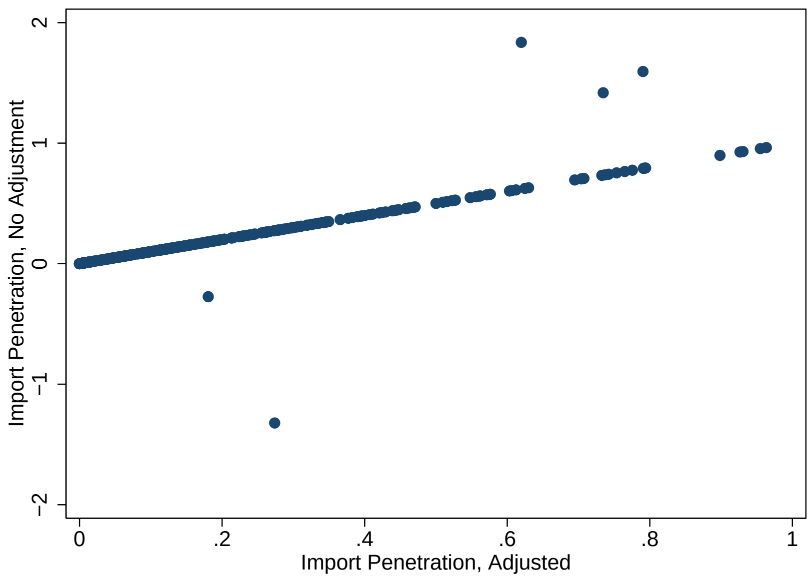


Figure 19: Import Penetration vs. Adjusted Import Penetration, 1992

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.

- 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 combinations, 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 7.1 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:

$$ln(MPI)_{ijt} = \rho L5.ln(MPI)_{ij,t-1} + \beta_1 ln\Delta ImportPen._{jt} + \beta_2 ln\Delta Materials_{it} + \epsilon_{ijt} \tag{7.4}$$

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 18, 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:

$$ln\Delta MPI_{ijt} = \beta_1 ln\Delta ImportPen._{jt} + \beta_2 ln\Delta Materials_{it} + \epsilon_{ijt} \tag{7.5}$$

We run this model for the full period (column 2), and for each of the years individually in Table 18, 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.

Table 18: 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.

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 20 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 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.)

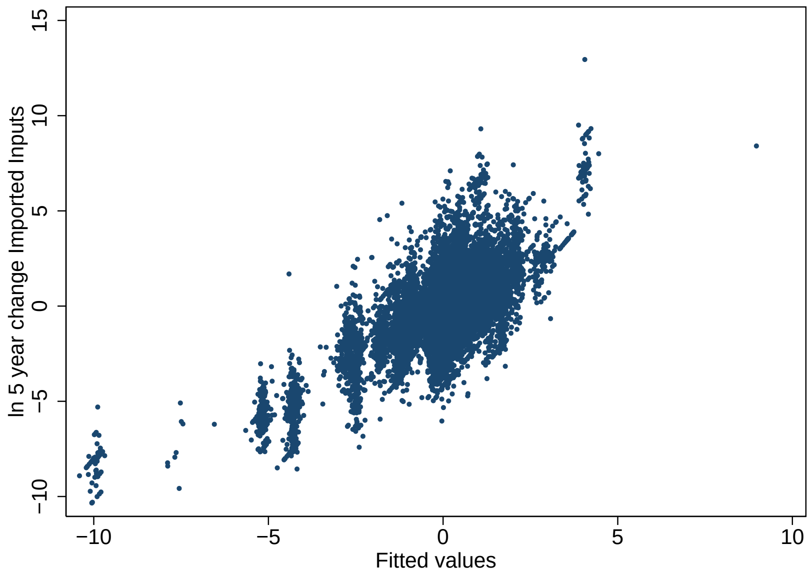


Figure 20: Out-of-Sample Test of Intermediate Imports, 1992

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.

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

$$MPI_{ijt} = MPI_{ij,t-1} * exp(1.14 * ln\Delta ImportPen. + .46 * ln\Delta MaterialsCost) \quad (7.6)$$

For years in between benchmark years, we use a weighted average of the forward extrapolation from the previous benchmark year (using formula 7.6, and the backwards extrapolation from the subsequent benchmark year (which also uses a formula similar to 7.6 to extrapolate backwards using the regression coefficients).

$$MPI_{ij,t+s} = \frac{k-s}{k} MPI^F_{ij,t+s} + \frac{s}{k} MPI^B_{ij,t+s} \quad (7.7)$$

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 21, 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.

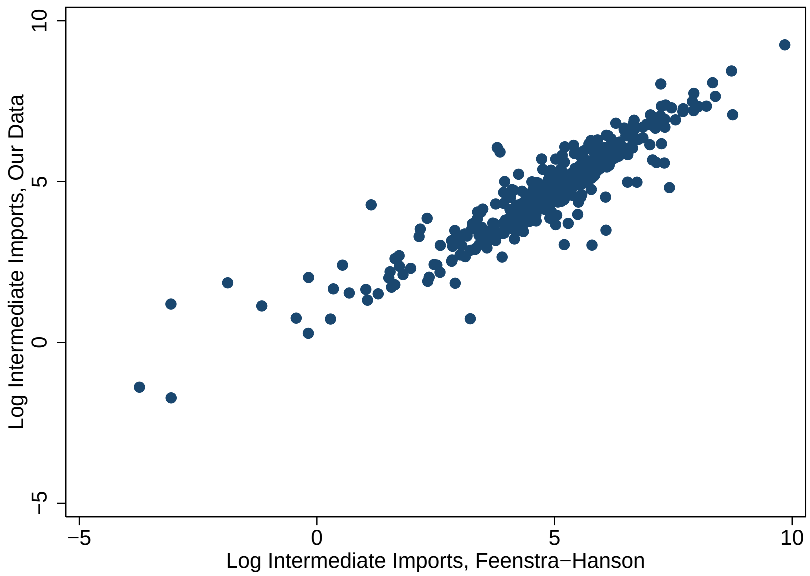


Figure 21: Feenstra-Hanson Comparison, 1990

- 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.

7.1.2 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.[26]

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

- 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 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 20 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 18, 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 22 below).

Table 19: 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

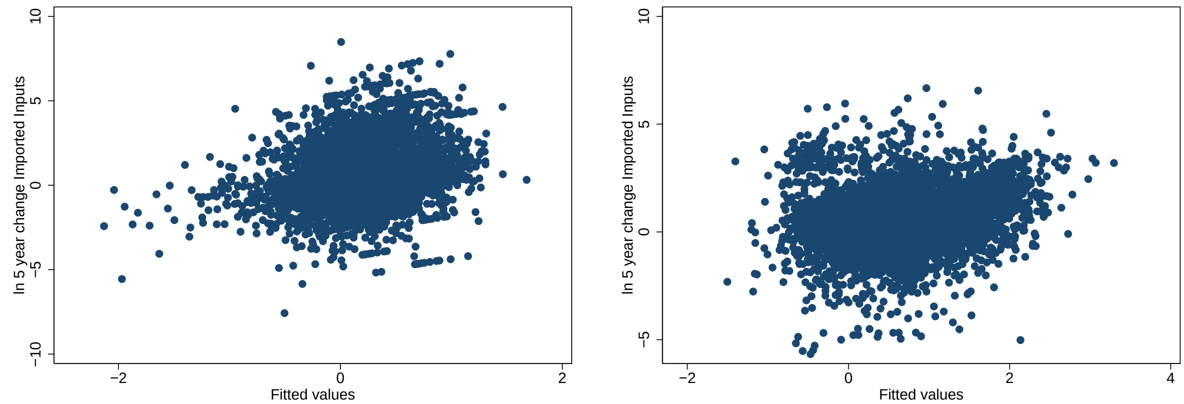
Notes: Standard errors in parenthesis. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. The dependent variable in column is the log of imported intermediate inputs in column (1), and the log 5 year change of imported intermediates in columns (2)-(6). Columns (2) and (4) were run on the full sample, while columns (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.

- Given the somewhat rough out-of-sample test results in Figure 22, 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.

- 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 20.

- 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:

$$MPI_{ijt} = MPI_{ij,t-1} * exp(.99 * ln\Delta Imports + .76 * ln\Delta MaterialsCost) \qquad (7.8)$$



(a) 2002

(b) 2007

Figure 22: 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.

Table 20: 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.

For years in between benchmark years, we use a weighted average of the forward extrapolation from the previous benchmark year (using formula 7.8, and the backwards extrapolation from the subsequent benchmark year (which also uses a formula similar to 7.8 to extrapolate backwards using the regression coefficients).

$$MPI_{ij,t+s} = \frac{k-s}{k} MPI^F_{ij,t+s} + \frac{s}{k} MPI^B_{ij,t+s} \tag{7.9}$$

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.