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Globalization and the China Shock: A Reassessment of Autor, Dorn, and Hanson

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LINK TO ABSTRACT

In a seminal article entitled "The China Syndrome: Local Labor Market Effects of Import Competition in the United States" and published in 2013 in the *American Economic Review*, David Autor, David Dorn, and Gordon Hanson (ADH) estimated the effect of imports of manufactured goods from China from 1990 to 2007 on employment in manufacturing, overall (un)employment, and wages in the USA during the same period. The authors concluded:

> Our analysis finds that exposure to Chinese import competition affects local labor markets not just through manufacturing employment, which unsurprisingly is adversely affected, but also along numerous other margins. Import shocks trigger a decline in wages that is primarily observed outside of the manufacturing sector. Reductions in both employment and wage levels lead to a steep drop in the average earnings of households. These changes contribute to rising transfer payments through multiple federal and state programs, revealing an important margin of adjustment to trade that the literature has largely overlooked. (ADH, 2159)

By mid-February 2026, the ADH "China Syndrome" article had been cited over 7,000 times according to Google Scholar, and ADH's related "China Shock" article (Autor, Dorn, and Hanson 2016) has been cited over 2,200 times. The basic

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approach of the "China Syndrome" article has been used to study the effect of Chinese imports on voting and political expression (Autor, Dorn, Hanson, and Majlesi 2020), on research and development expenditures and patents of US firms (Autor, Dorn, Hanson, Pisano, and Shu 2020), and on marriage and out-of-wedlock birth (Autor, Dorn, and Hanson 2019).

In this comment, I question ADH's conceptualization of the problem, the validity of their empirical approach, and the interpretation of their findings. I provide evidence that their empirical approach does not identify the ceteris paribus effect of Chinese imports on US employment, wages, and social welfare payments. Nor, in my opinion, does it identify what I refer to as the equilibrium effect of Chinese imports on US employment, wages, and social welfare payments. My conclusion is that ADH's estimates are largely uninformative.

Before presenting my evidence, however, it is important to document that ADH focuses on Chinese imports as the cause of the consequences purportedly documented in their work—if the titles of the "China Syndrome" and "China Shock" articles are not sufficient. Here are statements from the ADH 2013, "The China Syndrome" (italicization and boldface added by me):

> We find that local labor markets that are exposed to rising low-income-country imports due to ***China's rising competitiveness*** experience increased unemployment, decreased labor-force participation, and increased use of disability and other transfer benefits, as well as lower wages. (2125)

> To ***identify the supply-driven component of Chinese imports***, we instrument for growth in Chinese imports to the United States using the contemporaneous composition and growth of Chinese imports in eight other developed countries. (2129)

> We more conservatively estimate that ***Chinese import competition*** explains 16 percent of the US manufacturing employment decline between 1990 and 2000, 26 percent of the decline between 2000 and 2007, and 21 percent of the decline over the full period. (2140)

> A $1,000 per worker increase in a CZ's ***exposure to Chinese imports*** during a decade is estimated to reduce mean weekly earnings by −0.76 log points. (2146)

> We estimate that a $1,000 ***increase in Chinese import exposure*** leads to a rise in transfer payments of $58 per capita (1.01 log points in the logarithmic specification). (2148)

> The ***consequences of China trade for US employment, household income, and government benefit programs*** may contribute to public ambivalence toward globalization and specific anxiety about increasing trade with China. (2159)

I emphasize ADH's focus on China and Chinese imports because I show that the empirical analysis of ADH does not actually estimate the effect of Chinese imports; instead, under the most charitable interpretation of their findings, they estimate the effects of imports from many countries on US employment and wages. A less charitable interpretation, which I believe is the most accurate, suggests that we learn little about the impact of imports and, in particular, the impact of Chinese imports, from ADH's estimate.

It is not surprising that greater imports of manufactured goods reduce U.S. employment in manufacturing. It is the China claim that gets ADH most of the attention and something, as documented by their statements, they highlight and lean into, presumably to hype the appeal of their article. But it is not clear whether there was a unique "China Shock." Imports from many countries expanded in the late 1990s and early 2000s, and manufacturing in the US declined. It is an old story, and China's role in it is not particularly large as Donald Boudreaux has pointed out in a number of blog posts (link). Others have noted that the counterfactual outcome implied by the ADH 2013 analysis, that absent Chinese imports, there would simply be fewer imports, is nonsensical (Winship 2025). World trade would not stop in place. Finally, three previous studies have assessed the empirical approach of ADH 2013, although not as I do, and found that modifications to it reduce the magnitude and significance of ADH 2013's estimates of the effect of Chinese imports (Goldsmith-Pinkham, Sorkin, and Swift 2020; Borusyak, Hull, and Jaravel 2022; Francis 2026).

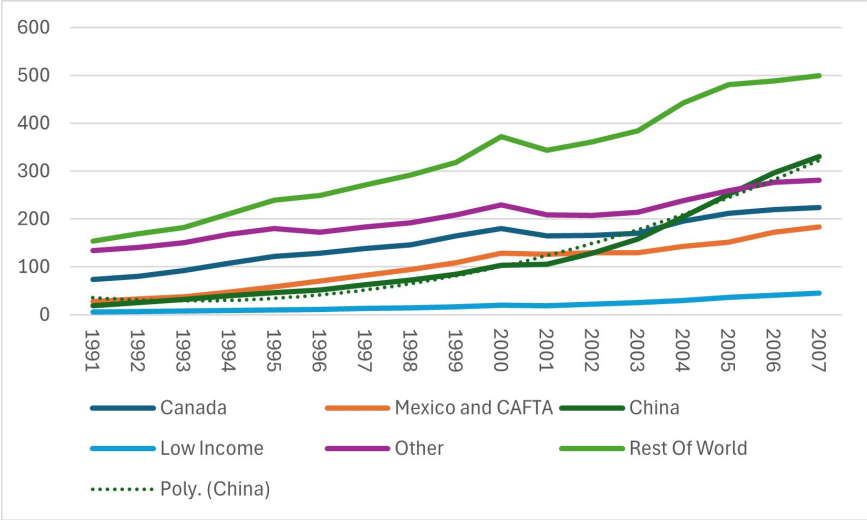
ADH's sole focus on China is conceptually wrong, and that fact leads them into the empirical problems I document. From the perspective of a domestic firm or a worker in a particular geographic location working in a specific industry, the critical issue is import competition in that industry and locale—not just Chinese imports. Thus, the correct measure of trade exposure is all (net) imports and not just Chinese imports, or Chinese imports and non-Chinese imports.[2] Indeed, Morrison (2018) showed that much of the growth of Chinese imports during the period from 1990 to 2017 came at the expense of other pacific-rim countries because of firms moving production to China. Data from ADH also show that imports grew for many countries, not just China or low-income countries.

Figure 1 shows imports into the USA from six exporter regions: Canada, Mexico and CAFTA, China, what ADH refers to as low-income countries, other countries, and rest of world. The data were obtained from David Dorn's Data Page (link). During this period, imports were growing in general, although Chinese imports grew more rapidly than imports from other countries, or country groups,

[2] Several authors (see for example Feenstra, Ma, and Xu 2019) have noted that "net" exports—exports minus imports—are the conceptually appropriate measure of trade exposure and that export growth is positively correlated with import growth.

particularly after 2000.[3] Also, while difficult to see in the figure, I have fit a second order polynomial trendline to the Chinese series to see whether there was an inflection point around 2001 and China's entry into the World Trade Organization.[4] It is not obvious that there was such an inflection point, as claimed by ADH, and that inflection point purportedly motivates their empirical approach. However, while ADH discuss entry into the WTO as a source of exogenous variation, their empirical analysis makes no use of this or any other policy variation but instead asserts that the growth of Chinese imports is mostly (but undoubtedly not fully) a result of supply-side shocks (e.g., massive Chinese subsidies for exports).

Figure 1: Imports (in billions) to USA by Exporter and Year

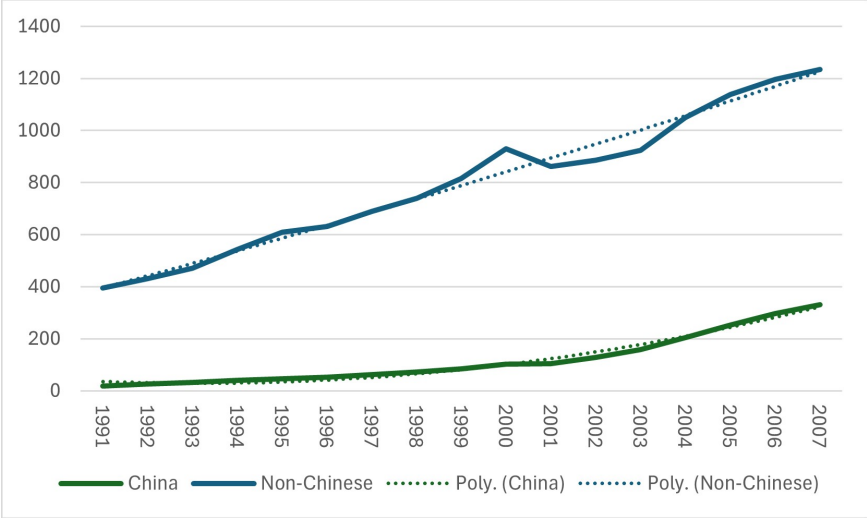


The general rise in imports is illustrated better in Figure 2, which compares imports from China and other countries. As shown in Figure 2, imports from other countries were much larger and rising faster than imports from China. Here, too, I fit a second order polynomial trendline, and it is clearer in this graph that there was no obvious break in trend of Chinese imports in 2001.

[3] From the David Dorn Data Page, I used china9114.dta (https://www.ddorn.net/data/china9114.zip). These are not the exact same data as used by ADH. In all analyses below, I use the exact same data as ADH (2013). The data in Figures 1 and 2 reveal more clearly that imports from other countries were substantial and growing during the period.

[4] While a more sophisticated approach could be taken to detect a break point, I make the argument to highlight that ADH do not actually use any policy variation in their analysis despite much discussion implying so. The numbers underlying Figure 2 are as follows: Imports from China rose by 21% per year between 2001–2007 versus 18.6% per year from 1991–2000. I thank an anonymous reviewer for these figures.

Figure 2: Imports (in billions) to USA by China, and Non Chinese Countries by Year



Figures 1 and 2 show that, while Chinese imports grew significantly from 1991 to 2007, imports from other countries also grew by similar absolute amounts, although Chinese imports grew faster proportionally. It is also the case that Chinese imports did not dominate many manufacturing industries. According to data from David Dorn, even by 2007, the Chinese share of total industry-specific imports was 15% or less for 50% of the manufacturing industries considered by ADH (https://www.ddorn.net/data.htm). Additionally, the Chinese share was 50% or greater for only 13% of the industries. Thus, the ADH's sole focus on imports from China is conceptually misplaced. As noted earlier, domestic firms and workers do not necessarily care about the exporter country. Trade exposure reflects all imports (and exports) in the industry and locale, not just imports from China. Using only the quantity of Chinese imports to measure trade exposure leaves open the possibility of omitted variable bias due to the correlation between imports from other countries and Chinese imports, and correlations between Chinese imports and unmeasured factors.

It is this fundamental, obvious, and uncontroversial conceptualization—that total imports in an industry and geography are what matters to domestic industries and workers, not Chinese imports alone—that motivated my re-analysis of ADH. ADH understand this issue but chose to focus on China because of the infeasibility of obtaining causal estimates of the effect of all imports in an industry on US employment and wages in that industry. ADH claim to have an empirical approach that can identify the effect of just Chinese imports on US manufacturing employment, wages, and social welfare payments. Unfortunately, their empirical approach fails to do so in the sense of estimating a ceteris paribus effect of Chinese imports.

I argue further that their approach fails to do so for the broader effect of Chinese imports and responses to growing Chinese imports from other countries (what I refer to as the equilibrium effect).

To understand why the ADH approach fails, a bit of review is in order. ADH used an instrumental variables approach to estimate the effect of Chinese imports in a commuting zone (CZ) on employment, wages, and welfare payments in the CZ. However, as just described, the appropriate model should include all imports and not just Chinese imports, or Chinese imports and non-Chinese imports. Using just Chinese imports raises the possibility that there is an omitted variable (e.g., other imports) that will produce biased estimates. To successfully get around this problem, it is necessary that the instrument used by ADH to isolate exogenous variation in Chinese imports is uncorrelated with imports from low-income countries, Mexico-CAFTA, or any other country and any unmeasured factors that affect employment. Imports from other countries belong in the model, either in addition to imports from China or as part of the sum of imports. I show that this requirement, which is referred to as the exclusion restriction, does not hold. The instrument that is used to predict Chinese imports is correlated with imports from several other countries. That means that the effect that ADH estimate is not the ceteris paribus effect of Chinese imports but instead, at best, the effect of Chinese imports and imports from other countries that respond to a change in Chinese imports, which I will refer to as the equilibrium effect of Chinese imports.

However, I provide further evidence that the results obtained by ADH are unlikely to even measure the equilibrium effect, and are instead likely to include effects of unmeasured factors besides omitted exports from other countries that make it impossible to interpret their estimates. In the end, ADH estimates provide little useful information about the effects of trade on US manufacturing, which is self-evident and known. Globalization (trade) has led to a decline in US manufacturing and the cause of the decline should be labelled as just that—globalization. Germany, Mexico, Canada, and all other international trading partners are as culpable for the decline in US manufacturing employment as is China.

Likely Mismeasurement of Chinese (and all) imports

Before getting to the problem of the exclusion restriction, there is, perhaps, an even more basic data measurement problem. Remarkably, ADH never actually measure imports from China or any other country in any industry and location. Instead, ADH construct Chinese (and other countries') imports by using the national share of employment in an industry in that location to allocate total Chinese

(and other countries') imports in an industry to that locale based on this share. For example, if Cleveland had 10% of all workers in the Fabricated Structural Metal Products industry, then ADH assigned 10% of Chinese imports in that industry to Cleveland. This approach assumes that all firms in an industry competed in a proportional (to employment) way with Chinese imports. But this is unlikely given the relatively low share of Chinese imports in many industries and heterogeneity of firms in an industry. Chinese imports were likely to be more important to some manufacturers and locales than others. Consider the Fabricated Structural Metal Products (SIC 344) industry. This industry has many small- to medium-sized firms as well as large international ones, and domestic firms are distributed widely throughout the USA. It is unlikely that each of these firms were equally affected by Chinese imports. Cannon Boiler Works in New Kensington, PA (Pittsburgh CZ) may have lost none of its customers to Chinese imports whereas a large, internationally owned firm like Babcock and Wilcox may have lost many of its customers. ADH ignores this possibility and assumes that all firms were equally affected conditional on their share of employment in that industry. No evidence was provided to support this assumption.

The unjustified, and unlikely to be true, assumption used by ADH to construct import exposure makes it highly likely that the primary measure of imports used in ADH is measured with substantial error, and that measurement error is unlikely to be random.[5] In fact, as my description of the Fabricated Structural Metal Products industry suggests, the measurement error in the construction of ADH's import measures is likely to be related to geography.

Given this hypothesis, I re-estimated the ADH regression models used to estimate the effect of Chinese imports on employment (the main result of the ADH article), first by dropping the geographic weight, and second by stratifying the sample by the size (population) of the geography (CZ) (using a weighted regression). Table 1 presents these estimates.

The first result in Table 1 that merits comment is that I was able to replicate the ADH results exactly.[6] The estimate (−0.60) indicates that the ADH measure of Chinese imports is negatively associated with manufacturing employment. Dropping the use of the geographic weight and estimating unweighted regressions, however, dramatically changes the estimate—not by 5% or 20%, but 50%. This is not a trivial change, and it changes the qualitative interpretation of the conclusions. At a minimum, it implies that the effect of Chinese imports on domestic manufacturing employment is half as large as the article states. The difference between weighted and unweighted regression estimates also indicates significant heterogeneity of ef-

[5] Several authors have used other measures of Chinese imports and obtained significantly different results than ADH (Jakubik and Stolzenburg 2021; Bloom et al. 2024).

[6] ADH deserve credit for making such an easy-to-use replication package available to researchers.

fects by geography. Yet there is no theory in ADH 2013 that would generate such a result. In contrast, my simple story about mismeasured Chinese imports explains the result quite well.

TABLE 1: Estimates of the Effects of Imports from China on Manufacturing Employment: Weighted and Unweighted Regression Estimates

	Full Sample		Small CZs	Large CZs
Weighted Regression	Yes Replication	No	Yes	Yes
(Δ imports from China to US)/ worker	−0.60 (0.10)	−0.30 (0.10)	−0.12 (0.06)	−0.69 (0.10)
Instrument	ADH China	ADH China	ADH China	ADH China
First-stage Coefficient	0.63 (0.09)	0.75 (0.18)	0.75 (0.28)	0.60 (0.09)
Notes: Data and analyses are based on the replication package provided by ADH. Estimates are the instrumental variables estimate of the effect of imports from China. Timepwt48 is the weight ADH used in their regressions. Small CZs are defined as Timepwt48<=0.0003. Large CZs are defined as Timepwt48>0.0003. Standard errors are in parentheses.				

Next, I stratified the sample by the magnitude of the geographic weight (i.e., population size of CZ) and re-estimated the model using a weighted regression, as in ADH.[7] Results from models using the stratified samples differ significantly. In areas with relatively small populations (45% of sample CZs), Chinese imports have a relatively small and statistically insignificant effect on manufacturing employment. In areas with larger populations, Chinese imports have a significant and negative effect on manufacturing employment. The difference between the two estimates is not 50%, but 575%! An almost 6X difference in magnitude is something to be concerned about in terms of the credibility of all the ADH results, which are based on an untested assumption related to the construction of the key variables of interest.

The heterogeneous effects by geography shown in Table 1 may reflect true heterogeneity, although why there would be such variation is not addressed by ADH. Such variation is, however, consistent with my hypothesis that the dependent variable is measured with significant error, that the error is non-random, and that it varies by geography. Overall, the extreme sensitivity of the estimates to weighting and stratifying the sample by the population of the CZ suggests that the ADH findings are not robust and likely biased and misleading. And given that the key right-hand side variable is estimated (stochastic), which was ignored by ADH, standard errors associated with the ADH estimates are also incorrect.

Again, it is worth noting that ADH never actually measure the imports of China or any other country in an industry and location. All imports and instru-

[7] Estimating unweighted regression within the stratified sample had little effect on estimates.

ments for those imports are constructed (estimated) using the geographic share of employment in industry as a weight ("share") with no assessment, even anecdotally, of the accuracy of the estimates. The results in Table 1 raise serious doubts about the validity of the key measures of imports, and also about the entire ADH analysis. Indeed, the criticisms of ADH 2013 by Goldsmith-Pinkham, Sorkin, and Swift (2020) and Borusyak, Hull, and Jaravel (2022) center around the use of the "share" that was used to construct imports and the instruments for Chinese and other countries' imports. These two papers raise questions as to whether the "share" is exogenous and whether that biases ADH's estimates. My argument that constructing imports in a geography based on these "shares" and the assumption that imports across areas are an identical function of these "shares" puts flesh on the statistical arguments offered by Goldsmith-Pinkham, Sorkin, and Swift (2020) and Borusyak, Hull, and Jaravel (2022). The shares would not be exogenous if, for example, imports from China and other countries are more competitive in some geographic areas than others, either because of the strategic motives of international exporting firms and their domestic competitors, or because of some type of "gravity" explanation, in which distance is an important determinant of product flows, which is common in the trade literature. It is obvious that the shipping distance from China to California is significantly less than the shipping distance from China to New York. Goldsmith-Pinkham, Sorkin, and Swift (2020) make a similar comment in passing:

> Indeed, it is hard to conceive of a model of an 'optimizing China' that would generate random patterns of exports across a wide swathe of the economy. (31)

The geographic heterogeneity of estimates of the effect of Chinese imports on employment in manufacturing, which is difficult to reconcile with a coherent conceptual model, is direct evidence consistent with the statistical arguments made by Goldsmith-Pinkham, Sorkin, and Swift (2020) and Borusyak, Hull, and Jaravel (2022) about the (unlikely) validity of the ADH approach.

Re-analysis Using a Correctly Specified Model

I next assess the validity of the ADH approach through the lens of the exclusion restriction of their instrumental variables approach. To remind the reader, ADH used an instrumental variables approach to estimate the effect of Chinese imports in a commuting zone (CZ) on employment, wages, and social welfare payments. I have already described how ADH constructed a CZ-specific measure of Chinese (other country) imports and the potential problems with that measure. ADH also used an instrument for Chinese imports in an industry using Chinese

imports in that industry to eight other countries: Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. ADH develop similar instruments for imports from low-income countries, Mexico-CAFTA (Central American countries), and what they refer to as all middle- and high-income countries.

It is important to note that all of the instruments used by ADH are constructed the same way. For Chinese imports, the instrument is based on Chinese imports to the eight aforementioned countries. For imports from Mexico-CAFTA countries, the instrument is also based on imports from these countries to the eight aforementioned countries. The validity of the Chinese instrument, as opposed to the validity of the other instruments, is made only by assertion. ADH argue that the Chinese instrument is valid because it reflects unique supply-side factors that made Chinese imports more competitive. While it is clear that China adopted an export strategy that included large subsidies and oppression of labor to achieve its goal, that does not exclude possible demand shifts and common productivity shocks that ADH argue characterize other instruments (Goldsmith-Pinkham, Sorkin, and Swift 2020).

To assess the importance of the potential misspecification of the model used by ADH, I estimated a series of regressions in which the dependent variable is employment in US manufacturing in an industry and CZ but with different country imports as the key variable of interest. The four countries, or country groups, are those used by ADH in Table 2 and Appendix Table 4: China; low-income countries; Mexico-CAFTA; and middle- and high-income countries. For each of these four countries or country-groups, I instrumented for that country's imports using the ADH instrument for Chinese imports.

These series of regressions go to the heart of the interpretation of ADH results. If the parameter of interest is the ceteris paribus effect of Chinese imports on US employment, then the instrument for Chinese imports needs to be uncorrelated with imports from all other countries and other unobserved confounders. If the parameter of interest is the equilibrium effect of Chinese imports, then the instrument for Chinese imports may be correlated with imports from other countries. In this interpretation, the ADH estimate is the effect of Chinese imports and the effect of changes in other imports due to a change in Chinese imports. However, using the Chinese import instrument for other country's imports also estimates the equilibrium effect of Chinese imports. It is the effect of a change in other country's imports and the effect of Chinese imports due to a change in Chinese imports. As the description sounds, the equilibrium effects are mechanically related to each other.

TABLE 2: Estimates of the Effects of Imports from China, Low Income Countries, Mexico-CAFTA and Other Countries on Manufacturing Employment

	China	Low-Income Countries		Mexico-CAFTA		Middle- and High-Income Countries	
	Appendix Table 4 Replication	Low-income Instrument	China Instrument	Appendix Table 4 Replication	China Instrument	Appendix Table 4 Replication	China Instrument
(Δ imports from Country to US)/ worker	−0.60 (0.10)	−3.82 (1.44)	−12.98 (5.41)	−1.87 (0.68)	−38.64 (166.13)	−0.04 (0.03)	0.75 (0.24)
Instrument	ADH Instrument For China	ADH Instrument for Low Income	ADH Instrument For China	ADH Instrument Mexico-CAFTA	ADH Instrument For China	ADH Instrument for All Other	ADH Instrument For China
First-stage							
China	0.63 (0.09)		0.03 (0.01)		0.01 (0.04)		−0.50 (0.18)
Low Inc		0.39 (0.13)					
Mex.-CAF				1.15 (0.27)			
Middle- and High-Income Countries						0.44 (0.06)	

Notes: Data and analyses are based on the replication package provided by ADH. Estimates are the instrumental variables estimate of the effect of imports from the country listed in the column on manufacturing employment. First-stage estimates are listed in the bottom half of the table, and the instrument is indicated in the row above the first-stage estimates. All regressions are weighted using Timepwt48. Standard errors are in parentheses.

Estimates from these regressions are presented in Table 2, and the first point to note is that I was able to replicate estimates reported by ADH (Appendix Table 4).[8] The replicated estimates are identified in the column headings. Second, the interesting estimates in Table 1 are those related to the First Stage and the instrument for Chinese imports. The instrument for Chinese imports is strongly related to Chinese imports (coefficient of 0.63), but it is also significantly correlated with imports from low-income countries (coefficient 0.03) and imports from middle- and high-income countries (coefficient of −0.50). These results imply but do not demonstrate that an increase in imports from China is associated with an increase in imports from low-income countries. Chinese imports do not apparently crowd out imports from low-income countries and instead crowd in imports from these countries. Why would this be the case if the instrument for Chinese imports is reflecting a unique, supply-side China shock? Chinese imports should crowd out other imports. One answer is that there are unmeasured factors correlated with both the instrument for Chinese imports and employment. Indeed, this is likely unless there is a plausible explanation for why a supply-side shock that advantages Chinese imports would lead to more imports from low-income countries. In con-

[8] ADH once again deserve credit for making such an easy-to-use replication package available to researchers.

trast, the results related to middle- and high-income countries suggest a crowding out of imports from middle- and high-income countries. While this seems plausible, it seems inconsistent with the results from low-income countries, and it cannot be differentiated from an omitted variable problem including imports from other countries outside of these four groups. Finally, the instrument for Chinese imports is not significantly related to imports from Mexico-CAFTA.

Considered together, these results are not easily reconciled. A unique, China supply-side shock should crowd out imports from other countries. That is the case only for middle- and high-income countries. Why not Mexico-CAFTA and low-income countries? Again, once it is demonstrated that the instrument for Chinese imports is correlated with other variables including, potentially, those not measured, any ad hoc story about crowding or crowding out imports from other countries is questionable. There are missing variables that are likely correlated with the instrument for Chinese imports that are part of the First Stage, reduced form, and IV estimates. But even coming up with an ad hoc story to explain these results is not obvious. As noted, the exogeneity of the instrument for Chinese imports is by assertion and supported by storytelling, not evidence. The evidence here suggests that the instrument does not reflect a unique, supply-side China shock that crowds out other imports. Therefore, the claim of exogeneity should be viewed as suspect, as should the results of ADH.

Now consider the IV estimates.[9] As described earlier, at best, the ADH IV estimate of the effect of Chinese imports on US employment is measuring the equilibrium effect—the effect of a change in Chinese imports and the change in other imports due to the change in Chinese imports. There also may be confounding from unmeasured factors that are reflected in the equilibrium effect. The IV estimate of the Chinese equilibrium effect is negative, suggesting that imports due to a change in Chinese imports (and the associated changes in imports from other countries and possible influence of unmeasured factors) crowd out domestic production and decrease employment in manufacturing. However, the IV estimates of the effect of imports from low-income countries, Mexico-CAFTA, and middle- and high-income countries all obtained using the instrument for Chinese imports also measure the same equilibrium effect, although scaled differently. For example, consider a simple model in which there are Chinese and non-Chinese imports. Assume that there is a supply-side China shock that causes Chinese imports to rise, and this causes a decrease in non-Chinese imports. The causal pathway is from the China shock to more Chinese imports to fewer non-Chinese imports to a de-

[9] Applying the suggested changes to the ADH (2013) approach outlined in Borusyak, Hull, and Jaravel (2022) has relatively little effect on point estimates, as they show in their Table 4 (Borusyak, Hull, and Jaravel 2022, 209). For example, IV estimates of the equilibrium effect of imports from middle- and high-income countries range between 0.38 and 0.75, which is similar to the variation of estimates shown in Table 4 of Borusyak, Hull, and Jaravel (2022).

crease in employment. This assumes some net growth in total imports because employment has decreased. In this case, the IV estimate of the equilibrium effect of Chinese imports would be equal to the IV estimate of the equilibrium effect of non-Chinese imports scaled by the effect of Chinese imports on non-Chinese imports. An important part of this conceptualization is that any correlation between Chinese imports and imports from other countries is caused by the supply-side China shock. If that is not the case, which would be so if there were unmeasured confounders, then scaling by the effect of Chinese imports on non-Chinese imports would not result in the same estimates. The corollary is that estimates that are not equal after scaling suggest confounding from omitted variables.

The IV estimate of the equilibrium effect of imports from low-income countries is negative, and very large (20X the effect of Chinese imports). A simple scaling story, as described above, would suggest a coefficient of around −20 (0.6 divided by 0.03) instead of −13, which is the observed IV estimate. But this simple story is not likely to be the case, because imports from other countries besides low-income countries were likely changing as were, perhaps, omitted influences. The upshot is that, again, once it has been demonstrated that the instrument for Chinese imports is correlated with other variables including, potentially, those not measured, any ad hoc story is questionable. The IV estimate is not really interpretable in these cases.

The IV estimate of the equilibrium effect of middle- and high-income countries is positive, about the same magnitude of the effect of Chinese imports, and significant. In a simple case with just Chinese imports and imports from middle- and high-income countries, the IV estimates would be mechanically related. In this case, one would scale the IV estimate of the equilibrium effect of imports from middle- and high-income countries by −0.5, yielding an estimate of 1.2 instead of the observed 0.75. The absence of a mechanical relationship reflects the fact that lots of other variables besides imports from middle- and high-income countries may be correlated with the instrument for Chinese imports and reflected in the IV estimate. Any claim to a specific interpretation is unjustified.

TABLE 3: Estimates of the Effects of Total Imports (China, Low Income Countries, Mexico-CAFTA and Other Countries) on Manufacturing Employment

	All Country Instruments	China Instrument
(Δ imports from Country to US)/ worker	−0.01 (0.02)	−2.24 (3.31)
First-stage		
China	−0.07 (0.16)	0.17 (0.25)
Low Inc	−1.67 (0.83)	
Mex.-CAF	−6.04 (1.98)	
Middle- and High-Income	0.56 (0.06)	
Notes: See notes to Table 2.		

An alternative model to those used by ADH and those shown in Table 2 is to include imports from all the countries in the same model. If the ADH (2013) claim is correct and the instrument for Chinese imports is exogenous, then that instrument could be used to isolate the effect of a change in total imports on US manufacturing that is due to China. This estimate has the interpretation of a local average treatment effect (LATE). It measures the effect of increased imports that is due to the purported Chinese "shock". Table 3 presents these results. In the first column, I present estimates of the effect of total imports on US manufacturing employment using instruments developed by ADH (2013) for each country, or group of countries. The IV estimate is small and not significant. In the next column, I show results using just the supposedly exogenous Chinese instrument. The Chinese instrument has no significant or meaningful correlation with total imports (first stage) and the IV estimate is imprecisely estimated, as expected, when there is no first stage. It is difficult to go from this result to the claim by ADH that an increase in Chinese imports decreased employment. As shown in Table 3, the instrument for Chinese imports is not correlated with the aggregated measure of imports. There is no net increase in this aggregated measure of imports. This result is important to understand because it negates any claim by ADH (2013) that their estimates pertain to trade exposure more broadly and not just China. While it is clear by their statements that ADH (2013) want to place some special importance on China trade, even if they were more circumspect and wanted to appeal to a broader, LATE interpretation of their findings, it would not be valid to do so.

I also conducted a similar reanalysis for (log) weekly wages, which ADH highlights as an important outcome affected by Chinese imports. Table 4 presents the results and has a similar presentation format as Table 2. While not shown, the

First Stage estimates and the correlations between the instrument for Chinese imports and imports from other countries are the same as described earlier. For this outcome, I was also able to replicate the estimates in ADH, and they are shown in Table 4 in the column labelled as "ADH Replication." These (replication) estimates indicate that imports from China are associated with significant declines in wages in non-manufacturing industries but not in manufacturing industries. ADH argued that this combination of results is possible (not demonstrated) because the supply of workers in non-manufacturing increased, lowering wages in non-manufacturing; the remaining workers in manufacturing were of higher, unobserved skill with higher wages negating the expected decrease in wages from a decrease in demand. It's a story, but whether or not it's correct is something to be determined, not accepted.

Turning to the other IV estimates of the equilibrium effect, my reading of the results is that they do not adhere to a simple story where IV estimates from Chinese imports are rescaled by the association between the instrument for Chinese imports and imports from the given country. This is just another way to say that the IV estimates reflect an unknown set of influences including measured effects of the Chinese instrument on imports from other countries and likely unmeasured effects. Interpretation of such estimates is uncertain. For example, the IV estimate of the equilibrium effect of imports from middle- and high-income countries was associated with an increase in employment, but the equilibrium effect of imports from middle- and high-income countries is not associated with a change in wages in either manufacturing or non-manufacturing.

TABLE 4: Estimates of the Effects of Imports from China, Low Income Countries, Mexico-CAFTA and Other Countries on Log Weekly Wages

	China	Low-Income Countries		Mexico-CAFTA		Middle- and High-Income Countries	
All Industries	ADH Replication	Low-income Instrument	China Instrument	Mexico-CAFTA Instrument	China Instrument	Middle- and High-Income Instruments	China Instrument
(Δ imports from Country to US)/ worker	−0.76 (0.25)	−6.00 (3.35)	−16.52 (8.23)	−5.68 (2.24)	−49.19 (209.38)	−0.17 (0.13)	0.95 (0.51)
Manufacturing							
(Δ imports from Country to US)/ worker	0.15 (0.48)	−0.56 (4.03)	3.28 (10.64)	−6.09 (2.09)	9.76 (54.00)	−0.14 (0.14)	−0.19 (0.59)
Non-Manufacturing							
(Δ imports from Country to US)/ worker	−0.76 (0.26)	−6.73 (3.79)	−16.56 (7.70)	−4.30 (2.11)	−49.32 (207.65)	−0.15 (0.14)	−0.15 (0.14)

Notes: Data and analyses are based on the replication package provided by ADH. Estimates are the instrumental variables estimate of the effect of imports from the country listed in the column on manufacturing employment. First-stage estimates are listed in the bottom half of the table, and the instrument is indicated in the row above the first-stage estimates. All regressions are weighted using Timepwt48. Standard errors are in parentheses.

Conclusion

As others have noted, ADH's use of Chinese imports as a measure of trade exposure is incomplete (Feenstra, Ma, and Xu 2019; Winship 2025). However, it is not only because exports matter, as noted in these articles, but also, and more importantly, because domestic firms and workers care only about import competition in general, and not the specific country that produces the import. A correctly specified model examining the effects of trade exposure would include all (net) imports, or (net) imports from each country. ADH did not conduct such an analysis. Instead, they argued that China's entry into the World Trade Organization (WTO) in 2001 and its purposeful, government-led plan to promote exports represent an exogenous source of variation in imports and that the effect of Chinese imports can be isolated from broader trends in trade. This is speculation by ADH that is not particularly supported in Figures 1 and 2, and, more importantly, ADH never actually incorporated this argument into their empirical analysis. Instead, ADH created an instrument for Chinese imports into the US using Chinese imports to eight other developed nations. Note that this approach does not exploit entry into WTO, which ADH imply is an exogenous source of variation in Chinese imports. That argument was simply misdirection. While it is likely that the expansion of Chinese imports reflected significant supply-side influences, it does not mean that changes in Chinese imports were exclusively a result of these forces. This raises the question of whether the instrument for Chinese imports based on Chinese ex-

ports to eight countries reflects more than just supply-side forces. If so, ADH IV estimates are not interpretable as the effect of a China shock.

In this article, I have shown that there is substantial heterogeneity of estimates by geography. In smaller CZs (bottom half of distribution), an increase in Chinese imports has no effect on employment in manufacturing. In larger CZs, the effect is highly significant and six times larger. ADH (2023) did not explore this heterogeneity, and there does not seem to be an easy explanation. I provide one possibility that the key independent variable, Chinese imports, is likely to be measured with significant error, and this measurement error is likely to differ by geography and is a possible explanation of the heterogeneous findings. In fact, I find it remarkable that imports from China in an industry in a locale, which is the key measure of the analysis, are not actually measured, but instead estimated under the assumption that Chinese imports in an industry and locale are proportional to that locale's share of national employment in that industry. This approach ignores any strategic action by Chinese exporters and domestic producers. I view that evidence alone as sufficient to question the ADH results and their conclusions.

In addition to that criticism, I showed that the ADH instrument for Chinese imports is correlated with imports into the US from low-income countries and middle- and high-income countries and uncorrelated with imports from Mexico-CAFTA. At a minimum, this changes the interpretation of the ADH IV estimates to that which I referred to as the equilibrium effect—the effect of Chinese imports and the changes in other imports caused by a change in Chinese imports. However, once I demonstrated that the instrument for Chinese imports is correlated with imports from other countries, it suggested that the instrument is not strictly valid. Moreover, if the cause of these changes in imports was from a supply-side China shock, then we would expect Chinese imports to crowd out imports from other countries. This is not the case in general, and is only apparent for imports from middle- and high-income countries. Overall, the lack of a clear, entirely exogenous change to Chinese imports, combined with the absence of consistent evidence that other imports were crowded out, along with empirical estimates that do not align in a coherent way to ad hoc stories about import competition, suggest that the ADH IV estimates are uninformative and should be interpreted as such. Claims otherwise, for example, by Autor and Hanson (2025) in a recent New York Times article about a second China shock, are unjustified and misleading.

Data and Code

Data and code used in this research are available here

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