Trade Shocks: The Next Generation*

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Abstract

This paper explores the intergenerational effects of childhood import shocks using linked full-count Census data between 1910 and 1940 and a novel identification strategy that isolates quasi-random import growth. We show that individuals exposed to import growth in their first 10 years of life report lower incomes and reduced upward mobility 30 years later, with effects that fall most heavily on the left tail of the income distribution. More exposed individuals also experience an increased probability of moving between states and a reduced probability of living in an urban location in adulthood. Intergenerational structural change plays a critical role in our results, with import competition reducing the probability that sons work in high-earning occupations regardless of their father's income level.

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1 Introduction

A fundamental issue in the study of international trade is the extent to which its effects persist; the welfare consequences of trade depend to great extent on how individuals adjust to it, and how quickly. Indeed, an enormous body of work has attempted to trace the effects of import competition on individual and local outcomes over time. The life of an economy, however, is measured in decades, not years, and the effects of trade shocks are felt not only by those exposed to them directly but those in the generations that follow. Put another way, it is not only the response of the contemporaneous generation, but also that of the generation that follows, that matters for the long-run consequences of trade. This response, however, has received extremely little attention from scholars.

In this paper, we explore the intergenerational effects of trade in the United States between 1910 and 1940, taking advantage of linked full-count Census data and a novel identification strategy to examine the effects of childhood import exposure on individuals' economic outcomes three decades into the future. Exploiting quasi-random variation in local import growth, we show that the effects of childhood trade shocks are still felt 30 years later. In particular, sons who are exposed to import competition in their youth report lower incomes and reduced upward mobility relative to their fathers. Such individuals are more likely to move between states but less likely to live in the South or in urban settings in adulthood. They are also more likely to work in low-wage occupations such as general or farm labor, suggesting a role for the intergenerational transmission of trade shocks in the structural adjustment of the American economy in the early twentieth century.

To identify childhood exposure to trade, we employ the methodology of Greenland and Lopresti (2024), which leverages changes in aggregate price levels and pre-existing industry variation in reliance on per-unit, or specific tariffs to identify changes in ad valorem equivalent (AVE) tariff levels. The intuition is straightforward: when tariffs are nominally defined, increasing price levels reduce the AVE level of protection, while declining price levels increase it. Given the fact that approximately two-thirds of US tariffs were specified in per-unit terms as of 1900, and that price levels more than doubled between 1900 and 1920, there is considerable scope for such variation in this period.

Using digitized tariff schedules covering nearly 3000 tariff lines annually for the years 1900, 1905, and 1910, we calculate the share of tariff revenue generated by specific tariffs for each of 66 tradable Census industries. The variation across industries in reliance on specific tariffs is considerable, with a "specific tariff share" of tariff revenue at the 25^{th} percentile of

the distribution equal to 25%, approximately one-third of the specific tariff share at the 75^{th} percentile. We combine these data with county-level employment to construct a local measure of exposure to trade liberalization among sons born between 1900 and 1910. Specifically, we interact county-level specific tariff shares in the year that an individual is born with aggregate price changes over the ensuing 10 years to create a measure of childhood exposure to import growth that varies both across counties and over time. We use this measure as an instrument for local childhood import growth, measured as the employment-weighted average of log industry import growth in an individual's first 10 years of life.

The instrument is strongly predictive of local import growth, with an interquartile reduction in our county-level measure of "realized protection" corresponding to an 84% decadal increase in county-level average imports in our baseline specification. Import growth, in turn, predicts long run income outcomes; an individual born into a county at the 75^{th} percentile of childhood import growth reports an income 30 years later approximately 8% lower than an observationally equivalent individual at the 25^{th} percentile. These results are robust to a host of individual and county-level controls and alternative samples.

This baseline result is driven in large part by trade's effect on occupational choice. Individuals exposed to import competition in their youth are more likely to work as farm or general laborers, and less likely to work in higher-paying sales, clerical, managerial, or other professional occupations. We also find evidence of mechanisms beyond occupational choice in driving our results: more exposed individuals are, on average, less likely to complete 8^{th} grade or live in urban locations, but are more likely to move between states.

To more directly assess the intergenerational component of the effects of childhood trade shocks, we explore the relationship between sons' outcomes and the economic characteristics of fathers. First, we explore the effect of import growth on the probability of upward mobility – that a son will move to a higher income rank than his father – and again find a result in the negative. Specifically, an interquartile increase in import growth reduces the probability of upward mobility across generations by approximately 5 percentage points. To explore this further, we allow for the effect of trade exposure on 1940 log income to vary across individuals as a function of fathers' incomes. We show that the long-run consequences of childhood import growth are primarily driven by those born into lower-income households. Indeed, the estimated effect of trade on 1940 income is nearly twice as large among the bottom quintile of the father's income distribution as it is in the top. While trade exposure reduces long-run income growth and upward mobility across all income groups, its effects

are most concentrated among the most economically disadvantaged.

Our paper contributes to a large literature analyzing the effects of local import competition on labor market outcomes.[1] While the majority of this literature considers short- or medium-run effects, a relatively small number of studies have emphasized that the effects of exposure may be persistent (Autor et al., 2021; Kovak and Morrow, 2024; Autor et al., 2025) and may indeed grow over time (Dix-Carneiro and Kovak, 2017). Likewise, a small minority of work has assessed the effects of trade shocks on linked individual-level outcomes, rather than on local aggregates (Autor et al., 2014; Hummels et al., 2014; Greenland and Lopresti, 2024). In this paper we are able to combine both elements, studying the effects of trade shocks on long-run outcomes at the individual level.

We also contribute to the literature focused on understanding the drivers of intergenerational mobility more generally. Some of this work emphasizes spatial determinants in childhood (Chetty and Hendren, 2018; Rothstein, 2019) or at the time of immigration Abramitzky et al. (2021) in determining intergenerational mobility. Others exploit large and permanent shocks such as the abolition of slavery (Ager et al., 2021), the Great Migration (Derenoncourt, 2022), WWII plant openings (Garin and Rothbaum, 2024). While we find an important role for local shocks in explaining intergenerational mobility, we also find subsequent labor market opportunities through human capital adjustments, migration and occupational choice to be important determinants.

Three studies stand out as closest to ours in their explicit emphasis on the intergenerational effects of trade. First, Ahsan and Chatterjee (2017) explore the effects of the 1991 trade liberalization in India on intergenerational occupational mobility. Among a sample of approximately 8000 individuals, they find that, by increasing the employment share among higher skill occupations, liberalization promoted upward mobility. Second, Colantone et al. (2024) employ linked income data from Chetty and Hendren (2018), averaged at the local level, to explore the effects of import competition from China on intergenerational mobility among a cohort of US individuals born between 1980 and 1982. Similar to our results, they find reduced upward mobility in response to import shocks and argue that this is driven by lower wage growth combined with increased persistence in occupational choice in the face of trade exposure. Finally, Mitra et al. (2024) examine the effects of the US-Vietnam

[1] Among many others, see Autor et al. (2013), Hakobyan and McLaren (2016), Feler and Senses (2017), Pierce and Schott (2020), Batistich and Bond (2023), and Choi et al. (2024) for studies on the US, Kovak (2013) and Dix-Carneiro and Kovak (2017) Brazil, Bombardini and Li (2020) and Erten and Leight (2021) on China, Dauth et al. (2014) and Dauth et al. (2017) on Germany, Topalova (2007) and Edmonds et al. (2010) on India, Erten et al. (2019) on South Africa, and Colantone and Stanig (2018) on the UK.

Bilateral Trade Agreement on occupational mobility between Vietnamese generations. They find that the agreement increased absolute mobility, but to a greater extent for children of higher-income parents, such that relative mobility declined. They find that these results are driven at least in part by higher levels of human capital accumulation.

Our work complements these studies in its use of the full-count Census data, which allows us to control for a host of individual characteristics for millions of sons, rather than focusing on labor market-level aggregates or a relatively small number of individuals. The rich nature of the Census data allows us to directly explore numerous mechanisms of adjustment including human capital, migration, and occupational choice.

The rest of the paper proceeds as follows. Section 2 discusses our data and empirical methodology. Section 3 presents our primary results. Section 5 concludes.

2 Data and Methodology

We make use of data from multiple sources. We begin with a sample of boys 10-years old and younger who live in households with a father present in the 1910 Census, using data from IPUMS (Ruggles et al., 2024). Fathers are defined as men between the ages of 17 and 70 who have fewer than 10 sons. We match the sons observed in 1910 Census to the 1940 Census using the linking algorithm from Price et al. (2021) as described in the "Census Tree" data developed in Buckles et al. (2024).[2] From the Census we take data on personal characteristics of the father, including race, age, and nativity, as well as occupation, industry, and county of residence. Since individual earnings data are not collected until the 1940 Census, we follow Abramitzky et al. (2021) to impute earnings for fathers in 1900 and 1910 and sons in 1940, based on a specification in which we regress log income in 1940 on occupation, race, state of residence, a quadratic in age and a full set of interactions of aggregate occupation category and region fixed effects. We adjust this measure of imputed income to include self-employed farmers following Collins and Wanamaker (2022). Specifically, we use the 1960 Census to calculate the ratio of average earnings for farmers to those of farm laborers by region and race, and use this ratio to scale wage earnings of farm laborers in 1940 to calculate income for farmers.[3] We then calculate the income rank of each son relative to all

[2]In robustness exercises we also adopt the linking algorithm of Abramitzky et al. (2020) in which we employ the "ABE NYSIIS standard" linking which employs the Abramitzky, Boustan, and Eriksson linking algorithm, with NYSIIS standardized names.

[3]We repeat all our analysis using reported 1940 earnings for sons and show all our estimates to be robust.

sons in a given birth cohort, and of each father relative to all fathers with sons in the same cohort. When merged to our primary measures of trade exposure, this yields an estimating sample of 3.8 million individuals present in 1910 and 1940 and linked to their fathers in 1900 and 1910. We report summary statistics on the variables of interest in Table 1. The average income increases slightly between fathers and sons throughout our sample. The linked sample consists primarily of white sons, born to fathers, approximately 20% of whom are foreign-born.

Table 1: Summary Statistics

Variable	Mean	SD	IQR	N
$\ln(\widehat{Income}_{j,1940})$	6.88	0.49	0.65	3,868,666
$\ln(\widehat{Income}^{Father}_{j,1900})$	6.54	0.50	0.78	3,868,666
$\Delta \ln(Imports_{ct})$	1.46	0.98	1.52	3,868,666
$\ln(1 + AVE_{ct})$	0.19	0.05	0.04	3,868,666
ΔRP_{ct}	-0.23	0.25	0.36	3,868,666
$\Delta \ln(Imports_{it})$	0.54	0.29	0.22	2,429,181
$White_j$	0.94	0.23	0.00	3,868,666
$Black_j$	0.05	0.23	0.00	3,868,666
$Literate^{Father}_j$	0.92	0.28	0.00	3,868,666
$Immigrant^{Father}_j$	0.18	0.39	0.00	3,868,666

Notes: Table provides summary statistics for individuals born between 1900-1910 later observed in 1940 who could be matched to their father. All observations are male. Data taken from Ruggles et al. (2024) and linked with the algorithm outlined in Price et al. (2021). Measures of import growth realized protection and tariff levels taken from data constructed in Greenland and Lopresti (2024).

We combine these data with data on US imports and tariff rates from the *Foreign Commerce and Navigation of the United States*. In particular, we rely on legislated tariffs from the 1900, 1905, and 1910 tariff schedules, as well as annual import data between 1900 and 1920.[4] These are digitized and concorded to Census industries. To isolate quasi-random variation in exposure to import competition, we follow the approach of Greenland and Lopresti

[4]The 1900 and 1905 tariff schedules correspond to the Dingley Tariff of 1897, while 1910 schedule reflects the changes made by the Payne-Aldrich Tariff of 1909.

(2024), who combine aggregate price movements with pre-existing variation in the prevalence of per-unit, or specific, tariffs to generate variation in ad valorem equivalent (AVE) tariff levels. Specifically, they show that changes between periods t and $t+1$ in the AVE tariff of imported product i can be written as a function of the product's initial AVE level, price growth, and the share of tariff revenue generated by specific tariffs, or the "specific tariff share" of the product:

$$\Delta \ln(1 + AVE_{it}) = \ln\left(1 - \frac{\Delta p_{it}}{p_{it+1}} STS_{it} \frac{AVE_{it}}{1 + AVE_{it}}\right). \tag{1}$$

STS_{it} is defined as the ratio of revenue generated by specific tariffs to total import duties on product i at time t. Intuitively, the protection afforded by nominally defined tariffs falls as prices increase and increases as prices fall. As a result, once policymaker have chosen AVE_{it} and STS_{it}, AVE levels will vary inversely with price levels – even in the absence of changes to tariff policy. To address the concern that initial tariff levels are themselves endogenous, we follow Greenland and Lopresti (2024) and exploit only cross-product variation in specific tariff shares and aggregate price movements. Our primary measure, changes in "realized protection", is thus defined for product i as

$$\Delta RP_{it} = \ln\left(1 - \frac{\Delta p_t}{p_{t+1}} STS_{it}\right). \tag{2}$$

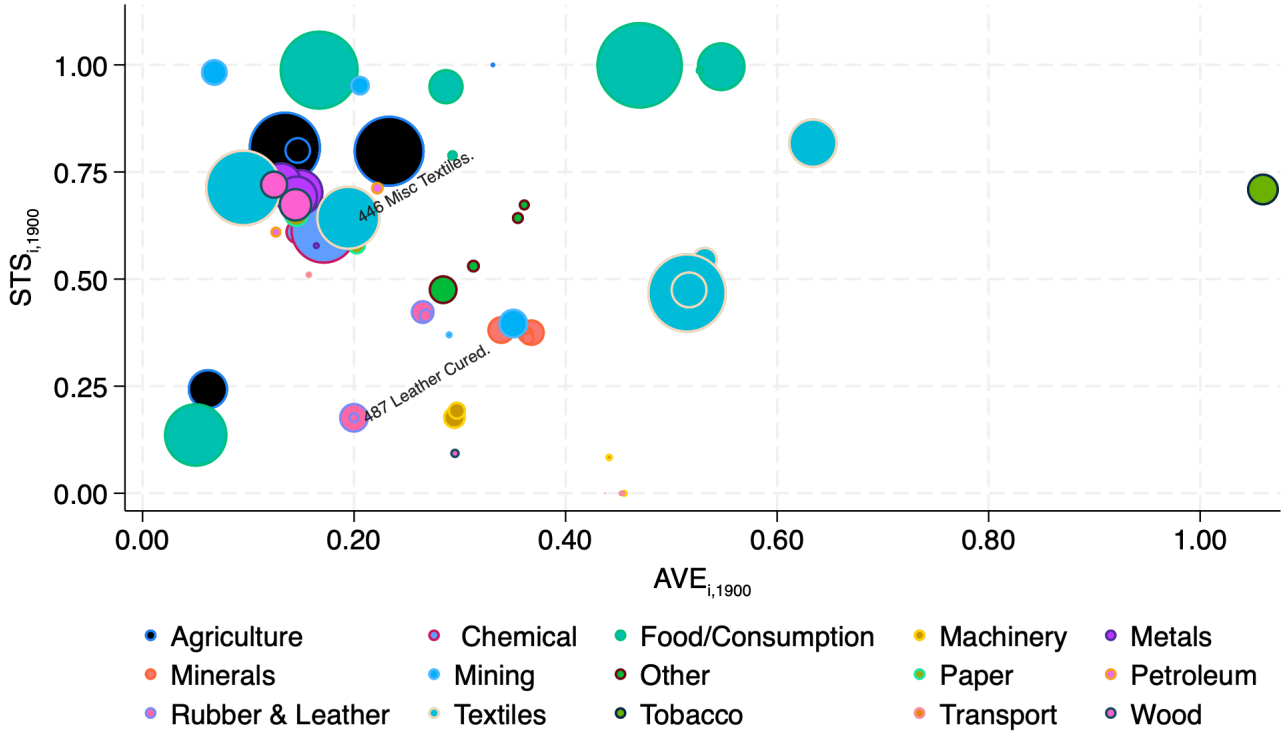
We control separately for AVE levels, such that we are exploiting variation over time in exposure to trade conditional on an industry's initial level of protection.

This identification strategy requires variation across products in the use of specific tariffs as well as variation over time in price levels. The period we study is appealing on both counts. Specific tariffs are pervasive during this era of US trade policy. As of 1900, under the Dingley Tariff, approximately 67% of all tariff revenue is generated by specific tariffs.[5] Further, there is considerable variation in specific tariff shares even among goods with similar AVE levels. To see this, consider Figure 1, in which we report specific tariff shares and AVE levels at the 1950 Census industry level. Consider industry 445 – "Miscellaneous textile mill products" – and industry 487 – "Leather: tanned, cured, and finished" – each of which has a 1900 AVE of approximately 20%. Despite facing nearly identical levels of protection, industry 487 has a specific tariff share of less than 20%, while industry 446 draws more than 65% of its tariff revenue from specific tariffs. As a result, in the face of price movements,

[5]This falls to 57% in the 1910 data, under the Payne-Aldrich Tariff of 1909.

industry 446 will face substantial changes in AVE tariff levels relative to industry 487.

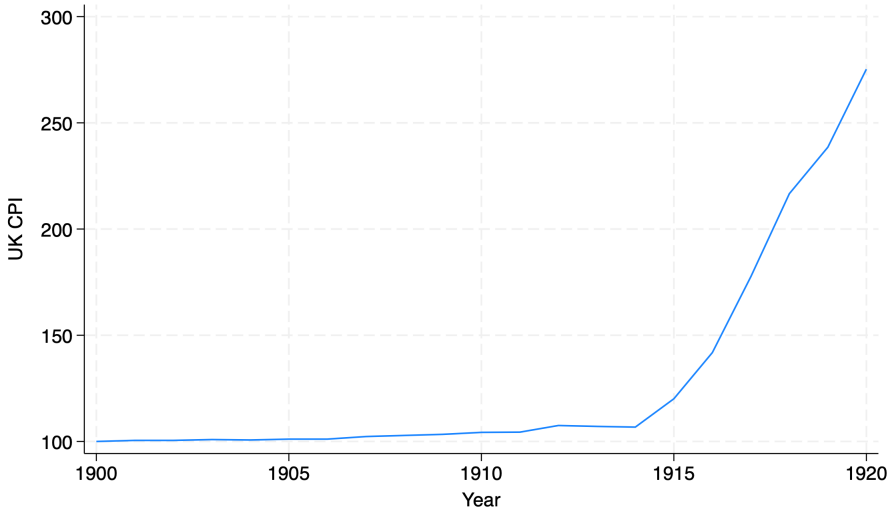
Figure 1: Tariff Policy, 1900



Notes: Figure displays 1900 policy variables, STS, and AVE. STS indicates proportion of AVE tariff level used that is subject to inflationary/deflationary exposure. AVE is the ad valorem equivalent level of tariff exposure under 1900 prices. Data taken from Greenland and Lopresti (2024) and mapped to 1950 census industries. Colors indicate 1950 industry groups and marker size is proportional to share of 1900 US imports.

Price changes are considerable in this period as well. In our primary measure of realized protection we exploit variation in UK price levels in order to mitigate concerns that US imports and prices both reflect, e.g., domestic demand shocks. As is shown in Figure 2, price levels are relatively stable between 1900 and 1914, rising by a modest 7%. During the WWI years that follow, on the other hand, prices rise rapidly, ultimately ending 275% higher than their 1910 level. This price growth, combined with industry variation in the importance of specific tariffs, underpins our estimation, as children born in 1905 and later will potentially face substantial inflation-driven changes in realized AVE levels. The difference in inflation levels before and during WWI also suggests that it will be important to account for differences between birth cohorts. We return to this in detail below.

Figure 2: UK CPI



Notes: Figure displays UK CPI price growth from 1900-1920. This is the price variation used to construct our IV changes in realized protection describe in Equation 2.

To construct measures of local exposure to trade, we first aggregate tariff-level ad valorem equivalent rates and specific tariff shares – for approximately 2750 tariff lines annually – to 66 Census industries. We similarly aggregate imports, taken from annual editions of the *Statistical Abstract of the United States*, to the Census industry level. We calculate both AVE_{it}, defined as duties collected relative to the value of imports, and ΔRP_{it}, as defined in equation 2 above. For ΔRP_{it}, we calculate 10-year changes for each birth-year cohort between 1900 and 1910.[6] Sons born in 1900 will thus face a "childhood shock" based on tariff levels as of 1900 and price changes between 1900 and 1910, while the shock for those born in 1901 will be a function of birth-year tariffs and price growth between 1901 and 1911, and so on.

We aggregate these industry-level measures to the county level, weighting by each indus-

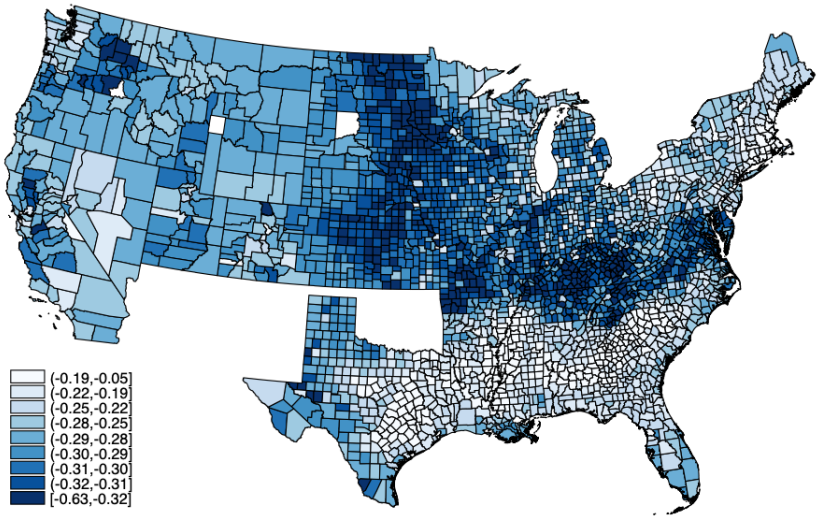
[6] As we have tariff data for 1900, 1905, and 1910, "start of period" tariffs will correspond to 1900 tariffs for the years 1900-1904, 1905 tariffs for 1905-1909, and 1910 tariffs for 1910.

try's 1900 labor share of tradable employment within the county.

$$
\begin{aligned}
AVE_{ct} &= \sum_i \frac{L_{ict}}{L_{ct}} AVE_{it} \\
\Delta RP_{ct} &= \sum_i \frac{L_{ict}}{L_{ct}} \Delta RP_{it} \\
\Delta \ln(Imports_{ct}) &= \sum_i \frac{L_{ict}}{L_{ct}} \Delta \ln(Imports_{it}) \qquad (3)
\end{aligned}
$$

We display the average 10-year change in realized protection in our sample in Figure 3. While specific tariffs are ubiquitous in our data, they are used most frequently in agriculture – with the notable exception of cotton, which is duty free in this period. As a result, as prices rise between 1900 and 1920, the agricultural Midwest and Plains regions face the steepest decline in realized protection. The deep South, heavily reliant on cotton, experiences much more modest changes in AVE levels, while the Northeast, West, and Industrial Midwest exhibit more mixed exposure patterns. We discuss the implications of this regional clustering for our estimation in detail below.

Figure 3: County Changes in Realized Protection



Notes: This map displays the average county-level changes in realized protection across all cohorts in our sample–individuals born 1900-1910 who could be linked to themselves in 1940 and to their father. Changes in industry-level realized protection are defined in equations 2 and mapped spatially in equation 3. The darkest blue bin indicates that 32%-63% of tariff protection was eroded by inflation

Given these data, our primary specification takes the following form:

$$X_{j,1940} = \beta_0 + \beta_1 \Delta \ln(\widehat{Imports}_{ct}) + \beta_2 X_{j,1900} + \beta_3 X_{c,1900} + \gamma_t + \epsilon_{jt} \tag{4}$$

Here, $X_{j,1940}$ is a 1940 outcome of interest – most frequently log income – for sons linked between 1910 and 1940, and $\Delta \ln(\widehat{Imports}_{ct})$ is childhood county-level import growth, which we instrument for using county-level changes in realized protection. We control for a host of individual- and county-specific controls, $X_{j,1900}$ and $X_{c,1900}$, respectively. We describe these in detail below. Additionally, we include fixed effects for the son's birth year, γ_t, in all specifications.

3 Results

We begin by exploring the effect of childhood trade shocks on adult income. In Table 2, we regress log predicted income in 1940 against county-level childhood import growth, instrumenting with realized protection as described in equation 4. In column 1, we include only birth-year fixed effects to account for time-varying macroeconomic conditions. The point estimate suggests that a doubling of import growth during childhood reduces predicted log earnings by nearly 8%. This implies that a one standard deviation increase in import growth in childhood lowers log earnings in adulthood by 0.15 standard deviations.[7] In column 2, we additionally control for childhood county-level ad valorem equivalent tariff levels to account for the possibility that initial tariff levels may be jointly correlated with subsequent import growth and long-run economic outcomes. The coefficient on $\ln(1 + AVE_{jct_0})$ reveals that children born in locations with higher initial levels of protection have higher wages in adulthood. While the inclusion of this covariate increases the precision of our main estimates, it leaves the interpretation unchanged – as tariff protection erodes, childhood exposure to import competition rises, yielding lower wages in adulthood.

To address the possibility that exposure to import competition may be correlated with individual demographic characteristics, we include controls for race of the son, and indicator variables for the father's literacy and immigrant status in column 3. In column 4 we introduce state-by-urban fixed effects to account for differences in long-run outcomes driven by location-specific characteristics. Urban status is potentially correlated with long-run eco-

[7] The interquartile range of import exposure in the estimation sample is 1.46, implying an interquartile increase in import growth reduces log income by 0.235, relative to an interquartile range of 1.06.

Table 2: The Effect of Childhood Import Exposure on Adult Income

	$\ln(Predicted\ Income_{j,1940})$					
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta\ln(\widehat{Imports}_{jct_{(0,10)}})$	-0.078*	-0.080***	-0.107***	-0.031***	-0.063***	-0.055***
	(0.042)	(0.026)	(0.021)	(0.007)	(0.012)	(0.010)
$\ln(1 + AVE_{jct_0})$		3.038***	2.026***	0.446***	0.310***	0.280***
		(0.482)	(0.317)	(0.090)	(0.110)	(0.095)
White_j			0.534***	0.560***	0.563***	0.543***
			(0.034)	(0.023)	(0.022)	(0.021)
Black_j			0.064*	0.139***	0.140***	0.142***
			(0.033)	(0.023)	(0.022)	(0.021)
$\text{Father Literacy}_{jt_0}$			0.176***	0.118***	0.117***	0.107***
			(0.013)	(0.005)	(0.005)	(0.005)
Father is an immigrant			0.027	-0.015***	-0.020***	-0.022***
			(0.017)	(0.005)	(0.005)	(0.005)
$\ln(Father\ Income_{jct_0})$						0.108***
						(0.007)
1^{st} Stage Coeff.	-4.89	-4.92	-4.86	-3.62	-2.35	-2.35
1^{st} Stage F	48.28	158.77	161.62	79.84	53.62	53.65
1σ	-.15	-.16	-.21	-.06	-.13	-.11
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Urban-State FE	No	No	No	Yes	Yes	Yes
County Controls	No	No	No	No	Yes	Yes
Obs.	3,868,666	3,868,666	3,868,666	3,868,666	3,868,666	3,868,666

Notes: Table presents regression of childhood county-level import exposure shock on log predicted earningsCollins and Wanamaker (2022) in 1940. Sample is comprised of all male individuals born between 1900 and 1910 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

nomic outcomes in a way that varies across states; long-run trends for individuals born in Atlanta, for instance, might differ from those in Chicago. Inclusion of these fixed effects reduces the point estimate of interest, but again leaves it statistically and economically significant. In column 5 we introduce 1900 county-level controls for the share of employment accounted for by agriculture and by manufacturing as well as the share of the population living in an urban location, each interacted with a birth-year fixed effect. This allows for cohort-specific variation in long-run outcomes as a function of county characteristics that may be correlated with import growth. Finally, in column 6, to account for the possibility that import exposure in childhood may be concentrated among fathers of particular eco-

nomic status, we directly control for the father's pre-period predicted income as of 1900. While the magnitude is reduced by a third compared to our specification in column 1, the estimated effect is still economically and statistically significant. A one standard deviation increase in childhood import exposure reduces log earnings in adulthood by 0.11 standard deviations. Moving forward, we take the specification reported in column 6 as our benchmark specification.

The results above speak to the persistent effects of childhood trade shocks, but do not directly address any intergenerational aspect. To what extent do the estimated effects operate through a child's father specifically? We address this question in several ways. First, we turn to a direct measure of intergenerational mobility: whether sons achieve a higher rank in the income distribution than their fathers. In Table 3, we replace the dependent variable with an indicator variable equal to 1 if a son's rank in the income distribution in 1940 exceeds that of his father in 1900 and sequentially introduce controls as before.

The table reveals a clear pattern: import exposure in childhood dramatically reduces the probability of upward mobility. Our most parsimonious specification – controlling only for cohort fixed effects and father's initial position in the income distribution shows that a one standard deviation increase in childhood import exposure reduces the probability of surpassing one's father in the income distribution by approximately 5.3%. This result is robust across all specifications. Our most restrictive specification, found in column 6, shows a still substantial but slightly smaller 3.2% effect.

The message from the above tables is clear: The effects of local childhood trade shocks persist well into adulthood. In Table 4 we explore the robustness of this result to a host of alternative specifications. We focus on income in adulthood as the outcome of interest, and report analogous results for upward mobility in the appendix. In column 1 we use occupational income scores as our measure of income in 1940 in the benchmark specification from Table 2. This is an alternative measure of wages that employs no spatial variation or adjustments for self-employed individuals (which primarily impacts farmers in our sample). While the point estimate of interest is smaller, the distribution of predicted income is tighter than that of observed income, which implies a similar share of the interquartile range.[8] In column 2 we include 1910 county fixed effects, exploiting only within-county variation in exposure to import growth across cohorts. As expected, the estimated impact decreases in

[8]The interquartile range of the log predicted income is approximately 0.63. The implied effect of an interquartile increase in import growth is a reduction in predicted income of 0.073, or around 12% of the interquartile range.

Table 3: The Effect of Childhood Import Exposure on Intergenerational Mobility

	Upward Mobility					
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta\ln(\widehat{Imports}_{jct_{(0,10)}})$	-0.054**	-0.058***	-0.076***	-0.024***	-0.036***	-0.033**
	(0.022)	(0.013)	(0.011)	(0.006)	(0.012)	(0.013)
Income $\text{Rank}^{Father}_{j,1900}$	-0.008***	-0.009***	-0.010***	-0.011***	-0.011***	-0.008***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
$\ln(1+AVE_{jct_0})$		1.822***	1.373***	0.429***	0.264***	0.242***
		(0.243)	(0.176)	(0.067)	(0.079)	(0.083)
White_j			0.295***	0.316***	0.321***	0.323***
			(0.016)	(0.023)	(0.023)	(0.023)
Black_j			0.038**	0.073***	0.076***	0.072***
			(0.016)	(0.024)	(0.024)	(0.023)
$\text{Father Literacy}_{jt_0}$			0.104***	0.079***	0.078***	0.079***
			(0.005)	(0.003)	(0.003)	(0.003)
Father is an immigrant			0.006	-0.016***	-0.020***	-0.019***
			(0.011)	(0.005)	(0.005)	(0.005)
$\ln(Father\ Income_{jct_0})$						-0.197***
						(0.053)
1^{st} Stage Coeff.	-4.81	-4.87	-4.81	-3.63	-2.35	-2.35
1^{st} Stage F	58.33	172.99	169.77	80.02	53.66	53.73
1σ	-.11	-.11	-.15	-.05	-.07	-.07
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Urban-State FE	No	No	No	Yes	Yes	Yes
County Controls	No	No	No	No	Yes	Yes
Obs.	3,868,666	3,868,666	3,868,666	3,868,666	3,868,666	3,868,666

Notes: Table presents regression of childhood county-level import exposure shock on the probability of intergenerational upward mobility. Sample is comprised of all male individuals born between 1900 and 1910 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

magnitude, suggesting that spatial variation to import exposure encompasses an important dimension of the long run effects.

While all counties are exposed to import competition to some extent, there is clear spatial clustering in our instrument, as depicted in Figure 3. To ensure that we are not simply identifying long-run differences in intergenerational mobility in the North versus the South, for example, in column 3 we introduce region specific birth-year fixed effects. This specification exploits spatial variation in the specific tariff share within a region and year for identification. Here, too, we find a statistically significant and quantitatively meaningful effect of the childhood import shock on wages in adulthood. In column 4 we include fixed effects for father's occupation interacted with birth-year fixed effects in order to address the possibility that coincident shocks to particular occupations may drive our results. This

Table 4: The Effect of Childhood Import Exposure on Adult Income, Robustness

	$\ln\left(\widehat{Income}_{j,1940}\right)$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta\ln(\widehat{Imports}_{ct})$	-0.036***	-0.023**	-0.064***	-0.047***	-0.027**	-0.109***	-0.057***
	(0.008)	(0.010)	(0.010)	(0.010)	(0.013)	(0.027)	(0.010)
$\ln(1+AVE_{ct_0})$	0.072	0.385**	0.290***	0.208**	0.315***	0.192*	0.347***
	(0.057)	(0.184)	(0.093)	(0.087)	(0.093)	(0.113)	(0.095)
$\ln(\widehat{Income}^{Father}_{j,1900})$	0.056***	0.103***	0.108***	0.047***	0.108***	0.127***	0.093***
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.006)
$\Delta\ln(\widehat{Imports}_{c\mathbf{T}})$					-0.134***		
					(0.044)		
1^{st} Stage Coeff.	-2.35	-1.42	-2.69	-2.36	-2.37	-25.94	-2.95
1^{st} Stage F	53.65	28.14	75.48	53.47	31.23	28.38	53.12
1σ	-0.08	-0.05	-0.13	-0.09	-0.05	-0.19	-0.12
Spec. Change	Dep. Var. ln(Occ. Score)	1910 County FE	Region × Year FE	Father Occ-Yr FE	1930-1940 Shocks	1900-1904 Cohort	1905-1910 Cohort
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,868,666	3,868,666	3,868,666	3,868,533	3,860,399	1,884,530	1,984,136

Notes: Table presents regression of childhood county-level import exposure shock on log earnings in 1940. Sample is comprised of all male individuals born between 1900 and 1910 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). With the exception of column 6, the dependent variable is log of observed income in 1940. In column 6 the dependent variable is predicted income as constructed by Collins and Wanamaker (2022). Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year except in column 4. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

leaves our primary conclusions unaffected.

A unique feature of our identification strategy is the ability to construct time-varying shocks. This allows us to explore directly whether childhood exposure itself drives long run outcomes, or if exposure to a childhood shock is merely predictive of exposure to future shocks, and it is those that determine 1940 income levels. This latter case may occur, for example, if industry-level trade shocks are persistent and sons are likely to live in locations with similar industrial composition to those of their father. To our knowledge, such a possibility has not been explored in the intergenerational literature. To address this, in column 5 we construct an import growth shock based on the import growth between 1930 and 1940 in the son's 1940 county of residence. As one would expect the estimated effect of the contemporaneous shock is larger in magnitude than the childhood shock. Moreover, its inclusion decreases our baseline estimate by about half, suggesting that some component of the effect of childhood shocks may be due to correlation with subsequent shocks. However, the effect of the childhood shock remains significant and economically meaningful.

Finally, in columns 6 and 7 we estimate our baseline specification separately for individ-

uals born between 1900 and 1904 and those born between 1905 and 1910. This helps address concerns that period-specific shocks, most notably those driven by World War I, might explain our results. While our early cohorts, born 1900-1904, would have been just entering the labor market during WWI, those born in 1905-1910 would have still been children. Although the first stage and point estimates reveal heterogeneity in the effect of import growth on long-run earnings, our results are not driven exclusively by either cohort. The implied effect of an interquartile increase in trade for the two groups is quite similar: a reduction in log income of 0.25 and 0.20, for the 1900-1904 and the 1905-1910 cohorts, respectively.

Prior work has emphasized long run persistence in familial wealth and status even in the face of immediate economic loss Ager et al. (2021), suggesting the important heterogeneity throughout the income distribution. For example, wealthier parents may be able to smooth out consumption across longer horizons and minimize the effects of adverse shocks on their children. To explore this possibility, we estimate our baseline specification separately by income quintiles based on the father's predicted income in 1900. The estimates, presented in Table 5, suggest that the income effects of childhood exposure to import shocks are primarily concentrated in the left-tail of the parental income distribution. Children born to fathers in the bottom 20% of the income distribution see a 7% reduction in their adult earnings for a doubling of import growth, equivalent to a standardized effect of a 0.13 standard deviation reduction in long run earnings. The magnitudes are similar for sons born to fathers in quintiles two and three. By contrast, children born to fathers in the top 20% of the income distribution experience an earnings effect only half as large. These results suggest that the channels through which childhood exposure impacts long-run outcomes may be heterogeneous throughout the income distribution.

4 Other Outcomes and Mechanism

The richness of the full count Census data allows us to explore a host of additional outcomes in order to better identify the channels through which adult incomes may be impacted by childhood trade shocks. In this section, we explore whether such shocks results in variation in educational outcomes, cross-state migration, the probability of living in the South or in an urban environment, or the probability of being locked into a father's occupation. As in our prior analysis, we continue to explore heterogeneity in responses by father's income. Figure 4 presents the estimates from this exercise. Each cell contains the point estimate and standard

Table 5: The Effect of Childhood Import Exposure on Adult Income Throughout the Income Distribution

	$\ln\left(\widehat{Income}_{j,1940}\right)$				
	Q1	Q2	Q3	Q4	Q5
$\Delta\ln(\widehat{Imports}_{ct})$	-0.070***	-0.077***	-0.065***	-0.003	-0.030***
	(0.010)	(0.015)	(0.018)	(0.021)	(0.008)
$\ln(1 + AVE_{ct_0})$	0.140	0.394***	0.318***	0.161*	0.203***
	(0.168)	(0.126)	(0.103)	(0.093)	(0.071)
1^{st} Stage Coeff.	-2.25	-2.32	-2.45	-2.51	-2.88
1^{st} Stage F	76.22	75.97	56.82	13.67	36.96
1σ	-0.13	-0.15	-0.13	-0.01	-0.06
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Obs.	773,733	773,733	773,734	773,733	773,733

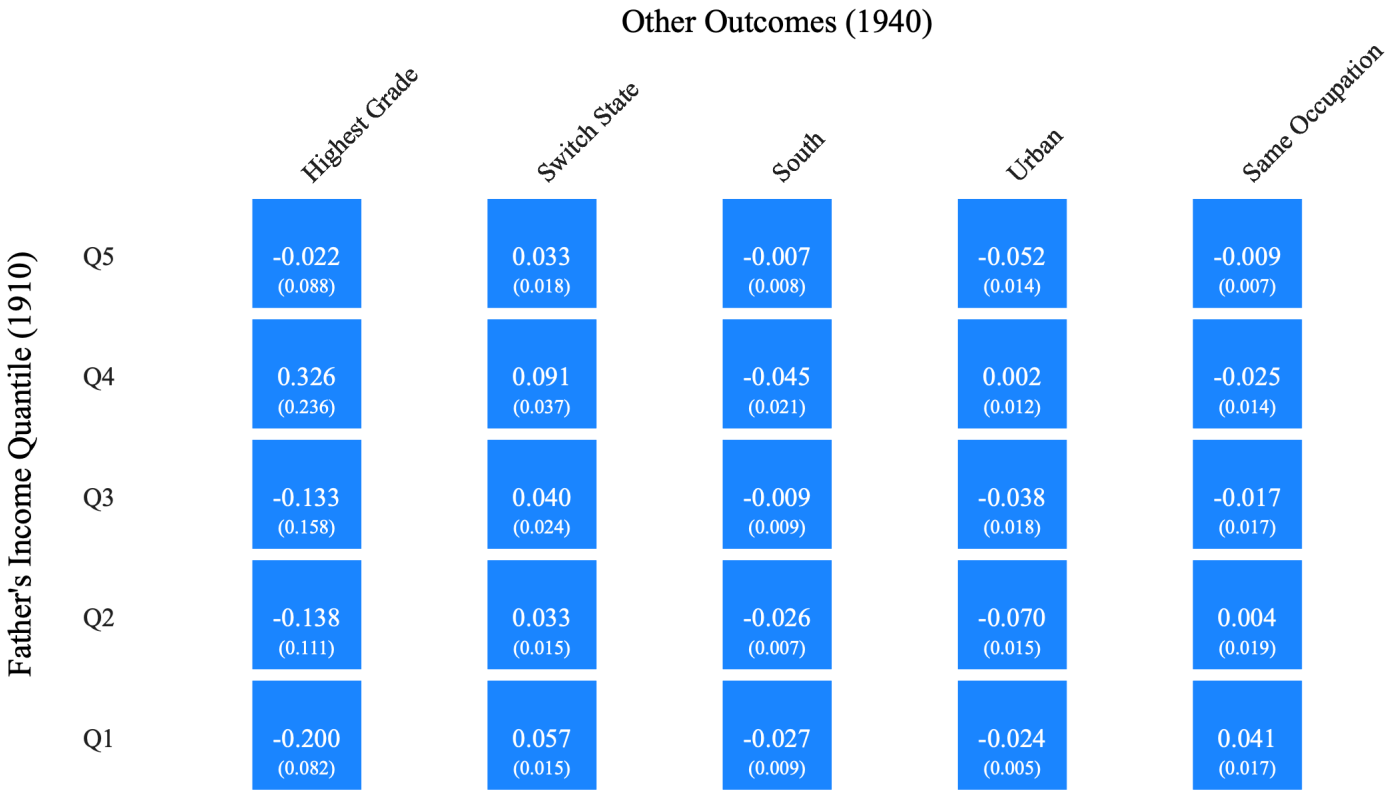
Notes: Table presents regression of childhood county-level import exposure shock on log earnings in 1940. Sample is comprised of all male individuals born between 1900 and 1910 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). The dependent variable is log of predicted income in 1940 following Collins and Wanamaker (2022). Each column provides separate estimates for sons born to fathers in each respective 1900 quintile. Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

error for the childhood import shock. We explore the various adjustment mechanisms across the columns, with each row corresponding to father's income quintiles.

Estimates suggest that among the lowest income quintile, import competition is robustly associated with fewer years of education and a decreased likelihood of living in the South. Similarly, sons born into the bottom quintile of the income distribution are most likely to remain in their father's occupation. By contrast, higher trade exposure is associated with an increased likelihood of a move to another state and a decreased likelihood of living in an urban area independent of the father's income status. These results suggest that occupational lock-in and reduced educational attainment may have played a role in driving our results, particularly at the bottom end of the income distribution.

Consistent with our findings on income and educational attainment, we next document that childhood exposure to trade shocks substantially reduces the likelihood of being employed in a high-paying occupation in adulthood, particularly for children born into the bottom income quintiles. As above, in Figure 5, rows correspond to different income quintiles and columns correspond to occupation groups, ascending in median income, with cells reporting the estimated effect of trade exposure on the probability of entry into a particular occupation group in adulthood. The estimated coefficients within a row sum to zero and

Figure 4: Effect of Import Exposure on Other Outcomes

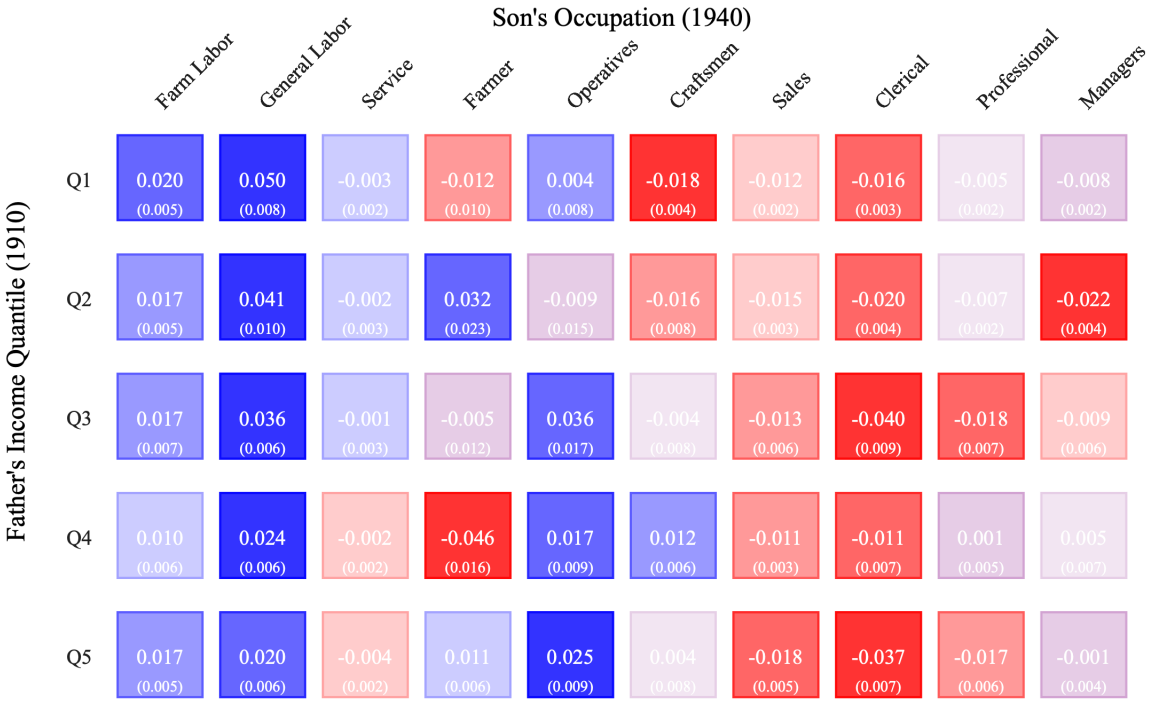


Notes: Figure displays effect of childhood import growth, instrumented with realized protection, on various outcomes in 1940. Sample is comprised of all male individuals born between 1900 and 1910 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

are coordinated ordinarily from dark blue – largest positive value – to dark red – largest negative value – to aid in identifying broad patterns in occupational access.

Our results suggest that childhood exposure to import competition pushed sons born into the bottom end of the income distribution out of craft and clerical work and into general and farm labor. While nearly all sons throughout the income distribution are pushed toward these lower-earning positions, the difference in magnitudes is stark: a doubling of childhood import exposure is associated with a 7% increase in entry to these lowest wage occupations, and only a 3% increase for the highest income groups. Higher-income sons shift disproportionately towards the middle-paying occupations.

Figure 5: Effect of Import Exposure on Occupational Transitions



Notes: Figure displays effect of childhood import growth, instrumented with realized protection, on the probability of working in each respective occupational category for sons of fathers in each respective 1900 quintile. Sample is comprised of all male individuals born between 1900 and 1900 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

Finally, in Table 6, we directly account for the effect of childhood import shocks on fathers. We consider two dimensions related to father's labor market opportunities. In Panel A of the table, we first include indicator variables for the industry and occupation of the father in 1900. This addresses the concern that different industries and occupations may have been trending differently throughout this era in a way that was correlate with trade exposure while also being predictive of son's long run outcomes. If anything, inclusion of these controls sharpens the result that the effects of childhood import shocks disproportionately impacts children of economically disadvantaged fathers. We find no significant effect of childhood import exposure on long run earnings for children born to parents in the top two quintiles of the income distribution, while those in the bottom three quintiles experience statistically and economically significant declines.

In Panel B, we explore whether father's industry-level trade exposure, in addition to

Table 6: Father's Controls

	$\ln\left(\widehat{Income}_{j,1940}\right)$				
	Q1	Q2	Q3	Q4	Q5
	Panel A: Controlling for Father's Occupation and Industry				
$\Delta\ln(\widehat{Imports}_{ct})$	-0.058***	-0.060***	-0.040***	-0.006	-0.007
	(0.009)	(0.013)	(0.012)	(0.016)	(0.006)
$\ln(1+AVE_{ct_0})$	0.100	0.306***	0.181**	0.094	0.133**
	(0.148)	(0.108)	(0.073)	(0.067)	(0.057)
1^{st} Stage Coeff.	-2.25	-2.32	-2.47	-2.51	-2.88
1^{st} Stage F	75.85	75.36	57.86	13.67	37.02
1σ	-0.11	-0.12	-0.08	-0.01	-0.02
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Obs.	773,724	773,721	773,729	773,725	773,726
	Panel B: Controlling for Father's Industry Shock				
$\Delta\ln(\widehat{Imports}_{ct})$	-0.064***	-0.081***	-0.051***	-0.011	0.002
	(0.008)	(0.016)	(0.014)	(0.020)	(0.012)
$\ln(1+AVE_{jct_0})$	0.139	0.485***	0.310**	0.203*	0.324***
	(0.194)	(0.158)	(0.117)	(0.120)	(0.095)
$\Delta\ln(\widehat{Imports}_{it})$	-0.052***	-0.052***	-0.056***	-0.064***	-0.080***
	(0.011)	(0.011)	(0.008)	(0.016)	(0.010)
1^{st} Stage Coeff.	-2.18	-2.22	-2.44	-2.36	-4.3
1^{st} Stage F	34.93	36.03	30.13	4.1	28.05
1σ	-0.12	-0.15	-0.1	-0.02	0.01
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Obs.	556,555	535,134	403,136	537,098	397,258

Notes: Table presents regression of childhood county-level import exposure shock on log earnings in 1940. Sample is comprised of all male individuals born between 1900 and 1910 who could be linked to themselves in 1940 and to their father. Data taken from Ruggles et al. (2024) and linked following Price et al. (2021). The dependent variable is log of observed income in 1940. In column 6 the dependent variable is predicted income as constructed by Collins and Wanamaker (2022). Each column provides separate estimates for sons born to fathers in each respective 1900 quintile. Change in log imports is instrumented with change in realized protection ΔRP_{ct} defined in equation 3. Shock varies by cohort and county of birth. Import and tariff data taken from Greenland and Lopresti (2024). Standard errors clustered at the state and year except in column 4. *, **, *** indicate p-values less than 10%, 5%, and 1% respectively.

local exposure, might directly impact the income of the son in adulthood. Specifically, we include a measure of import growth in father's industry and similarly instrument it with the industry-level measure of changes in realized protection.[9] As in Panel A, the county-level shock is only significantly related to long-run outcomes for sons born into the bottom of the income distribution. Interestingly, we find that industry level exposure matters throughout the income distribution. Indeed, the effects are largest for those in the upper two quintiles;

[9]Note that this measure can only be constructed for the tradable sectors and therefore this sample excludes sons with fathers in the nontradable sector.

sons of high-income fathers are more sensitive to the direct industry level shock than their low income counterparts. One possible explanation for this heterogeneity is that while all children are affected by family income levels, low-income chldren are more dependent on local investment and infrastructure (Feler and Senses, 2017) compared to high income individuals.

5 Conclusion

Despite their importance for long-run welfare, the intergenerational effects of trade shocks have received relatively little attention from scholars. In this paper we help to fill this gap by exploring the effect of childhood import growth on adult labor market outcomes between 1910 and 1940. Exploiting quasi-random variation in tariff protection across industries and regions, we show that children more exposed to import competition in their youth earn lower incomes and work in less appealing occupations 30 years late. Higher tariff levels were associated with lower import growth and better long-run outcomes for children. Such individuals are also less educated and less likely to live in urban locations in adulthood. These effects are especially pronounced for sons born into the lowest income quintile We find trade exposure both at the county level and in fathers' sectors of employment important in driving these results.

A large and still active literature has emphasized the persistent effects of trade in the late 20^{th} and early 21^{st} centuries. Our results here suggest that such effects predate those emphasized in this literature by decades, are potentially even more persistent than has been recognized, and pass from one generation to the next. Moreover, while much is known about the disruptive effects of trade shocks in the modern era of US manufacturing decline, ours is a period of rapid industrial expansion. Our results here suggest that the distributional effects of trade are still important outside of periods of secular manufacturing decline.

These results also raise the prospect of further work exploring policies to mitigate the negative consequences of trade. This was an era of substantial policy innovation, with changes in laws related to compulsory education, women's suffrage, collective bargaining rights, and government taxation and spending. The availability of full count Census data over many decades allows for detailed exploration of the role these and other policies may have played in promoting adjustment to trade shocks. Now as then, trade is a powerful force in shaping economic and social outcomes. Policies that aid in making it a force for good are as important now as they have ever been.

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A Appendix

Figure A.1: Unconditional Income Quantile Transitions

