

The China Shock's Surprising Impact on Intergenerational Income Mobility

Hanqi Liu*

University of Calgary

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Abstract

Import competition is known to cause lasting income losses for manufacturing workers, but its effects on their children have been largely overlooked. One might expect these losses to make family background matter more for children's income, since poorer families have little to draw on while richer ones can insulate theirs. Using matched employer-employee data that link children to their parents, I find the opposite: import competition from China increases income mobility among Canadian manufacturing families. In heavily exposed families, where a child ends up depends about 20% less on where their parents stood. Children from poorer families climb the income ladder; they escape by moving out of their parents' industries into ones that offer similar pay on average but where family background matters less. Children from richer families fall; they stood to follow their parents into high-paying industries, and the shock takes those jobs away.

Keywords: International trade, China Shock, intergenerational income mobility, inequality

JEL Codes: F13, F16, J62

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1 Introduction

International trade alters the wages, employment, and occupational trajectories of workers it directly hits. A displaced worker is visibly displaced; her child is not. Growing up in a household reshaped by the same shock, the child may follow a different path — a different industry, a different income — and look as though she chose it. But if that path was redrawn for her before she was old enough to have a stake in it, the shock does not end with the worker it displaces. It reaches forward into the next generation and sets the terms of a child's economic life. What ends with the worker is a labor market adjustment; what reaches the child is a change in economic opportunity — a change that shows up as intergenerational income mobility, how much a child's income depends on their parents'. Despite the obvious connection, remarkably little is known about how trade shocks affect it. I fill this gap by studying the effect of the China Shock, China's accession to the World Trade Organization (WTO) in December 2001, on the intergenerational income mobility of children of manufacturing workers in Canada.

The China Shock could raise or lower income mobility. When parents working in manufacturing lose their income, they have less to invest in their children, and the shock passes down as a poorer economic start. The child inherits the disadvantage, and mobility falls. But the same shock can also dislodge children from their parents' industry and region. If they are better poised to move into the new jobs, where they end up no longer follows from where their parents stand, so mobility rises.

The China Shock is a good candidate to study this question, given how hard it hit manufacturing employment across developed economies ([Autor, Dorn and Hanson, 2013](#); [Dauth, Findeisen and Suedekum, 2014](#); [Balsvik, Jensen and Salvanes, 2015](#)). The surge in Chinese imports that Canada faced was comparable in magnitude to that in other developed economies.¹ Isolating its effect, however, is difficult, because in many countries manufacturing employment was already declining when the shock arrived. In the United States, it had been shrinking for decades, so any change in children's outcomes could reflect the shock or the trend that preceded it. Canada alleviates this concern, because there the shock arrived as a break rather than a continuation. After entering the North American Free Trade Agreement (NAFTA) in 1994, Canada's manufacturing employment share rose from 14% to 15.3% by 2000. The trend then reversed abruptly, and the share fell to 10.2% by 2011, giving back a decade of gains. This reversal is therefore far harder to attribute to a pre-existing trend, and Canada offers a cleaner setting to isolate the shock's

¹Chinese imports surged across developed economies after the shock: China's share of manufacturing imports rose from about 3% to 14% in Canada, 9% to 26% in the United States, and 4% to 12% in Germany between 2000 and 2011; import shares are computed using data from the CEPII database.

effect.

To investigate the effect of the China Shock on the next generation, I need to observe both what a family looked like before the shock and how its children fared as adults. The Canadian Employer-Employee Dynamics Database (CEEDD) lets me follow the same family across both, with longitudinal administrative tax data covering the universe of tax filers from 2001 to 2019 and linking children to their parents. My sample is manufacturing families, those with a child born between 1986 and 1992 and at least one parent in manufacturing in 2001, the year I use to capture each family’s pre-shock position.²

I measure income mobility by how strongly a child’s income percentile rank depends on their parents’; the weaker this dependence, the higher the mobility. This measure is rank-rank persistence (Chetty et al., 2014a).³ Children are ranked on their average income at ages 27–31, a standard proxy for permanent income, against others born the same year. Parents are ranked on total parental income in 2001 against other families whose child was born the same year.⁴ These manufacturing families, the main sample I study, sit mostly in the middle and upper middle of the 2001 income distribution.

I measure exposure to the China Shock at the industry level, from the growth in an industry’s imports from China (Autor et al., 2014). Each family is assigned the exposure of its parents’ 2001 industry, averaged across both parents when both worked in manufacturing and taken from that parent otherwise.⁵ Since the data link parents to children, this exposure, like every comparison in the paper, is defined at the family level. I estimate the effect of the China Shock on income mobility by comparing children whose parents worked in industries with different levels of exposure.

To make the size of the effects easier to picture, I anchor continuous trade exposure to three benchmark industries that recur throughout the paper. I take an industry at average exposure to the trade shock as the mild case (wire and spring products), an industry about one standard deviation above the average as the severe case (leather products), and

²Those born in 1986 were old enough that the shock preceded their post-secondary schooling and early-career choices; those born in 1992 are the youngest whose adult income is reliably observed before the data end in 2019.

³Rank-rank persistence runs from 0 to 1 and measures relative mobility. At 0, a child’s rank is unrelated to their parents’ and mobility is complete; at 1, it is fully inherited. Canada is among the most mobile of rich countries, at about 0.24, against 0.30 in the U.K. (Rohenkohl, 2023) and 0.34 in the U.S. (Chetty et al., 2014a).

⁴Parental income is the sum across both parents, or the single parent’s. Ranks are robust to zero and transitory incomes and stable across the life cycle (Chetty et al., 2014a). Parental income rank would ideally average over several pre-shock years, but the CEEDD begins only in 2001; the 2001 rank is nonetheless close to one averaged over the child’s teenage years; see detailed discussions in Section 2.3.

⁵Because Canadian import growth may partly reflect domestic demand shocks, I instrument it with Chinese import growth in six other high-income economies. Regressions control for a range of family, regional, and pre-shock industry characteristics, so that exposure is identified from within-sector variation rather than from industries that differed before the shock.

an industry about 2.7 standard deviations above the average, among the most exposed of all, as the extreme case (apparel knitting mills). Exposure is assigned at the family level, so a mild-case family is one facing the level of exposure this industry experienced.

The shock hits the parents directly and reaches the children through them, so I begin with the parents. Manufacturing families more exposed to the shock earn less over 2002–2011. Relative to a mild-case family, a severe-case family's cumulative loss over the decade comes to about 15% of its 2001 annual income, or roughly \$15,000 at the average 2001 parental income; for an extreme-case family, the loss reaches about 35%, or roughly \$35,000.

The next step is to see whether the shock reaches children and affects income mobility. When a shock hits these families, one might expect family background to matter more, not less. Children from richer families, cushioned by their money and resources, stay unscathed, while children from poorer families are dragged down, so mobility falls. Surprisingly, the opposite happens. The shock loosens the link between children's income and their parents', and mobility rises. Rank-rank persistence falls from 0.23 in the mild case to 0.21 in the severe case and 0.187 in the extreme case, a drop of roughly 20%. To see how large this is, set it against how persistence moves on its own. [Connolly and Haeck \(2024\)](#) document that across cohorts born from 1975 to 1985 in Canada, rank-rank persistence shifts from 0.233 to 0.245, about 5% over a full decade. By comparison, the shock moves persistence downward four times as far, and against the direction it was already heading.

A rise in mobility sounds like good news. It can mean children of poorer families climbing past where their parents stand, but it can also arise in a darker way, when richer families fall toward the poorer ones. To tell which, I need to see where each generation ends up, not just where the parents start. So for the parents, I now use their rank after the shock, measured when the child was a teenager, rather than their 2001 starting point. On average, the shock pushes both parents and children down the income distribution.

This fall, however, is not shared evenly. Compare two families whose parents worked in the same extreme-case industry but began far apart — one at the 10th percentile of the 2001 distribution, the other at the 90th. In the richer family, parents and child fall together: the child drops 0.91 percentile ranks and the parents 0.96 percentile ranks, the child tracking the parents down. In the poorer family, the parents barely move, rising about 0.5 percentile ranks, while the child's rank rises about 2.74, more than five times as much, pulling away from where the parents had stood. The gain is concentrated among poorer families, the loss among richer ones. Because a percentile is worth far more at the top, these rank movements imply very different sums: over a working life to age 65, the

poorer family's child gains about \$40,230, close to a full year's income for such a family, while the richer family's parents lose about \$64,950. The same shock binds one child to their parents' fate and cuts the other loose from it.

The rise in mobility is not uniform across families. It is concentrated among families in the second quartile; among the top half it barely moves. The rise is also about twice as large for girls as for boys, and for children of immigrant parents as for children of native-born parents. Among children whose parents were in the bottom fifth of the income distribution, those from extreme-case families are about 13% less likely to remain in the bottom fifth as adults than those from mild-case families. For these children, the climb up the economic ladder is real.

Why does mobility rise, and rise mainly for children from poorer families? It turns on two things. The first is whether a child leaves their parents' industry. Children usually follow career paths close to their parents', taking advantage of the contacts and experience a family passes down (Lo Bello and Morchio, 2022). In Canada, as a baseline, a 17-to-19-year-old already working in 2001 had about a one-in-ten chance of working in their own parents' manufacturing industry. However, the trade shock pushes children out of that path. For a child from a family at the 50th percentile, moving from the mild case to the extreme case cuts the chance of working in their parents' industry by about 5%. But children from richer families are more likely to stay. Facing the same extreme hit, a child from a family at the 90th percentile is about 14% more likely to stay than one at the 10th.

The second is which industry children enter. For those who leave, I compare the industry they move into with their parents', the one they would otherwise have inherited. The comparison weighs what they gain by leaving against what they give up, and it turns on two features of an industry. The first is the industry's mobility, meaning how loosely it ties a child's income to their parents'. In some industries, such as tobacco, this tie is strong, so those who start poor tend to stay poor and those who start rich stay rich. In others, such as food service, the tie is loose, and where children end up has little to do with where they began. On this margin, children of every family background move into industries with higher mobility than the manufacturing ones they leave, trading a career that runs in the family for one that does not. The gap is widest for children from poorer families, which suggests that the rise in mobility is concentrated among them.

The other feature is the industry's returns, which measure how much more an industry pays a worker than other industries would, holding family background fixed. For example, oil is a higher-return industry than retail. On this margin, whether a child gains or loses returns by leaving their parental industry depends on where their family started, and the two cases divide at about the 20th percentile of parental income. For families

below the 20th percentile, the move is lateral in returns. Their parents' manufacturing industries were low-return to begin with and declining, even as they had low mobility and bound children's incomes tightly to their origins. The sectors these children move into, personal care or food service, are themselves low-return, so these children give up almost no returns while gaining mobility. This is therefore escape. Leaving frees them from a tie that would have held them near the bottom regardless of their own ability. For families above the 20th percentile, the move costs returns, and the loss grows with where the family started, largest at the top. Their parents worked in industries with returns worth inheriting, but the shock pushes their children out of them. A child who leaves moves to a lower-return industry than the one they would have inherited. A child who stays remains in a declining sector. In sum, the same manufacturing industry is a floor for one family and a ladder for another. When the shock hollows it out, it loosens the floor that had held one child near the bottom and pulls away the ladder that would have carried the other up.

The mobility literature points to two other channels that could raise income mobility. The first is geography. Mobility is known to vary across regions ([Chetty et al., 2014a](#); [Connolly, Corak and Haeck, 2019](#)), so a shock that relocated children to more mobile places could raise mobility. However, I find that the trade shock has no measurable effect on whether a child works in the same Census Division as their parents. The second is human capital ([Becker and Tomes, 1979](#)). Parental income affects the education children receive, and education in turn affects mobility. Lacking direct data on education, I use an AKM decomposition ([Abowd, Kramarz and Margolis, 1999](#)) to isolate the component of a child's income that follows them across firms, their own skill, and I find that the shock leaves no measurable effect on it.

This paper is, to my knowledge, the first to study the intergenerational effects of a trade shock on income mobility using family-level evidence. A large body of work documents the negative and persistent effects on directly affected manufacturing workers ([Topalova, 2010](#); [Autor, Dorn and Hanson, 2013](#); [Autor et al., 2014](#); [Acemoglu et al., 2016](#); [Pierce and Schott, 2016](#); [Dix-Carneiro and Kovak, 2017](#)). This paper studies the persistent impact on an important but overlooked group, the children of those manufacturing workers, and tracks the income of both generations with administrative tax records. The few studies that do follow trade shocks across generations look at other outcomes in developing countries — educational mobility in China ([Lou and Li, 2022](#)), occupational mobility in Vietnam ([Mitra, Pham and Marchand, 2026](#)) and India ([Ahsan and Chatterjee, 2017](#)). But a child can hold on to their schooling and their occupation while the income tying them to their parents comes loose, precisely what a rank-rank measure is built to catch.

The study closest to mine, [Colantone, Ottaviano and Takeda \(2024\)](#), uses commuting-

zone variation in China Shock exposure and finds the shock lowered upward mobility in the U.S. My design differs in what it measures and in the mechanisms it can test empirically. Given the linkage between children and their parents in my data, exposure is defined at the family level, not the region: I separate the children the shock directly affected from those who merely lived nearby. A commuting-zone estimate gives one number per place, the net change for everyone in it. My results show what this net is made of: children from poorer and richer families move in opposite directions, so the net can rise even when part of it is a loss.

Showing and explaining this contrast by family background is my key contribution. Manufacturing has long been seen as a source of good jobs, offering steady, well-paid work to people without a university degree. But after the China Shock, following a parent into that sector meant different things depending on where the family started. For children from poorer families it was staying near the bottom; for children from richer families, inheriting a job that paid well, though one in a declining sector. The shock freed the first group from a floor and cut the second off from a ladder that was giving way in any case. Whether a sector is a floor or a ladder therefore depends not only on how fast it is declining, but on which families were standing on it.

My paper also contributes to a growing literature on the drivers of income mobility in developed countries, including neighborhoods, schools, occupations, and local economic booms (Chetty and Hendren, 2018; Biasi, 2023; Haeck and Laliberté, 2025; Bütikofer, Dalla-Zuanna and Salvanes, 2025). Beyond these domestic forces, I study a force from outside the domestic economy, a huge global trade shock. Moreover, the China Shock is one instance of a broader class, because automation and AI share the same defining feature, striking some industries and sparing others. For any shock of this kind, my paper shows that what it does to the next generation depends not just on how hard it hits, but on what it hits, whether the ground beneath those families was a floor or a ladder.

The paper proceeds as follows. Section 2 details the data sources and variable measurement. Section 3 presents the empirical strategy and the results. Section 4 explores the underlying mechanisms, and Section 5 concludes.

2 Data and measurement

2.1 Data sources and intergenerational sample

Administrative tax data I use the Canadian Employer-Employee Dynamics Database (CEEDD), which spans 2001 to 2019 and combines administrative tax files into a matched

employer-employee panel. On the employee side, it contains T1 individual tax returns for the universe of tax filers, T4 Statements of Remuneration Paid for wage earnings, and Records of Employment (ROE). The Longitudinal Immigration Database (IMDB) is also integrated into the CEEDD to identify immigrant status. The T4 slips connect workers to the employers that issued them. A worker who switches employers within a year receives multiple slips, in which case I assign them to the employer from which they earned the most. On the employer side, the CEEDD includes T2 corporate tax returns and T2S50 ownership schedules, which provide the firm's industry classification and its number of employees. A worker's recorded industry is the industry code of their assigned employer, concorded to the four-digit 2012 NAICS level, and this assignment is the basis for both parental and child industry throughout the paper.

Trade data To construct measures of trade exposure, I use Canadian trade data from the Canadian International Merchandise Trade (CIMT) database and domestic shipment data from the Annual Survey of Manufactures (ASM), which surveys businesses with annual sales above CAD 30,000. Import and export values, originally reported at the 10-digit and 8-digit Harmonized System (HS) levels, respectively, are mapped to 86 four-digit 2012 NAICS manufacturing industries using the concordance provided by the International Accounts and Trade Division of Statistics Canada. For the instrumental variable, I draw Chinese export flows to six other high-income countries from the UN Comtrade database: Denmark, Finland, the Netherlands, Norway, Spain, and Sweden. All income and trade values are deflated to 2012 constant Canadian dollars.

Intergenerational sample I construct the parent-child linkage by following the procedure used by Statistics Canada in constructing the Intergenerational Income Database (Corak and Heisz, 1999), which underlies most existing work on intergenerational mobility in Canada. Specifically, I use the T1 Family File (T1FF) in the CEEDD to match each child aged 15 to 19 to their parents through unique census family identifiers. When a child appears with different parents across ages 15 to 19, which may reflect changes in family composition such as divorce or remarriage, I retain the linkage from the earliest year in which a match is made.

My main analysis sample consists of all linked children from the 1986 to 1992 birth cohorts with at least one parent employed in manufacturing in 2001. I refer to these children and their parents as manufacturing families. The two ends of the cohort window are pinned down jointly by data coverage and by the ages at which the key variables are measured, which I describe in Section 2.2. The parent-child link and trade exposure are both

measured over ages 15 to 19, so the 1986 cohort is the oldest whose exposure window falls entirely within the CEEDD panel. This cohort also sits at a developmentally meaningful moment. When the shock began in 2002, these children were 16 and had not yet settled their post-secondary plans. At the other end, labor market outcomes are measured from age 27 onward, so the 1992 cohort is the youngest whose outcomes can be observed by the 2019 endpoint. The resulting sample contains approximately 400,000 children.⁶

2.2 Measurement of trade exposure

I measure trade exposure by the growth in Chinese import penetration at the industry level following the methodology of [Autor et al. \(2014\)](#):

$$\Delta IP_{j\tau} = \frac{\Delta M_{j\tau}^{Can-Chn}}{Y_{j,2000} + M_{j,2000} - X_{j,2000}} \quad (1)$$

where $\Delta M_{j\tau}^{Can-Chn}$ is the change in imports for industry j from China to Canada over the period τ . The denominator is initial domestic absorption in the base year 2000, before the China Shock, where Y_j , M_j , and X_j represent total shipments, imports, and exports, respectively.⁷

I define the shock window τ for a child born in year b as the period when they are aged 15 to 19, that is, from year $b + 15$ to $b + 19$. The window therefore varies across cohorts, so children whose parents work in the same industry face different values of $\Delta IP_{j\tau}$ depending on their birth year. This window serves two purposes. It coincides with the ages at which I construct parent-child matches. It is also the window over which the mobility literature normally measures parental income to characterize the family resource environment ([Chetty et al., 2014a](#); [Connolly and Haeck, 2024](#)), since these years span human capital accumulation and the school-to-work transition. To the extent that the trade shock reaches children through parental income, measuring exposure over this window aligns the timing of the shock with the timing of parental income. This cohort-specific window is used throughout the paper. The one exception is Section 3.1, where the trade exposure on parents is measured over the fixed period 2002 to 2011, for reasons I explain there.

⁶According to the vetting requirement of Statistics Canada’s Research Data Centres, the count reported here is rounded. Disclosure rules also prevent me from reporting counts for the subsamples used in the analysis that follows, such as the number of individuals by gender or by immigrant status.

⁷Total shipments measure the dollar value of products sold by manufacturing establishments, and they are reported on a net selling value basis, free on board plant, excluding discounts and allowances.

Instrumental variable A central challenge with Equation (1) is that observed changes in import penetration are determined by both supply and demand factors. A positive domestic demand shock in Canada raises the demand for imported goods and the demand for domestic labor at the same time. For example, if Canadian consumers spend more on furniture, purchases of both Chinese imports and domestically produced furniture rise, so imports and local employment increase together for reasons unrelated to import competition. Unobserved technological shocks within Canadian industries can work in the same way, shifting import demand and labor demand jointly and further confounding the causal interpretation. An OLS regression would attribute employment growth driven by these domestic factors to rising imports. Because trade exposure operates directly on parents of these manufacturing families, the OLS estimate of the effect on parental income is biased upward and understates the true negative effect. The consequences for children's income mobility are less clear, since mobility responds to the shock primarily through the parental margin and the sign of the bias is not determined a priori.

To address this endogeneity, I isolate the component of Chinese import growth driven by China's rising productivity and falling trade costs. I construct an instrument based on China's export growth to six other high-income economies, namely Denmark, Finland, the Netherlands, Norway, Spain, and Sweden:

$$\Delta IPO_{j\tau} = \frac{\Delta M_{j\tau}^{Other-Chn}}{Y_{j,2000} + M_{j,2000} - X_{j,2000}} \quad (2)$$

where the numerator captures the growth in imports from China in the comparison countries, and the denominator is unchanged from Equation (1). The identification strategy relies on the assumption that the rise in Chinese exports to these economies is driven by a common supply shock originating in China rather than by demand conditions specific to any single destination.

The choice of these six economies reflects two considerations. First, they are high-income economies with industrial structures comparable to Canada's, but each is small enough in the Chinese export market that it cannot shape China's pricing or export strategy in the way that the United States, Japan, or Germany plausibly can. Their import growth from China is therefore more likely to reflect China's rising productivity and falling trade costs than destination-specific demand. Second, the United States is excluded when measuring the instrument. Given the high degree of economic integration between the U.S. and Canada, demand shocks are transmitted across the border at high frequency, so an instrument built on U.S. imports from China would be contaminated by the very Canadian demand variation it is meant to purge. I discuss how I control for U.S. import

competition in Section 3. The distribution of import penetration growth has a long right tail. To prevent a small number of industries from driving the estimates, I winsorize both the trade shock and the instrument at the 99th percentile.

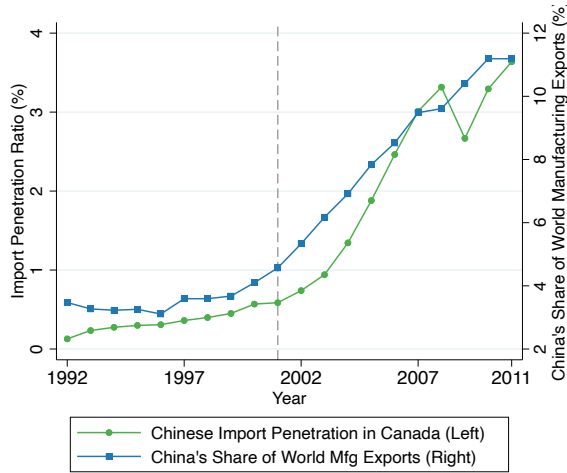
Descriptive evidence of trade exposure Figure 1(a) documents how large the China Shock was in both global and Canadian markets. China's share of world manufacturing exports held at roughly 3% from 1992 to 2001, as shown by the blue line, and Chinese imports in Canada remained near 0.5% of manufacturing absorption over the same period, as shown by the green line. Both series were flat through 2001 and rose sharply thereafter, reaching 12% and 3.7% respectively by 2011. Chinese import penetration in Canada rose more than sevenfold over the decade.

Unlike the United States and many other developed countries, where manufacturing employment had been declining for decades, Canada's manufacturing employment was still rising before the China Shock, in both levels and share, as shown by both the purple and red lines in Figure 1(b). The share rose over the NAFTA years, from 14% in 1994 to 15.3% in 2000, then turned down in 2002 and fell to 10.2% by 2011, ending the period well below its 1994 level. The absence of a pre-existing downward trend makes the reversal easy to date and helps isolate the impact of the China Shock by ruling out that the post-2002 fall is a continuation of decline.

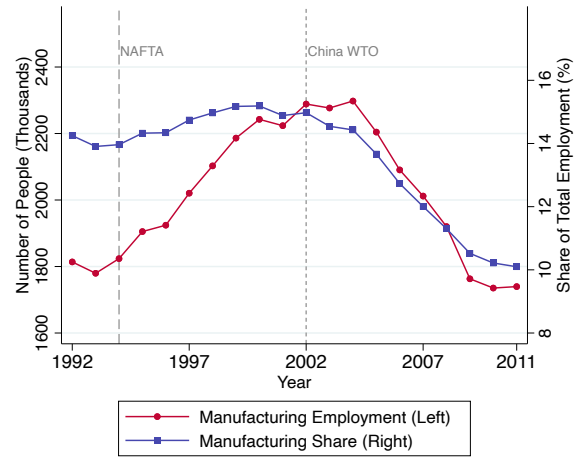
Figure 1(c) plots trade exposure by industry. The horizontal axis measures the growth in Chinese import penetration from 2002 to 2011, constructed as in Equation (1), which captures cumulative trade exposure for each industry. The vertical axis measures pre-shock labor intensity, proxied by the share of production workers in industry employment in 1992. Each point is a four-digit NAICS industry, and points are colored by ten broad sectors for legibility.

Two features stand out. First, trade exposure is heavily concentrated in labor-intensive sectors. The industries with higher levels of trade exposure are also the ones with higher initial shares of production workers. Trade theory predicts this pattern, since exports from a labor-abundant economy such as China fall most heavily on labor-intensive industries in destination markets. Autor et al. (2014) document the same concentration for the United States. In Canada, the most exposed four-digit industries, concentrated in Machines/Electrical and Textiles/Apparel/Leather, saw import penetration rise by over 20 percentage points. Capital- and resource-intensive sectors, such as Chemicals/Petroleum, Food/Tobacco, and Transportation, cluster near the vertical axis, with import penetration growth below 3 percentage points.

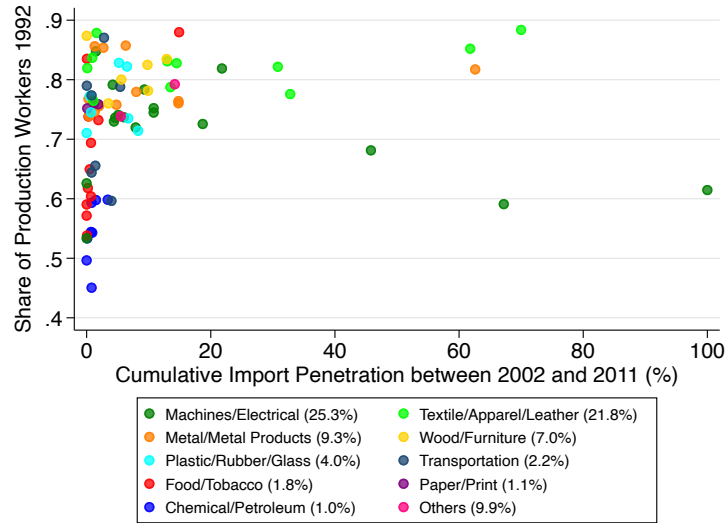
Second, and crucially for the identification strategy, there is large variation in expo-



(a) Manufacturing Trade Trends



(b) Manufacturing Employment Trends



(c) Trade Exposure and Production Worker-Intensity by Industry

Figure 1: Canadian Manufacturing: Trends and Industry Exposure

Notes: Panel (a) plots Canadian manufacturing imports from China, scaled by 2000 domestic absorption (green), together with China's share of world manufacturing exports (blue); data sources are described in Section 2. The vertical line marks 2001, the last year before the China Shock. Panel (b) plots annual manufacturing employment levels (red) and shares (purple), aggregated from seasonally adjusted monthly data (Statistics Canada Table 14-10-0355-01). The first vertical line marks the entry into force of NAFTA in 1994; the second marks 2002, the year in which manufacturing employment peaked. Throughout the paper I date the onset of the shock to 2002, the first full year after China's WTO accession in December 2001. In panel (c), each point is a four-digit NAICS manufacturing industry. The horizontal axis is the cumulative growth in Chinese import penetration from 2002 to 2011, constructed with Equation (1). The vertical axis is the share of production workers in industry employment in 1992. For visualization, industries are grouped into 10 broad sectors by color, and one industry with import penetration growth above 100 percentage points is truncated. Numbers in parentheses in the legend are unweighted averages of the cumulative trade shock within each broad sector.

sure among industries of similar labor intensity. High labor intensity is necessary but not sufficient for high trade exposure. The upper-left region of the figure contains a number of labor-intensive industries that faced little import competition, such as Plastics and Rubber Products. This dispersion also holds within sectors: four-digit industries belonging to the same three-digit sector often sit far apart along the horizontal axis. The trade shock is therefore not collinear with labor intensity. My empirical specification exploits variation in trade exposure across four-digit industries while including fixed effects for the 21 three-digit NAICS sectors, which absorb differences in skill composition and other characteristics common to industries within a sector.

2.3 Measurement of intergenerational income mobility

Income and income ranks In the CEEDD tax records, total income is pre-tax income from all sources, including wages and salaries, self-employment earnings, investment income, and government transfers. This definition follows the convention in the mobility literature ([Chetty et al., 2014b](#)).

Child income is measured as the average of their total income over ages 27 to 31, conditional on appearing in the tax records in at least one year of that window. Averaging over years with available data in this five-year window helps reduce the influence of transitory annual shocks, and the window is chosen to fall after the ages at which income profiles have largely stabilized. I also report results using an alternative window, ages 25 to 29, in the Appendix as a robustness check. Parental income is total family income, summed across both parents where two are present, and is measured in 2001.⁸ Previous literature typically measures parental income as an average over the five years when the child was aged 15 to 19, which smooths transitory fluctuations ([Connolly and Haeck, 2024](#)). That window is not usable as a pre-shock measure here. For every cohort from 1986 to 1992, some or all of the ages 15 to 19 fall after 2002, so parental income measured over that window is itself an outcome of the trade shock, and conditioning on it to recover the effect of the shock on children’s income mobility would absorb part of the effect I aim to estimate.

⁸A challenge common to tax-based mobility studies is non-filing, which produces missing income observations. The standard remedy is to impute zero income for non-filers, on the grounds that those who do not file are disproportionately low- or zero-income individuals. The CEEDD allows me to refine this practice in two ways. First, for an individual with verified employment activity in the T4 or ROE records but no T1 filing in a given year, I use the T4 and ROE earnings to impute the missing T1 income. Second, for individuals who remain missing after this step, I follow [Connolly and Haeck \(2024\)](#) and treat parents and children asymmetrically: I impute zero for a parent who remains unmatched in the employment record, but leave the observation missing for children. Young adults have substantially higher rates of missing records than parents, driven by outmigration, non-filing, and delayed filing, so zero imputation would introduce more measurement error for children than dropping them does.

Parental income must therefore be measured in a pre-shock year, and 2001 is the only pre-shock year the CEEDD covers. It is also the last year before the shock, so it captures the family’s economic position at the time the shock hit. What this requirement costs is that a single year of income is more exposed to transitory fluctuations than a five-year average. I return to this below, using Figure 2(a) to show that the cost is small.

I convert both parental and child income into percentile ranks, constructing these two distributions separately: children are ranked against all other children in Canada born in the same year, and parents are ranked against all parents whose children were born in the same year. A child’s income rank gives their position within their own birth cohort, and their parental rank gives the economic resources available to them relative to other families of their cohort. Ranking within cohort also removes any common shift in the income distribution across birth years. Compared with log income levels, ranks accommodate the zero incomes that a log specification cannot, and they are less sensitive to transitory fluctuations and to life-cycle position (Chetty et al., 2014a).

Measuring intergenerational mobility Intergenerational income mobility is measured as the relationship between child and parental income ranks. Following Chetty et al. (2014a), let $rank_i^c$ denote the child income rank of child i and $rank_i^p$ the parental income rank of that child’s family, both as defined above. I estimate

$$rank_i^c = \omega + \delta \cdot rank_i^p + \varepsilon_i. \quad (3)$$

The slope δ is the rank-rank persistence, which measures relative mobility: a higher δ means a child’s economic position is more tightly tied to their parents’, that is, lower mobility. In practice δ lies between 0 and 1: at 0, a child’s rank is unrelated to their parents’ and mobility is complete; at 1, it is fully inherited. The fitted value $\omega + \delta \cdot rank_i^p$ measures absolute mobility at parental rank $rank_i^p$, the expected rank of a child born to parents at that economic position.

Descriptive evidence of income ranks and income mobility Figure 2(a) plots the unconditional mean child income rank against parental income rank, with red markers using parental income measured in 2001 and blue markers using the conventional measure, the average of parental income over the years the child was aged 15 to 19. The rank-rank slope is 0.23 when parental income is measured in 2001, in line with prior estimates for Canada (Connolly and Haeck, 2024; Haeck and Laliberté, 2025). A slope of this magnitude places Canada among the most mobile of rich countries, against roughly 0.30 in the

United Kingdom and 0.34 in the United States. The relationship departs from linearity in the tails, steepening below the 20th percentile of parental rank and again at the very top, a pattern that appears in many other developed countries, including the United States (Chetty et al., 2014b), Australia (Deutscher and Mazumder, 2020), and France (Kenedi and Sirugue, 2023). The two sets of markers are also nearly indistinguishable. The slope is 0.24 under the conventional measure, which answers the measurement question left open above. Little is lost by measuring parental income in a single pre-shock year.⁹

Figure 2(b) narrows the focus to manufacturing families, defined as families in which at least one parent worked in manufacturing in 2001. Parents are divided into ventiles of the 2001 parental rank distribution, and the figure plots the share of families in each ventile with a parent in manufacturing. That share rises steadily through the lower and middle ranks, forms a plateau of roughly 23% between the 60th and 80th percentiles, and then declines through the top. The bottom is nearly empty, with only 0.2% of families in the lowest ventile having a parent in manufacturing, against 4.7% in the next. The families this paper studies are therefore concentrated in the middle and upper middle of the Canadian income distribution.

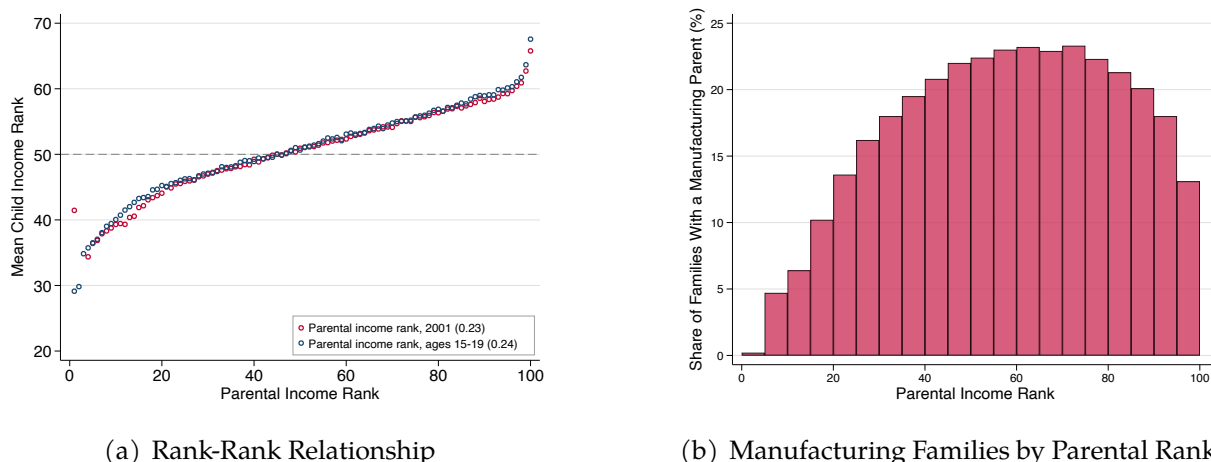


Figure 2: Intergenerational Mobility and the Composition of Manufacturing Families

Notes: Panel (a) plots mean child income rank against parental income rank for all families in the estimation sample. Red markers measure parental income in 2001; blue markers use the average of parental income over the years the child was aged 15 to 19. Numbers in parentheses in the legend are the unconditional rank-rank slopes. The dashed horizontal line marks a child income rank of 50. Panel (b) shows the share of manufacturing families within each ventile of the 2001 parental income rank distribution, expressed as a proportion. A manufacturing family is one in which at least one parent worked in manufacturing in 2001.

⁹Two further checks point the same way. For manufacturing families, the sample this paper studies, the correlation between the two measures is 0.87, with $R^2 > 0.76$. I also restrict the sample to families in which both parents are at least 40 years old in 2001, an age range over which income profiles are comparatively flat, and the estimated effect of the trade shock on income mobility is very similar.

3 Intergenerational impacts of the China Shock

This section presents the identification strategy and empirical findings in six parts. First, I establish the direct impact of the China Shock on parents. Second, I estimate its impact on the intergenerational income mobility of manufacturing families. Third, I ask which of the two generations moved, and by how much. Fourth, I show how this impact varies with the parent's gender, the child's gender, and the family's immigrant background. Fifth, I examine whether the shock helped children escape the bottom of the income distribution. Finally, I report the robustness checks.

3.1 The impact on parents

To establish the first-order effects of the China Shock on parents, I begin by estimating its impact on cumulative parental income over the decade from 2002 to 2011. This measures the total economic resources available to the family over the decade in which the child grew up. I estimate the following specification by two-stage least squares:

$$\tilde{E}_{ij}^p = \beta_0 + \beta_1 \Delta IP_{ij} + W_i' \beta_2 + \gamma_{yob} + \gamma_s + \gamma_{cd} + \varepsilon_i \quad (4)$$

where, for child i whose parents worked in industry j in 2001, $\tilde{E}_{ij}^p = \sum_{t=2002}^{2011} E_{ijt}^p / E_{ij,2001}^p$ is cumulative parental income over 2002 to 2011, normalized by parental income in 2001. Following [Autor et al. \(2014\)](#), this normalization captures long-run parental income relative to its pre-shock level. A family whose income remained at its 2001 level throughout the decade would have $\tilde{E}_{ij}^p = 10$. To limit the influence of very small baseline incomes, I winsorize 2001 parental income at the bottom 0.5 percentile before constructing the ratio. The key explanatory variable, ΔIP_{ij} , is the cumulative growth in Chinese import penetration in industry j from 2002 to 2011, defined in Equation (1). When both parents worked in manufacturing, ΔIP_{ij} is the average of the shocks to the two parents' industries. The corresponding instrument is constructed as in Equation (2).

The vector of control variables W_i contains an indicator for whether at least one parent is an immigrant, an indicator for the child's gender, and log parental income in 2001. I include three sets of fixed effects. The parents' birth cohort effects, γ_{yob} , absorb the stage of the income life cycle at which parents entered the decade after the trade shock. The three-digit NAICS sector effects, γ_s , absorb time-invariant sector characteristics measured at a coarser level than the treatment, such as skill requirements that shape long-run income growth. The Census Division effects, γ_{cd} , absorb time-invariant local labor market conditions. For families in which both parents worked in manufacturing, both the sector

and the birth cohort fixed effects are assigned from the father, since the father was the higher-earning manufacturing parent in the large majority of these families.

The fixed effects narrow the comparison to families with a similar economic background before the shock, living in the same Census Division, with parents in the same three-digit manufacturing sector, who differ only in the trade exposure of their four-digit industry. Common shocks to a region or to a broad sector are therefore differenced out, and what remains is variation in exposure across closely related industries. Industries within the same three-digit sector may nonetheless have been on different trajectories for reasons unrelated to Chinese competition. To address this, I include three industry-level controls measured at the four-digit level. To capture lasting effects of NAFTA, I control for U.S. import penetration in 2001, measured as in Equation (1) with the numerator replaced by imports from the United States. Using the 2001 Census microdata, I also control for the share of workers with a high school education and the log number of workers aged 16 to 65. These two factors are correlated with exposure to Chinese imports, since low-skill and labor-intensive industries were the most affected, and both plausibly shape children's outcomes on their own, through the local employment base and through the transmission of skills within the family. Standard errors are clustered at the Census Division level to account for spatial correlation in the error terms.

Table 1 presents the impact of the China Shock on cumulative parental income. Column (1) is estimated by OLS and the others by 2SLS, instrumenting the change in import penetration in Equation (1) with China's exports to other high-income countries as defined in Equation (2). The OLS estimate is positive and statistically insignificant, while the 2SLS estimate is negative and statistically significant. This contrast is consistent with the endogeneity concern raised in Section 2.2: domestic demand shocks could raise imports and parental income at the same time, biasing OLS upward. The instrument isolates the supply-driven component of the shock, and the 2SLS estimate shows that trade exposure reduces cumulative parental income over the decade that followed.

Columns (3) through (5) vary the set of fixed effects, holding the demographic controls and three-digit sector effects fixed throughout. Column (3) replaces the parents' birth-year effects with Province $\times$ parents' birth-year effects, absorbing province-specific trends that vary across parental cohorts. Column (4) instead replaces the Census Division and birth-year effects with their interaction, absorbing the same trends at a finer geographic level. Column (5) adds the four-digit industry characteristics described above to the specification in Column (3). The point estimate is stable at roughly -0.65 across Columns (3) and (4), and attenuates to -0.502 in Column (5), where it remains statistically significant.

To interpret the coefficient estimates, I compare manufacturing families that experi-

enced different levels of trade exposure. As explained above, trade exposure is assigned at the family level, averaging across both parents' industries where both worked in manufacturing. A family-level trade exposure of 0.1 corresponds to an increase in import penetration equal to 10 percent of the industry's year 2000 domestic absorption. To make such a number concrete, I anchor each level of trade exposure to a manufacturing industry that faced comparable import competition between 2002 and 2011. I define a mild-case family as one facing exposure equal to the average across manufacturing families in the sample, roughly the increase in import penetration experienced by the wire and spring products industry. A severe-case family faces exposure about one standard deviation higher, comparable to leather products. An extreme-case family faces exposure about 2.7 standard deviations above the average, among the most exposed of all and comparable to apparel knitting mills. The same three industries anchor the benchmarks used later for parental income rank and for child income mobility.

Reading Column (5), manufacturing families more exposed to the shock earned substantially less over 2002 to 2011. Relative to a mild-case family, a severe-case family lost the equivalent of about 15% of one year of its 2001 income over the full decade, or roughly \$15,000 at the average 2001 parental income of \$100,000. For an extreme-case family, the cumulative loss reached about 35% of one year of baseline income, or roughly \$35,000.

3.2 The impact on intergenerational income mobility

I now turn to the central question of the paper, whether the China Shock alters the intergenerational income mobility of manufacturing families. Building on the rank-rank framework, I estimate the following specification by two-stage least squares:

$$\begin{aligned} \text{rank}_i^c = & \beta_0 + \beta_1 \Delta IP_{ij\tau} + \beta_2 \text{rank}_i^p + \beta_3 (\Delta IP_{ij\tau} \times \text{rank}_i^p) \\ & + W_i' \beta_4 + \gamma_b + \gamma_s + \gamma_{cd} + \varepsilon_i \end{aligned} \quad (5)$$

For child i whose parents worked in industry j in 2001, the outcome rank_i^c is their own income rank at ages 27 to 31, and rank_i^p is their parental income rank in 2001, both defined in Section 2. The trade exposure variable $\Delta IP_{ij\tau}$ is defined in Equation (1) and measured over the five years when the child was aged 15 to 19, so that treatment timing varies across birth cohorts. I recenter $\Delta IP_{ij\tau}$ on its birth cohort mean, so that zero denotes a family at the average level of exposure within its own cohort. The corresponding instrument is constructed as in Equation (2) over the same window, and both $\Delta IP_{ij\tau}$ and its interaction with rank_i^p are instrumented. The term ε_i is the residual.

I include the same controls and fixed effects as in Equation (4), with three changes:

Table 1: Effect of Trade Exposure on Cumulative Parental Income

	(1) OLS	(2)	(3) 2SLS	(4)	(5)
Cumulative Trade Shock	0.116 (0.287)	-0.673** (0.293)	-0.656** (0.296)	-0.641** (0.289)	-0.502* (0.289)
Demographics	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
CD FE	Yes	Yes	Yes		Yes
Parent YoB FE	Yes	Yes			
Province $\times$ Parent YoB FE			Yes		Yes
CD $\times$ Parent YoB FE				Yes	
Industry Characteristics					Yes
First-stage F-stat		291.3	227.1	232.8	226.8
R^2	0.07	0.06	0.06	0.06	0.06

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. The table reports estimates of Equation (4). Column (1) reports OLS estimates; Columns (2) to (5) report 2SLS estimates. Demographics include parents' immigrant status, the child's gender, and log parental income in 2001. Sector fixed effects are defined at the 3-digit NAICS level of the parents' 2001 manufacturing industry. Industry Characteristics include U.S. import penetration, the log number of workers aged 16 to 65, and the share of workers with a high school education, all measured at the 4-digit NAICS level in 2001. Because the dependent variable is measured for parents, fixed effects use the parent's year of birth, denoted "Parent YoB".

log parental income in 2001 is dropped, the parents' birth cohort effects are replaced by the child's birth cohort effects γ_b , and the baseline four-digit industry characteristics are interacted with the child's birth cohort dummies, since treatment timing differs across cohorts. Standard errors are clustered at the Census Division level.

This specification has two coefficients of interest. The first is β_2 . For manufacturing families at the average level of trade exposure, it gives the rank-rank persistence across generations, namely relative income mobility, conditional on the controls and fixed effects. The second, and the one central to this paper, is the interaction term β_3 . A negative β_3 means that the shock weakens the link between parental rank and child rank and therefore raises relative mobility. The same coefficient can also be read as the slope of the marginal effect of the shock with respect to family background. The marginal effect at parental rank rank_i^p is $\beta_1 + \beta_3 \cdot \text{rank}_i^p$, so a negative β_3 means the effect of the shock is less favorable for children from richer families. Because parental rank runs from 1 to 100, β_1 alone is the marginal effect at a parental rank of zero, a point outside the support of the data. I therefore report the marginal effect at points within the observed range rather than interpreting β_1 on its own.

Throughout this section and the rest of the paper, trade exposure is measured over the five-year window when children are aged 15 to 19 and is recentered on its birth cohort mean, so that zero denotes a family at the average level of exposure in its child’s birth cohort. One standard deviation of this recentered family-level measure is 0.136, equivalent to 13.6 percentage points of an industry’s year 2000 domestic absorption. A mild-case family sits at the cohort average, a severe-case family one standard deviation above it, and an extreme-case family 2.7 standard deviations above it, among the most exposed in the sample. I anchor each benchmark to a manufacturing industry facing comparable import competition, averaging the industry’s recentered exposure across the 1986 to 1992 cohorts: wire and spring products for the mild case, leather products for the severe case, and apparel knitting mills for the extreme case. The benchmarks describe distances from the cohort average rather than absolute levels of import penetration, and because exposure is assigned at the family level and averaged across both parents when both worked in manufacturing, a family at a given benchmark need not work in the anchoring industry.¹⁰

Panel A in Table 2 reports estimates of Equation (5). Column (1) is estimated by OLS and Columns (2) through (6) by 2SLS. Columns (1) through (5) include the interaction term and vary the geographic and cohort controls. I discuss the results in Column (6) in Section 3.3.

First, the coefficient on Parental Income Rank is 0.233 and is stable across Columns (1) through (5). Because trade exposure is recentered on its cohort-specific mean, this coefficient is the rank-rank slope at the cohort average level of exposure: among manufacturing families at that level, a child whose parents ranked 10 percentiles higher in 2001 ranks on average 2.3 percentiles higher at ages 27 to 31. This is close to the rank-rank persistence of 0.245 estimated for the 1985 birth cohort in Canada as a whole (Connolly and Haeck, 2024). My estimate is slightly lower, implying slightly higher relative mobility of manufacturing families after conditioning on the controls.

Second, the interaction between Trade Shock and Parental Income Rank, β_3 , is the central estimate of this paper. It is -0.126 in Column (5), negative and statistically significant, which shows that import competition flattens the rank-rank slope between children and their parents and therefore raises relative mobility. Relative to a mild-case family, the rank-rank slope falls by 0.017 for a severe-case family and by 0.046 for an extreme-case family, declines of 7% and 20% against the baseline persistence of 0.233.

To interpret the magnitude of this change, I turn to the national trend in income mobility in Canada as a yardstick. Connolly and Haeck (2024) show that rank-rank persistence

¹⁰Although trade exposure is measured over different time windows in Section 3.1 and in this section, the benchmark industries are the same, because the ranking of industries by exposure is stable over time.

in Canada drifted from 0.233 to 0.245 between the 1975 and 1985 birth cohorts, a rise of more than 5% over a decade, in the direction of declining relative mobility. That figure describes the national distribution, averaging across families the shock reached and families it did not, whereas mine is an effect within manufacturing families; the comparison is one of scale rather than of like with like. On that scale, the decline I estimate for extreme-case families moves persistence nearly four times as far as a decade of national drift, and in the opposite direction. Movements of this size within an exposed group leave little trace in national averages, which pool exposed and unexposed families together.

3.3 Connecting the impact on children and parents

The results in Section 3.1 show that the trade shock reduces cumulative parental income, and the results in Section 3.2 show how it reshuffled children relative to the families they came from. Neither says how far the two generations moved relative to each other. A flatter rank-rank slope can arise because children from poorer families climbed toward the positions their parents once held, or because families higher up slid down toward the poorer ones. Separating the two requires putting both generations on the same scale, and percentile ranks provide one. I therefore estimate Equation (5) a second time, replacing the child's own income rank with the parental income rank measured over the years the child was aged 15 to 19, and leaving the regressors, instruments, controls, and fixed effects unchanged. The two versions differ only in whose rank appears on the left-hand side, so the shock is allowed to reshuffle each generation on the same terms and the two estimates of β_3 are directly comparable. Throughout this section, the mild, severe, and extreme benchmarks are as defined in Section 3.2, computed from the same distribution of exposure over the five-year window when children are aged 15 to 19.

The impact on parental income rank by family background Panel B in Table 2 reports the estimated effects of the trade shock on parental rank. Column (1) is estimated by OLS and Columns (2) through (6) by 2SLS. Columns (1) through (5) include the interaction term and vary the geographic and cohort controls. Column (6) drops the interaction, so that the coefficient on trade exposure gives the average effect on parental rank across all manufacturing families, without distinguishing families of different backgrounds.

Several results stand out from Panel B of Table 2. First, the coefficient on parental rank, which gives the relationship between a family's baseline economic position in 2001 and its position when the child was aged 15 to 19 for families experiencing the average level of trade exposure, is stable at 0.86 across Columns (1) through (5). A family whose income

Table 2: Effect of Trade Shock on Intergenerational Income Mobility

	(1) OLS	(2)	(3)	(4) 2SLS	(5)	(6)
<i>Panel A. Child Income Rank</i>						
Trade Shock	4.289*** (1.450)	7.676*** (1.780)	7.868*** (1.752)	7.420*** (1.797)	8.822*** (1.714)	-0.519 (0.578)
Parental Income Rank	0.233*** (0.009)	0.233*** (0.009)	0.233*** (0.009)	0.233*** (0.008)	0.232*** (0.009)	0.233*** (0.009)
Trade Shock $\times$ Parental Income Rank	-0.074*** (0.022)	-0.119*** (0.027)	-0.121*** (0.026)	-0.117*** (0.027)	-0.126*** (0.026)	
First-stage F-stat – Trade Shock		3095.32	3099.27	3126.88	3517.29	79.49
First-stage F-stat – Interaction		1642.92	1658.45	1874.97	1302.72	
R^2	0.08	0.06	0.06	0.06	0.06	0.06
<i>Panel B. Parental Income Rank, Measured at Child Ages 15–19</i>						
Trade Shock	-0.125 (0.764)	1.071 (1.174)	0.896 (1.123)	1.034 (1.090)	1.937* (1.087)	-2.227*** (0.457)
Parental Income Rank	0.862*** (0.006)	0.862*** (0.006)	0.862*** (0.006)	0.862*** (0.006)	0.859*** (0.006)	0.862*** (0.006)
Trade Shock $\times$ Parental Income Rank	-0.014 (0.010)	-0.050*** (0.019)	-0.049*** (0.018)	-0.048*** (0.018)	-0.051*** (0.017)	
First-stage F-stat – Trade Shock		3372.79	3374.11	3413.15	3654.57	70.39
First-stage F-stat – Interaction		1792.7	1802.24	2038.12	1429.09	
R^2	0.74	0.68	0.68	0.68	0.68	0.68
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
CD FE	Yes	Yes	Yes		Yes	
Child YoB FE	Yes	Yes				
Province $\times$ Child YoB FE			Yes		Yes	
CD $\times$ Child YoB FE				Yes		Yes
Industry Characteristics $\times$ Child YoB					Yes	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. The table reports estimates of Equation (5). Panel A uses the child's income rank as the outcome; Panel B replaces it with parental income rank measured when children were aged 15 to 19. Column (1) reports OLS estimates; Columns (2) to (6) report 2SLS estimates. Column (5) is the preferred specification. Column (6) omits the interaction term and reports the average effect of trade exposure. Demographics include parents' immigrant status and the child's gender. Sector fixed effects are defined at the 3-digit NAICS level of the parents' 2001 manufacturing industry, and "YoB" denotes the child's year of birth. Industry Characteristics include U.S. import penetration, the log number of workers aged 16 to 65, and the share of workers with a high school education, all measured at the 4-digit NAICS level in 2001, each interacted with child year-of-birth dummies. The control sets listed at the bottom apply to both panels.

rank was one percentile higher in 2001 stands on average 0.86 percentiles higher during the child's adolescence. Family position is thus highly persistent for mild-case families.

Second, the shock moves manufacturing families down the income distribution on average. Column (6) drops the interaction and estimates this average effect at -2.227. Because exposure enters linearly, the effect scales with the level of exposure a family faced: relative to a mild-case family, a severe-case family falls about 0.3 percentile ranks over the child's adolescence and an extreme-case family about 0.8.

Third, this average result in Column (6) describes families only by how heavily they were hit, not by where they started. Column (5), my preferred specification, allows the effect to vary with baseline position. The marginal effect of the shock is $\beta_1 + \beta_3 \cdot \text{rank}_i^p$, with $\hat{\beta}_1 = 1.937$ and $\hat{\beta}_3 = -0.051$, so it declines as the baseline parental rank (rank_i^p) rises and changes sign near the 38th percentile. Families below this point move up relative to comparable but less exposed families, and families above it move down. Holding the baseline parental rank fixed and comparing an extreme-case family with a mild-case family, a family at the 10th percentile gains 0.52 percentile ranks, while a family at the 90th percentile loses 0.96. As Figure 2(b) shows, families above the 38th percentile are more numerous in the sample, so the average effect in Column (6) remains negative.

Since a percentile is worth different amounts at different points in the distribution, the gap in dollar terms is wider than the gap in ranks. I use a back-of-the-envelope calculation to translate these rank movements into income levels. Each rank movement is converted at the slope of the parental income distribution around the position these families reached during the child's adolescence, and the resulting annual amount is accumulated over the remainder of the parents' working lives. Parents at the 10th percentile of the 2001 distribution were on average 40 years old, leaving 25 years to retirement at age 65, while parents at the 90th percentile were on average 43, leaving 22 years. Holding the baseline parental rank fixed, comparing an extreme-case family with a mild-case family, and discounting at 3% per year, the gain of 0.52 percentile ranks at the 10th percentile is worth about \$15,176 in present value, while the loss of 0.96 percentile ranks at the 90th percentile is worth about \$64,950. The loss at the top is more than four times the gain at the bottom.¹¹

The impact on child income rank by family background I now return to the children and read Panel A in Table 2 in the same way, starting from the average effect, since the

¹¹The calculation discounts a constant annual amount over the remaining working life at an annual rate of 3%, with payments treated as arriving at the beginning of each year. It rests on three assumptions: that the change in relative position persists to retirement, that the shape of the income distribution remains as it was during the child's adolescence, and that the annual dollar value of a rank movement does not vary over the remaining working life. It is intended to convey the magnitude of the estimated rank movements rather than to serve as a welfare calculation.

baseline rank-rank coefficient was discussed in Section 3.2. Column (6) drops the interaction term, so the coefficient on trade exposure gives the average effect on child rank across manufacturing families. That coefficient is -0.519 and statistically insignificant: relative to a mild-case family, a child from an extreme-case family ends up 0.19 percentile ranks lower.

Column (5), my preferred specification, allows the effect to vary with baseline position. With $\beta_1 > 0$ and $\beta_3 < 0$, the marginal effect of the shock on child rank is positive at the bottom, crosses zero at the 70th percentile, and turns negative above it, so the shock is less favorable for children from richer families than for children from poorer ones. Holding the baseline parental rank fixed and comparing an extreme-case family with a mild-case family, a child from the 10th percentile gains 2.74 percentile ranks, while a child from the 90th percentile loses 0.91.

A simple back-of-the-envelope calculation translates these rank changes into income levels. Each rank change is converted at the slope of the child income distribution around the average adult position reached by children from that background, and the resulting annual amount is accumulated over the remainder of the children's working lives. Unlike their parents, whose ages differ by three years between the 10th and the 90th percentile of the 2001 distribution, children from poor and rich backgrounds differ in age only negligibly. I take the children to be 29 years old and working until they retire at 65, and discount at 3% per year. On the same extreme-to-mild comparison, the gain of 2.74 percentile ranks for a child from a 10th percentile family background is worth about \$40,230 in present value, while the loss of 0.91 percentile ranks for a child from a 90th percentile background is worth about \$14,852. For children from families at the bottom, \$40,230 is close to a full year of average individual income in 2001.¹²

Placing the two sets of estimates for parents and children side by side answers the question this section opened with. The interaction coefficient is -0.051 for parents and -0.126 for children, so the shock reshuffles the child generation about two and a half times as strongly as it reshuffles their parents relative to the same measure of family background. The point at which the marginal effect changes sign also differs, at the 38th percentile for parents and the 70th for children, so the range of families whose position improved relative to less exposed families is considerably wider among children than among parents. The difference is concentrated at the bottom. At the 10th percentile the parents gain 0.52 percentile ranks and their children 2.74, whereas at the 90th percentile the parents lose 0.96 and their children 0.91.

Columns (5) and (6) of both panels in Table 2 describe one movement. Children from

¹²The same assumptions apply as for the parental rank change and its implied change in income levels.

poorer manufacturing families move up and children from richer ones move down, and it is because these two movements run in opposite directions that the average effect in Column (6) is indistinguishable from zero while the rank-rank slope falls by a fifth for extreme-case families.

3.4 Heterogeneous effects

I examine the heterogeneous effects of the trade shock on income mobility along three dimensions. I first reassign trade exposure using only the mother's industry and, separately, only the father's industry. I then split the sample by the child's gender and by whether the child has at least one immigrant parent. Figure 3 presents these results, with each set of splits in a different color. Figure 3(a) reports the coefficient on Parental Income Rank, which recovers the rank-rank slope between children and parents for manufacturing families at the cohort-specific average level of trade exposure. Figure 3(b) reports the coefficient on the interaction between Trade Shock and Parental Income Rank, which recovers how the rank-rank slope changes with trade exposure, where a negative value means that the trade shock raises mobility. Full results are in Appendix Table A1.

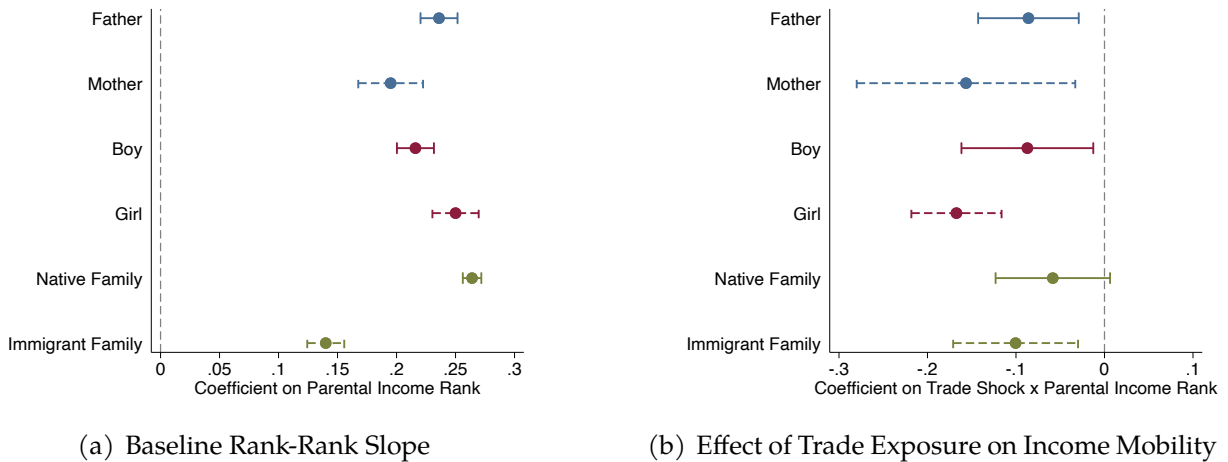


Figure 3: Heterogeneous Effects of Import Competition on Mobility

Notes: Each marker plots a coefficient from Equation (5), estimated separately by subgroup using the specification in Column (5) of Table 2. Horizontal bars denote 95% confidence intervals. Panel (a) plots the coefficient on Parental Income Rank and Panel (b) plots the coefficient on Trade Shock $\times$ Parental Income Rank. In the first pair of estimates, trade exposure is assigned from the father's or the mother's industry alone. The second pair splits the sample by the child's gender. The third pair distinguishes children with at least one immigrant parent from children of native-born parents.

Mothers' and fathers' industries I first examine whether the effect varies by which parent was employed in manufacturing in 2001. The baseline specification averages exposure across both parents when both worked in manufacturing. Here I instead assign exposure from the father's industry alone, within families in which the father worked in manufacturing, and separately from the mother's industry alone, within families in which the mother worked in manufacturing. The two subsamples overlap, because families in which both parents worked in manufacturing enter both. The contrast therefore reflects both the parent whose industry carries the shock and the different composition of the two subsamples.

The baseline rank-rank slope is lower when exposure is assigned from the mother's industry, at 0.195, than from the father's, at 0.236. Among mild-case families, then, a child's income rank is more tightly tied to their parental rank when exposure runs through the father's industry than when it runs through the mother's, that is, relative mobility is lower. The interaction coefficient is -0.156 for mothers and -0.086 for fathers, so the shock flattens that slope by nearly twice as much when it reaches the family through the mother's industry. Scaling each estimate by the difference in exposure between an extreme-case and a mild-case family, and expressing the result relative to each group's own baseline slope, persistence falls by 30% when exposure runs through the mother's industry and by 13% when it runs through the father's.

This pattern is consistent with pronounced gender segregation within Canadian manufacturing in 2001 in my data, where female workers were concentrated in textiles and apparel, and male workers were concentrated in machinery and fabricated metals. Textiles and apparel contracted sharply after China's WTO accession, leaving little room for reemployment within the sector, whereas the industries employing fathers contracted less. A given increase in exposure may therefore close off the child's path into the parental industry more completely when that industry is the mother's, and it is this closing off that raises mobility, as Section 4 discusses.

Sons and daughters The path into the parental industry is not equally open to sons and daughters. Grenet et al. (2024) show that sons are more likely than daughters to enter their father's industry, and the same holds in my data. Among children who follow their parents into the same three-digit industry, the large majority are sons.¹³ In a sector where women made up less than 30% of the workforce in 2001, that path is the one most exposed to the shock. If closing it off is what raises mobility, the effect should differ by the child's gender.

¹³Section 4.2.1 describes how I measure whether a child works in their parents' industry.

Figure 3 shows that the rank-rank slope is lower for sons, at 0.216, than for daughters, at 0.250, so within mild-case families a daughter's income rank depends more on her parental rank than a son's does. The interaction coefficient is -0.167 for daughters and -0.087 for sons, so persistence falls by 24% for daughters and by 15% for sons between a mild-case and an extreme-case family.

The pattern in the point estimates points toward the mechanism I take up below. Sons are more likely than daughters to enter their father's industry, so sons may hold a stronger prior attachment to manufacturing and remain more likely to follow that path even after the shock. Daughters' weaker prior attachment leaves them freer to reallocate to other industries, which is consistent with their larger point estimate.

Immigrant and native-born families Finally, I compare children with at least one immigrant parent to children of native-born parents. Immigrant background matters for mobility; the mobility literature documents that second-generation immigrants are the more mobile group in Canada (Aydemir, Chen and Corak, 2009), as they are in the United States (Abramitzky et al., 2021), Norway (Hermansen, 2016), and Sweden (Bratu and Bolotnyy, 2023). I investigate whether this pattern holds after the trade shock.

Figure 3 shows that the baseline persistence in immigrant families is 0.140, barely half the 0.264 in native-born families, consistent with the literature above. The interaction coefficient is -0.100 for immigrant families and -0.058 for native-born families, so persistence falls by 26% and by 8% respectively between a mild-case and an extreme-case family.

The larger response of children from immigrant families again appears in the group with the weaker prior tie to the parental industry. Compared with immigrant children, native-born children may have more industry-specific skill and social networks to inherit, which would anchor them to their parents' sector and leave their mobility less changed by the shock.

Children's attachment to the parental industry Although the three splits are estimated on different samples, together they provide suggestive evidence that the larger impact of the trade shock on mobility appears in the group with the weaker prior attachment to the parental industry. A similar mechanism operates in the United States. Kahn, Oldenski and Park (2026) find that the China Shock narrowed Black-white employment and wage gaps, in part because Black workers were less attached to manufacturing than white workers by 2000 and were therefore better placed to move into the sectors the shock expanded. If attachment is the operative margin here, it should be visible in where children work. Section 4 explores whether exposed children leave the parental industry and what kind

of industries they move into.

3.5 Escaping the bottom quintile

The analysis so far has shown the impact of the trade shock on relative mobility between children and their parents. Another important margin is upward mobility, whether the trade shock changed the probability that a child born into the bottom quintile of the parental income distribution remained there as an adult. I therefore restrict the sample to children whose families were in the bottom quintile of the 2001 parental income distribution, and ask whether they are also in the bottom quintile of their own cohort's income distribution in adulthood. Within this sample parental income rank varies only within a narrow band, so I estimate a version of Equation (5) that omits parental rank and its interaction with trade exposure. All columns in Table 3 report two-stage least squares estimates of a linear probability model, adding controls progressively across columns.

The coefficient is close to zero across Columns (1) through (3) of Table 3. It turns negative and statistically significant in Column (4), which adds the baseline industry characteristics interacted with birth cohort. These characteristics could be correlated with trade exposure, since low-skill and labor-intensive industries were highly exposed, and they also bear on whether a child remains at the bottom, so omitting them biases the estimate toward zero. Column (4) is therefore the preferred specification here. Reading Column (4), moving from a mild-case to an extreme-case family lowers the probability of remaining in the bottom quintile by 3.7 percentage points, a 12.9% reduction relative to the baseline probability of 28.7%.

In Section 3.3, I show that when an extreme-case family and a mild-case family start at the same 10th percentile, the child from the extreme-case family ends up 2.74 percentile ranks higher. The bottom-quintile results describe that gain from a different angle, and they show that a shift of this size is enough to carry a meaningful share of these children over the boundary of the bottom quintile and move upward along the economic ladder.

3.6 Robustness checks

I subject the main results on the impact of the trade shock on income mobility to seven robustness checks, reported in Appendix D. First, to rule out that industries hit by import competition were already on different mobility trajectories, I conduct a placebo test that assigns the shock to an older cohort born between 1963 and 1967, whose adolescence and entry into the labor market predate China's WTO accession. Second, to separate the shock from the global financial crisis, I restrict the sample to cohorts whose exposure windows

Table 3: Effect of Trade Shock on the Probability of Remaining in the Bottom Quintile

	(1)	(2)	(3)	(4)
	Children Remaining in the Bottom Quintile			
Trade Shock	-0.030 (0.043)	-0.029 (0.045)	-0.029 (0.046)	-0.102** (0.050)
Demographics	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
CD FE	Yes	Yes		Yes
Child YoB FE	Yes			
Province $\times$ Child YoB FE		Yes		Yes
CD $\times$ Child YoB FE			Yes	
Industry Characteristics $\times$ Child YoB				Yes
R^2	0.01	0.01	0.01	0.01

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. The sample is restricted to children whose families were in the bottom quintile of the 2001 parental income distribution. All Columns report 2SLS estimates. Column (4) is the preferred specification. All variables, instruments, and control sets are defined as in Table 2.

Table 4: Effect of Trade Shock by Parental Income Quartile

	(1) 1st Quartile	(2) 2nd Quartile	(3) 3rd Quartile	(4) 4th Quartile
Trade Shock	8.573 (7.803)	13.235** (6.264)	3.321 (9.137)	1.267 (10.040)
Parental Income Rank	0.231*** (0.065)	0.240*** (0.019)	0.229*** (0.012)	0.304*** (0.019)
Trade Shock $\times$ Parental Income Rank	-0.107 (0.370)	-0.299* (0.164)	-0.057 (0.143)	-0.039 (0.114)
R^2	0.03	0.04	0.05	0.03

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. Each column reports 2SLS estimates of Equation (5) within one quartile of the 2001 parental income distribution, using the specification in Column (5) of Table 2. All variables, instruments, and control sets are defined as in Table 2.

close before 2008. Third, I measure child income over an earlier age window, ages 25 to 29. Fourth, to address the concern that the surge in Chinese imports to the comparison economies reflected demand shocks correlated with Canada's, I expand the comparison group beyond the original six countries. Fifth, I restrict the sample to families in which both parents were at least 40 in 2001, since income could be more variable earlier in the life cycle. Sixth, I cluster standard errors at the sector level rather than the Census Division level. Seventh, to rule out the Western Canadian resource boom as the source of the mobility gains, I exclude two provinces, Alberta and Saskatchewan. The placebo test cannot distinguish the coefficient from zero, which is what the identifying assumption predicts; across the other six checks, the coefficient keeps its sign, magnitude, and significance.

4 Mechanism

I find that the China Shock increases intergenerational income mobility among manufacturing families. This section asks why, and whether the channels involved are particular to import competition in Canadian manufacturing or generalizable to other settings. First, I examine which families account for the aggregate increase in mobility. Second, I ask whether the increase arises from sectoral reallocation, with exposed children working outside their parents' industry rather than following them into it, and I characterize the industries they move into. Third, I evaluate whether the trade shock changes the skills these children brought to the labor market or the wage premiums of the firms that employed them. The evidence points to the second and third of these. The destinations children reach after leaving the parental industry, and the firm premiums attached to them, offer the best explanation for the rise in mobility. Both of these channels, the loosening of industry inheritance and the reallocation of children across firms paying different premiums, operate whenever a parental sector declines. What is more specific to this setting is the position of manufacturing in the Canadian income structure, and hence what a child forgoes by leaving it. In Appendix C, I investigate three alternative accounts: geographic reallocation, the transmission of general human capital, and the role of the parent who did not work in manufacturing.

4.1 Mobility effects by family background

Having established that the trade shock raises the income mobility of manufacturing families, I next ask which families account for this aggregate increase. I re-estimate Equation (5) separately within each quartile of the 2001 parental income distribution. Table 4

reports the results. The estimated coefficient on the interaction between Trade Shock and Parental Income Rank is negative in every quartile. Only in the second quartile is it statistically significant, at the 10% level, and its magnitude there is large, at -0.299. For the top half of the distribution the estimates are close to zero. For the bottom quartile the estimate is larger than those in the top half but is far too imprecise to interpret, since manufacturing families are scarce at the bottom of the income distribution, as Figure 2(b) shows.

The aggregate increase in mobility is therefore not a uniform lift. The only quartile in which it is measurable is the lower middle of the distribution. One reading of this concentrated effect is that the parental sector offers these families a position worth inheriting, yet they lack the buffers that protect families at the top from labor market disruptions. The next section traces two behavioral responses, whether children leave the parental industry and what kind of industry they move into.

4.2 Career choices of children

Existing work documents strong connections between children’s careers and their parents’, with children frequently working in the same industry or occupation, drawing on contacts and industry-specific experience passed down from their parents (Hellerstein and Morrill, 2011; Lo Bello and Morchio, 2022). If the trade shock raises income mobility, a natural place to look is whether it loosens these links. I therefore ask whether exposed children still work in a parent’s industry, and how this choice varies with family background.

4.2.1 Leaving the parental industry

I track each child’s industry of employment annually between ages 27 and 31 and define the modal industry as the one in which the child worked in the largest number of years with employment records, breaking ties in favor of the industry held at the older age. Industry is measured at the three-digit level, which is fine enough to distinguish the sectoral characteristics that matter for the analysis below while remaining broad enough that ordinary job changes within a line of work are not counted as exits.

I construct two binary outcomes. The first, P_{ia} , equals one if child i at age a is employed in the same three-digit industry in which a parent worked during the 2001 baseline, and zero otherwise. The second, P_i , equals one if child i ’s modal industry is that same industry, and zero otherwise. For dual-manufacturing households, I assign the father’s 2001 industry, following the rule used for sector fixed effects in the main specification.

Industry inheritance is not evenly distributed across family backgrounds. Figure 4(a) plots, based on children’s modal industry, the share of children working in a parent’s 2001

industry against parental income rank. The share rises steadily through the distribution, from about 2% among children of the lowest-ranked families to about 7% at the very top. Children from richer manufacturing families are therefore the ones with the most to inherit, and the estimates below show that they are also the ones the shock displaces least.

Table 5 reports estimates of Equation (5) with P_{ia} as the outcome in Columns (1) through (5) and P_i in Column (6). Two results stand out. First, the shock pushes children out of their parental industry. Holding family background at the 50th percentile, the marginal effect of the trade shock is $\hat{\beta}_1 + \hat{\beta}_3 \times 50$. Both coefficients keep the same sign across every column, and the modal-industry estimate lies in the middle of the age-specific range, so it summarizes the age profile. In that column the marginal effect is -0.013. Relative to a mild-case family, a child from an extreme-case family is 0.48 percentage points less likely to work in a parent's industry. Against a baseline in which 10% of Canadian 17-to-19-year-olds employed in 2001 worked in the same three-digit manufacturing industry as a parent, that is a reduction of roughly 5%.¹⁴

Second, the effect runs in opposite directions for children from poorer and richer families. The coefficient on Trade Shock $\times$ Parental Income Rank is positive at every age, so the push out of the parental industry weakens as parental rank rises. In the modal-industry column the marginal effect, $\hat{\beta}_1 + \hat{\beta}_3 \times \text{rank}$, turns positive above the 77th percentile. The same shock that pushes poorer children out of the parental industry makes the richest children more likely to stay in it. Comparing an extreme-case family with a mild-case family, a child from a family at the 90th percentile is 1.4 percentage points more likely to stay than a child from a family at the 10th, a difference of 14% against the same baseline.

Taken together, the shock loosens the tie between parents' and children's industries, but it does so unevenly: children from lower-income families are more likely to leave the parental sector, while children from higher-income families are more likely to stay.

4.2.2 Industry characteristics

The trade shock pushes children out of their parents' industries. The next question is what kind of industry they move into. I characterize industries along two dimensions, both estimated on the national sample of children born between 1986 and 1992 and their parents, rather than on manufacturing families alone.

¹⁴The ideal baseline would be the share of children in the same age range as my sample who worked in a parent's industry before the shock. That quantity is not available in the CEEDD. The parent-child link is built from the T1 Family File and can only be formed while children still appear in their parents' census family, so children observed at ages 27 to 31 before 2002 cannot be linked to their parents. This 10% should therefore be read as a reference point for the order of magnitude rather than as the counterfactual rate for this sample.

Table 5: Effect of Trade Shock on the Probability of Working in a Parent's Industry

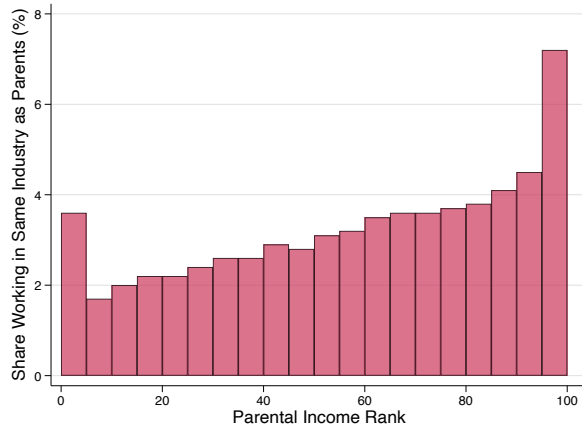
	(1) Age 27	(2) Age 28	(3) Age 29	(4) Age 30	(5) Age 31	(6) Modal Industry
Trade Shock	-0.05325*** (0.01452)	-0.04504*** (0.01416)	-0.03513** (0.01386)	-0.02805** (0.01183)	-0.01208 (0.00811)	-0.03783*** (0.01288)
Parental Income Rank	0.00045*** (0.00002)	0.00037*** (0.00002)	0.00030*** (0.00002)	0.00024*** (0.00001)	0.00017*** (0.00001)	0.00030*** (0.00002)
Trade Shock $\times$ Parental Income Rank	0.00070*** (0.00018)	0.00058*** (0.00018)	0.00048*** (0.00017)	0.00032** (0.00015)	0.00012 (0.00009)	0.00049*** (0.00016)
R^2	0.01	0.01	0.01	0.01	0.01	0.01

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. The table reports 2SLS estimates of Equation (5) as a linear probability model, where the dependent variable is an indicator equal to one if the child is employed in the same 3-digit NAICS industry in which a parent worked in 2001. Columns (1) to (5) use employment at each age from 27 to 31; Column (6) uses the child's modal industry over that window. For dual-manufacturing households the father's 2001 industry is used. All columns use the specification in Column (5) of Table 2. All variables, instruments, and control sets are defined as in Table 2.

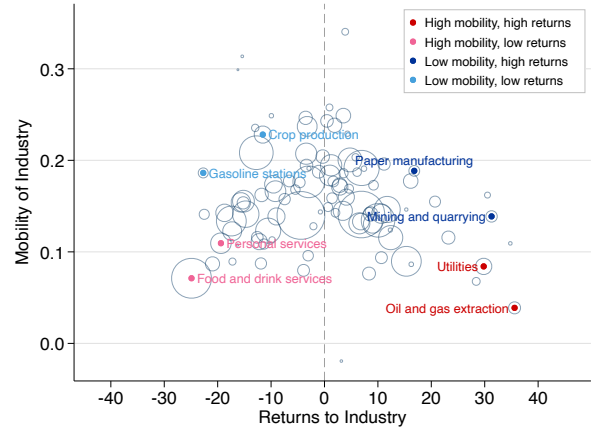
Table 6: Effect of Trade Shock on Worker and Firm Wage Premiums

	(1)	(2) Worker Effect	(3)	(4)	(5) Firm Premium	(6)
Trade Shock	0.0594 (0.039)	0.0587 (0.039)	0.0594 (0.040)	0.0351*** (0.012)	0.0349*** (0.011)	0.0363*** (0.011)
Parental Income Rank	0.0020*** (0.000)	0.0020*** (0.000)	0.0020*** (0.000)	0.0008*** (0.000)	0.0008*** (0.000)	0.0008*** (0.000)
Trade Shock $\times$ Parental Income Rank	-0.0009 (0.001)	-0.0009* (0.001)	-0.0009 (0.001)	-0.0006*** (0.000)	-0.0005*** (0.000)	-0.0006*** (0.000)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
CD FE	Yes	Yes		Yes	Yes	
Child YoB FE	Yes			Yes		
Province $\times$ Child YoB FE		Yes			Yes	
CD $\times$ Child YoB FE			Yes			Yes
Industry Characteristics $\times$ Child YoB FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.04	0.04	0.04	0.02	0.02	0.02

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. The dependent variable in Columns (1) to (3) is the estimated worker fixed effect $\hat{\alpha}_i$ from the AKM model, and in Columns (4) to (6) the average estimated firm fixed effect $\bar{\psi}_i$ over ages 27 to 31; both are measured in log points. Columns (2) and (5) are the preferred specifications for each outcome variable, matching Column (5) of Table 2. All variables, instruments, and control sets are defined as in Table 2.



(a) Industry Inheritance by Family Background



(b) Industry Mobility and Returns

Figure 4: Industry Attachment and Intergenerational Mobility: Descriptive Patterns

Notes: Panel 4(a) reports the share of children working in the same 3-digit NAICS industry as their parents did in 2001, by ventile of the parental income distribution. The sample is restricted to children whose parents worked in manufacturing in 2001 and who appear in the tax files at ages 27 to 31; the child's industry is assigned from their modal industry over that window. Panel 4(b) plots the mobility of three-digit industries against their returns, both constructed as described in Section 4. The mobility of an industry is measured by its rank-rank slope: a higher slope means a child's adult income depends more on where their family started, that is, lower mobility. Marker size is proportional to 2011 employment. For illustration, one industry whose rank-rank slope is above 0.4 is excluded, and the labelled industries are the two closest to each corner of the figure among those employing at least 50,000 workers. Both panels are descriptive and do not condition on the controls used in the regression analysis.

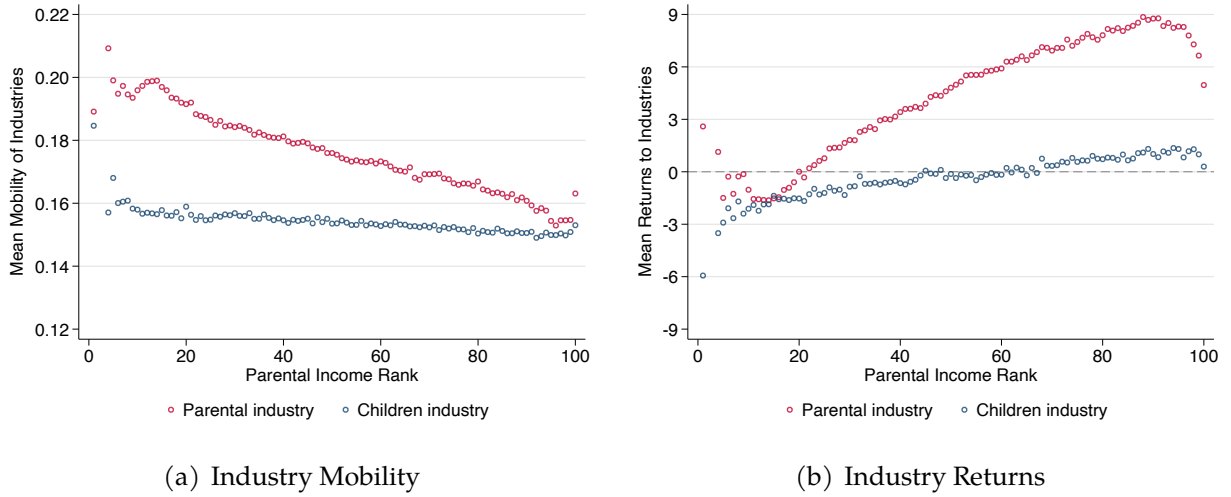


Figure 5: Comparison Between Children and Parents

Notes: The sample is restricted to children whose modal industry over ages 27 to 31 differs from the 2001 manufacturing industry of their parents, using the father's industry where both parents worked in manufacturing. Panel (a) plots industry mobility and Panel (b) plots industry returns. The mobility of an industry is measured by its rank-rank slope: a higher slope means a lower mobility. In both panels, each point is an average across families at a given parental income rank, in red for the industries the parents worked in and in blue for the industries the children ended up in. Both panels are descriptive and do not condition on the controls used in the regression analysis.

The first dimension is an industry's mobility. For each industry j , I regress the income rank of children whose own modal industry is j on their parental income rank, and I refer to the estimated coefficient as the rank-rank slope for that industry. A lower slope means that a child's adult economic position depends less on where their family started, so I treat industries with lower slopes as the more mobile ones. As I discuss below with Figure 4(b), food and drink services is one example of a high-mobility industry, where a child's rank depends comparatively little on their parents'.

The second dimension is an industry's returns. Among children from the same family background, working in some industries places a child higher in the income distribution, and I refer to this income premium as the returns to that industry. I measure it following Haeck and Laliberté (2025), who use the same approach for occupations, estimating

$$\text{rank}_i^c = \sum_{k=1}^{100} \beta_k \mathbf{1}\{\text{rank}_i^p = k\} + \delta_{j(i)} + \varepsilon_i \quad (6)$$

where $\mathbf{1}\{\text{rank}_i^p = k\}$ is an indicator for the parental rank of child i being at percentile k , so that the β_k are a full set of parental percentile effects, and $\delta_{j(i)}$ is a fixed effect for the modal industry j of child i . The industry fixed effects are normalized to have mean zero

across the estimation sample. Among children with the same parental rank, δ_j measures how much higher or lower a child's rank is when working in industry j than in the average industry.

An industry can rank high on one dimension and low on the other, so the two together describe what a child gains and gives up by leaving their parent's industry. Figure 4(b) plots mobility against returns for three-digit industries. Mobility is measured by the rank-rank slope estimated within each industry, so lower values on the vertical axis correspond to higher mobility. The size of each marker is proportional to the number of workers in the industry in 2011.¹⁵

For illustration, I highlight four groups of industries, restricting attention to those that employ a substantial number of workers. Industries in red combine high mobility with high returns. A child working in utilities or oil and gas extraction earns above average and ends up in a position that depends comparatively little on where their parents started. Industries in pink offer high mobility but low returns, as in food and drink services and personal care services, where family background matters little, but the pay is low as well. Industries in dark blue pay well but transmit family position strongly, as in paper manufacturing and mining and quarrying. Industries in light blue, such as crop production and gasoline stations, offer neither.

Two other features of the figure also matter. First, the two dimensions are only weakly related across industries, with a correlation of -0.28 between the rank-rank slope and returns. High returns do not systematically come at the cost of high persistence. Where a child sorts after leaving the parental industry is therefore not a movement along a fixed trade-off, and the destination that loosens the tie to family background need not be the one with the lowest returns.

Second, manufacturing occupies a distinctive position. Across the 21 three-digit manufacturing industries, the average rank-rank slope is 0.18, against 0.16 across the remaining industries, while the average return is +3.0 percentiles, against -0.7.¹⁶ The sector these children stood to inherit pays slightly above average but transmits family position more strongly than most. Within manufacturing, the industries most exposed to Chinese competition are also those with the lowest returns and the highest persistence. Wire and spring

¹⁵Ideally the size of each marker would be proportional to the number of children in my sample working in the industry, but Statistics Canada's vetting rules do not permit releasing those counts. I use total employment from the 2011 National Household Survey instead to convey the relative size of each industry.

¹⁶These are within-industry slopes and are not directly comparable to the national rank-rank slope of about 0.23, which is unconditional. Conditioning on industry removes the component of persistence that operates across industries, and 90% of industries have a slope below the national figure, with a mean of 0.16. [Haeck and Laliberté \(2025\)](#) report the analogous comparison for occupations, where conditioning reduces the rank-rank slope from 0.24 to 0.17.

products, the mild-case anchor, has a slope of 0.16 and a return of +4.1. Leather products, the severe-case anchor, has a slope of 0.31, the second highest of any industry in the sample, and a return of -15.4 percentiles. Apparel, which includes the extreme-case anchor, follows at 0.24 with a return of -13.0.

Taken together, Figure 4(b) shows that where a child ends up working is not neutral. Because industries differ both in how tightly a child's position tracks the family's and in what they pay conditional on that background, where a child sorts is itself associated with intergenerational income transmission. If the shock disrupts the career paths children would otherwise have inherited and pushes them to re-sort across industries, that reallocation is a natural place to look for the changes in aggregate mobility.

4.2.3 The quality of exit

For children whose modal industry over ages 27 to 31 differs from their parents' 2001 manufacturing industry, I compare the industry they left with the industry they entered, using the father's industry where both parents worked in manufacturing. Each industry carries the two measures constructed above, its mobility and its returns, so the comparison shows what these children gain by leaving and what they give up.

Figure 5 plots both measures against parental income rank, mobility in Panel (a) and returns in Panel (b). Each dot is an average across families at a given parental rank, in red for the parents' industries and in blue for the children's.

Figure 5(a) shows that mobility rises with parental rank for both generations. At every parental rank the blue dots lie below the red, so the industry a child moves into transmits income less strongly than the manufacturing industry their parents worked in. The vertical gap is wider at the bottom of the parental income distribution, so the industries that children from poorer families end up in differ most in mobility from the ones their parents worked in. Few manufacturing families sit at those lowest ranks, however, as Figure 2(b) shows.

Returns follow a similar gradient in Figure 5(b), with children from richer families, and their parents, working in industries that pay a higher return. The relationship departs from monotonicity in both tails, and the spread across parental ranks is wider for parents than for children. The two curves cross at around the 20th percentile. Below that point the blue and red dots sit at about the same height, so children enter industries paying roughly what their parents' industries paid. Above it the blue dots lie below the red, and the gap widens with parental rank, so children from richer families move into industries that pay less than the ones their parents worked in.

Read together, the two panels describe a trade-off that falls unevenly across family

background. The main results show that the trade shock raises income mobility among manufacturing families, most clearly among families in the second quartile of the 2001 parental income distribution, where a wide destination gap coincides with a large number of families. The two panels account for that pattern in two steps. The first is who leaves. Children from poorer families are more likely to leave the parental industry, while children from richer families are more likely to stay. The second is where those who leave end up, and here the destinations differ across three groups of families. Children from families below the 20th percentile move into industries where family background matters less, such as personal care and food services, and conditional on their background these industries pay about what their parents' manufacturing industries paid, so leaving costs them almost nothing in returns. Children from families in the second quartile, the group whose mobility rises most clearly, also move into higher-mobility industries, but ones paying somewhat less than their parents' industries, so they gain mobility at a small cost in returns. Children from richer families move into industries paying less still, since the gap in returns widens with parental rank, so for them the same move that loosens the tie gives up a sizable premium.

4.3 Worker skills versus firm premiums

The analysis of mechanisms so far has focused on the industry level. I now move below the industry and ask which components of a child's income the trade shock affects. A child's adult income reflects both what they carry with them across employers, such as their skills and abilities, and what comes with the firm employing them. The distinction matters for how a rise in income mobility brought about by the trade shock should be read. Gains in the first component mean these children bring more to the labor market, while gains in the second mean they are hired by better-paying employers. To separate them, I apply the AKM model of [Abowd, Kramarz and Margolis \(1999\)](#), which splits income into a worker component that follows a worker across employers and a firm component that is the wage premium paid by the employing firm. I then ask how the trade shock affects each component for children whose parents worked in manufacturing.

This AKM decomposition also speaks to a channel I cannot examine directly. Parental income could affect the education children receive, which in turn affects their adult income ([Becker and Tomes, 1979](#)), but the CEEDD does not contain educational attainment. To the extent that schooling raises a child's productivity wherever they work, its contribution is absorbed by the worker component. Education may also improve access to high-premium firms, however, a channel the decomposition cannot separate from other determinants of

firm matching.

I estimate the AKM model on the full CEEDD population rather than restricting attention to the birth cohorts in my main specifications. Using the full population maximizes the size of the connected set of firms and workers and improves the precision of the estimated worker and firm fixed effects.¹⁷ The AKM specification takes the form:

$$\log Y_{ift} = \alpha_i + \psi_{F(i,t)} + \beta_t + W'_{it}\gamma + \mu_{ift} \quad (7)$$

where Y_{ift} denotes the T4 earnings from the primary job of worker i at firm f in year t , and $F(i, t)$ indexes the firm employing worker i in year t . The worker fixed effect α_i captures the portion of income that follows a worker across employers, which includes whatever education and general human capital contribute to income. The employer fixed effect $\psi_{F(i,t)}$ captures the wage premium paid to all employees at a given firm, reflecting firm-specific rents, productivity differentials, or compensation policies that are independent of who the firm employs. The two are separately identified from workers who move between firms. The vector W_{it} includes a quartic polynomial in age, which controls for life-cycle earnings profiles and returns to labor market experience, together with indicators for marital status. The year fixed effects β_t absorb aggregate macroeconomic shocks and business cycle fluctuations common to all workers, and μ_{ift} is the residual.

I retrieve the estimated fixed effects, $\hat{\alpha}_i$ and $\hat{\psi}_f$, and map them to my primary analysis sample, children born between 1986 and 1992 from manufacturing families. Since the worker fixed effect is time-invariant by definition, I assign each child their single estimate. The firm fixed effect requires more construction, since children may switch employers between ages 27 and 31. I track the primary firm f at which the child was employed in each year over that window and take the average firm fixed effect across those years, $\bar{\psi}_i$, which measures the typical quality of the employers the child worked at over that window. The average is taken only over the years in which the child has an employment record, so it is based on fewer than five years for children with intermittent employment. Finally, I use these two components, the worker fixed effect $\hat{\alpha}_i$ and the average firm fixed effect $\bar{\psi}_i$, as dependent variables in my main specification, Equation (5). This lets me ask whether the observed changes in income mobility are driven by changes in human capital acquisition or by changes in access to high-paying firms.

Columns (1) to (3) in Table 6 report the effect of the trade shock on the child's worker component. The coefficient on Parental Income Rank is positive and stable across specifications, so at average exposure children from richer families have higher worker fixed

¹⁷See full details of the sample selection in Appendix B.

effects, which is what one would expect if family income shapes the human capital children bring to the labor market. Neither the coefficient on Trade Shock nor the interaction term is stable across specifications, and the worker-side estimates are far less precise than their firm-side counterparts, with standard errors several times larger. The data therefore do not show a measurable effect of the trade shock on the worker component.

The firm premium results in Columns (4) to (6) tell a different story, and three results stand out. First, the coefficient on Parental Income Rank is positive and statistically significant across all three columns, so at average exposure children from richer families work at firms paying higher premiums. This echoes Figure 5, where on average children from richer backgrounds work in industries with higher returns. Second, the interaction between Trade Shock and Parental Income Rank is negative and statistically significant in every column. The more exposed the parents' industry, the less a family's position in 2001 carries over into the pay premium of the firm that eventually employs the child. Family background still matters, but exposure erodes how much of it reaches the next generation through this margin. Third, the marginal effect of the shock, $\hat{\beta}_1 + \hat{\beta}_3 \times \text{rank}^p$, declines with parental rank and changes sign near the 70th percentile, close to the crossover in the child income rank results of Section 3.3. Relative to a mild-case family, a child from an extreme-case family at the 10th percentile of parental income works at a firm paying about 1.1% more, while a child at the 90th percentile works at a firm paying about 0.4% less.

Where the shock leaves a measurable mark is on the firm side, and in opposite directions across family backgrounds. Children of higher-income manufacturing parents work at firms paying lower premiums than they otherwise would have, while children of lower-income parents work at firms paying more. The worker component shows no comparable pattern, though the estimates there are too imprecise to rule out a channel operating through the skills these children bring to the labor market.

This result points to something about what family position was transmitting in the first place. If part of what the shock disrupts is access to well-paying firms, then part of what a manufacturing parent's position delivered to the next generation was a route into firms that paid a premium. As import competition contracted those firms' industries, that route narrowed for the children whose family position would otherwise have opened it, while children without such a route to begin with moved into better-paying firms than they otherwise would have. What these estimates cannot separate is whether the decline at the top reflects contraction of the high-premium firms themselves, a re-sorting of these children into lower-premium firms, or some combination of the two.

5 Conclusions

Since the late twentieth century, trade liberalization has moved millions of workers out of manufacturing in developed economies and into manufacturing in developing economies. This movement has spurred a rich literature investigating the impact of globalization on employment, wages, and within-generation inequality. An equally important question concerns the children of these workers. Do the children born in this age of rapid globalization face genuinely different opportunities in life, and if so, how should policy respond? This paper has asked how the China Shock, one of the largest trade shocks in history, changed economic opportunities for the children of manufacturing workers in Canada.

I find that the China Shock increases the intergenerational income mobility of these families, so that where a child ends up depends less on their family background, but the increase is not shared evenly. The shock moves children from poor and rich families in opposite directions, with those from poor families rising relative to their peers and those from rich families falling. Two things explain this divergence: whether a child leaves their parents' industry, and how the industry they enter compares with the one they left. First, children from poorer families are more likely to leave, while children from richer families tend to stay. Second, those who leave move into jobs such as personal care and food service, where their family background matters less. For children from these backgrounds, these industries are worth about as much as the manufacturing work they would otherwise have inherited, so leaving costs them little. For them, leaving is an escape. Children from richer families fare differently: they stood to inherit a good job, and the shock took it away.

The policy implications are clear but surprising. The costs of the trade shock for manufacturing workers are real: lost jobs, falling incomes, fracturing communities. But assistance is usually built to return families to where they stood before a shock, and here that logic breaks down. For the poorer families, where they stood in manufacturing before was a floor that held their children near the bottom. Sending them back is not help. The shock was closer to a release than a loss for their children.

For richer families, the loss was real, but its lesson points the same way. The good jobs their children stood to inherit were valuable only as long as one industry survived and prospered, and that is exactly what can no longer be assumed. No industry can now count on being spared: trade took some, automation is taking others, and AI will choose its own industries. Any inheritance tied to a single industry is therefore fragile. The China Shock did not create this fragility; it only revealed it. Extending the life of declining industries is short-sighted for the same reason, since the cost falls on a generation that had no say in

the bet. What both groups need is the same thing: portable human capital. For children from richer families it is protection against a fragile inheritance; for children from poorer families it is the footing to make leaving a choice rather than a fall.

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Appendix

A Heterogeneous Effect of Trade Shock by Subgroup

Table A1: Heterogeneous Effect of Trade Shock by Subgroup

	(1) Boy	(2) Girl	(3) Father	(4) Mother	(5) Native	(6) Immigrant
Trade Shock	5.420** (2.713)	12.497*** (1.972)	5.435*** (1.862)	13.631*** (4.424)	3.609 (2.524)	7.452*** (2.208)
Parental Income Rank	0.216*** (0.008)	0.250*** (0.010)	0.236*** (0.008)	0.195*** (0.014)	0.264*** (0.004)	0.140*** (0.008)
Trade Shock $\times$ Parental Income Rank	-0.087** (0.038)	-0.167*** (0.026)	-0.086*** (0.029)	-0.156** (0.063)	-0.058* (0.033)	-0.100*** (0.036)
R^2	0.03	0.04	0.03	0.02	0.08	0.01

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. This table reports the full estimates underlying Figure 3. Each column reports 2SLS estimates of Equation (5) on one subsample. Columns (1) and (2) split children by gender, Columns (3) and (4) by whether the manufacturing parent is the father or the mother, and Columns (5) and (6) by whether the family is native-born or immigrant. All columns use the specification in Column (5) of Table 2. All variables, instruments, and control sets are defined as in Table 2.

B AKM estimation details

This section describes the estimation sample for the AKM model in Section 4.3. The estimation window runs from 2009 to 2019. It begins after the labor market recovered from the Great Recession and covers the early-career years of my analysis cohorts, since the 1986 cohort turned 23 in 2009.

To ensure data quality and computational feasibility, I impose several sample restrictions. I define the primary job as the position generating the highest annual T4 earnings, breaking ties randomly. I limit the analysis to workers aged 18 to 54 with a recorded marital status. To focus on wage employment, I exclude individuals whose net self-employment income, which includes net commissions, fishing, and farming revenues, exceeds the T4 earnings from their primary job. On the firm side, I restrict the sample to incorporated firms with at least \$50,000 in revenue and at least five employees. Finally, I limit the sample to individuals and firms observed at least twice during the period, and estimate the model on the largest connected set of workers and firms linked by job movers, which is the set over which the worker and firm fixed effects are separately identified.

My sample selection broadly follows the CEEDD-based AKM applications of [Dostie et al. \(2023\)](#) and [Tino \(2025\)](#), with three departures motivated by the analysis of young

workers moving out of manufacturing. First, the prior literature requires the primary job alone to clear the annualized full-time-minimum-wage threshold (\$18,733 in 2012 CAD). I instead apply this threshold to total T4 earnings across all jobs, and require the primary job to clear a separate minimum of \$5,000. This accommodates young workers who reach full-time-equivalent earnings through multiple jobs during early-career stabilization. Second, I retain non-market sectors (Education, NAICS 61; Health Care, 62; and Public Administration, 91), which the prior CEEDD literature typically excludes. My sector-switching analysis identifies these as important destinations for children leaving manufacturing, so excluding them would discard much of the variation I aim to study. Third, I set the minimum firm-size threshold at five employees rather than two for computational tractability.

C Other mechanisms

C.1 Geographic mobility

A natural alternative mechanism is geographic mobility. [Autor et al. \(2014\)](#) document that workers in declining local labor markets relocate to regions with better economic prospects, and this logic can extend across generations. If trade exposure pushes children into different industries, it may also induce geographic relocation. Alternatively, children may choose to remain in their home region to access local family resources and networks.

Following the construction used for industry switching in Section 4, I track each child's Census Division of residence annually between ages 27 and 31 and define the modal Census Division as the one in which the child is observed in the largest number of years, breaking ties in favor of the Census Division of residence at the older age. I construct two binary outcomes. The first, G_{ia} , equals one if child i at age a resides in the Census Division in which a parent lived during the 2001 baseline, and zero otherwise. The second, G_i , equals one if the child's modal Census Division is that same one, and zero otherwise. For dual-manufacturing households, I assign the father's 2001 Census Division, consistent with the household assignment rule used in the baseline specification.

Table A2 reports estimates of Equation (5) with G_{ia} and G_i as the outcomes. The coefficient on Parental Income Rank is negative in every column and statistically significant from age 29 onward, so children from higher-ranked manufacturing families are somewhat less likely to remain in the Census Division their parents lived in. The coefficient on Trade Shock is also negative throughout, though never significant. The interaction between Trade Shock and Parental Income Rank is positive in every column, which would indicate that exposure makes children from higher-ranked families more likely to stay, but

none of these estimates approaches conventional significance and the standard errors are of the same order as the point estimates.

The trade shock therefore does not appear to move children from different family backgrounds apart geographically. Whatever asymmetry the shock produces across family backgrounds operates through the industries children enter rather than through where they live. This also means the industry results in Section 4 are not picking up relocation to different local labor markets.

C.2 The intergenerational transmission of general human capital

A second alternative mechanism is that what parents pass on to their children is not income or industry attachment but a type of skill. Parents in higher-ranked positions within their firms often hold managerial or supervisory roles built on organizational and cognitive skills that carry across industries, and they may pass these on through the home environment and through what children observe of their parents' work. Parents in lower-ranked positions are more likely to perform tasks specific to the manufacturing production process, and this industry-specific human capital depreciates when the industry contracts, leaving fewer outside options.

Testing this mechanism directly would require occupation identifiers to separate managers from production workers, which the CEEDD does not contain. I therefore construct a proxy, the within-firm income rank, defined as an individual's percentile rank in their firm's T4 earnings distribution in 2001. The proxy rests on the assumption that where a worker sits in their firm's wage hierarchy is informative about their occupational role, with workers near the top more likely to occupy managerial positions and workers near the bottom more likely to perform production tasks. Two limitations follow. The measure is purely relative, so it removes differences in firm size and firm pay, and a worker near the top of a small firm may earn less than a worker in the middle of a large one. Firm size in manufacturing is itself related to trade exposure. I therefore treat the results below as suggestive.

I construct the within-firm income rank using the full universe of Canadian tax filers in 2001, restricting to active workers aged 18 to 65 and to firms with at least five employees to ensure meaningful within-firm hierarchies. For each firm, I assign every employee a percentile rank based on T4 earnings, using T4 earnings rather than total income to isolate the within-firm labor market hierarchy from other income sources. For manufacturing families, I assign the household the within-firm rank of the parent employed in manufacturing in 2001. Where both parents worked in manufacturing, I assign the rank of the parent with

Table A2: The Impact on Probability of Residing in the Same Census Division by Age

	(1) Age 27	(2) Age 28	(3) Age 29	(4) Age 30	(5) Age 31	(6) Modal CD
Trade Shock	-0.03888 (0.05775)	-0.03999 (0.03814)	-0.03057 (0.03829)	-0.01281 (0.03028)	-0.01240 (0.03786)	-0.03791 (0.03595)
Parental Income Rank	-0.00011 (0.00023)	-0.00030 (0.00020)	-0.00033* (0.00017)	-0.00033** (0.00014)	-0.00032*** (0.00011)	-0.00034* (0.00018)
Trade Shock $\times$ Parental Income Rank	0.00100 (0.00084)	0.00089 (0.00057)	0.00069 (0.00059)	0.00053 (0.00051)	0.00028 (0.00052)	0.00076 (0.00054)
R^2	0.001	0.001	0.001	0.001	0.001	0.001

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses. The table reports 2SLS estimates of Equation (5) as a linear probability model, where the dependent variable is an indicator equal to one if the child resides in the same Census Division in which a parent lived in 2001. Columns (1) to (5) use residence at each age from 27 to 31; Column (6) uses the child's modal Census Division over that window. For dual-manufacturing households the father's 2001 Census Division is used. All columns use the specification in Column (5) of Table 2. Coefficients are reported to five decimal places given their magnitude. All variables, instruments, and control sets are defined as in Table 2.

Table A3: Mechanism: General Human Capital and the Non-Manufacturing Parent

	(1) General Human Capital Children Income Rank	(2) Children Income Rank	(3) Age 27	(4) Age 28	(5) Age 29	(6) Age 30	(7) Age 31	(8) Modal Industry
Trade Shock	3.975** (1.648)	4.124** (2.063)	-0.0162 (0.011)	-0.0509** (0.022)	-0.1119** (0.044)	-0.1733*** (0.057)	-0.2593*** (0.091)	-0.1389*** (0.050)
Parental Income Rank	0.110*** (0.004)	0.214*** (0.011)	0.0001 (0.000)	0.0004*** (0.000)	0.0006*** (0.000)	0.0009*** (0.000)	0.0012*** (0.000)	0.0007*** (0.000)
Trade Shock $\times$ Parental Income Rank	-0.050** (0.021)	-0.106** (0.041)	0.0003 (0.000)	0.0008* (0.000)	0.0020** (0.001)	0.0031*** (0.001)	0.0049*** (0.002)	0.0025** (0.001)
R^2	0.044	0.049	0.004	0.003	0.002	0.002	0.002	0.002

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, clustered at the Census Division level, are reported in parentheses.

the higher T4 earnings, on the grounds that this captures the highest level of potentially transferable skills available to the child within the household.

Column (1) of Table A3 reports the results with the full set of controls and fixed effects, with the within-firm rank in place of the parental income rank. The baseline slope is 0.110, so a parent standing one percentile higher within their firm's earnings distribution in 2001 is associated with a child standing 0.110 percentiles higher in adulthood. The interaction with Trade Shock is -0.050 and significant at the 5 percent level, so import competition weakens this transmission in the same direction as it weakens the transmission of parental rank documented in Section 4.

This result does not establish the firm hierarchy as a channel operating independently of family income. A worker's position within their firm's earnings distribution and their earnings level move together by construction, so the two rank measures are highly correlated, and the specification in Column (1) cannot separate what the hierarchy carries from what income position carries. The estimate is therefore consistent both with the reading that managerial and organizational skills are less portable across industries than the term general human capital suggests, and with the reading that the within-firm rank is a noisier proxy for the same underlying family position that the main specification already measures.

What Column (1) does show is that the finding is not specific to how a parent's labor market position is measured. Whether that position is taken as the family's place in the national income distribution or as the parent's place within their own firm, import competition weakens the extent to which it reaches the next generation. I treat this as a descriptive complement to the main results rather than as evidence for a particular mechanism.

C.3 The role of the non-manufacturing spouse

The preceding analyses treat the household as a single economic unit, with trade exposure assigned at the household level. In this subsection, I separately examine the role of the non-manufacturing spouse in two-parent households where only one parent worked in manufacturing and the other in a non-manufacturing sector. This dual-sector household structure allows me to assess whether the non-manufacturing parent provides an intra-household buffer against the trade shock, either through income that buffers the household against the manufacturing parent's earnings losses, or through access to alternative career paths for the child.

I restrict the sample to two-parent families with this dual-sector structure and assign each parent an individual income rank in 2001, computed within the full national popu-

lation of parents whose children were born in the same year. Let rank_i^{Mfg} denote the income rank of the manufacturing parent and rank_i^{NonMfg} the rank of the non-manufacturing spouse. Trade exposure $\Delta IP_{ij\tau}$ is assigned based on the manufacturing parent's industry j and is recentered around the cohort mean. I estimate:

$$\begin{aligned} \text{rank}_i^c = & \theta_0 + \theta_1 \Delta IP_{ij\tau} + \theta_2 \text{rank}_i^{Mfg} + \theta_3 (\Delta IP_{ij\tau} \times \text{rank}_i^{NonMfg}) \\ & + W_i' \theta_4 + \gamma_b + \gamma_s + \gamma_{cd} + \varepsilon_{ib} \end{aligned} \quad (8)$$

where the controls W_i and the fixed effects γ_b , γ_s , and γ_{cd} are defined as in my main specification, Equation (5).

The coefficients of Equation (8) characterize different aspects of the relationship between the shock and child outcomes. The coefficient θ_2 captures the rank-rank relationship between the manufacturing parent and the child. The coefficient of interest, θ_3 , measures how the effect of trade exposure on the child income rank varies with the non-manufacturing spouse's rank. The specification omits the main effect of the spouse's rank, so θ_3 absorbs both the differential effect of exposure across spousal ranks and any level relationship between spousal rank and child rank that is correlated with exposure. I therefore read it as a descriptive pattern rather than as an estimate of intra-household buffering. Column (2) of Table A3 reports the estimates. The coefficient on Trade Shock is 4.124 and the interaction term is -0.106, both significant at the 5 percent level. The negative interaction means that the association between exposure and child rank is weaker in households where the non-manufacturing parent ranked higher, which is the direction intra-household resources would predict, though the omitted main effect prevents me from attributing it to buffering.

To assess whether the non-manufacturing spouse provides an alternative employment pathway for the child, I re-estimate Equation (8) using two binary outcomes. The first, N_{ia} , equals one if child i at age a is employed in the non-manufacturing parent's 2001 industry, and zero otherwise. The second, N_i , equals one if the child's modal industry between ages 27 and 31 is that same industry, and zero otherwise. The modal industry is defined as in Section 4. Columns (3) to (8) of Table A3 report the estimates. The interaction term is positive in every column, rises with the age at which industry is measured, and is significant from age 28 onward, reaching 0.0049 at age 31. In the modal industry column it is 0.0025. Children whose non-manufacturing parent ranked higher are more likely to enter that parent's industry when the manufacturing parent's industry was more exposed, and this tendency strengthens as the children age. Read alongside the industry exit results in Section 4, the non-manufacturing parent's industry appears to be one destination for

children leaving the manufacturing parent’s industry. The caveat about the omitted main effect applies to these columns as well.

D Robustness checks

I conduct seven robustness checks.

D.1 Placebo test

A central identifying assumption is that the observed changes in income mobility are driven by exposure to the China Shock during adolescence and early career formation. If instead the results reflect pre-existing secular trends, where industries eventually hit by import competition were already on different mobility trajectories before the shock, the baseline estimates would be biased.

To assess this assumption, I conduct a placebo test using an older cohort whose labor market outcomes were established before the onset of the China Shock. I focus on children born between 1963 and 1967 and their parents. Since this period predates China’s WTO accession, future import competition should have no predictive power over the mobility outcomes of this group if the main estimates are causal.

I use two data sources for this analysis, linking the Intergenerational Income Database (IID) to the 2001 Census (20% sample). The IID provides income levels, from which I compute income ranks for children at ages 27 to 31 and for their parents when the children were 15 to 19. The 2001 Census provides demographic information and employment industries. By matching individual identifiers, I identify the industries in which the parents of this older cohort were employed in 2001, and I use these industry codes to assign the cumulative trade shock over 2002 to 2011 to their pre-shock outcomes. Because industry is observed in 2001 rather than during the children’s adolescence, this assignment requires that a parent’s industry in 2001 is informative about the industry the household was attached to earlier, and it restricts the sample to parents still holding an industry record in 2001.

In assigning the trade shock, I adapt the household rule to reflect the labor market structure of this older generation. For the main analysis, covering the 1986 to 1992 cohorts, parents were active in the 2000s, when dual-earner households were common. For the placebo cohort, whose parents were active in the 1980s, the male-breadwinner model was more prevalent in manufacturing households, so I use the father’s industry when assigning the shock in dual-manufacturing households. This departure from the averaging

rule used in the main analysis reflects the different structure of manufacturing households in that era, but it does mean the placebo does not replicate the main specification exactly.

The link to the 20% Census sample yields a geographically sparser dataset than the administrative universe used in the main analysis. This sparsity makes clustering at the Census Division level infeasible, since too few observations fall within each cluster. I therefore cluster standard errors at the three-digit industry level. This choice is also consistent with the level at which the placebo treatment varies, since exposure is defined by industry-specific import growth.

The results appear in Column (1) of Table A4. Neither the coefficient on the trade shock nor its interaction with parental income rank is statistically significant, which is what the identifying assumption predicts. The point estimates are not small, however, and the standard errors are several times those in the main sample, so the test rules out pre-existing trends only within a wide confidence interval. The interval for the interaction term includes zero as well as magnitudes comparable to the baseline estimate. I therefore read the placebo as consistent with the identifying assumption rather than as decisive evidence for it.

D.2 The global financial crisis

In the main specification, trade exposure varies across industries and across birth cohorts, and for the later cohorts the five-year exposure window overlaps with the global financial crisis that began in 2008. Since the crisis fell most heavily on the same manufacturing industries that Chinese import competition had already weakened, the estimated effect of trade exposure may partly reflect the downturn rather than import competition alone.

To address this concern, I restrict the sample to children born between 1986 and 1988. For the youngest cohort in this restricted sample, born in 1988, the exposure window spans 2003 to 2007, so the exposure window of every cohort closes before the onset of the crisis. Trade exposure is therefore measured entirely over pre-crisis years, and any correlation between the shock and the downturn cannot enter through the timing of the treatment.

Column (2) of Table A4 reports the results. The coefficient on Trade Shock is 6.166 and the interaction with Parental Income Rank is -0.092, both statistically significant. The baseline rank-rank slope is 0.240, close to the 0.232 in the main specification. The interaction term is about a quarter smaller in magnitude than the baseline estimate of -0.126, which is consistent with the crisis reinforcing the effect of import competition in the later cohorts, but the sign, the significance, and the order of magnitude are unchanged. Relative to a mild-case family, the rank-rank slope still falls by 0.034 for an extreme-case family,

a decline of 14% against the baseline persistence in this subsample.

Two limitations of this check are worth noting. First, restricting to three cohorts removes the variation in exposure timing that the full sample provides, and the estimates are correspondingly less precise. Second, the restriction moves the exposure window before the crisis but not the outcome window: children in these cohorts reach ages 27 to 31 between 2013 and 2019, so their adult incomes are still measured in a post-crisis labor market. What the check rules out is that the measured treatment picks up the downturn, not that the downturn left their eventual incomes untouched.

D.3 Alternative age windows for child income

In the baseline analysis, the child income rank is constructed from average earnings between ages 27 and 31 as a proxy for permanent income. One concern with this window is uneven coverage across cohorts. Because the CEEDD income data end in 2019, children born in 1992 are observed only once within the window, at age 27, while earlier cohorts are observed for all five years. To assess whether this drives the results, I re-estimate the model using average income over ages 25 to 29, under which the 1992 cohort is observed three times rather than once. Column (3) of Table A4 reports the results. The coefficient on Trade Shock is 8.026 and the interaction with parental rank is -0.119, both significant at the 1 percent level and close to the baseline estimates. The baseline rank-rank slope falls from 0.229 to 0.209 under the earlier window, which is the direction life-cycle attenuation predicts.

D.4 Robustness to an alternative instrument

The identifying assumption requires that the surge in Chinese imports to the comparison countries stems from China's internal supply-side productivity growth rather than from demand shocks correlated with those in Canada. In the baseline analysis, the instrument is constructed from trade flows to six European economies (Denmark, Finland, the Netherlands, Norway, Spain, and Sweden). To check that the estimates are not driven by an idiosyncratic demand shock in any single country, I expand the comparison group to include Italy and Austria, both high-income economies with trade structures comparable to the original six and without a large direct trade relationship with Canada. Column (4) of Table A4 reports the results. The coefficient on Trade Shock is 11.257 and the interaction with parental rank is -0.150, both significant at the 1 percent level. The interaction is somewhat larger in magnitude than the baseline estimate of -0.126, so the alternative instrument does not weaken the pattern documented in the main results.

D.5 Robustness to parents' age

Haider and Solon (2006) show that annual earnings approximate lifetime income most closely when observed in the early to mid forties. In the baseline sample, parental income is measured in the 2001 cross-section, where the average parental age is already over 40. To check that the results do not depend on the younger parents in the sample, I restrict to parents aged 40 and older in 2001, the range in which annual income tracks lifetime position most closely. Column (5) of Table A4 reports the results. The coefficient on Trade Shock is 7.069 and the interaction with parental rank is -0.102, both significant at the 1 percent level and close to the baseline estimates.

D.6 Alternative inference: clustering by industry

In the baseline specification, standard errors are clustered at the Census Division level. Trade exposure is assigned at the family level, since a household with one manufacturing parent takes that parent's industry shock and a household with two takes the average, so the treatment does not vary purely by industry. Abadie et al. (2023) argue that the clustering level should follow the design by which treatment is assigned, which in this setting points to the family and the local labor market rather than the industry. Even so, unobserved components of the error term may be correlated within parental industries, and I therefore report inference clustered at the three-digit industry level as an alternative.

Manufacturing contains only three two-digit industries, too few for cluster-robust inference, so I use the 21 distinct three-digit industries. This number is below the range usually considered adequate for cluster-robust asymptotics, and the resulting standard errors should be read as an illustration of how much precision is lost under a coarser clustering assumption rather than as a preferred alternative. Column (6) of Table A4 reports the results. The coefficient on Trade Shock is 8.660 and remains significant at the 10 percent level, and the interaction with parental rank is -0.124 and remains significant at the 5 percent level, with standard errors roughly twice those in the baseline.

D.7 Excluding resource-intensive provinces: Alberta and Saskatchewan

Concurrent with the rise in Chinese import competition, Western Canada experienced a substantial resource boom driven by surging global oil prices, with Alberta and Saskatchewan most affected. Two features of this boom could confound the estimates. First, it changed where children of manufacturing families went after leaving the parental industry, since energy and its supporting industries offered unusually high returns in these provinces

over the same period, so a rise in mobility there need not reflect the trade shock. Second, resource revenues reached other provinces through federal fiscal transfers, so the boom was not confined to the provinces where the resources were extracted.

The province by child birth cohort fixed effects in the main specification absorb differences in provincial conditions that apply uniformly within a province and cohort. They do not absorb effects of the boom that vary with a family's trade exposure, which is what the first concern above describes. I therefore replicate the baseline analysis excluding these two provinces. Column (7) of Table [A4](#) reports the results. The coefficient on Trade Shock is 9.095 and the interaction with parental rank is -0.122, both close to the baseline estimates and significant at the 1 percent level.

Table A4: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Placebo Test	Control Financial Crisis	Child Income Rank 25–29	Alternative IV	Restriction on Parents Age	Cluster at Industry Level	Exclude AB & SK
Trade Shock	10.430 (8.760)	6.166*** (1.860)	8.026*** (2.100)	11.257*** (2.320)	7.069*** (1.850)	8.660* (4.510)	9.095*** (1.770)
Parental Income Rank	0.223*** (0.010)	0.240*** (0.010)	0.209*** (0.010)	0.229*** (0.010)	0.215*** (0.010)	0.229*** (0.010)	0.228*** (0.010)
Trade Shock $\times$ Parental Income Rank	-0.184 (0.130)	-0.092*** (0.030)	-0.119*** (0.030)	-0.150*** (0.030)	-0.102*** (0.030)	-0.124** (0.060)	-0.122*** (0.030)
R^2	0.10	0.03	0.02	0.03	0.02	0.03	0.03

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses, clustered at the Census Division level except in Columns (1) and (6), which cluster at the industry level. Each column reports 2SLS estimates of Equation (5) from a separate regression, with the child income rank as the dependent variable. All columns use the specification in Column (5) of Table 2 unless the column heading indicates otherwise. All variables, instruments, and control sets are defined as in Table 2.