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# The Value of Specific Skills under Shock: High Risks and High Returns

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**Abstract**

We study the causal effects of negative and positive demand shocks on the returns to specific skills by using variation from international trade shocks. To measure specific skills, we use task information from an official data set for career guidance and merge this information with a large register data set. Our results show that negative demand shocks result in larger earnings losses for workers with specific skills than for those with general skills, but workers with specific skills also profit much more from positive demand shocks. Thus, demand shocks lead to risk-return trade-offs for workers with specific skills.

Keywords: demand shocks, human capital specificity, skill bundles
JEL Classification: J24, F16, I20

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# 1 Introduction

Human capital theory predicts that workers with specific skills have higher adjustment costs in response to negative demand shocks than workers with general skills, because specific skills cannot be transferred across industries, occupation, or firms. In line with this prediction a large literature documents that workers with specific skills experience larger earnings losses in response to firm closures and mass layoffs (Couch and Placzek, 2010; Gathmann and Schönberg, 2010; Hijzen et al., 2010; Jacobson et al., 1993; Robinson, 2018). Others show that workers with specific skills experience more difficulties adjusting to technological change (e.g., Hanushek et al., 2017) and economic turbulence (e.g., Lamo et al., 2011). Yet, others show that workers with specific skills have larger reallocation costs in response to local labor market shocks stemming, for example, from industrial regulation policies (e.g., Walker, 2013) or import competition (e.g., Traiberman, 2019; Utar, 2018; Yi et al., 2017).

However, human capital theory also predicts that increasing labor demand drives up the rents to specific skills, because workers with specific skills cannot easily be substituted, such that their outside options and, thus, their bargaining position improves with increasing labor demand (Becker, 1964; Lazear, 2009). Although understanding how returns to specific human capital respond to positive demand shocks provides important insights about the value of specific skills in dynamic labor markets and the substitutability of workers across firms, evidence on this relationship is scarce. A notable exception are Jäger and Heining (2019), who show that small firms pay their incumbent workers higher wages in response to positive demand shocks stemming from co-worker deaths.

This study uses variation from import and export shocks in Germany as a lens through which it studies the causal effects of negative *and positive* demand shocks on the returns to specific skills. Trade shocks provide an ideal quasi experiment for this purpose. First, trade shocks induce both, negative and positive demand shocks. Second, many papers suggests that trade shocks are related to substantial variation in labor demand by showing that variation of trade shocks leads to substantial employment variation (e.g., Autor et al., 2013; Dauth et al., 2014). In this sense, Germany provides an ideal context for our analysis, because the country was exposed to a substantial increase in import competition from Eastern Europe and China at the same time that German exports to those countries were accelerating. These dynamics allow us to exploit substantial regional variation within and across different industries and occupations to analyze how returns to specific and general skills vary by negative and positive demand shocks.

We combine three unique data sources. First, to measure the specificity of skills, we rely on the BERUFENET, an occupational task data set constructed by the German employment agency for career guidance and job placement. Comparable to the U.S. O*Net, the BERUFENET contains information on the required tasks, equipment, working conditions, and required qualifications for all occupations in Germany. Following Eggenberger et al. (2018), we use this data to construct a specificity measure that captures the transferability of the skill bundle of a particular occupation to the skill bundle requirements in the overall labor market. This specificity measure, that closely corresponds to Lazear's skill-weights definition of specific human capital, captures the probability of finding a new job that requires a similar skill bundle.

Second, to measure the trade flows between Germany and other countries, we follow Autor et al. (2014) and Dauth et al. (2014) by using trade data from the United Nations (UN) Commodity Trade Statistics Database (Comtrade). This dataset, which contains detailed information about commodity types, provides information on trade flows between more than 170 countries.

Third, to follow workers' careers over long periods, we use register data from the Federal Employment Agency of Germany, the Integrated Employment Biographies (IEB). This data contains highly accurate information about workers' wages, employment status, and common demographic characteristics (e.g., age, nationality, and education), thereby allowing us to analyze the long-term consequences of international trade for workers with specific and general skills. Moreover, this data allows us to link workers to firms, so that we can account for detailed firm characteristics.

Our empirical analysis relies on two main identification assumptions. First, we assume that German workers and firms were unable to foresee which industries, and therefore which labor market regions, would be affected by the demand shocks stemming from increasing international trade. While this assumption is key to all approaches following Autor et al. (2014), it is even more important for our specific analysis, because the expected consequences of increasing international trade may have influenced firms' demand for specific skills and workers' occupational choices. Thus, international trade might influence workers' human capital investments.

On one hand, to reduce the negative consequences of import shocks, workers may have chosen occupations and industries that demand less specific human capital. On the other hand, firms may have increased their demand for specific skills in anticipation of new export opportunities. To deal with this concern, we restrict our sample to only West German workers who chose their jobs before 1990, and we calculate their skill specificity according to their baseline job in that year. Before

1990, it was virtually impossible to predict the fall of the Iron Curtain and the resulting acceleration of international trade between Germany and the former Soviet bloc countries in the 1990s and 2000s (Chevalier and Marie, 2017; Fuchs-Schündeln, 2008).

Second, we must assume that the trade exposure measures do not reflect domestic shocks to German industries. To tackle this issue and isolate the effects of trade from other confounding factors, we follow common practice by building on the estimation strategy developed by Autor et al. (2013) and Dauth et al. (2014): We instrument the increase in trade exposure from China and Eastern Europe to Germany with the trade between these low-wage countries and other "third-party" high-income countries.

Our results show that workers with specific skills do not only suffer more from negative demand shocks, they also profit much more from positive demand shocks than workers with general skills. In response to the average increase of exports throughout our observation period, the earnings of a worker with a high level of skill specificity (75 percentile) increased by approximately 14 percent more than the earnings of a worker with an average level of skill specificity. In contrast, specific-skilled workers lost approximately 8 percent more in response to the average rising imports throughout our observation period. Workers with very general skills (25 percentile) gained approximately 12 percent less from exports than the workers with an average level of skill specificity. However, they also lost approximately 7 percent less from average increasing exports.

As workers with specific skills profit more from positive demand shocks but suffer more from negative ones, the demand shocks led to more heterogeneous effects for workers with specific skills than for workers with general skills. Thus, our results suggest that investments in occupation-specific skill bundles lead to a risk-return trade-off when labor markets become more dynamic. We also find qualitatively similar results for workers who remain in their firm and occupation. This result highlights that not only job mobility, but also intra-firm wage bargaining is an essential tool for firms in responding to changes in labor demand.

Additionally, our results uncover important heterogeneities. We find that specific skills matter less for young workers in response to negative demand shocks—at least, if they are mobile and switch occupations and labor markets. Thus, our results suggest that young workers either adjust to the consequences of demand shocks (Lazear, 2009), or that they are worse matched such that specific skills matter less for them (Fredriksson et al., 2018). Moreover, we show that specific skills matter less for workers who have entered the labor market after the fall of the iron curtain.

Most likely, because workers learn about the developments in the labor markets and adjust to the consequences of demand shocks.

The results remain robust for different sub-samples and regression specifications. First, the trade shocks are not correlated with our outcome variables throughout the pre-treatment period (the 1980s) suggesting that the implicit parallel trends assumption holds. Second, our results remain robust if we control for a set of very detailed industry dummies thus comparing workers within the same narrowly defined industry. Third, the results hold if we restrict our sample to a very homogeneous sample that only includes workers who hold an apprenticeship degree. Third, we find similar effects when we run our regressions on the industry instead of the regional level.

Our results contribute to at least two strands of the literature. First, our results relate to the literature analyzing how portable human capital is across firms, occupations, and industries (Leighton and Speer, 2020; Cortes and Gallipoli, 2018; Gathmann and Schönberg, 2010; Kambourov and Manovskii, 2009; Poletaev and Robinson, 2008; Robinson, 2018). This literature largely relied on worker mobility in response to negative demand shocks to evaluate the importance of firm specific human capital for worker mobility. By analyzing how the returns to specific skills respond to positive demand shocks, our results demonstrate that the specificity of a worker's human capital is not purely determined by a fixed "technological" substitutability between the worker's skill bundle and the skill bundle of other workers. In contrast, the returns to specific skills are endogenous to market parameters. By demonstrating that the earnings of workers with specific human capital change with the labor demand of the relevant labor market, even in the absence of worker mobility, we offer new insights into the micro-economic foundations of the return to specific skills.

Second, we contribute to the literature analyzing the labor market consequences of international trade. Within this literature a number of papers emphasize the role of specific human capital as main determinant for the adjustment costs in response to trade shocks. Traiberman (2019) estimates an occupational choice model and shows that the largest share of adjustment costs in response to import exposure arises because workers must switch occupations and lose their rents to occupation specific skills. Utar (2018) finds that workers with manufacturing-specific skills suffer lager losses in response to imports shocks.

Although Dauth et al. (2021) briefly touch on the relationship between export exposure and industry specific human capital, this is the first paper providing an extensive analysis of how trade shocks affect the returns to specific skills. Thus, we contribute to the literature by showing that both import and export exposure influence the returns to specific skills, such that workers with specific skills are not the main losers of increasing trade exposure. Indeed, in Germany, on average they even profit more than workers with general skills. Moreover, we show that specific skills matter less for young than for old workers, and younger workers change their occupations and labor markets in response to increasing exports. These results suggest that workers adjust to trade shocks in the longer run.

The remainder of this paper is structured as follows. Section 2 presents our theoretical model. Section 3 explains our estimation strategy. Section 4 presents the data and explains the empirical construction of our measure for occupational specificity. Section 5 shows our empirical results and robustness checks, and Section 6 concludes.

# 2 Specific occupations and demand shocks

This section provides a simple theoretical framework that is based on Lazear's (2009) "skill-weights" model of human capital to guide the interpretation of our empirical results. The first subsection presents the theoretical model and the second subsection discusses it's implications for our empirical analysis.

## 2.1 A skill weights approach

According to the skill-weights model, all skills are "general" in that other jobs[1] that value each of these skills always exist. Nonetheless, these general skills are used in different jobs in different combinations and with different weights attached to them, thereby, giving skill combinations varying degrees of specificity.

In Lazear's (2009) basic model, there are two completely general skills, $A$ and $B$. Different jobs require these skills in different combinations to produce output. The relative weight of skill $A$ in job $i$ is denoted with $\lambda_i$ , where $0 < \lambda_i < 1$. Suppose a worker's current job $i$ requires $\lambda_i$. The productivity of a worker with skills $A$ and $B$ in job $i$ is given by:

$$y_i = \lambda_i A + (1 - \lambda_i) B \tag{1}$$

[1]Lazear originally formulated the model such that skill bundles vary between firms and not occupations. However, we transfer the idea to occupations and assume that skill bundles vary between occupations, because this approach is more consistent with the structure of our data. Moreover, the literature on human capital specificity suggests that occupational skill bundles are more tied to workers' compensation than firm-, industry-, and even occupation-specific skills (Gathmann and Schönberg, 2010; Poletaev and Robinson, 2008).

Thus, workers with different investments into $A$ and $B$ will have different productivity levels in different jobs depending on $\lambda_i$.

We assume that there are two time periods. The first period corresponds to the base period before any demand shocks occurred. The second period is characterized by an unexpected demand shock. In the first period workers chose jobs and invest in skills, i.e., they receive random assignments to jobs and skills $A$ and $B$. In the second period, they can switch to other jobs, and the number of job offers in the second period depends on the "thickness" of the market. A thick labor market is one in which workers receive many offers for a given amount of search effort and a thin market is one in which they receive few offers.

For simplicity, we normalize the wage in the first period to zero $w_0 = 0$. The wage in the second period is determined by a Nash bargaining framework. More specifically, at the beginning of the second period, a worker might leave his or her initial job and accept a new job that requires a different skill weight $\lambda_j$. This $\lambda_j$ is a realization of the random variable $\lambda$, which has the probability density function $f(\lambda_j)$, representing the distribution of skill weights (or jobs) in the labor market. If the worker would only get one draw from $f(\lambda)$ at the beginning of the second period; then, after comparing the wages in the new and the old job, the worker decides whether to switch to the new job with $\lambda_j$. The Nash bargaining solution thus implies that the worker's wage in the second period is:[2]

$$w_2 = \frac{1}{2}\{[\lambda_1 A + (1-\lambda_1)B] + [\lambda_j A + (1-\lambda_j)B]\} \tag{2}$$

What are the model's implications for the wages of workers with specific and general skill investments in the presence of unexpected negative—or positive—demand shocks? Consider the case in which the worker has invested such that $A > B$.[3] In this case, the worker prefers to work in jobs with a high $\lambda$, because these jobs use more of the worker's abundant skills and thus produce more output given the worker's skill combination. With each additional draw of $\lambda$, the worker has a chance to find a higher-$\lambda$ job, which improves his or her outside option. The highest ex-

[2] As the worker's productivity in job 1 (in firm 1) is $y_1 = \lambda_1 A + (1-\lambda_1)B$, firm 1 would be willing to pay up to this amount for the worker's services. Thus the disagreement outcomes are the productivity of the worker in current firm 1 and the productivity of the worker in firm $j$, i.e., the worker's outside option, which is drawn at the beginning of the second period. Although the worker will move to the firm that makes the most efficient use of his or her skill bundle, the same wage will be paid in both the new and the old firms. If the worker's productivity is higher in the old firm, the worker stays, and the new firm constitutes his or her outside option. If the worker's productivity is higher in the new firm, then the worker changes, and the old firm constitutes his or her outside option.

[3] Although the model works the other way around for workers specializing in skill $B$, the logic is identical.

pected draw of $\lambda_j$ with $N$ draws can be written as $E(Y) = \int_0^1 (1 - F(y)^N)dy$ with $F(.)$ denoting the cumulative distribution function of $\lambda$ (see Online Appendix A). The expected wage in the second period with $N$ draws can thus be written as:[4]

$$w_2 = \frac{1}{2}\{\lambda_1 A + (1-\lambda_1)B + \Big[(\int_0^1 (1-F(y)^N)dy)\cdot A + (1-\int_0^1 (1-F(y)^N)dy)\cdot B\Big]\} \quad (3)$$

If we take the derivative of $w_2$ with respect to $N$, we obtain:

$$\frac{\delta w_2}{\delta N} = \frac{1}{2}\{-\int_0^1 (F(y)^N ln(F(y)))dy \cdot (A - B)\} \quad (4)$$

This expression is clearly positive, because $F(\lambda)^N lnF(\lambda)$ is negative. Moreover, the larger the difference between $A$ and $B$, i.e., the more unbalanced or idiosyncratic the worker's initial investment, the larger the derivative becomes. Thus, for a given investment, an unexpected increase in $N$ has a larger positive effect on wages for workers with more specific—idiosyncratic—investments than for workers with more balanced investments. In contrast, an unexpected decline in $N$ has a larger negative effect for workers with specific investments.

## 2.2 Implications for our empirical analysis

In our empirical analysis, we focus on exogenous changes in labor demand stemming from the trade shocks triggered by the fall of the Iron Curtain. Thus, the model's first period corresponds to the period before the fall of the Iron Curtain, and the second period corresponds to the period after the fall of the Iron Curtain. We proxy changes in the market thickness by changes in international trade volumes. For example, Lazear (2009) mentions that empirical proxies for changes in market thickness (job search costs and offer frequencies) might include business cycles or regional population densities. Thus, we assume that workers receive more job offers when exports increase (i.e., export exposure increases the demand for output, and workers receive more job offers) and fewer job offers when imports increase (i.e., import exposure reduces the demand for output, and workers receive fewer job offers). Thereby, we specifically consider that trade shocks can affect local job opportunities, either through the direct effect on the own industry, or through local spillovers (see e.g., Helm 2020).

Two key features distinguish this model from a classical model of human cap-

[4]As in the case with one single draw ($N = 1$), the wage is the same in both firms. However, in the case with multiple draws, a higher number of draws $N$ leads to a higher outside option and a lower probability that the worker stays with his or her initial option.

ital investments. First, workers do not have to change jobs to receive higher or lower wages. Given the Nash bargaining structure of our framework, for a wage increase/decrease to occur, it is sufficient that the increasing/decreasing number of job offers increases/decreases the workers' outside options. We analyze this feature in Subsection 5.5 by showing that immobile and mobile workers have virtually the same returns to specific skills in response to demand shocks.

Second, workers in our model cannot anticipate the trade shocks and therefore experience varying returns to specific skills in response to demand shocks. In contrast, in Lazear's original model workers build adequate expectations about the number of job offers in the future. Thus, workers adjust to expected demand changes and returns to specific skills do not change in response to them. Subsection 5.6 analyzes this feature by comparing the effects for workers in the early 1990's who were unable to anticipate the fall of the iron curtain with workers in the 2000's who already witnessed a decade of trade exposure from the east. We indeed find that specific skills only matter for the first sample but not for the latter.

# 3 Empirical strategy

This paper examines whether workers with specific skills are faced with different labor market consequences of accelerating international trade (imports and exports) than workers with general skills. Therefore, we analyze how workers in occupations with different combinations of skills—i.e., specific or general—adjust to import and export exposure on regional labor markets.

## 3.1 *Main specification*

We define regional labor markets according to the classification of the German Federal Office for Building and Regional Planning (BBR), a classification based on commuter flows between municipalities. This classification assigns geographic regions to (205) functional sub-economies (Kropp and Schwengler, 2011). Following Autor et al. (2013) and Dauth et al. (2014), we assign trade flows to regions according to their industry structure and define local labor market exposure to import competition ($ImE$) as follows:

$$\Delta ImE_r^{East \to D} = \sum_j \frac{L_{rjt_0}}{L_{jt_0}} \frac{\Delta IM_j^{East \to D}}{L_{rt_0}} \tag{5}$$

where $\Delta IM_j^{East \to D}$ stands for the change of industry $j$'s imports (i.e., imports of industry $j$'s final goods) from Eastern Europe and China to Germany between the periods $t_0$ and $t+10$. We divide this change by region $r$'s total labor force ($L_{rt_0}$) and weight the measure by region $r$'s share of total (national) industry employment at time $t_0$ ($\frac{L_{rjt_0}}{L_{jt_0}}$). We calculate an analogous measure for exports ($\Delta ExpE_r^{D\rightarrow East}$). The variation of our main explanatory variables stems from two sources: initial differences in manufacturing employment across regions and the specialization of import- or export-intensive industries within the local manufacturing sector.

We use these measures for trade exposure in the following regression equation:

$$\begin{aligned} Y_i = \alpha + \beta_1 \Delta ImE_{r(i)}^{East\rightarrow D} + \beta_2 \Delta ExpE_{r(i)}^{East\rightarrow D} \\ +(\beta_1^I \Delta ImE_{r(i)}^{East\rightarrow D} + \beta_2^I \Delta ExpE_{r(i)}^{D\rightarrow East}) \times Spec_{o(i)s(i)} + \tau Spec_{o(i)s(i)} \\ +\delta w_i + X_i^{'}\gamma + Z_i^{'}\theta + S_i + J_i + \epsilon_i \end{aligned} \tag{6}$$

where $Y_i = \sum_{t=1}^{t_0+10} Y_{it}/Y_{t0}$ denotes individual $i$'s cumulative labor market earnings over a 10-year period following the base year $t_0$. We normalize the sum by the workers base year earning ($Y_{t0}$), such that it measures earning changes relative to the workers' earning in $t_0$. The right hand side contains a full set of interaction terms between our trade exposure measures ($\Delta ImE_{r(i)}^{East\rightarrow D}$ and $\Delta ExpE_{r(i)}^{East\rightarrow D}$) and the measure for workers' demeaned (occupational) skill specificity in the base year ($Spec_{o(i)s(i)}$).[5] Section 4.2 describes the construction of $Spec_{os}$ in detail.

The coefficients of the interaction terms between workers' skill-specificity and trade exposure—$\beta_1^I$ and $\beta_2^I$—are the coefficients of main interest, and they measure the extent to which the consequences of international trade differ for workers with general and specific skills. First, they measure the extended consequences of positive demand shocks in response to increasing trade exposure, i.e., they also include any effects that occur after workers have resorted into different occupations, regions, or industries after the base year. Second, we consider that the trade demand shocks induce spillovers between industries in the same labor market region, i.e., even if workers are employed in firms or industries that are not directly affected by trade shocks, they may benefit or suffer from demand shocks to other firms and industries in the region. These spillover effects can be either caused by changes in outside options (Paul Beaudry et al., 2012) or agglomeration effects (see e.g., Dix-Carneiro and Kovak, 2017 or, for evidence from Germany, Helm, 2020). However, Section 5.7 additionally presents all specifications on the industry level and the results remain qualitatively the same.

The remainder variables indicate further control variables that we measure in

[5] We define an individual's occupational skill specificity on the basis of the occupation ($o$) the individual practiced in 1990 and on the federal state ($s$) level.

the base year $t_0$ to avoid bad controls. $w_i$ describes the workers' annual earnings in the base year.[6] The vector $X_i$ contains individual-level controls for the worker's age, nationality, and education level in three categories (i.e., no secondary schooling, apprenticeship, tertiary education). The vector $Z_i$ contains firm-level controls (plant-size). $S_i$ denotes federal state (i.e., Bundesland) fixed effects.

Finally, vector $J_i$ controls for four broad industry groups (consumer goods, production goods, capital goods, and other goods). Trade shocks are likely to have different effects for different industries even if those industries are located within the same region. In an additional specification, we control for 198 granular industry categories. As the effect of the own-industry shock on earnings changes is absorbed in this specification, the trade variables do not capture the overall regional effects of trade anymore. Instead, this specification only picks up the aforementioned spillover effects from other local industries, i.e., changes in outside options or agglomeration spillovers. To allow for correlation in error terms of workers originally employed in the same labor market regions, we cluster the standard errors at the regional (BBR) level.

## 3.2 *Identification of interaction terms* $\beta_1^I$ *and* $\beta_2^I$

That OLS estimations of the isolated terms $\beta_1$ and $\beta_2$ are biased if $\Delta ImE_r^{East\to D}$ and $\Delta ExpE_r^{East\to D}$ are endogenous or contain measurement error is well established in the literature. On one hand, changes in import exposure may correlate with domestic demand shocks to German industries, so that workers' income and changes in import exposure might correlate with unobserved shocks to product demand. On the other hand, workers' incomes and changes in export exposure may correlate with unobserved shocks to product supply. Moreover, as we assign trade exposure to regions based on the regional industry structure, we have good reason to believe that $\Delta ImE_r^{East\to D}$ and $\Delta ExpE_r^{East\to D}$ suffer from measurement error.

To overcome these sources of bias, we can simply follow the common solution to this problem and instrument the increase in trade exposure from Eastern Europe and China to Germany with the trade between these low-wage countries and other "third-party" high income countries (e.g., Autor et al., 2013; Dauth et al., 2014;

[6] If earnings are closely related to workers' productivity, $w_i$ should capture most of the workers' time-constant unobserved productivity differences that may persist between workers in the base year.

Helm, 2020).[7] More specifically, we construct the following instruments:

$$\begin{aligned}\Delta IV\_ImE_r^{East\rightarrow D} &= \sum_j \frac{L_{rjt-3}}{L_{jt-3}} \frac{\Delta IM_j^{East\rightarrow Other}}{L_{jt-3}} \quad \text{and} \\ \Delta IV\_ExpE_r^{East\rightarrow D} &= \sum_j \frac{L_{rjt-3}}{L_{jt-3}} \frac{\Delta EXP_j^{East\rightarrow Other}}{L_{jt-3}}\end{aligned} \tag{7}$$

Where $\Delta IM_j^{East\rightarrow Other}$ and $\Delta EXP_j^{East\rightarrow Other}$ denote the trade change between Eastern Europe, China and other "third-party" high wage countries (in the observation period 1990-2000).[8]

Recent literature shows that identification in shift-share designs requires either exogeneous industry shocks (and takes industry shares as given, Borusyak et al. 2022) or exogenous industry shares (taking industry shifts as given, Goldsmith-Pinkham et al. 2020). Given the high industry-level variation in the timing and intensity of the Eastern Europe/China trade shock (see Section 5), we follow Autor et al. (2014) in arguing that our story is more consistent with import shock exogeneity rather than employment share exogeneity. Moreover, Barth et al. (2020) show that estimates of the effect of trade on regional employment in Europe are identified primarily by the import shocks. To check for industry-level orthogonality, Borusyak et al. (2022) recommend regressing current shocks on past outcomes. We validate our results by undertaking this falsification-test in Subsection 5.2.

However, in our case the coefficient estimates of main interest are not the isolated coefficients for trade exposure but the interaction terms between trade exposure and workers' human capital specificity ($\beta_1^I$ and $\beta_2^I$). The identification of these interaction terms additionally requires that the workers' skill specificity ($Spec_{os}$) and the omitted variable(s) are jointly independent of the instruments. Nizalova and Murtazashvili (2016) show that if the source of heterogeneity and the omitted variable(s) are jointly independent of an exogenous variable, then the OLS estimate of the interaction term between the exogenous and the endogenous variable is consistent. Thus, our first stage and reduced form estimates for the interaction terms between our instruments and our measure for workers' skill specificity will be consistent if $(Spec_{os}, \epsilon_i)$ is jointly independent of our instruments—even if $Spec_{os}$ correlates with

[7] We use the same instrument countries as Dauth et al. (2014): Australia, Canada, Japan, Norway, New Zealand, Sweden, Singapore, and the United Kingdom. To mitigate any possible simultaneity bias, we follow the literature and use lagged employment (3 years before the start of the period) to construct the instrument and apportion trade flows from the East to the labor market regions. Using contemporaneous employment shares to construct the instrument has no significant effects on our results.

[8] In order to tackle the problem of workers sorting across industries in anticipation of future trade exposure, we distribute the trade values across German regions according to lagged $(t-3)$ industry employment shares.

$\epsilon_i$. As a result, the 2SLS estimates of $\beta_1^I$ and $\beta_2^I$ will be consistent,[9] because 2SLS estimates are a combination of the reduced form and the first stage estimates.[10] See Online Appendix B for a more detailed discussion of this identification assumption.

$(Spec_{os}, \epsilon_i)$ may not be jointly independent of the instruments if workers' job choice and human capital investments depend on their expectations about the future developments of international trade. For example, workers may have chosen whether to invest in general or specific skills in response to the expected trade exposure in their sector. More able workers may have expected increasing international trade to result in lower job stability in certain industries. Given such expectations, and wanting to reduce the negative consequences of potential job loss, they may have chosen occupations and industries that demand less specific human capital. If this selection were to occur, $(Spec_{os}, \epsilon_i)$ would not be jointly independent of $\Delta ImE_r^{East \to D}$ and $\Delta ExE_r^{East \to D}$ and potentially would also not be jointly independent of the instruments that correlate with $\Delta ImE_r^{East \to D}$ and $\Delta ExE_r^{East \to D}$. As a result, our estimates of $\beta_1^I$ and $\beta_2^I$ would be biased.

To overcome this source of bias, we base our analysis on a restricted sample that only includes West German workers who chose their jobs before 1990.[11] Before 1990, the German population was unable to foresee the fall of the Iron Curtain, which triggered trade between Germany and the former Soviet bloc countries in the 1990s and 2000s (Chevalier and Marie, 2017; Fuchs-Schündeln, 2008). Thus, we can plausibly assume that, before 1990, workers did not choose their jobs in anticipation of the rapid globalization of trade throughout the 1990s and 2000s. Indeed, a variety of papers have exploited this historical setting as a quasi-natural experiment (e.g., Huber and Winkler, 2019; Brülhart et al., 2012; Glitz, 2012; Redding and Sturm, 2008).

We run our main regression equation (6) on this restricted sample and we instrument $\Delta ImE_r^{East \to D}$, $\Delta ExE_r^{East \to D}$, $\Delta ImE_r^{East \to D} \times Spec_{os}$, and $\Delta ExE_r^{East \to D} \times Spec_{os}$ by $\Delta IV\_ImE_r^{East \to D}$, $\Delta IV\_ExpE_r^{East \to D}$, $\Delta IV\_ImE_r^{East \to D} \times Spec_{os}$ and $\Delta IV\_ExpE_r^{East \to D} \times Spec_{os}$. In other words, we use interaction terms between workers' skill specificity and the "third-party" trade as additional instruments (see Amodio and Martinez-Carrasco, 2018 and Aghion et al., 2005 for similar approaches).

[9] Bun and Harrison (2018) rely on the same argument to show that interaction terms between endogenous and exogenous variables can be used as instruments for those endogenous variables.

[10] See Acemoglu et al. (2004) for a similar argument. They exploit the mobilization of World War II for analyzing how female labor supply effects the wage structure in the mid-century. Their main specification includes an interaction term between a time variable that may be related to other unobserved time trends and a variable for state-specific female labor supply that may be related to other unrelated state-specific effects.

[11] We do not have data on East German workers before 1991 and thus cannot include them.

# 4 Data and measures

This section describes our data sources and the creation of our measures. For our analysis, we use three main data sources. First, we use individual data from the Integrated Labour Market Biographies (IEB). Second, to construct our specificity measure, we merge the IEB data with a skill database from the BERUFENET. Third, to measure regional import and export exposure, we merge the IEB data with information from the United Nations Commodity Trade Statistics Database (Comtrade).

## 4.1 *Employment histories*

The data for workers' labor market outcomes stems from the Integrated Employment Biographies (IEB) provided by the German Federal Employment Agency (BA). The data contains precise register information about the employment histories of all employees required to make German social security contributions (i.e., all German employees who are not self-employed or civil servants). Unique personal and establishment identifiers identify all individuals and establishments, so that we can follow all workers and establishments over more than 40 years. The data contain labor market information about workers' employment status, wages, education, establishments, occupations, and the location of their workplaces. It also contains standard demographic information such as age, gender, and nationality.

We restrict our sample to male West German[12] employees who held a stable full-time job for at least 300 workdays in the base year of 1990,[13] and we follow these workers throughout the observation period (1990–2000). We focus our analysis on men, who exhibit more stable labor market patterns than women. Women also have more missing spells in the data and as our results reveal that assigning zeros when workers have gaps in the data adds a substantial amount of measurement error, we focus our main results on men. However, we present results for women in the robustness section.

As mentioned in the previous section, we can plausibly assume that in 1990 workers were unable to foresee the trade integration of Germany and Eastern Europe.

[12] Although we can follow West German employees and firms from 1975, we can only follow East German employees and firms from 1991.

[13] The workers must be reported as full-time workers by their main employer at least once during the base year. Additionally, we require that they have a strong labor force attachment (i.e., are employed for at least 300 days of that year) and earn more than the marginal part-time income threshold. This definition may include workers with interrupted employment, such as workers on sabbatical, on maternity leave, or on sick leave. We do not include workers registered as apprentices.

Most previous papers on the labor market effects of international trade also analyze the period between 2000 and 2010, which spans China's entry into the WTO. However, previous research on Germany shows that the trade integration of Eastern Europe had much stronger consequences for the labor market of West Germany than China's entry to the WTO (Dauth et al., 2014). The reason is that Germany already tended to import labor-intensive goods from Eastern Europe in the 1990s, and China's entry into the WTO mainly led to a diversion of German import flows from other countries. As a result, the workers' job choices in the 2000s had already been a consequence of increasing international trade throughout the 1990s (see Simon, 2018, for evidence that German workers chose apprenticeship training occupations with more specific skills when they were hit by trade shocks in the 1990s). Therefore, our main results rely on the period between 1990 and 2000. However, we do show results for the period between 2000 and 2010 in Section 5.4.

We further follow Dauth et al. (2021) and Autor et al. (2014) by applying two additional data restrictions. First, to ensure that workers had finished their entire education before entering the sample and were below the legal retirement age of 65 throughout the entire 10-year observation period, we restrict our sample to only those workers who were between ages 22 and 54 in the base year 1990. Second, we exclude individuals who died or emigrated during the 10-year window after the base year.

For all remaining workers, we create balanced panels capturing their employment histories for the entire ten-year period after the base year. As inactivity, unemployment, or early retirement may be consequences of accelerating international trade, we include periods with no labor market income as zero earnings. Thus, we also assign zero labor earnings if workers had gaps in their observed employment histories because the majority of the missing values are due to inactivity or early retirement.[14] We then calculate the total annual labor earnings (measured in 2010 Euros) for each worker by multiplying his daily wages by the total duration of all employment spells in that year.[15]

Although the employment and earnings data are highly reliable, because the BA collects this information for calculating social security benefits, the data have three minor limitations. First, the education variables are sometimes inconsistent

[14] Although this approach is common in the literature, it may overstate the negative consequences of trade shocks, because workers who have gaps in their employment histories may have since become civil servants or become self-employed. Thus, to emphasize the robustness of our results, we additionally present results for which we excluded workers with gaps in their employment histories.

[15] We do not include earnings from employment data that cannot be observed for the entire observation period, e.g., marginal employment.

and contain missing values.[16] To eliminate these inconsistencies and to impute the missing values of the education variables, we follow the standard approach of previous studies (e.g., Dustmann et al., 2009) and apply the imputation procedure of Fitzenberger et al. (2006).[17] Second, the earnings data are censored (top-coded) for high-wage workers at the annual German social security contribution ceiling, which applies to approximately 10 percent of all workers. To impute the missing upper tail of the wage distribution, we again follow the standard approach in the literature (e.g., Card et al., 2013) and apply a two-stage stochastic imputation procedure.[18] Third, as a result of the regulations for data protection and server restrictions of the Institute for Employment Research (IAB), which provided us with the data, we have access to a 52 percent random sample of the target population for this study.

## 4.2 *Skill data and specificity*

We measure human capital specificity at the occupational level by approximating the transferability of skill bundles across occupations. We focus on occupational skill bundles because the literature on human capital specificity suggests that occupational skill bundles are more tied to workers' compensation than firm-, industry-, or even occupation-specific skills (Gathmann and Schönberg, 2010; Poletaev and Robinson, 2008).[19]

More specifically, we use skill data from the BERUFENET database, an expert database and information portal of the German Federal Employment Agency (Bundesagentur für Arbeit, BA). The BERUFENET data, which is very similar to the U.S. O*NET data, contain information on the required skills, equipment

[16] As the BA does not need this information for administrative purposes, it records these variables with lower quality than the earnings and employment variables.

[17] Specifically, we perform an imputation in the style of the IP1 procedure described in Fitzenberger et al. (2006). If an individual is observed in multiple parallel spells in the same period, we assign all observations to the individual's highest education category. As a worker's highest education cannot decline over time, we then forward extrapolate an individual's highest educational degree to all following spells. Additionally, in case of missing data, we write an individual's degrees back to the age when these degrees are typically obtained (as observed in the data).

[18] In a first stage, we fit a series of Tobit models for each year and education group. In a second stage, we calculate the imputed values for each censored observation using the estimated parameters of the first-stage models plus a random draw from the associated (left-censored) distribution. The control variables contain the worker's gender, age, $age^2$, a dummy for "older" individuals, tenure, and tenure squared. We then use these imputed values for a second round of imputations, where we include each worker's average log wage in all other periods and the average annual wage of his current co-workers (leave-out means). If a worker is only observed once, we set his mean wage in all other years to the sample mean and include a dummy in the model. Similarly, we set the wage of the co-workers to the sample mean and include a dummy if a worker is the firm's only employee.

[19] Earlier studies suggested that industry and occupation-specific skills are more important for workers' wage development than firm-specific human capital (Gibbons et al., 2005; Kambourov and Manovskii, 2009; Neal, 1995; Parent, 2000).

used, working conditions, and required qualifications for all occupations in Germany. Since 2003, the BA has been building the BERUFENET for career guidance and job placement and has continuously updated the data. Thus, we assume that workers possess the skills that experts consider essential for performing the required tasks in their occupation. To date, the data contains approximately 3,900 (8-digit) occupations (Dengler et al., 2014).

To measure the skill requirements of occupations, BA experts collect data on occupational skill requirements and qualifications from training or study guidelines and from applications and job offers. Using this information, the experts assign a bundle of core skills (i.e., skills essential for performing the relevant tasks in an occupation) to each occupation.[20] Thus, overall, the data contains information on approximately $8,000$ different skill items. Table 1 gives an example by listing the core skills for the occupations "tool mechanic," "precision mechanic," and "construction carpenter."

Following Matthes and Vicari (2017), we used the BERUFENET data to calculate the distances between all pairs of occupational skill bundles in the German labor market by calculating the Euclidian distance between their respective skill vectors.[21] These distances depend on the total number of skills in each occupation, and the number of shared skills between two occupations. In the example in Table 1, the skill distance between tool- and precision mechanics is smaller than that between tool mechanics and construction carpenters, as tool mechanics share four of their nine core skills with precision mechanics but only one skill with construction carpenters.

—Table 1 about here—

Gathmann and Schönberg (2010) and Poletaev and Robinson (2008), for example, have calculated similar distance measures and have directly used them to assess how transferable skills are between different occupations. However, for the purpose of our study, we calculate a weighted average skill distance from any one occupation to all other occupations in the labor market (in the base year $t_0$). This average distance represents our measure of specificity of an occupational skill bundle, according

[20]The BA uses the German word "Kompetenzen" (competencies) for skills.

[21]Occupations are classified at the 3-digit level plus a one-digit indicator for (at most) four "requirement levels." These requirement levels represent the complexity of the tasks and the education level that is commonly required to work in the occupation. Together, these two dimensions result in 422 $(3 + 1)$-digit combinations and 88,831 pairwise distance measures.

to Lazear's 2009 model:

$$Spec_{os} = \sum_{p=1}^{N} dist_{op} \cdot \frac{L_{ps\,t_0}}{L_{s\,t_0}} \tag{8}$$

where $dist_{op}$ is the skill distance between a given occupations $o$ and a second occupation $p$, and $\frac{L_{ps}}{L_s}$ is the relative employment share of occupation $p$ (in year $t_0$ and federal sate $s$) to weight the skill distances by the number of alternative jobs available to a worker. While the simple distance measures of previous studies only allowed assessing how transferable skills between different occupations are, our index measures the overall demand for the skill bundle of the respective occupation $o$ (in the worker's federal state $s$).

Because the index of equation (8) takes into account that the specificity of workers' skills is endogenous to the use of skills in the market, the index closely follows the theoretical concept of Lazear's skill weights approach. For example, an individual with a skill bundle used in only very few occupations might still be very general if those few occupations demanding similar skills (including the worker's own) are large and offer a considerable number of jobs. As a result, our specificity measure correlates with the size of a worker's occupation, because the distance to jobs in the same occupation is zero. However, our measure does more than simply reflect differences between small and large occupations. Indeed, the correlation between the size of occupations and skills specificity is only moderate (Pearson correlation 0.55). Most of the variations stem from the variation of skill distances. Thus, smaller occupations are not necessarily more specific than larger ones. For example, very small occupations, such as pharmacist, are general, while large occupations, such as building construction worker, are specific.

We calculate our measure for skill specificity on the level of federal states instead of calculating it at the level of local labor markets (the level on which we calculate the trade exposure), because a specificity measure calculated at a more desegregated regional level is strongly driven by local industry clusters that also drive the trade shocks. For example, machining metal operators who only share a limited skill set with other occupations become very general as these workers tend to cluster in manufacturing regions. In contrast, commercial workers, who tend to cluster less, become substantially more specific although they share a large set of skills with many other occupations. Nevertheless, Appendix A shows results for which we have used a specificity measure that is calculated on the level of local labor markets.

Table 2 displays the 10 most and the 10 least specific occupations.[22] Both groups

[22] Table 2 excludes occupations with an employment share of less than 0.1% in our main sample.

of occupations contain both low- and high-wage occupations. For example, while the group of occupations with the least specific skill requirements contains low-wage occupations, such as sales assistants, and high-wage occupations, such as pharmacists, the group of occupations with the most specific skill requirements contains journalists and floor layers.

—Table 2 about here—

Table 3 displays the distribution of skill specificity for seven broad occupational classes. While occupations in construction/building and natural science/IT appear to have on average the most specific skill requirements, occupations in business/administration/law and commerce/tourism appear to have the least specific skill requirements. However, the distributions differ widely between those occupational classes. For example, the results reveal relatively wide distributions for both general and specific occupations for traffic/logistics and manufacturing. In contrast, occupations in construction/building are primarily specific, while the occupations in business/administration/law are primarily general.

—Table 3 about here—

Workers in the same occupations can work in different industries. Therefore, Table 4 shows the distribution of workers' skills specificity over five broad industry classes. The table reveals that workers in energy/construction have by far the most specific skills, while workers in trade/maintenance have on average the most general skills. Workers in manufacturing and services have a skill distribution relatively similar to the overall distribution, with a mean close to zero and a relatively large variance. The main reason for this outcome is that these sectors are by far the largest in the German economy. However, the results also show that a great variety of workers with different skills work in these sectors. For example, the manufacturing sector contains not only blue-collar workers, whose skills are on average specific, but also workers, such as office clerks, whose skills are on average general.

As previously mentioned, we calculate the specificity of individuals' skill bundles based on the occupations they were practicing in the base year (1990), implicitly assuming that individuals meet the typical skill requirements of an occupation (according to BERUFENET). Our measurement is inaccurate if there is a mismatch between workers occupations and skill sets, for example, workers might be over- or under-qualified for the occupation they were practicing in the base year. We expect

Occupations with less than 0.1% are rather unusual occupations, such as, for example, equine managers that might be very specific but, given their small size, will not drive our results.

the skill measurement error to bias our estimates towards zero. In Online Appendix H we thus perform robustness checks with a skill specificity measure that takes into account an individual's occupational history. However, our main results are very robust to this alternative specification.

—Table 4 about here—

### 4.3 *Trade data*

To measure trade shocks, we use trade data from the UN Commodity Trade Statistics Database (Comtrade). These data provide information on trade flows between more than 170 countries and contains detailed information on commodity types. To merge the trade data with our labor market data, we follow Dauth et al. (2014) who use a crosswalk from the UN Statistics Division that allows to link each product category in Comtrade (consisting of 1031 SITC rev. 2/3 product codes) to one of the NACE industries in our labor market data. This crosswalk allows to unambiguously assign 92 percent of all commodities to single industries. To calculate trade values for the remaining commodities, we use national employment shares from 1978 to calculate weighted averages of trade values across multiple industries. In line with Dauth et al. (2014), we drop all workers in industries related to the primary sector and fuel products, because these industries are subject to specific trade restrictions. These data restrictions leave us with detailed trade data on 97 NACE (WZ73) manufacturing industries.

## 5 Results

### 5.1 *Descriptive statistics*

Table 5 shows the descriptive statistics we use to analyze the differences in observable characteristics between workers with very specific and very general skills. For this purpose, we divided our sample into two sub-samples. The first sub-sample contains the 33 percent of workers with the most general skills; the second one contains those 33 percent with the most specific skills. The third column shows the differences between both sub-samples, and descriptive statistics for the entire sample appear in the fourth column of the table.

—Table 5 about here—

The first row reveals that average base year earnings of workers with the most specific skills are significantly larger than for those with the most general skills. In

1990, workers with the most specific skills earned approximately 3,715 Euros (i.e., approximately eight percent) more than those with the most general skills. On one hand, more able workers may have self-selected into occupations with a more idiosyncratic demand for skills (see Neal, 1998 for a similar argument). On the other hand, workers with specific skills are more likely to earn above market wages, because they commonly share the returns to their investments in specific human capital with their employers (Becker, 1964). Thus, even in the absence of ability differences, human capital theory predicts that workers with specific skills should earn more than those with general skills (Eggenberger et al., 2018). Moreover, the results reveal that workers with specific skills are (a) more educated and (b) somewhat less likely to have German citizenship. The last row shows the average local skill specificity for workers with the most and least specific skills. The results reveal a slight tendency that workers with very specific skills cluster in other regions than those with very general skills. However, although the difference in the mean local skill specificity is significantly different from zero, it is, with only a 10'th of a standard deviation, very small.

Figure 1 shows the development of total imports and exports between the East (i.e., Eastern Europe, and China) and Germany between 1980 and 2000. The figure reveals that German trade with the East was negligible before the end of the 1980s. In 1990, German imports from and exports to the East suddenly increased, and the growth in trade persisted through 2000. For example, German exports to the East increased from approximately 18 billion Euros in 1990 to approximately 70 billion Euros in 2000. Imports increased at similar magnitudes. Thus the data clearly indicates a shock in the development of trade after the fall of the Iron Curtain, a shock that workers were unlikely to have been able to anticipate.

—Figure 1 about here—

Figure 2 shows the regional variation of workers' exposure to increasing imports and exports between 1990 and 2000 and the regional variation of workers' skill specificity in 1990. Panels A and B show how exports and imports increased on average per worker across the different labor market regions of West Germany. We created the measures for average trade exposure per worker by combining the trade data with the industrial structure of the labor market regions (see Section 3). Between 1990 and 2000 imports increased on average by 2,967 Euros per worker and exports by approximately 2,680 Euros.

However, both maps reveal considerable variation in exports and imports across regions. Moreover, although import and export exposure are strongly correlated across regions, the figure still reveals substantial variation between export and import-oriented regions. For example, the labor market region of Bremerhaven, now a region with high rates of unemployment, was strongly exposed to import exposure (i.e., imports increased by approximately $2,700$ Euros per worker) while exports increased by only approximately 470 Euros. In contrast, Bodensee, a region that has become famous for hosting many small and medium-sized technology-companies, experienced a similar increase in imports ($3,600$ Euros), but—with approximately $5,150$ Euros per worker—a much stronger increase in exports.

—Figure 2 about here—

Panel C of Figure 2 shows the regional variation of workers' skill specificity in 1990. Although the figure reveals some regional variation in workers' skill specificity, only a few regions with an average skill specificity that lies above or below one standard deviation of the mean exist (we standardize our measure for skill specificity).

## 5.2 *Main identification assumption: joint independence and pre-trends*

Our main identification assumption is that $(Spec_{os}, \epsilon_i)$ is jointly independent from our instruments. As in regular instrumental variable approaches, we cannot empirically test whether this assumption is true. However, the two following analyses provide additional support for our main identification assumption.

First, if workers' skill specificity correlates with both the error term and our instruments, $(Spec_{os}, \epsilon_i)$ cannot be jointly independent from our instruments. As $(Spec_{os}, \epsilon_i) \neq 0$ is very likely, orthogonality between workers' skill specificity and our instruments for trade becomes a necessary condition. Therefore, Table 6 provides the results from a regression of workers' skill specificity on our instruments for trade exposure. Column I shows the results without control variables, and column II shows the results with control variables (as described in Section 3). For completeness, columns III and IV also show regressions of the workers' skill specificity on our measures of regional trade exposure. All coefficient estimates are very small (i.e., no coefficient estimate is larger than 0.01 standard deviations of the dependent variable) and not significantly different from zero at conventional levels. Thus, the results support the validity of this condition.

—Table 6 about here—

Second, workers with different levels of skill specificity living in different labor market regions might have had different levels of wage-growth, even before our examination period. If our results would be driven by pre-existing long-run differences between workers with different levels of skill specificity $\epsilon_i$ would be likely to correlate with our instruments and $(Spec_{os}, \epsilon_i)$ would jointly depend on them. Therefore, we conduct a falsification exercise as suggested by Borusyak et al. (2022) and regress *past* changes of workers' wage growth on *future* changes in their trade exposure (and the respective interactions with their skill specificity in 1990).

Figure 3 shows the trade coefficients and 95% confidence intervals of nine separate regressions that relate a worker's cumulative earnings before the trade shock ('8$x$-'90 pre-period earnings) to the trade exposure of the worker's 1990 labor market region ('90-'00 trade shock). The coefficients for the trade shock and the specificity interactions are close to zero and insignificant for all pre-period estimations. Thus, these results provide no evidence that workers with different levels of specificity living in different labor markets regions in 1990 had different levels of wage growth in the 1980s.

—Figure 3 about here—

### 5.3 *Main results: 2SLS estimates*

This section shows the estimates of the relationship between earnings effects of trade shocks and workers skill specificity according to variants of the two-stage least square model (2SLS) presented in Section 3. The estimations in Table 7 stem from a sample that followed workers between 1990 (the base year) and 2000. The table reports the estimated effects of trade exposure on the cumulative labor earnings between 1990 and 2000. Thus, the job (or the occupation, respectively) and region of the base year 1990 determine the skill specificity and the trade flows that we assign to each worker in this sample. To instrument trade exposure of German industries, we use trade exposure from other high-income countries.

—Table 7 about here—

The first column of Table 7 starts with the most parsimonious specification. The specification includes only our core variables of regression equation (6) and the workers' earnings in the base year to account for the workers' unobserved heterogeneity before the trade shock. The isolated coefficient estimates of import and export exposure show the effect for workers with an average level of skill specificity. As expected, the coefficient estimate for import exposure is negative, and the one for export exposure is positive. While the isolated coefficient for import exposure is marginally not significant, the one for export exposure is significantly different from zero at the 5 percent level.[23]

The coefficient estimates of the interaction terms—our estimates of main interest—show that the effects of unexpected trade shocks differ for workers with specific and general skills. Both coefficient estimates are precisely estimated at conventional levels and point in the expected direction. The coefficient estimate for the interaction term between workers' skill specificity and import exposure is negative, and the one between workers' skill specificity and export exposure is positive.

The lower part of the table reports the Sanderson-Windmeijer F-statistics for all three 2SLS specifications.[24] These F-statistics allow us to assess the power of instruments in regressions with more than one endogenous variable. All F-values are large and reveal that our instruments have strong predictive power for all specifications.

Specification II adds further firm-level controls (i.e., firm size and four broad industry groups) and region-specific fixed effects, and specification III adds individual controls, (i.e., education, age, and a dummy variable for German nationality). Adding firm-level controls and region-specific fixed effects barely changes the results, while the individual control variables reduce the size of the interaction coefficients a bit. However, both coefficient estimates of the isolated trade effects now turn significant at the 5 percent level.

One concern might be that workers with specific skills who suffer from negative demand shock work in different industries (e.g., industries with many low skilled workers) than those who profit from rising exports. The last column of Table 7 (specification IV) adds a battery of 198 industry dummies to cover industry-specific effects. Adding these industry fixed effects drives the isolated coefficient estimates for import and export exposure much closer to zero, and the effects turn insignificant.

The interaction terms of specification IV now measure how the effects of trade shocks differ for workers with specific skills relative to the average earnings development within these workers' own industries. Thus, the industry fixed effects capture average industry-specific earnings changes—including earnings changes stemming

[23] Also Dauth et al. (2014) find that the average effect of import exposure on workers' median wages is insignificant. In Table C.1 in Online Appendix C, we estimate our main specifications without including the specificity measure and the respective interaction terms. We find essentially the same results for the isolated coefficients of import and export exposure as in Table 7.

[24] The Sanderson-Windmeijer statistic adjusts for endogenous covariates that might be highly correlated with each other. The theoretical justification for the validity of this statistic relies on the assumption of homoskedastic errors. However, to the best of our knowledge, no other method addresses the problem of multiple endogenous regressors if standard errors are non-homoskedastic. Thus, the Sanderson-Windmeijer statistic is most frequently reported in settings with multiple endogenous regressors and non-homoskedastic errors (e.g., Helm, 2020; Akerman et al., 2022).

from industry-specific trade shocks. As a result, the interaction terms of specification IV are somewhat smaller than those of specification III. As we are interested in the overall effects of trade—including the effect on workers' own industry, specification III will remain our preferred specification instead of specification IV.

Let us, for example, consider a worker with a skill specificity level of approximately 0.63, which corresponds to the 75 percentile of our skill specificity measure and is similar to the skill specificity of workers in the field of technical research and development (specificity of 0.64) or workers in building construction (specificity of 0.59). For a 1000 Euro increase of export exposure between 1990 and 2000, we estimate that this worker's earnings increased by approximately 5.3 percent more than the average earnings of a worker at the mean level of specificity. Throughout the period between 1990 and 2000 exports increased by approximately $2,680$ Euros per worker leading to a total earnings mark up of approximately 14 percent of the worker's base year earnings.

In contrast, for a 1000 Euro increase of imports, the worker of our example loses approximately 2.7 percent more than the average worker in his or her industry. As imports increased by approximately $2,967$ the effect amounts to approximately 8 percent of his or her base year earnings. Instead, a worker with a low level of skill specificity (25 percentile $\approx -0.55$) gains approximately 12 percent less from increasing exports than the average worker in the same industry, but also loses approximately 7 percent less in response to increasing imports.

Online Appendix D visualizes the earnings effects for workers with specific and general skills and shows that workers with specific skills profit on average more from increasing international trade than those with general skills. However, they also experience larger wage inequality. Online Appendix E analyses whether our main earnings effects are rather related to changes of wages or employment opportunities. The results suggest that the largest share of the earnings effects stem from wage changes instead of changes in employment opportunities. This result is consistent with previous evidence by Dauth et al. (2017) who find that, in Germany, employment changes in response to increasing trade exposure are largely related to young workers' entry behavior and returnees from non-employment. Moreover, Dauth et al. (2021) do not find evidence that increasing trade exposure leads to more mass-layoffs or firm closures.

Online Appendix F shows the OLS and reduced form results, and it shows that our results hold when we restrict our sample to only include manufacturing workers or apprenticeship graduates. Moreover, we find very similar results for women. Online Appendix G shows that our results hold when we use an alternative trade exposure measure considering that trade effects might differ for occupations within regions. Finally, Online Appendix H shows that our results also hold when we use a measure for workers' skill specificity that takes into account that workers might have had multiple jobs before 1990.

There are two remaining concerns. First, high-ability workers, who are more capable of exploiting the rents from positive demand shocks and cope with the costs of negative ones, might have more specific skills than low-ability workers. In such a scenario $(Spec_{os}, \epsilon_i)$ would jointly depend on our instruments—even if $Spec_{os}$ would not correlate with them. However, in this scenario, the coefficient estimate $\beta_1^I$ for the interaction term between skill specificity and import exposure should be positive and *not* negative. Moreover, it is hard to come up with another unobserved factor that might correlate with skill specificity and lead to an upward bias for positive demand shocks but a downward bias for negative ones.

Second, between 1987 and 2001 approximately 2.8 million ethnic German immigrants came to Germany and increased the German population by approximately 3.5 percent (Glitz, 2012). If the immigrants from the former eastern bloc have self-selected into regions or industries with increasing trade exposure, the inflow of migrants might bias our wage effects. A classic migration model would predict that low-skilled migration, such as the one from the eastern bloc, puts pressure on the wages and employment of low skilled native workers whom they substitute. The migration effect for high skilled workers is more ambiguous and depends on the elasticity of substitution between skilled and unskilled workers and the substitutability between capital and labor (Dustmann et al., 2017). Thus, in theory, the inflow of East-European migrants might partially explain the negative wage and employment effects from import exposures as well as the positive wage and employment effects from rising exports.

However, although the literature documents quite large employment effect in response to migration from the east, they find no, or only small, wage effects. Moreover, the employment effects manifest predominantly among unskilled workers (e.g., Dustmann and Glitz, 2015; Dustmann et al., 2017; Glitz, 2012). In contrast, our analyses reveal strong wage effects that even manifest within educational groups as we show in Online Appendix D. These results appear to be at odds with the prediction from a migration model.

## 5.4 *Dynamic effects*

This subsection analyzes how the relationship between workers' skill specificity and trade exposure evolved over time. Therefore, Figure 4 shows the coefficient estimates of the interaction terms from regression equation (6) for ten different sub-periods. The first sub-period ranges from 1990 to 1991, the second from 1990 to 1992, and so forth until the tenth period, which ranges from 1990 to 2000. The coefficient estimates of Figure 4 stem from fully saturated specifications including the full set of industry dummies. In addition, Table I.1 in the Online Appendix presents all coefficient estimates in a table.

—Figure 4 about here—

In absolute values, the coefficient estimates of the interaction terms were relatively small immediately after the fall of the iron curtain and substantially increased throughout the mid 1990s to remain quite stable thereafter. Thus, we observe a strong response to the trade shocks in the medium-run after the fall of the iron curtain, but the effects remain relatively stable throughout the years between 1995 and 2000.

### 5.5 *Worker mobility and external labor market conditions*

Many previous studies have relied on involuntary labor mobility (e.g., firm closures and mass layoffs) to analyze the value of specific skills for workers' careers (Couch and Placzek, 2010; Hijzen et al., 2010; Jacobson et al., 1993). However, according to a Lazear type skill-weights model like ours, changes in labor demand should influence the returns to skills equally for movers and stayers—unless demand changes lead to unexpected and exogenous lay-offs that cannot be anticipated (see Lazear 2009, p. 933).

To analyze this idea, Table 8 presents six specifications that estimate our regression equation (6) for stayers and switchers. In this table, we compare the effects for workers who remain in their establishment, occupation, or labor market region and are continuously employed throughout the entire observation period[25] with the effects for workers who switch their establishment, occupation, or region. The first two specifications present the results for establishments, the second two for occupations, and the third two for local labor markets. Although restricting samples according to workers' labor mobility might change the composition of workers with respect to skill specificity, the results are remarkably similar for stayers and switchers.

—Table 8 about here—

[25] We specifically exclude workers who leave the labor market and return to their old firms and/or occupation, because these are likely to be workers on parental leave or workers who trained or studied.

Table 9 sheds more light on the relationship between demand shocks and worker mobility. The table presents results from equation (6) for which we have replaced the dependent variable by a dummy for (I) establishment switches, (II) occupation switches, and (III) switches of the local labor market.

—Table 9 about here—

Overall, we find very small and mostly insignificant effects of trade exposure on worker mobility—irrespective of whether workers have specific or general skills. Only for regional mobility, we find a positive coefficient estimate for the interaction term between import exposure and skill specificity that is marginally significant. This result provides weak evidence that the marginal worker tries to adjust to negative demand shocks by leaving his or her local labor market instead of their occupation. Instead, the results do not suggest that increasing import exposure led to a substantial number of exogenous layoffs. This conclusion is consistent with Dauth et al. (2021) who also cannot find evidence that import exposure increases mass layoffs and firm closures.

However, we might also be unable to detect mobility effects, because the effects of trade shocks on worker mobility are heterogeneous. On one hand, workers who are exposed to increasing labor demand might be poached by other firms, such that export exposure should increase worker mobility. On the other, hand firms might become more likely to match workers' outside offers, such that export exposure decreases worker mobility. Similar arguments apply for import exposure.

## 5.6 *Age and adjustment processes*

Young workers are more mobile than older workers. First, young workers' remaining careers last longer, such that new human capital investments and occupation changes are still efficient. Second, recent evidence suggests that young and inexperienced workers have a lower job-level match quality than older ones (Fredriksson et al., 2018). Thus, skill specificity should matter less for younger than for older workers. Third, young workers profit less from Germany's strong employment protection legislation than older ones.

Columns one and two of Table 10 replicate our main estimation for older ($> 40$ years) and younger workers ($\leq 40$). For older workers the results reveal precisely estimated coefficient estimates that are consistent with our main results. In contrast, the results for younger workers reveal a smaller and only marginally significant negative coefficient estimate for the interaction term between workers' skill specificity and import exposure. In addition, Online Appendix J replicates our analysis from Section 5.5 for young workers. The results reveal much stronger differences in earnings effects between young stayers and switchers. Particularly, for workers who switch their occupations and local labor markets. Moreover, young workers with specific skills become significantly more likely to switch their occupation and labor market when imports increase.

—Table 10 about here—

These results suggest that older workers might have higher adjustment costs than younger ones. On one hand, they might be better matched than younger workers. On the other hand, they might have invested more in specific human capital. Thus, the relationship between workers' skill specificity and increasing international trade might vanish in the long run. First, more and more older workers with high adjustment costs leave the labor market. Second, more young workers will enter the market, driving the relationship between workers' skill-specificity and trade exposure towards zero.

Therefore, the third column of Table 10 replicates our main specification for the period between 2000 and 2010.[26] This sample contains workers who have either experienced the consequences of increasing international trade with low-wage countries (Eastern Europe) for a decade or have entered the labor market during a period when these consequences were already known to them. The results reveal small and insignificant interaction terms suggesting that workers adjust in the long run.

## 5.7 *Regional-, versus industry-specific labor market thickness*

The model in Section 2 assumes that the demand shocks arising from trade exposure affect workers' earnings through their influence on local labor market thickness. However, the effects are also consistent with a model of industry-specific labor market thickness. Table 11 presents our results on the industry instead of the regional level. For this exercise, we have calculated all trade exposure measures on the level of 198 industry categories, i.e., the trade exposure is simply the future change in import and export exposure in the worker's industry of the base year 1990.

—Table 11 about here—

Columns I and II present results for which we have included workers of all industries in our sample. We assume a trade exposure of zero for workers in industries

[26]The sample for column five contains workers who were between 22 and 54 and held a stable full-time job in 2000. We follow these workers through 2010.

outside the manufacturing sector. The first specification includes the full set of controls from regression equation (6), obviously excluding the industry controls. All coefficient estimates point in the expected direction, i.e., the coefficient estimate on import exposure and the coefficient estimate of the interaction term between import exposure and skill specificity are negative. In contrast, the coefficient estimates on export exposure and the interaction term between export exposure and skill specificity are positive. However, only the latter two coefficient estimates are significantly different from zero. Column II additionally includes regional fixed effects for 205 local labor markets but the results barely change.

As the assignment of zero trade exposure to workers outside the manufacturing sector might substantially increase the measurement error, Specifications III and IV replicate our industry analysis on a sample that only includes manufacturing workers. While the magnitude and the sign of the coefficient estimates in III and IV barely change, we now find significant effects for both interaction terms—our coefficient estimates of main interest. Again, including fixed effects for the 205 local labor markets barely changes our results. The fifth specification includes both types of trade exposure with their interaction terms for skill specificity. The estimates of local and industry exposure barely change and the qualitative results remain very robust. This result is consistent with Autor et al. (2014), who also find that trade exposure at the industry and regional levels are largely orthogonal to each other, and that both levels of exposure appear to have a substantial impact on workers' earnings.

A common concern for shift-share estimators where the residual has a shift-share structure is that conventional standard errors might be downward biased, in our case due to correlations in industry level shocks across regions (Adão et al., 2019). An advantage of the industry-level regressions presented here is that they provide standard errors that account for the type of residual correlation discussed in Adão et al. (2019). Borusyak et al. (2022) show that by estimating a regression at the level of the shocks, one obtains valid ("exposure-robust") standard errors in the framework of Adão et al. (2019). Comparing the significance levels of the industry-level estimation thus allows us to infer if the standard errors in the regional-level estimations are likely to be biased. The significance levels for the industry- and the regional-level are very similar[27], thus, it seems unlikely that the regional-level

[27] Columns III and VI in Table 11 perform our main analysis using the industry-level exposure on a sample that only includes manufacturing workers (for workers outside the manufacturing sectors, i.e., Columns I and II, we have to assign zero trade exposure which might substantially increase the measurement error). Table F.1, Column III shows the same estimation for manufacturing workers on using the regional-level exposure. The results in Table 11 (industry-level exposure) go into the same direction as the ones in Table F.1 (regional-level exposure) and the significance levels are

confidence intervals are severely biased.

# 6 Conclusion

This paper analyzes the causal effects of negative and positive demand shocks on returns to specific skills by using variation from international trade shocks. Whereas previous studies showed that workers with specific skills experience larger earnings losses in response to negative demand shocks, our paper shows that they experience larger earnings gains to positive demand shocks. Our theory suggest that the heterogeneity that we uncovered is also relevant for economic shocks other than increasing international trade, such as technological change, a pandemic, or a financial crisis, all of which cause demand for some skills to decline and for others to increase.

However, such large demand shocks also produce larger heterogeneity among workers with specific skills than among workers with general skills. Our results demonstrate that the value of specific skills under shock consists on average of higher risks and higher returns. Moreover, the relationship between workers' skill specificity and increasing international trade helps to explain the rising inequality within education groups, as observed in many developed countries throughout the last decades.

Our results provide important insights for policy makers who want to reform education programs to prepare future workers for the increasing challenges of dynamic labor markets in a globalized world. The bundling of single skills in training programs (whether academic or vocational) is an important feature in this context and should be investigated more in future research. Whereas recent evidence might suggest that holding industry or occupation specific skill bundles has become more disadvantageous as labor markets have become more dynamic and workers more mobile, our results paint a different picture. We find that specific skills have higher net returns after a labor market demand shock, because they shield workers in occupations with increasing demand from labor market competition. However the returns to specific skill bundles are indeed more heterogeneous than the returns to general skill bundles.

very similar.

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# Figures in the text

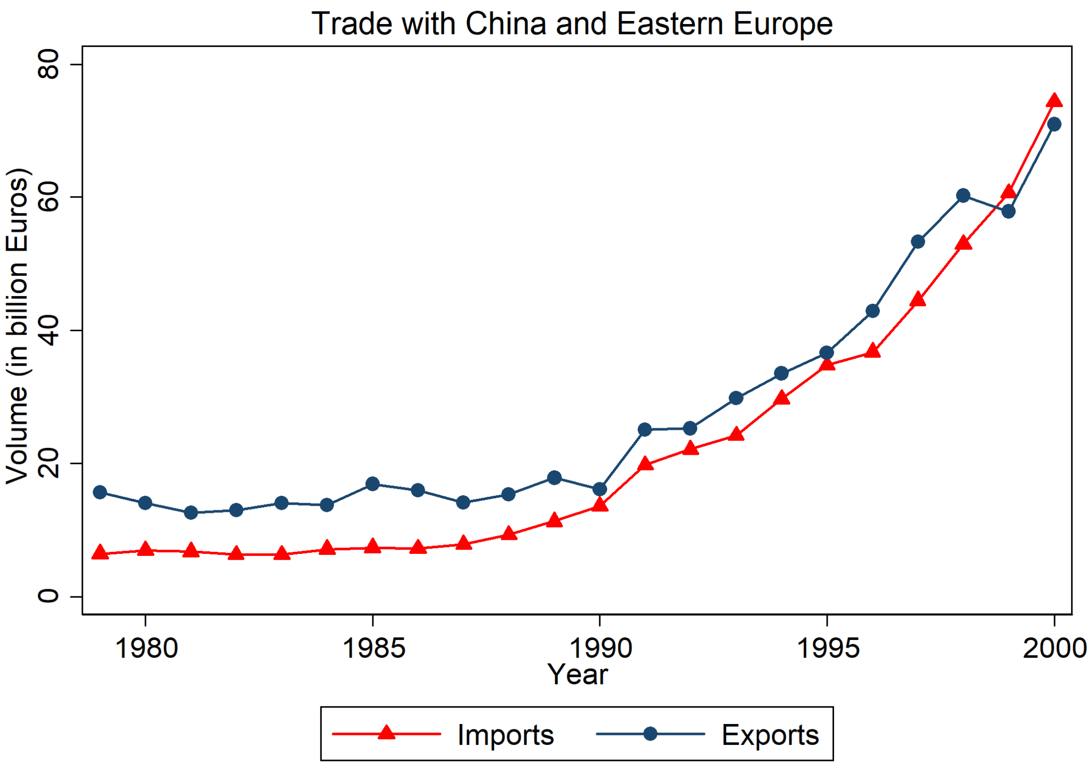


Figure 1: German Trade Volumes in Billion Euros

The figures show the development of imports and exports in commodities from Germany to Eastern Europe and China (excluding goods assigned to the primary sector).

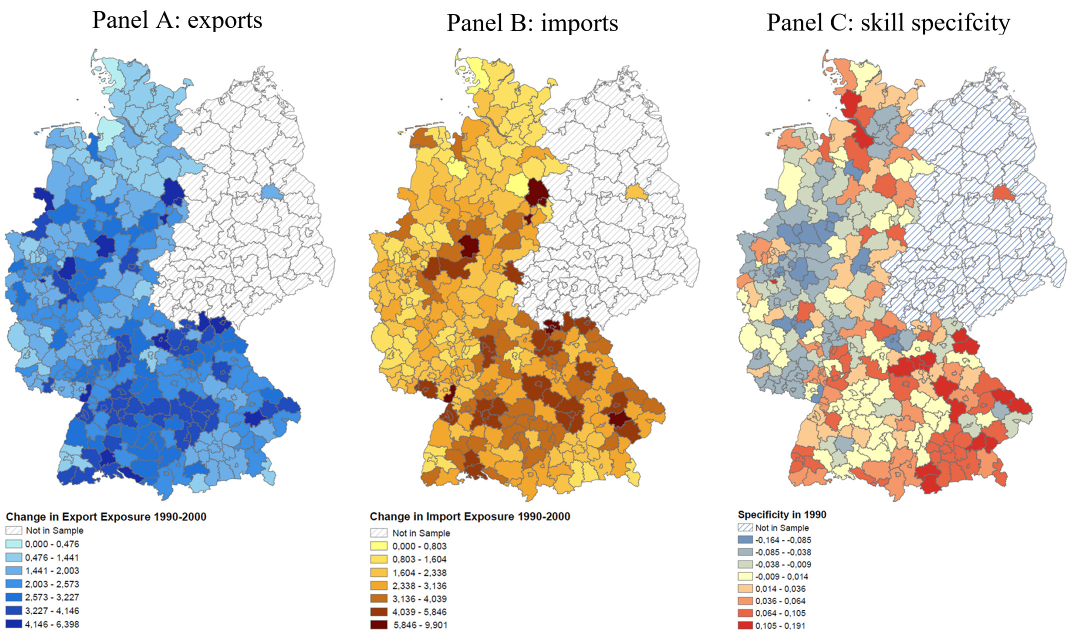


Figure 2: Change in Regional Import and Export Exposures per Worker (1990-2000)

Increase in imports (Panel A) and exports (Panel B) from China and Eastern Europe, 1990-2000 in 1000€ per worker. Panel C: distribution of average occupational specificity (standardized).

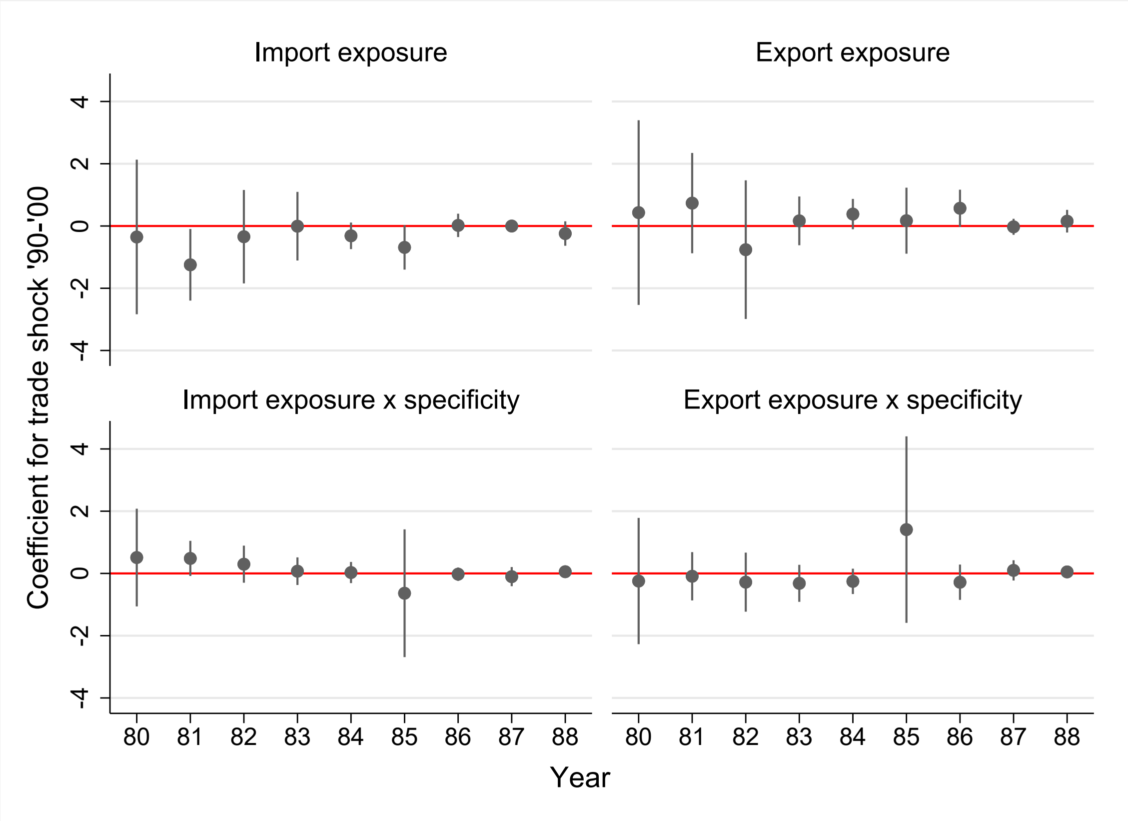


Figure 3: Pre-trends Falsification Tests

The figure reports the trade coefficients and 95% confidence intervals of nine separate regressions that relate a worker's cumulative earnings before the trade shock ('8x-'90 pre-period earnings) to the trade exposure of the worker's 1990 labor market region from 1990 to 2000 ('90-'00 trade shock). The dependent variable is the cumulative earnings that a worker obtained from the year indicated on the x-axis until 1990, normalized by the worker's annual earnings in the respective base year. The trade exposure is always measured as the increase of the trade flows between 1990 and 2000 (analogously to our main estimations). 2SLS regressions. All specifications include the control variables from Table 7, Column (IV).

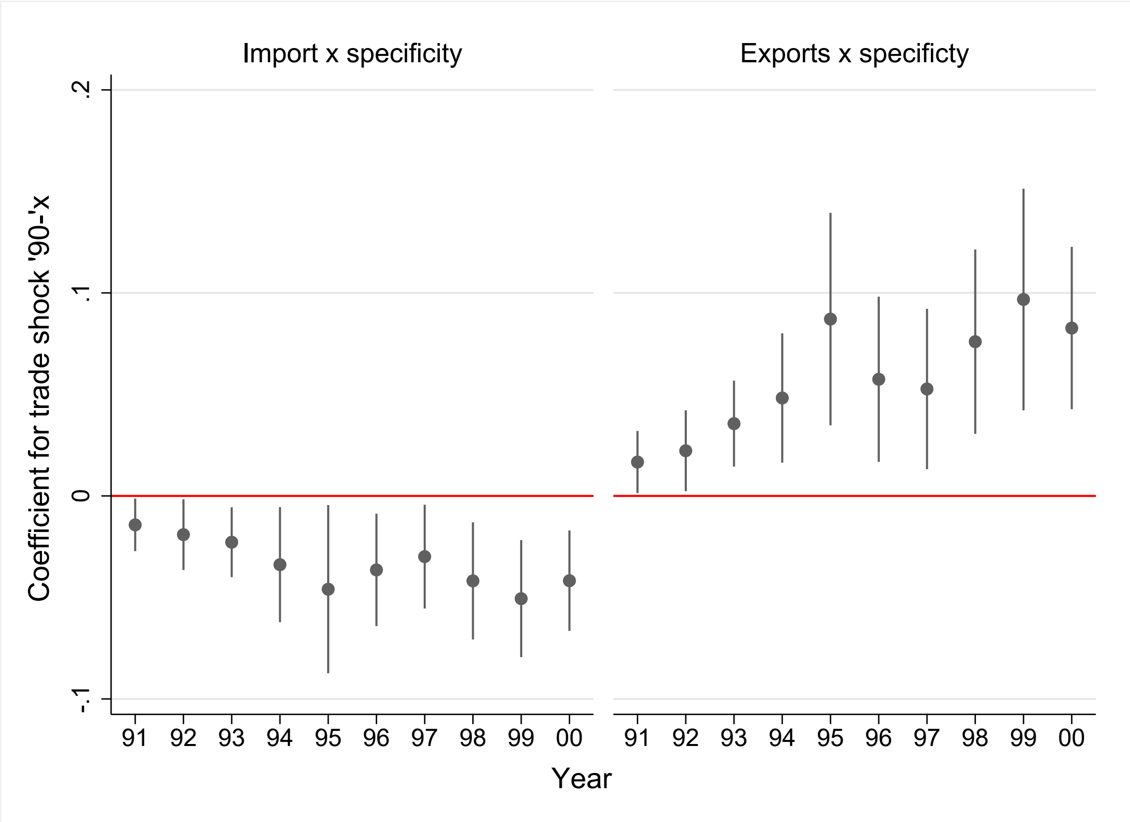


Figure 4: Dynamic of Conditional Net Earnings Effects of Trade Exposure over Time for Workers with Specific and General Skills

The figure reports the coefficients and 95% confidence intervals of the interaction terms of the specificity measure and the trade exposure for ten separate regressions that relate a worker's cumulative earnings during the indicated time period to the trade exposure of the worker's 1990 labor market region during the same time period. The dependent variable is the cumulative earnings that a worker obtained from 1990 through the year indicated on the x-axis. The trade exposure is measured as the increase of the trade flows between 1990 and the year indicated on the x-axis. More details can be found in Table I.1 in the Appendix, which presents the corresponding regression table.

# Tables in the text

Table 1: Examples of Skill Bundles in BERUFENET (simplified)

| Skill | Tool mechanic | Precision mechanic | Construction carpenter |
|---|---|---|---|
| Work according to technical drawings | x | x | |
| CNC programming | x | | |
| Precision engineering | x | x | |
| Mold making | x | | |
| Machine guidance | x | x | x |
| Metrology | x | x | |
| Fixture construction | x | | |
| Thermal treatment | x | | |
| Tool making | x | | |
| Mechanical engineering | | x | |
| Calibrating | | x | |
| Mounting | | | x |
| Planning | | | x |
| Carpentry | | | x |
| Timbering | | | x |
| Sawing | | | x |
| Sound insulation | | | x |
| Stair construction | | | x |
| Plastering | | | x |

Notes: Examples of skills (core competencies) listed in BERUFENET. A typical occupation requires about 10 to 12 core skills, but in principle there is no limit to the number of skills an occupation can require.

Table 2: Most Specific and Most General Occupations

| Most General | |
|---|---|
| *Job description* | KldB-2010 identifier |
| Commercial employees (office) | 714-2 |
| Sales assistants for retail services (no specialization) | 621-2 |
| Commercial employees (retail) | 612-2 |
| Mechanical engineers (industrial) | 251-2 |
| Metalworkers | 242-2 |
| Logisticians | 513-2 |
| Sales assistants for retail services (durables) | 622-2 |
| Pharmacists | 624-2 |
| Commercial employees (insurance/banking) | 721-2 |
| Commercial employees (industry) | 713-3 |
| **Most Specific** | |
| *Job description* | KldB-2010 identifier |
| Editors and journalists, authors | 924-4 |
| Surveyors | 312-2 |
| Photographers and photography technologists | 233-2 |
| Pharmacists | 818-4 |
| Traffic managers (road/ralway/air) | 515-2 |
| Floor layers | 331-2 |
| Interior designers, Visual marketing specialists | 932-2 |
| Teachers at educational institutions other than schools | 844-4 |
| Teachers in schools of general education | 841-4 |
| Teachers and researcher at universities and colleges | 843-4 |

Notes: The table shows the 10 most general and the 10 most specific occupations/job descriptions, along with their KldB 2010 (3+1)-digit identifiers. For this table, the specificity measure is calculated in 1990 and averaged over all federal states.

Table 3: Distribution of Skill Specificity by Occupational Classes

| | Standardized Specificity | | | |
|---|---|---|---|---|
| | 1st Quartile | Mean | 3rd Quartile | Count |
| Production/Manufacturing | −0.131 | 0.112 | 0.541 | 568,875 |
| Construction/Building | 0.627 | 0.745 | 0.910 | 152,920 |
| Natural Sciences/IT | 0.405 | 0.596 | 0.785 | 57,492 |
| Traffic/Logistics | −0.497 | 0.124 | 0.406 | 193,345 |
| Commercial/Tourism | −0.973 | −0.508 | 0.150 | 75,924 |
| Business/Administration/Law | −3.114 | −1.465 | −0.151 | 175,771 |
| Other | 0.664 | 0.859 | 1.104 | 70,048 |
| Total | −0.554 | 0.000 | 0.634 | 1,294,375 |

Notes: The table summarizes the distribution of the standardized specificity measure by occupational classes. The standardization is performed for the entire sample (n=1,294,375) for all rows.

Table 4: Distribution of Skill Specificity by Industries

| | Standardized Specificity | | | |
|---|---|---|---|---|
| | 1st Quartile | Mean | 3rd Quartile | Count |
| Manufacturing | −0.508 | −0.031 | 0.528 | 585,713 |
| Energy/Construction | 0.377 | 0.461 | 0.831 | 167,347 |
| Trade/Maintenance | −0.810 | −0.350 | 0.383 | 183,453 |
| Credit/Insurance | −0.341 | −0.112 | 0.325 | 127,392 |
| Services | −0.011 | 0.085 | 0.858 | 230,470 |
| Total | −0.554 | 0.000 | 0.634 | 1,294,375 |

Notes: The table summarizes the distribution of the standardized specificity measure by industry classes. The standardization is performed for the entire sample (n=1,294,375) for all rows.

Table 5: Descriptive Statistics Base Year 1990 by Skill Specificity

| | I<br>Most<br>Specific | II<br>Least<br>Specific | III<br>Difference<br>(I) - (II) | IV<br>All<br>Workers |
|---|---|---|---|---|
| Earnings base year (in EUR) | 48,030.1 | 44,315.2 | 3715.0*** | 45,370.0 |
| | (31101.5) | (24814.5) | (60.532) | (27509.4) |
| Apprentice (dummy) | 0.720 | 0.826 | -0.106*** | 0.780 |
| | (0.449) | (0.379) | (0.001) | (0.414) |
| University (dummy) | 0.181 | 0.059 | 0.121*** | 0.105 |
| | (0.385) | (0.237) | (0.001) | (0.306) |
| German (dummy) | 0.911 | 0.918 | -0.007 | 0.909 |
| | (0.284) | (0.274) | (0.001) | (0.288) |
| Age (years) | 38.28 | 37.60 | 0.683*** | 37.92 |
| | (9.315) | (9.451) | (0.020) | (9.428) |
| Average local specificity | 0.007 | -0.006 | 0.013*** | 0.000 |
| | (0.084) | (0.081) | (0.000) | (0.082) |
| Number of observations | 435,101 | 430,127 | | 1,294,375 |

Notes: The table summarizes observed characteristics of the workers in the sample in the base year 1990, separately for the 33 percent of workers who work in occupations with the most specific skill demand and the 33 percent of workers who work in occupations with the least specific skill demand. The third column reports the differences between both groups, along with t-tests. The last column reports the statistics for all workers in our main sample. Levels of significance: *** p<0.01.

Table 6: Workers' Skill Specificity and Instrumental Variables

| | I | II | III | IV |
|---|---|---|---|---|
| Instrument: imports | 0.001 | -0.000 | | |
| | (0.001) | (0.001) | | |
| Instrument: exports | 0.006 | 0.001 | | |
| | (0.004) | (0.003) | | |
| Import exposure | | | 0.009 | 0.007 |
| | | | (0.006) | (0.006) |
| Export exposure | | | -0.012 | -0.010 |
| | | | (0.010) | (0.010) |
| Base year earnings | No | Yes | No | Yes |
| Firm-level controls | No | Yes | No | Yes |
| Individual-level controls | No | Yes | No | Yes |
| 3-digit industry | No | Yes | No | Yes |
| R-squared | 0.000 | 0.147 | 0.000 | 0.143 |
| Clusters | 205 | 205 | 205 | 205 |
| Number of observations | 1,294,375 | 1,294,375 | 1,294,375 | 1,294,375 |

Notes: Table reports results (coefficients) of four OLS regressions. Dependent variable is the standardized measure of workers' skill specificity. Base year earnings are the workers' total income in the base year 1990. Firm-level controls include fixed effects for federal states and five plant-size groups. Individual-level controls include dummies for nationality, education level (no secondary schooling, apprenticeship, tertiary education), and age. 3-digit industry controls include dummies for 198 industries. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region.

Table 7: Trade Exposure and Individual Earnings (2SLS Estimates)

| | I | II | III | IV |
|---|---|---|---|---|
| Import exposure | -0.060 | -0.061 | -0.080** | -0.016 |
| | (0.040) | (0.047) | (0.039) | (0.028) |
| Export exposure | 0.130** | 0.084 | 0.108** | 0.033 |
| | (0.054) | (0.056) | (0.052) | (0.032) |
| Import × Specificity | -0.055*** | -0.059*** | -0.042*** | -0.036*** |
| | (0.017) | (0.017) | (0.013) | (0.011) |
| Export × Specificity | 0.111*** | 0.117*** | 0.083*** | 0.066*** |
| | (0.027) | (0.029) | (0.020) | (0.019) |
| Base year earnings | Yes | Yes | Yes | Yes |
| Firm-level controls | No | Yes | Yes | Yes |
| Individual-level controls | No | No | Yes | Yes |
| 3-digit industry | No | No | No | Yes |
| *Sanderson Windmeijer F-stat* | | | | |
| Import | 145.542 | 124.724 | 125.048 | 159.105 |
| Export | 126.391 | 124.823 | 124.637 | 141.527 |
| Imports × Specificity | 181.555 | 174.303 | 174.431 | 177.133 |
| Exports × Specificity | 130.687 | 134.236 | 134.353 | 140.892 |
| *P-value of joint significance* | | | | |
| Imports | 0.005 | 0.002 | 0.001 | 0.006 |
| Exports | 0.000 | 0.000 | 0.000 | 0.001 |
| R-square | 0.002 | 0.002 | 0.003 | 0.001 |
| Clusters | 205 | 205 | 205 | 205 |
| Number of observations | 1,294,375 | 1,294,375 | 1,294,375 | 1,294,375 |

Notes: Dependent variable: Cumulative earnings (normalized by base year income). Cumulative earnings are defined as the sum of total income from employment during the decade, divided by the base year income. All regressions control for the specificity of the workers occupations in 1990 and base year earnings (cumulative income in 1990). Firm-level controls include fixed effects for four broad industry groups (except for Column VI), federal states and five plant-size groups. Individual-level controls include dummies for nationality, education level (no secondary schooling, apprenticeship, tertiary education), and age. 3-digit industry controls include dummies for 198 industries. All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

Table 8: Trade Exposure and Individual Earnings for Stayers and Switchers

| | Establishment | | Occupation | | LLM | |
|---|---|---|---|---|---|---|
| | Stayer | Switcher | Stayer | Switcher | Stayer | Switcher |
| Import exposure | -0.175*** | -0.181*** | -0.194*** | -0.185*** | -0.197*** | -0.132*** |
| | (0.052) | (0.055) | (0.052) | (0.066) | (0.059) | (0.042) |
| Export exposure | 0.229*** | 0.183*** | 0.215*** | 0.242*** | 0.238*** | 0.125** |
| | (0.065) | (0.060) | (0.060) | (0.079) | (0.070) | (0.052) |
| Import × Specificity | -0.040*** | -0.039** | -0.039*** | -0.043* | -0.042*** | -0.040* |
| | (0.014) | (0.018) | (0.013) | (0.023) | (0.014) | (0.023) |
| Export × Specificity | 0.074*** | 0.103*** | 0.082*** | 0.099*** | 0.088*** | 0.093*** |
| | (0.021) | (0.026) | (0.019) | (0.032) | (0.020) | (0.026) |
| *Sanderson Windmeijer F-stat* | | | | | | |
| Import | 119.075 | 167.675 | 140.780 | 113.945 | 123.050 | 218.120 |
| Export | 131.577 | 113.941 | 123.774 | 118.449 | 118.911 | 126.929 |
| Imports × Specificity | 157.142 | 214.113 | 187.778 | 150.528 | 164.890 | 251.779 |
| Exports × Specificity | 125.095 | 150.879 | 130.360 | 141.496 | 129.850 | 145.373 |
| *P-value of joint significance* | | | | | | |
| Imports | 0.000 | 0.003 | 0.000 | 0.010 | 0.000 | 0.005 |
| Exports | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 | 0.000 |
| R-square | 0.002 | 0.005 | 0.003 | 0.004 | 0.001 | 0.006 |
| Clusters | | | | | | |
| Number of observations | 529,006 | 467,685 | 670,188 | 317,961 | 749,792 | 274,009 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. The sample includes only workers who remain in their initial establishment (Column I), occupation (Column III) or local labor market (Column V) for the whole observation period, and workers who switched out of their initial establishment (Column II), occupation (Column IV) or local labor market (Column VI), respectively, at some point during the decade. * p<0.1; ** p<0.05; *** p<0.01.

Table 9: Trade Exposure and Mobility

| | Establishment | Occupation | LLM |
|---|---|---|---|
| Import exposure | -0.001 | -0.007 | -0.004 |
| | (0.006) | (0.004) | (0.004) |
| Export exposure | -0.010 | 0.007 | -0.002 |
| | (0.011) | (0.007) | (0.005) |
| Import × Specificity | -0.000 | 0.001 | 0.002* |
| | (0.002) | (0.001) | (0.001) |
| Export × Specificity | 0.002 | -0.001 | -0.000 |
| | (0.002) | (0.002) | (0.002) |
| *Sanderson Windmeijer F-stat* | | | |
| Import | 125.048 | 125.048 | 125.048 |
| Export | 124.637 | 124.637 | 124.637 |
| Imports × Specificity | 174.431 | 174.431 | 174.431 |
| Exports × Specificity | 134.353 | 134.353 | 134.353 |
| *P-value of joint significance* | | | |
| Imports | 0.946 | 0.316 | 0.121 |
| Exports | 0.221 | 0.563 | 0.919 |
| R-square | 0.001 | -0.000 | 0.001 |
| Clusters | 205 | 205 | 205 |

Notes: N = 1,294,375. Dependent variables: Column I: Dummy for leaving establishment (withing the decade following the base year). Column II: Dummy for leaving occupation. Column III: Dummy for leaving local labor market. 2SLS Regressions. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

Table 10: Trade Exposure and Individual Earnings for Young and Old Workers

| | Age > 40 | Age ≤ 40 | 2000-2010 |
|---|---|---|---|
| Import exposure | -0.021 | -0.135*** | -0.026** |
| | (0.051) | (0.042) | (0.012) |
| Export exposure | 0.042 | 0.183*** | 0.033*** |
| | (0.073) | (0.052) | (0.011) |
| Import x Specificity | -0.051*** | -0.025* | 0.007 |
| | (0.016) | (0.015) | (0.009) |
| Export x Specificity | 0.073*** | 0.083*** | 0.001 |
| | (0.027) | (0.021) | (0.007) |
| *Sanderson Windmeijer F-stat* | | | |
| Import | 131.629 | 121.193 | 73.058 |
| Export | 127.133 | 123.109 | 45.538 |
| Imports × Specificty | 180.756 | 169.917 | 51.535 |
| Exports × Specificty | 129.513 | 139.303 | 46.203 |
| *P-value of joint significance* | | | |
| Imports | 0.003 | 0.004 | 0.021 |
| Exports | 0.006 | 0.000 | 0.009 |
| R-square | 0.001 | 0.004 | 0.003 |
| Clusters | 205 | 205 | 205 |
| Number of observations | 526,888 | 767,487 | 1,495,394 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Column I includes only workers older than 40 years of age, Column II includes only workers younger than 40 years. Column III replicates our main specification for the period between 2000 and 2010. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

Table 11: Effects of Industry Level Trade Exposure on Individual Earnings

| | I | II | III | IV | V |
|---|---|---|---|---|---|
| Reg.Import exposure | | | | | -0.077** |
| | | | | | (0.039) |
| Reg.Export exposure | | | | | 0.098* |
| | | | | | (0.052) |
| Reg.Import × Specificity | | | | | -0.038*** |
| | | | | | (0.012) |
| Reg.Export × Specificity | | | | | 0.052** |
| | | | | | (0.020) |
| Ind.Import exposure | -0.005 | -0.004 | -0.006 | -0.005 | -0.004 |
| | (0.005) | (0.005) | (0.005) | (0.004) | (0.003) |
| Ind.Export exposure | 0.021* | 0.019* | 0.016 | 0.014 | 0.019*** |
| | (0.013) | (0.010) | (0.012) | (0.009) | (0.006) |
| Ind.Import × Specificity | -0.003 | -0.003 | -0.004** | -0.004** | -0.002 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.001) |
| Ind.Export × Specificity | 0.024*** | 0.024*** | 0.014*** | 0.014*** | 0.023*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.002) |
| All controls | Yes | Yes | Yes | Yes | Yes |
| Region fixed effects | No | Yes | No | Yes | No |
| *Sanderson Windmeijer F-stat* | | | | | |
| Import_Reg | | | | | 217.543 |
| Export_Reg | | | | | 202.116 |
| Import_Reg × Specificty | | | | | 348.444 |
| Export_Reg × Specificty | | | | | 512.993 |
| Import_Ind | 42.147 | 42.354 | 40.480 | 40.769 | 491.201 |
| Export_Ind | 36.316 | 38.569 | 37.221 | 39.653 | 1267.396 |
| Export_Ind × Specificty | 51.442 | 51.021 | 48.892 | 48.682 | 742.258 |
| Export_Ind × Specificty | 73.783 | 73.755 | 59.709 | 61.449 | 1659.210 |
| *P-value of joint significance* | | | | | |
| Imports_Reg | | | | | 0.003 |
| Exports_Reg | | | | | 0.0179 |
| Imports_Ind | 0.377 | 0.326 | 0.101 | 0.0633 | 0.332 |
| Exports_Ind | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| R-square | 0.003 | 0.003 | 0.000 | 0.001 | 0.003 |
| Clusters | 93 | 93 | 93 | 93 | 198 |
| Number of observations | 1,294,375 | 1,294,375 | 557,003 | 557,003 | 1,294,375 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. Reg.Import/Reg.Export measures trade exposure on the regional level, as in our main regressions. Ind.Import/Ind.Export measures trade at the industry level. Specifications I, II and V include workers in all industries, where trade exposure in non-manufacturing industries is equal to zero. Specifications III and IV include only workers in manufacturing industries. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states) and individual-level controls (nationality, education level, age). Specifications II and IV additionally include dummies for 205 local labor markets. All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period 3-digit industry (industry trade exposure) or 3-digit region (Specification V), respectively. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

# Appendix A Specificity at the level of local labor markets

Table A.1 compares results from equation (6) with a specificity measure calculated on the level of local labor markets (Column I) with a specificity measure calculated at the level of federal states (Column II). The coefficient estimates of the baseline terms are similar in both specifications. However, the coefficient estimates between trade exposure and skill specificity are substantially more compressed in the first than in the second column. In more detail, the coefficient estimate of the interaction term between local skill specificity and export exposure in Column I is only half as large in the one in Column II. The coefficient estimate for the interaction term between local import exposure and skill specificity in Column I is not significantly different from zero anymore.

These results reveal that the global specificity measure is more deterministic for workers' wages than the local one. One reason is that the local specificity is strongly related to regional industrial clusters that also drive the trade shocks. Another reason might be that the marginal worker with specific skills is able to leave the local labor market in response to a negative demand shock—particularly, when his or her skills are less specific outside the local labor market. See Section 5.5 for further details.

Table A.1: Specificity at the level or local labor markets

| | LLM205 Specificity | Federal State Specificity |
|---|---|---|
| Import exposure | -0.081** | -0.080** |
| | (0.039) | (0.039) |
| Export exposure | 0.095** | 0.108** |
| | (0.048) | (0.052) |
| Import × Specificity | 0.025 | -0.042*** |
| | (0.017) | (0.013) |
| Export × Specificity | 0.045** | 0.083*** |
| | (0.022) | (0.020) |
| *Sanderson Windmeijer F-stat* | | |
| Import | 123.361 | 125.048 |
| Export | 129.302 | 124.637 |
| Imports × Specificty | 218.161 | 174.431 |
| Exports × Specificty | 129.555 | 134.353 |
| *P-value of joint significance* | | |
| Imports | 0.089 | 0.001 |
| Exports | 0.001 | 0.000 |
| R-square | 0.003 | 0.003 |
| Clusters | 205 | 205 |
| Number of observations | 1,294,375 | 1,294,375 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. The local specificity measure (LLM205 Specificity) calculates the specificity measure based on local employment shares (205 regions) instead of federal state employment shares. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

# Online appendix A Expected value of $\lambda$

In this section, we derive an expression for the highest expected value of $\lambda$ with $N$ independent draws, which we use in Section 2. Let $f_\lambda(\lambda)$ denote the probability density function and $F_\lambda(\lambda)$ the cumulative density function of the random variable $\lambda$. Consider $N$ independent draws of $\lambda$ and let $Y$ denote the highest of these $N$ draws, i.e., $Y = max(\lambda_1, \lambda_2, ..., \lambda_N)$. The max of these $N$ independent draws can be written as (Paarsch and Golyaev, 2016, e.e.,):

$$E(Y) = \int_{-\infty}^{+\infty} y f_y(y) dy \tag{A.1}$$

The cumulative density function of $Y$, i.e., $F_Y(y)$ can be written as:

$$\begin{aligned} Pr(Y \leq y) = F_Y(y) = Pr[(\lambda_1 \leq y) \cap (\lambda_2 \leq y) \cap ... \cap (\lambda_N \leq y)] = \\ \prod_{n=1}^{N} Pr(\lambda_n \leq y) = F_\lambda(y)^N \end{aligned} \tag{A.2}$$

As in our case $f_\lambda(\lambda)$ is a continuous function with support $[0, 1]$ we can write:

$$E(Y) = \int_0^1 y F'_y(y) dy \tag{A.3}$$

Integrating by parts, we get:

$$E(Y) = [yF_y(y)]_0^1 - \int_0^1 F_y(y) dy \tag{A.4}$$

Since $F(0) = 0$ and $F(1) = 1$, we get:

$$E(Y) = 1 - \int_0^1 F_y(y) dy \tag{A.5}$$

Finally, replacing $F_y(y)$ by its equivalent expression from above:

$$E(Y) = 1 - \int_0^1 F_\lambda(y)^N dy = \int_0^1 1 - F_\lambda(y)^N dy \tag{A.6}$$

# Online appendix B Identification of interaction terms

This section provides more intuition on our identification assumption by clarifying our approach with an example of a regression equation with only one interaction term. Therefore, we depart from the following regression equation:

$$y_i = \beta_0 + \beta_x x_i + \beta_{xw} x_i w_i + \beta_w w_i + \epsilon_i \tag{B.1}$$

where $y_i$ represents the dependent variable and $\epsilon_i$ is the error term. $x_i$ and $w_i$ are two endogenous variables with $cov(x_i, \epsilon_i) \neq 0$ and $cov(w_i, \epsilon_i) \neq 0$. Furthermore, let $z_i$ be an instrument for $x_i$ with $cov(z_i, \epsilon_i) = 0$ and $cov(z_i, x_i) \neq 0$.

The first stages of this model are

$$x_i = \pi_{11} + \pi_{12} z_i + \pi_{13} z_i w_i + \pi_{14} w_i + \epsilon_1 \tag{B.2}$$

$$x_i w_i = \pi_{21} + \pi_{22} z_i + \pi_{23} z_i w_i + \pi_{24} w_i + \epsilon_2 \tag{B.3}$$

$$\tag{B.4}$$

and the reduced form is

$$y_i = \pi_{31} + \pi_{32} z_i + \pi_{33} z_i w_i + \pi_{34} w_i + \epsilon_3 \tag{B.5}$$

As $z_i$ is exogenous ($cov(z_i, \epsilon_i) = 0$), $cov(z_i, \epsilon_1) = cov(z_i, \epsilon_2) = cov(z_i, \epsilon_3) = 0$, and we are able to identify $\pi_{12}$, $\pi_{22}$, and $\pi_{32}$. Moreover, we know from Nizalova and Murtazashvili (2016) that we can also identify $\pi_{13}$, $\pi_{23}$, and $\pi_{33}$ if $z_i$ and $(w_i, \epsilon_i)$ are conditionally independent—even if $w_i$ is endogenous. In our specific case, $w_i$ represents the workers skill specificity that needs to be independent of our instruments for international trade. Our approach satisfies this assumption by restricting our sample to only include workers who have chosen their jobs before the fall of the Iron Curtain. These workers were unable to have foreseen the consequences of international trade after the fall of the Iron Curtain. Therefore, that these workers have made their investments in specific and general skills in anticipation of the consequences of international trade is unlikely. Following Angrist and Pischke (2008) we can substitute the first stage expressions (B.3) and (B.4) into the relation of interest in equation (B.1) and rearrange the equation so that follows

$$
\begin{aligned}
y_i = \beta_0 + [\beta_x\pi_{12} + \beta_{xw}\pi_{22}]z_i + [\beta_x\pi_{13} + \beta_{xw}\pi_{23}]z_i w_i \\
+[\beta_x\pi_{14} + \beta_{xw}\pi_{24} + \beta_w]w_i + [\beta_x\epsilon_1 + \beta_{xw}\epsilon_2] + \epsilon_i
\end{aligned}
\tag{B.6}
$$

with $[\beta_x\pi_{12} + \beta_{xw}\pi_{22}] = \pi_{32}$, $[\beta_x\pi_{13} + \beta_{xw}\pi_{23}] = \pi_{33}$, $[\beta_x\pi_{14} + \beta_{xw}\pi_{24} + \beta_w] = \pi_{34}$, and $[\beta_x\epsilon_1 + \beta_{xw}\epsilon_2] + \epsilon_i = \epsilon_3$. Thus, $\beta_x = \frac{\pi_{32}\pi_{23} - \pi_{33}\pi_{12}}{\pi_{23}\pi_{12} - \pi_{22}\pi_{13}}$ and $\beta_{xw} = \frac{\pi_{33}\pi_{12} - \pi_{32}\pi_{13}}{\pi_{23}\pi_{12} - \pi_{22}\pi_{13}}$ are combinations of coefficients that we can identify given that the assumptions discussed in Section 3 hold.

# Online appendix C Baseline estimates

Table C.1: Trade Effects Without Specificity Interactions

| | I | II | III |
|---|---|---|---|
| Import exposure | -0.065 | -0.061 | -0.081** |
| | (0.040) | (0.040) | (0.039) |
| Export exposure | 0.137** | 0.133** | 0.110** |
| | (0.054) | (0.054) | (0.052) |
| Base year earnings | Yes | Yes | Yes |
| Specificity | No | Yes | Yes |
| All controls | No | No | Yes |
| *Sanderson Windmeijer F-stat* | | | |
| Import | 121.657 | 121.611 | 113.783 |
| Export | 124.389 | 124.508 | 121.798 |
| *P-value of joint significance* | | | |
| Imports | 0.101 | 0.123 | 0.039 |
| Exports | 0.012 | 0.014 | 0.036 |
| R-square | 0.000 | 0.002 | 0.003 |
| Clusters | 205 | 205 | 205 |
| Number of observations | 1,294,375 | 1,294,375 | 1,294,375 |

Notes: Dependent variable: Cumulative earnings (normalized by base year income). Cumulative earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. Column III includes controls for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

# Online appendix D Visualizing the risk-return trade-off

As workers with specific skills profit more from positive demand shocks but suffer more from negative ones, our results imply that dynamic labor markets that are characterised by both, positive and negative demand shocks, will induce more wage heterogeneity for workers with specific skills than for workers with general skills. Figure Figure D.1 aims at visualizing this effect for our specific case.

Figure D.1 shows the distribution of the conditional net effects of trade evaluated at the average regional trade intensities. The upper panel shows the distribution of conditional average net effects for workers with very specific skills (i.e., the 75 percentile of our skill specificity measure $Spec_{os} = 0.6$), and the lower panel for workers with very general skills (i.e., the 25 percentile of our skill specificity measure $Spec_{os} = -0.55$).[28] The figure reveals that the mass of the distribution for workers with specific and general skills lies in the area close to zero for both groups.

Nevertheless, the distribution of average net effects is much wider for workers with very specific skills than for those with very general skills. Comparing two extreme labor market regions of Bremerhaven (where imports increased by $2,700$ Euros and exports by 470 Euros) and Bodensee (where imports increased by $3,600$ Euros and exports by $5,150$ Euros) provides an intuitive example. The conditional average net effect for workers who were located in the labor market region of Bremerhaven in 1990 amounts to $-27$ percent (of the base-year income) for workers with very specific skills and $-18$ percent for workers with very general skills. In contrast, workers in the Bodensee region in 1990 were exposed to an average net effect of approximately 39 percent for workers with specific skills but only of approximately 7 percent for workers with general skills.

[28] As specification IV of Table 7 presents results relative to the average wage growth in the workers' own industry, using the full effects of specification III to produce Figure D.1 produces a more intuitive visualization of the results.

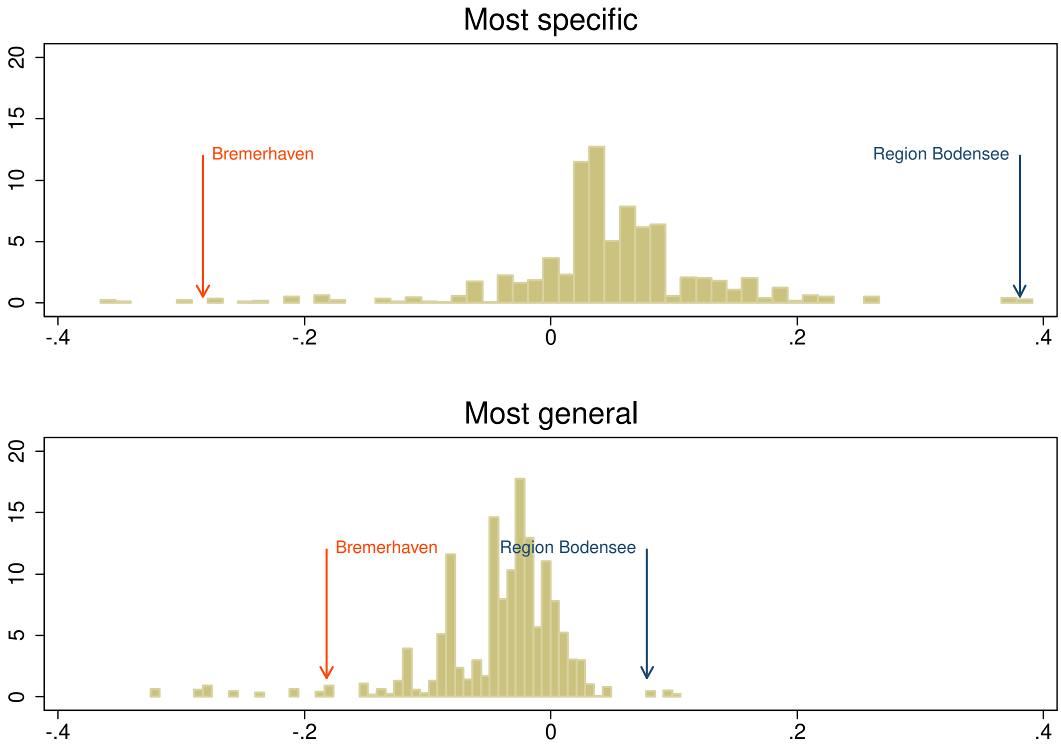


Figure D.1: Distribution of Conditional Average Net Earnings Effects of Trade Exposure

The figures show the distribution of German labor market regions with average conditional net earnings effects of trade exposure. The upper panel shows the effects for workers with specific skills (i.e., the 75 percentile of our skill specificity measure); the lower panel shows the effects for workers with general skills (i.e., the 25 percentile of our skill specificity measure).

# Online appendix E  Wage or employment effects

Table E.1 analyzes whether our earnings effects stem from reduced wages or reduced employment opportunities. Therefore, the first column of Table E.1 presents results from equation (6) on a sample of individuals with non-zero earnings throughout each year of our entire observation period. Workers in this sample are positively selected and, by definition, experience fewer spells of non-employment than the average worker of the entire sample. Thus, if reduced employment opportunities would mainly drive the earnings effects, we should expect to see smaller effect sizes for this specification. However, the results are very similar to our main results. Indeed, the absolute values of our effects are even larger. These differences in magnitudes suggest that assigning zero earnings to missing values introduces some measurement error that decreases the coefficient estimates of our main results.

Column II of Table E.1 provides some *back of the envelope calculations* for the pure wage effect. In more detail, Column II treats workers as if they were continuously employed throughout the entire observation period between 1990 and 2000. First, we have calculated the workers' average daily wages throughout their entire employment period. Second, we have extrapolated their employment spells to cover the entire observation period and multiplied the total days of employment with their average wages.[29] This measure gives us an estimate of their hypothetical earnings in the absence of non-employment spells.

Column I and Column II reveal that the effects of both approaches are qualitatively very similar. Comparing the magnitudes of the interaction terms across both specifications suggests that approximately 60 percent $(-0.025/-0.43)$ of the interaction term between skill specificity and import exposure and 76 percent of the one between skill specificity and export exposure are related to wage effects. However, we caution against over-interpreting the results of Column II, because they are based on rather strong assumptions.

The remainder columns of Table E.1 show results from regression equation (6) with a dependent variable measuring the share of non-employment days throughout the entire observation period. Column III shows the effects for workers without zero earnings, Column IV shows the effects for the entire sample.

Overall, the results reveal very small employment effects from increasing trade exposure. For example, workers with very specific skills experience approximately 0.2 percent more days of non-employment (eight days throughout the entire observation period of 10 years) if imports increase by 1000 Euro. Similarly, they experience

[29] As for our main specification we have normalized the hypothetical earnings by the workers earnings in the base year.

0.2 percent fewer days of non-employment if exports increase by 1000 Euros.

Table E.1: Price and Employment Effects

| | I<br>Overall<br>effect | II<br>Price<br>effects | III<br>Non-empl.<br>(non-zeros) | IV<br>Non-empl.<br>(all) |
|---|---|---|---|---|
| Import exposure | -0.199*** | -0.223*** | -0.002 | -0.009*** |
| | (0.056) | (0.057) | (0.001) | (0.003) |
| Export exposure | 0.232*** | 0.249*** | 0.001 | 0.009** |
| | (0.065) | (0.064) | (0.002) | (0.004) |
| Import × Specificity | -0.043*** | -0.025** | 0.002*** | 0.002*** |
| | (0.014) | (0.012) | (0.000) | (0.001) |
| Export × Specificity | 0.088*** | 0.067*** | -0.002*** | -0.002** |
| | (0.020) | (0.015) | (0.001) | (0.001) |
| *Sanderson Windmeijer F-stat* | | | | |
| Import | 132.623 | 132.623 | 132.623 | 125.048 |
| Export | 123.801 | 123.801 | 123.801 | 124.637 |
| Imports × Specificty | 180.110 | 180.110 | 180.110 | 174.431 |
| Exports × Specificty | 134.980 | 134.980 | 134.980 | 134.353 |
| *P-value of joint significance* | | | | |
| Imports | 0.000 | 0.000 | 0.001 | 0.000 |
| Exports | 0.000 | 0.000 | 0.001 | 0.012 |
| R-square | 0.003 | 0.001 | 0.001 | 0.001 |
| Clusters | 205 | 205 | 205 | 205 |
| Number of observations | 952,546 | 952,546 | 952,546 | 1,294,375 |

Notes: Dependent variables: Column I: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. Column II: Hypothetical cumulated daily wage (if always employed). Columns III and IV: Cumulated days not in employment during decade. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

# Online appendix F OLS, reduced form and 2SLS subsample estimations

This appendix shows the OLS and reduced form specifications and presents four analyses on sub-samples to investigate the robustness of our results. The first specification of Table F.1 shows the standard OLS results (including all control variables), and the second presents the results of the reduced form. Most OLS estimates are insignificant (excluding the interaction term between exports and workers' skill-specificity) and much smaller than the 2SLS estimates. In contrast, the reduced form parameters are precisely estimated at conventional levels. As in previous papers using this type of identification strategy, these results suggests that measurement error and simultaneity bias associated with German industry supply and demand shocks attenuate the naïve OLS estimates towards zero (Autor et al., 2013; Dauth et al., 2014; Helm, 2020).

Specifications three through six show the results for four different sub-samples. First, the third column presents results from a sub-sample including only workers in the manufacturing sector. The results for this sample are very similar to our main results for the whole sample, indicating that manufacturing workers and non-manufacturing workers are similarly affected by local spillover effects from industries in the same labor market region. Second, Table 5 reveals that workers with specific skills are better educated than workers with general skills, meaning that our results may capture unobserved ability differences—even after accounting for detailed worker and firm characteristics. Therefore, the fourth column presents results from a sub-sample including only workers whose highest degree is an apprenticeship degree. Apprenticeship graduates are a very homogeneous group of workers, because very few have dropped out of school and very few have obtained additional formal qualifications (e.g., an Abitur, a certificate that would allow them to study in a university) (Dustmann and Meghir, 2005). Moreover, apprenticeship training curricula precisely define the training content for apprenticeship training programs in Germany, and firms and vocational schools have to obey these training curricula to receive their training accreditation. Thus, apprenticeship graduates possess similar skills within occupations.

Table F.1: OLS, Reduced Form and 2SLS subsamples

| | OLS | Reduced | Manuf' | Appr' | Women |
|---|---|---|---|---|---|
| Import exposure | -0.038 | | -0.081 | -0.036 | -0.124*** |
| | (0.027) | | (0.056) | (0.033) | (0.042) |
| Export exposure | 0.025 | | 0.136* | 0.054 | 0.069 |
| | (0.040) | | (0.072) | (0.045) | (0.046) |
| Import × Specificity | -0.007 | | -0.037** | -0.034*** | -0.051*** |
| | (0.009) | | (0.019) | (0.011) | (0.016) |
| Export × Specificity | 0.037** | | 0.086*** | 0.064*** | 0.053** |
| | (0.015) | | (0.024) | (0.020) | (0.022) |
| Instr.: Import | | -0.011** | | | |
| | | (0.006) | | | |
| Instr.: Export | | 0.022 | | | |
| | | (0.016) | | | |
| Instr.: Import × Spec. | | -0.005** | | | |
| | | (0.002) | | | |
| Instr.: Export × Spec. | | 0.027*** | | | |
| | | (0.009) | | | |
| *Sanderson Windmeijer F-stat* | | | | | |
| Import | | | 84.403 | 132.967 | 210.333 |
| Export | | | 122.637 | 119.998 | 141.534 |
| Imports x Specificty | | | 96.791 | 182.578 | 224.734 |
| Exports x Specificty | | | 113.240 | 132.281 | 146.530 |
| *P-value of joint significance* | | | | | |
| Imports | 0.329 | 0.009 | 0.079 | 0.003 | 0.000 |
| Exports | 0.048 | 0.003 | 0.000 | 0.003 | 0.046 |
| R-square | 0.141 | 0.141 | 0.000 | 0.004 | 0.005 |
| Clusters | 205 | 205 | 205 | 205 | 205 |
| Number of observations | 1,294,375 | 1,294,375 | 557,003 | 1,009,128 | 665,108 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

As our information from the BERUFENET largely stems from these training curricula, our measure for workers' skill specificity is less likely to suffer from measurement error for apprenticeship graduates than for other workers. The second specification shows that the results for the sample of apprenticeship graduates remain very similar to our main results. Thus apprenticeship graduates do not appear more or less affected by international trade than workers with other types of education.

Third, our main estimations are based on a sample of only men. Running a separate regression for women may be informative for proving the robustness of our results. Therefore, the fifth column shows the results for a sub-sample of women. Again, the results remain similar to those of our main specification for men, even though for women, the negative effects of import exposure seem to be somewhat larger. Our descriptive results for Germany suggest that this result could be explained by women's being less likely to choose the most specific occupations.

# Online appendix G Occupation-specific labor market thickness

Occupations might be distributed across industries *and* regions, such that trade shocks happen to impact occupation more that have a relatively higher level of skill specificity. Therefore, equation (G.1) provides an alternative measure for trade exposure that reflects how the demand for workers' occupations in their region changes in response to positive or negative trade shocks:

$$\Delta ImE_{ro}^{East\to D} = \sum_j \frac{L_{rojt_0}}{L_{jt_0}} \frac{\Delta IM_j^{East\to D}}{L_{rot_0}} \tag{G.1}$$

$o$ represents the workers' occupation. The remainder variables and indices are defined as in equation (5). The measure of equation (G.1) varies for each occupation within each region and depends on the distribution of occupation across industries in each region. However, in contrast to the measure of equation (5), the measure of equation (G.1) assumes that there are no spillover effects across occupations, and it assigns zero trade exposure for occupations that only exist outside the manufacturing sector.

Table G.1 shows that the results are qualitatively similar to our main results, i.e., the coefficient estimate for the interaction term between skill-specificity and import exposure is negative and the one for skill-specificity and export exposure is positive. Nevertheless, as in Section 5.7 the negative coefficient estimate for the interaction term between skill specificity and import exposure is close to zero and insignificant as long as we assume zero trade exposure for occupations outside the manufacturing sector. It only becomes statistically and economically significant when we restrict our sample to workers manufacturing sector. Like in Section 5.7, the difference between both specifications suggests that assigning zero trade exposure to workers outside the manufacturing sector increases the measurement error and drives the coefficient estimates towards zero.

Table G.1: Occupational Trade Exposure

| | I<br>All workers | II<br>Manufacturing only |
|---|---|---|
| Import exposure | -0.009*** | 0.002 |
| | (0.003) | (0.003) |
| Export exposure | 0.002 | -0.008*** |
| | (0.003) | (0.002) |
| Import × Specificity | 0.001 | -0.013*** |
| | (0.005) | (0.004) |
| Export × Specificity | 0.022*** | 0.029*** |
| | (0.005) | (0.004) |
| *Sanderson Windmeijer F-stat* | | |
| Import | 185.155 | 135.918 |
| Export | 225.713 | 167.591 |
| Imports × Specificty | 253.877 | 207.752 |
| Exports × Specificty | 248.178 | 206.500 |
| *P-value of joint significance* | | |
| Imports | 0.000 | 0.000 |
| Exports | 0.000 | 0.000 |
| R-square | 0.006 | 0.002 |
| Clusters | 205 | 205 |
| Number of observations | 1,294,338 | 556,988 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. We calculate the occupational trade exposure by weighting the measure of industry-level imports/exports (see section 3) by the employment share of each industry in each occupation and region (205 local labor markets). All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Column I includes all workers, Column II includes only workers in the manufacturing sector (as of 1990). Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.

# Online appendix H Specificity based on individual labor market histories

We define an individual's occupational skill specificity on the basis of the occupation the individual practiced in 1990. Most individuals in our analysis have quite stable occupational trajectories. In the four years preceding the analysis (i.e., since 1987), 82 percent have not changed their occupation (based on detailed 3-digit occupational codes). We argue that for these individuals, the occupation held in 1990 accurately reflects the relevant skills. However, some individuals might have changed occupations shortly before 1990. These individuals might have additional skills in another occupation, which might be more, or less, specific than the one they are currently practicing. We thus create a skill specificity measure that calculates the average specificity of an individual's occupations between 1987 and 1990 (equally weighted) and repeat our main analysis.

Table H.1 reports the results of this robustness check. The estimates are almost identical to the ones in Table 7 (our main estimation), both in size and significance. We conclude that accounting for individual labor market histories does not significantly change our results and for sake of simplicity, we therefore stick with the simpler specification in our main analysis.

Table H.1: Specificity Based on Individual Labor Market Histories

| | I | II | III | IV |
|---|---|---|---|---|
| Import exposure | -0.059 | -0.061 | -0.080** | -0.016 |
| | (0.040) | (0.047) | (0.039) | (0.028) |
| Export exposure | 0.130** | 0.084 | 0.108** | 0.033 |
| | (0.054) | (0.056) | (0.052) | (0.032) |
| Import × Specificity | -0.058*** | -0.062*** | -0.045*** | -0.040*** |
| | (0.017) | (0.017) | (0.012) | (0.011) |
| Export × Specificity | 0.111*** | 0.117*** | 0.084*** | 0.068*** |
| | (0.027) | (0.029) | (0.019) | (0.018) |
| Base year earnings | Yes | Yes | Yes | Yes |
| Firm-level controls | No | Yes | Yes | Yes |
| Individual-level controls | No | No | Yes | Yes |
| 3-digit industry | No | No | No | Yes |
| *Sanderson Windmeijer F-stat* | | | | |
| Import | 147.818 | 126.509 | 126.881 | 161.406 |
| Export | 126.750 | 124.088 | 123.903 | 140.687 |
| Imports × Specificity | 185.107 | 177.673 | 177.807 | 180.450 |
| Exports × Specificity | 129.476 | 131.888 | 131.993 | 137.481 |
| *P-value of joint significance* | | | | |
| Imports | 0.003 | 0.001 | 0.000 | 0.001 |
| Exports | 0.000 | 0.000 | 0.000 | 0.000 |
| R-square | 0.002 | 0.002 | 0.003 | 0.001 |
| Clusters | 205 | 205 | 205 | 205 |

Notes: N = 1,294,375. Dependent variable: Cumulative earnings (normalized by base year income). Cumulative earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. All regressions control for the specificity of the workers occupations in 1990 and base year earnings (cumulative income in 1990). Firm-level controls include fixed effects for four broad industry groups (except for column VI), federal states and five plant-size groups. Individual-level controls include dummies for nationality, education level (no secondary schooling, apprenticeship, tertiary education), and age. 3-digit industry controls include dummies for 198 industries. All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

# Online appendix I Dynamic Effects

Table I.1: Dynamic Effects - Trade Exposure and Individual Earnings since 1991

| | 91 | 92 | 93 | 94 | 95 | 96 | 97 | 98 | 99 | 00 |
|---|---|---|---|---|---|---|---|---|---|---|
| Import exposure | -0.084*** | -0.099*** | -0.077*** | -0.066*** | -0.075*** | -0.062** | -0.065** | -0.089** | -0.095** | -0.080** |
| | (0.018) | (0.024) | (0.017) | (0.021) | (0.028) | (0.027) | (0.029) | (0.041) | (0.043) | (0.039) |
| Export exposure | 0.056** | 0.059** | 0.009 | -0.014 | 0.029 | 0.032 | 0.051 | 0.115** | 0.136** | 0.108** |
| | (0.024) | (0.027) | (0.020) | (0.029) | (0.043) | (0.034) | (0.034) | (0.053) | (0.065) | (0.052) |
| Import x Specificity | -0.014** | -0.019** | -0.023*** | -0.034** | -0.046** | -0.036*** | -0.030** | -0.042*** | -0.051*** | -0.042*** |
| | (0.007) | (0.009) | (0.009) | (0.014) | (0.021) | (0.014) | (0.013) | (0.015) | (0.015) | (0.013) |
| Export x Specificity | 0.017** | 0.022** | 0.036*** | 0.048*** | 0.087*** | 0.057*** | 0.053*** | 0.076*** | 0.097*** | 0.083*** |
| | (0.008) | (0.010) | (0.011) | (0.016) | (0.027) | (0.021) | (0.020) | (0.023) | (0.028) | (0.020) |
| *Sanderson Windmeijer F-stat* | | | | | | | | | | |
| Import | 57.236 | 37.577 | 41.018 | 37.625 | 21.298 | 83.274 | 137.310 | 103.425 | 78.433 | 125.048 |
| Export | 21.396 | 51.376 | 40.135 | 59.949 | 37.845 | 130.369 | 153.733 | 75.042 | 70.108 | 124.637 |
| Imports x Specificty | 25.141 | 25.089 | 28.973 | 28.715 | 12.209 | 64.361 | 133.596 | 128.987 | 139.501 | 174.431 |
| Exports x Specificty | 10.793 | 60.060 | 25.171 | 59.712 | 22.031 | 225.181 | 228.755 | 103.329 | 90.124 | 134.353 |
| *P-value of joint significance* | | | | | | | | | | |
| Imports | 0.000 | 0.000 | 0.000 | 0.004 | 0.014 | 0.008 | 0.017 | 0.006 | 0.001 | 0.001 |
| Exports | 0.024 | 0.034 | 0.004 | 0.005 | 0.004 | 0.022 | 0.025 | 0.002 | 0.001 | 0.000 |
| R-square | -0.000 | 0.000 | 0.001 | 0.001 | 0.001 | 0.001 | 0.002 | 0.002 | 0.002 | 0.003 |
| Clusters | 205 | 205 | 205 | 205 | 205 | 205 | 205 | 205 | 205 | 205 |

Notes: N = 1,294,375. The table reports the results of ten regressions that relate a worker's cumulative earnings during the indicated time period to the trade exposure of the worker's 1990 labor market region during the same time period. The dependent variable is the cumulative earnings that a worker obtained from 1990 through the year indicated on the column header, normalized by the worker's annual earnings in 1990. The trade exposure is measured as the increase of the trade flows between 1990 and the year indicated on the column header, according to the regions' industry structure (analogously to our main estimations). 2SLS Regressions. All specifications include the control variables from Table 7, Column (III), i.e., specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: ** $p<0.05$; *** $p<0.01$.

# Online appendix J Young worker mobility

This section repeats the analysis in Section 5.5 for young workers (age < 40). Table J.1 repeats the analysis of Table 8, comparing the wage effects of workers who remain in their establishment, occupation, or labor market region (and are continuously employed throughout the entire observation period) with workers who switch their establishment, occupation, or region. Again, the results are similar for stayers and switchers, with the exception that the coefficient estimates for the interaction terms of trade and specificity for switchers now seem to be smaller than the ones for stayers. In fact, for young workers, the interaction term of trade and specificity is no longer statistically significant for import exposure.

Table J.1: Trade Exposure and Individual Earnings for Stayers and Switchers (Young Workers)

| | Establishment | | Occupation | | LLM | |
|---|---|---|---|---|---|---|
| | Stayer | Switcher | Stayer | Switcher | Stayer | Switcher |
| Import exposure | -0.197*** | -0.186*** | -0.210*** | -0.213*** | -0.226*** | -0.132*** |
| | (0.055) | (0.061) | (0.055) | (0.073) | (0.065) | (0.048) |
| Export exposure | 0.256*** | 0.180*** | 0.227*** | 0.272*** | 0.271*** | 0.118* |
| | (0.058) | (0.066) | (0.055) | (0.087) | (0.069) | (0.060) |
| Import × Specificity | -0.039** | -0.039* | -0.045*** | -0.034 | -0.044*** | -0.029 |
| | (0.016) | (0.021) | (0.015) | (0.027) | (0.017) | (0.028) |
| Export × Specificity | 0.091*** | 0.112*** | 0.104*** | 0.098*** | 0.107*** | 0.091*** |
| | (0.022) | (0.028) | (0.020) | (0.034) | (0.024) | (0.031) |
| *Sanderson Windmeijer F-stat* | | | | | | |
| Import | 113.283 | 164.523 | 139.186 | 114.899 | 118.522 | 188.930 |
| Export | 132.328 | 112.102 | 123.120 | 115.490 | 115.636 | 123.788 |
| Imports × Specificity | 149.819 | 205.154 | 182.783 | 152.559 | 157.952 | 231.675 |
| Exports × Specificity | 125.752 | 149.872 | 132.206 | 143.841 | 130.274 | 147.029 |
| *P-value of joint significance* | | | | | | |
| Imports | 0.001 | 0.007 | 0.000 | 0.013 | 0.001 | 0.020 |
| Exports | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 | 0.004 |
| R-square | 0.004 | 0.006 | 0.004 | 0.005 | 0.003 | 0.009 |
| Clusters | | | | | | |
| Number of observations | 286,214 | 336,279 | 380,676 | 235,379 | 440,680 | 195,083 |

Notes: Dependent variable: Cumulated earnings (normalized by base year income). Cumulated earnings are defined as the sum of total income from employment during the decade, divided by the base year income. 2SLS Regressions. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. The sample includes only workers (age $\leq$ 40) who remain in their initial occupation (Columns I) or establishment (Column III) for the whole observation period, or workers who switched out of their initial occupation (Column II) or establishment (Column IV) at some point during the decade. Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. * p<0.1; ** p<0.05; *** p<0.01.

Table J.2 repeats the analysis of Table 9, examining the relationship between demand shocks and worker mobility (establishment switches, occupation switches, and switches of the local labor market). We again find a positive coefficient estimate for the interaction term between import exposure and skill specificity for regional mobility. The estimated coefficient is moderately significant and larger than the one for the whole sample in Table 9. Moreover, we now find a moderately significant coefficient for the interaction term between import exposure and skill specificity for occupational mobility. These results show that young workers, in comparison to older workers, are more likely to respond to increasing trade exposure with regional- or occupational mobility.

Table J.2: Trade Exposure and Mobility (Young Workers)

| | Establishment | Occupation | LLM |
|---|---|---|---|
| Import exposure | -0.002 | -0.009* | -0.004 |
| | (0.008) | (0.005) | (0.005) |
| Export exposure | -0.009 | 0.010 | -0.004 |
| | (0.014) | (0.008) | (0.007) |
| Import × Specificity | 0.002 | 0.002* | 0.003** |
| | (0.002) | (0.001) | (0.001) |
| Export × Specificity | -0.000 | -0.002 | -0.001 |
| | (0.003) | (0.002) | (0.002) |
| *Sanderson Windmeijer F-stat* | | | |
| Import | 119.361 | 119.361 | 119.361 |
| Export | 124.529 | 124.529 | 124.529 |
| Imports × Specificity | 169.917 | 169.917 | 169.917 |
| Exports × Specificity | 139.303 | 139.303 | 139.303 |
| *P-value of joint significance* | | | |
| Imports | 0.552 | 0.128 | 0.094 |
| Exports | 0.737 | 0.323 | 0.834 |
| R-square | 0.001 | -0.001 | 0.001 |
| Clusters | | | |
| Number of observations | 767,487 | 767,487 | 767,487 |

Notes: Dependent variables: Column I: Dummy for leaving establishment (withing the decade following the base year). Column II: Dummy for leaving occupation. Column III: Dummy for leaving local labor market. 2SLS Regressions. All specifications include control variables for specificity, base year earnings, firm-level controls (five plant-size groups, four broad industry groups, dummies for federal states), and individual-level controls (nationality, education level, age). All first stage regressions include the same set of control variables as the corresponding second stage. The sample includes only young workers (age $\leq$ 40). Robust standard errors (in parenthesis) are clustered on start-of-period labor market region. Levels of significance: * p<0.1; ** p<0.05; *** p<0.01.