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Competing for workers: labor market transmission of firm export shocks [1]

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Abstract

We study the transmission of firm-specific export shocks through labor markets, examining how these shocks influence workers' wages at competing firms via labor market competition. Using rich Danish administrative data, a flexible, empirically driven definition of workers' outside options, and an identification strategy based on world import demand shocks, we show that both own-firm and competitor export shocks raise incumbent workers' wages. These export-driven shocks also trigger competitive worker reallocation, as expanding exporters poach workers from other firms. The results indicate spillover effects of export shocks extending to non-exporting firms.

Key Words: labour market competition, export shocks, worker outside options.

JEL Classification: F16, J23, J31, J63

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1 Introduction

Shifts in international trade are among the most fundamental forces shaping labor market dynamics at the regional level (Autor et al. (2013); Dauth et al. (2014)) and directly impact the outcomes of workers employed at firms engaged in global trade (Hummels et al., 2014). However, what remains unclear is how trade shocks initially experienced by specific firms are transmitted to workers at other firms and whether labor market competition among firms plays a significant role in this process. When an exporting firm faces a rise in foreign demand, does the benefit remain concentrated within the firm, or does it spill over to competing firms through labor market competition? Understanding these spillover dynamics is crucial not only for the evaluation of trade policies but also for a broader comprehension of how globalization shapes labor market outcomes.

Intuitively, greater exposure to competing firms that rely more on foreign revenues should amplify the transmission of export shocks. However, from a theoretical standpoint, the labor market response to firm-level export shocks also depends on the degree of labor market imperfections (Garin and Silverio, 2024). In perfectly competitive labor markets, workers' outside options reflect overall market conditions, and shocks affecting some firms will quickly diffuse through wage adjustments across competing employers (Autor et al., 2013). In contrast, in markets with substantial frictions, firms may operate more independently, internalizing shocks rather than generating substantial spillovers. At the same time, the mechanism of transmission - whether through competitive wage pressure or worker reallocation across firms - is influenced by the labor market institutions. However, empirically identifying these effects remains a significant challenge.

This paper examines how firm export shocks are transmitted through labor markets, leveraging Danish register data, which allows us to match linked employer-employee records with detailed data on firm export activities. We build on recent advancements in the literature (Nimczik, 2019; Schubert et al., 2024) to identify complex patterns of workers' outside options, using job-to-job transition data. To establish causality, we implement an instrumental variable strategy that identifies the effects of both own-firm and competitor export shocks, using exogenous shifts from product- and importer-country-specific world import demand (WID) shocks. Additionally, we exploit granular variation in competitor shocks, identifying effects within narrowly defined industries, municipalities, and even firms, allowing us to isolate the role of labor market competition from other potential transmission mechanisms.

Empirical research often relies on predefined proxies for labor market boundaries, such as industries, occupations, or geographic regions. However, these proxies involve some degree of arbitrariness in determining which outside options are relevant and may fail to capture the complexity of worker mobility. Consequently, a more flexible approach to defining labor markets is necessary to accurately identify spillover mechanisms. Building on the methodological advancements of Schubert et al. (2024) and using comprehensive job transition data, we define workers' outside options through observed job-to-job transition probabilities across occupation-municipality cells. This approach allows the data itself to reveal which outside options are most relevant for workers in a given cell. Importantly, it recognizes potential asymmetries in relevance, meaning that the importance of jobs in cell A for workers in cell B does not necessarily imply that the reverse holds. We use these transition probabilities to construct firm-specific measures of exposure to export shocks experienced by competing firms within their relevant labor markets, allowing us to capture how labor market competition transmits export shocks across firms.

To identify the causal impact of competitor-induced export shocks, we use exogenous shifts in WID as an instrumental variable. By combining global trade fluctuations with pre-determined firm-level export portfolios (across products and importing countries), this method isolates export variations that are orthogonal to local economic conditions and firm-specific productivity shocks. This approach has been previously applied to identify the effects of own-firm trade shocks (Hummels et al. (2014), Mayer et al. (2021)). We follow the recently proposed guidelines by Borusyak et al. (2025) for shift-share instrumental variable designs and discuss the identification through the lens of the shift-based approach. However, WID shocks for a given firm may be correlated with those experienced by firms competing in the same labor market (Garin and Silverio, 2024). We address this issue by simultaneously identifying the effects of both own WID-driven shocks and competitor WID-driven shocks, ensuring that our identification strategy accounts for labor market spillovers. Our approach differs from Garin and Silverio (2024), who decompose the WID shock into an idiosyncratic firm-specific component and a common component shared by potential competitors within the same narrow industry. Crucially, we do not assume that labor market competitors are the same as product market competitors, which is a key element, as the effect of competitor shocks through labor markets is the central focus of our analysis.

Export shocks are transmitted between firms through various channels, including product market competition, input-output linkages, and housing market spillovers. However, by applying a battery of granular fixed effects, we ensure that the identified spillovers from competing firms are most likely driven by labor market competition, rather than these alternative mechanisms. In our most restrictive specification, the identification of competitive spillovers relies on comparisons across workers within the same firm. Since these workers should be exposed to the same firm-level, industry, and regional shocks, but face different shocks to their outside options, this design strengthens our ability to isolate labor market competition as the primary transmission mechanism.

Our empirical analysis yields several key findings. First, we provide strong evidence that workers' outcomes are shaped not only by their own firm's export performance but also by export shocks to competing firms within their labor market. Specifically, positive export shocks to competitors lead to higher wages for incumbent workers and increase the likelihood of worker mobility toward expanding firms. This dual response highlights the importance of both competitive wage renegotiations and worker reallocations as key mechanisms through which export shocks propagate across the labor market.

Second, we document significant heterogeneity in these effects across worker demographics and firm characteristics. Wage spillovers from competitor export shocks are particularly pronounced among highly educated workers, who exhibit strong wage bargaining responses. Conversely, mobility responses are more prevalent among lower-skilled employees. Danish collective agreements coexist with substantial local wage setting. However, collectively negotiated wage floors, standard rates, and agreement cycles are likely to be more binding for lower-skilled workers than for higher-skilled workers. This pattern might be consistent with the differential role of labor market institutions in shaping workers' responses. At the same time, different groups of workers are sorted among different firm types. Larger and more productive firms are not only more strongly affected by their own export shocks but also experience stronger spillovers from competitors. This highlights the differential role that firms play in the transmission of trade shocks.

Third, we explore whether these spillover effects extend beyond exporting firms, potentially affecting non-exporters that compete for similar workers. Our findings demonstrate a distinct worker mobility pattern: as competing exporters expand, non-exporting firms experience worker outflows toward them. At the same time, the results for wage competition between exporting and non-exporting firms are sensitive to the definition of labor markets. Allowing labor market definitions to differ between exporters and non-exporters reveals significant wage spillovers from exporters to non-exporting incumbents.

Our findings contribute directly to several branches of literature. First, the project aims to contribute to the literature on trade-induced labor market spillovers. Existing papers fall into one of two categories. Studies in the first group focus on the effects of trade shocks on labor markets defined at various levels of aggregation (e.g., Revenga (1992), Autor et al. (2013), Dauth et al. (2014), Acemoglu et al. (2016), Dix-Carneiro and Kovak (2017a)). Other papers analyze firm-level responses to trade shocks (such as Hummels et al. (2014), Macis and Schivardi (2016), Utar (2018), Garin and Silverio (2024)). We aim to bridge these two branches of literature by investigating how export shocks spread between firms competing in the same labor market. The recent study by Garin and Silverio (2024) is closely related to ours. Furthermore, our study shares significant similarities with a paper on wage adjustment to firm-level and market-level TFPQ shocks by Carlsson et al. (2016). An important distinction from these studies is that we do not rely on industry-based definitions to identify labor market competitors. Instead, we use a data-driven approach based on observed worker transitions.

Secondly, our paper relates to studies that explore the connection between workers' outcomes and their outside options, which are central to theoretical models of both perfectly and imperfectly competitive labor markets (Manning (2003), Pissarides (2000), Postel-Vinay and Robin (2002)). However, empirical evidence on how workers' external options influence wage-setting at the granular worker-firm level remains limited. Caldwell and Harmon (2019) defines external options through coworker networks to estimate how shocks to these options affect wages and worker mobility. Beaudry et al. (2012) provides evidence that changes in local industry composition influence worker wages, suggesting that the presence of high-paying jobs enhances outside options. Caldwell and Danieli (2024) constructs an index capturing the quality of workers' external options based on the distribution of similar workers across jobs. The study closest to ours is Schubert et al. (2024), which defines labor markets using worker transitions across occupations, focusing on the impact of employer concentration on wages. Our contribution to this literature lies in combining a flow-based approach to identifying outside options with trade-induced shocks to these options, allowing us to analyze how export shocks propagate through labor markets via competition for workers.

The remainder of the paper is structured as follows. Section 2 describes our data sources and provides descriptive statistics. Section 3 details our methodological approach to defining workers' outside options via observed worker transitions across jobs. Section 4 presents the empirical framework used to identify spillovers from export shocks across firms competing in the same labor market for workers. Section 5 discusses our primary results, including heterogeneity analyses and robustness checks. Section 6 concludes.

2 Data

We combine Danish administrative registers on firms, workers, and trade transactions into a matched product-worker-firm panel covering 2010–2017.

2.1 Data Sources

Firm- and worker-level data are drawn from multiple administrative registers:

- *Universal Harmonized Database Index (UHDI)*: firm-product-level annual import and export flows, including the source/destination country and trade values at the 8-digit CN product level.[2] UHDI integrates trade data from Intrastat (intra-EU) and Extrastat (extra-EU), covers the vast majority of Danish trade flows, and serves to distinguish importers from exporters.[3]

- *FIRM*: a comprehensive dataset on Danish firms, covering firm characteristics – sales, wage bill, employment in full-time equivalents, sector, and industry – and high-level trade data.

- *BACI*: this CEPII dataset contains annual trade flows between countries at the 6-digit HS level, enabling the construction of firm-level instruments based on world import demand.[4]

- *Monthly Spell-Level Data (BFL)*: administrative data tracking monthly wage payments of all workers in Denmark. It provides precise information on earnings and work duration, improving our ability to compute annual and daily wages. Each monthly record identifies the employing firm and establishment, the workplace municipality, the worker's DISCO occupation code, and hours and days worked, which allows us to track individual work histories at the establishment level and to identify monthly employment transitions.

- *Population Registry*: supplements worker information with demographic and educational data, including gender, age, and education.

[2]Due to annual changes in CN classifications, we apply a methodology similar to Van Beveren et al. (2012) to harmonize product identifiers over time.

[3]Extrastat has a comprehensive scope, including all firms trading outside the EU. In contrast, Intrastat's is not as exhaustive. Firms with minimal export/import interactions with EU countries are not mandated to report such trades. The reporting threshold ensures approximately 97% coverage of total intra-EU exports.

[4]For more details on BACI, see Gaulier and Zignago (2010).

2.2 Sample Construction

The unit of observation is a firm-year in the firm-level statistics and a worker-year in the estimation sample. Both panels cover 2010–2017.

On the firm side, we keep private-sector firms and drop public-sector employers and firm-years with missing sales, wage bill, or employment. Two size restrictions apply, and both bind in the exporter and the non-exporter sample. We drop individual firm-years with fewer than two full-time equivalents, and we drop the entire firm when its 2010 employment falls below ten full-time equivalents.

On the worker side, we keep individuals aged 18–60 employed at a firm satisfying these restrictions. We start from the monthly BFL records and define a job spell as a continuous worker-firm match. We first aggregate these records to the worker-year level. For each worker and year we retain the primary spell, defined as the spell covering the most months of employment in that year and, among spells of equal length, the one with the highest labor income. Annual earnings sum labor income over the months of that spell within the year, and the daily wage divides earnings by days worked, so that a worker-year records one employer, one occupation, and one workplace municipality. We also drop spells with missing occupation codes.[5] Occupation and municipality of work are taken from the BFL record; the skill groups reported in the descriptive tables use the first DISCO digit. The job-to-job transitions used to estimate transition probabilities in Section 3 are computed from the monthly records before this aggregation and restrict to full-time workers with a primary employment in every month, so that measured flows are not dominated by the moves of part-time job holders.

We split firms by their 2010 export status. The *Exporter Sample* consists of firms recorded as exporters in UHDI in 2010 whose 2010 export share of total sales exceeds the first quartile of the 2010 export-share distribution among 2010 exporters. So the restriction removes firms for which

[5]Around 10% of observations in the raw monthly data have missing or imputed occupation information. As private firms with more than 10 employees are committed to reporting occupational codes of their workers, the fraction of missing codes is less than 1%. We run several robustness checks for different spell samples, results are presented in Table 9.

exporting is a negligible source of revenue rather than firms with a moderate export orientation. The marginal firm in the exporter sample earns roughly 1% of its revenue from exports, and the recorded 2010 exporters excluded by the cutoff fall below that threshold and enter the non-exporter sample.[6] This is our core sample, allowing us to control for export activity and to construct firm-level instruments based on WID fluctuations. The *Non-Exporter Sample* consists of the remaining private-sector firms, and therefore includes firms with minimal or no export activity in 2010, firms that begin exporting after 2010, and firms whose export transactions are not recorded in UHDI. Sample membership is fixed on 2010 export status and never updated, so it does not respond to subsequent export shocks; firms in the exporter sample can consequently report zero exports in later years, and firms in the non-exporter sample can report positive ones.

Two further restrictions govern the trade data entering the instruments. Firm export portfolios are product-country revenue shares averaged over 2006–2010, and world import demand is measured from BACI net of Danish flows. We drop product-country pairs in which Danish exporters account for more than 25% of world imports, so that the identifying variation is not driven by Denmark's own weight in world demand (Section 4). Transition probabilities that define the relevance of outside options are estimated from 2010 flows (Section 3).

2.3 Descriptive Statistics

2.3.1 Firm-level Data

Table 1 reports firm-level descriptive statistics. The *Exporter Sample* contains 3,884 firms and 28,694 firm-year observations; the *Non-Exporter Sample* contains 11,871 firms and 82,289 firm-year observations.

On average, exporters are larger (85.67 employees vs. 46.07 among non-exporters) and generate higher sales (316.8 vs. 117.1 million DKK). Wage bills follow a similar pattern, higher among exporters than among non-exporters.

Exporter firms pay higher average wages (526,000 DKK vs. 475,000

[6] We check the robustness of our results to alternative thresholds in Appendix Table B12.

DKK per worker) and exhibit higher productivity, with sales per worker of 3.6 million DKK against 2.3 million DKK among non-exporters. As expected, exporters report substantially greater total export values, 90.9 million DKK against 0.6 million DKK.

Consistent with these definitions, 89% of exporter firm-years record strictly positive exports, compared to 4% of non-exporter firm-years. Export share averages 33% for exporters and 0.3% for non-exporters. Additionally, half of exporting firms operate in manufacturing, compared to just 10% among non-exporters.

2.3.2 Worker-level Data

Table 2 presents worker-level descriptive statistics. The *Exporter Sample* comprises 1,925,986 worker-year observations, covering 391,680 workers and 438,872 job spells, while the *Non-Exporter Sample* includes 3,931,742 worker-year observations, 856,544 workers, and 1,087,439 job spells. Workers observed in both types of firms over the period are counted in both columns, so the two worker counts do not sum to the number of distinct individuals in the pooled sample.

Employees in exporting firms differ systematically from those in non-exporting firms. Workers in exporting firms are older (42.14 vs. 36.98 years) and have a smaller female share (0.328 vs. 0.380). Employees at exporting firms also earn higher annual earnings (12.55 vs. 11.91 in logs) and daily wages (6.94 vs. 6.47 in logs), with turnover lower compared to non-exporters (0.166 vs. 0.250).

Occupational composition varies distinctly: high-skill occupations account for 0.446 of worker-years among exporters against 0.303 among non-exporters, while medium-skill (0.445 vs. 0.529) and low-skill occupations (0.109 vs. 0.168) are more prevalent in non-exporting firms. Table B1 in the appendix compares the pooled analysis sample with the entire population of workers who satisfy the age restriction but not the firm- and export-based ones. The analysis sample covers 5,857,728 of the 23,624,482 worker-year observations in that population. Relative to the population, workers in our sample are less likely to be female (0.363 vs. 0.506), of similar age (38.68 vs. 38.32 years), higher paid (12.13 vs. 11.65 log annual

Table 1: Descriptive Statistics: Firms

	Exporters	Non-exporters
Firm Size	85.67	46.07
	(342.60)	(246.44)
Sales (1000 DKK)	316,768.60	117,065.20
	(1,761,163)	(1,339,927)
Wage Bill (1000 DKK)	48,249.32	23,077.43
	(237,986.40)	(110,070.60)
Wage Bill per Worker (1000 DKK)	525.66	474.85
	(166.31)	(245.70)
Sales per Worker (1000 DKK)	3,647.52	2,293.14
	(14,227.73)	(18,988.77)
Total Export (1000 DKK)	90,853.89	570.64
	(481,314.30)	(15,181.61)
Exporter (Share)	0.89	0.04
	(0.31)	(0.19)
Export Share	0.33	0.003
	(0.48)	(0.04)
Manufacturing (Share)	0.50	0.10
	(0.50)	(0.30)
Unique Firms	3884	11871
N obs	28,694	82,289

Note: This table reports means of firm-level characteristics, with standard deviations in parentheses, separately for the two firm samples used in the analysis. The unit of observation is a firm-year and the period covered is 2010–2017. Public-sector firms, firms with missing core variables, and firms with fewer than 10 full-time equivalent employees in 2010 are excluded from both samples. The *Exporter Sample* consists of private-sector firms recorded as exporters in the UHDI trade register in 2010 and whose 2010 export share of total sales exceeds the first quartile of the 2010 export-share distribution among 2010 exporters; this is the core sample used for all firm-level instrument construction and for the estimates in Tables 4–6. The *Non-Exporter Sample* consists of the remaining private-sector firms and therefore also contains firms with a very low 2010 export share, firms that started exporting after 2010, and firms whose export transactions are not recorded in UHDI; it is the sample used in Table 7. *Firm Size* is the number of full-time equivalent employees. *Exporter (Share)* is the share of firm-year observations with strictly positive exports recorded in UHDI in that year, and therefore need not equal one in the exporter sample, which is defined on 2010 export status only. *Export Share* is the ratio of export revenue to total sales. *Manufacturing (Share)* is the share of firm-year observations in manufacturing. All monetary variables are deflated and expressed in thousands of 2010 Danish kroner. *Unique Firms* counts distinct firms and *N obs* counts firm-year observations.

earnings), less mobile across firms (0.222 vs. 0.275), and less concentrated in high-skill occupations (0.350 vs. 0.413).

3 Identifying Labor Markets

3.1 Conceptual Approaches

A worker's outside options are a central concept in labor economics, yet finding an empirical proxy that is both theoretically meaningful and practically implementable remains a significant challenge. Conventional definitions typically rely on fixed geographical, industry, or occupational boundaries (e.g., Autor et al. (2016), Azar et al. (2022)).

This approach implicitly assumes two key conditions. First, it assumes that outside options can be defined by a clear-cut boundary, where certain jobs are considered relevant while others are excluded, rather than adopting a more continuous measure of relevance. Second, it assumes that relevance is inherently symmetrical - meaning that if job B is a relevant outside option for a worker in job A, then the reverse must also be true. However, these assumptions overlook substantial heterogeneity and asymmetry in observed worker mobility patterns. Recent empirical evidence suggests that worker transitions between job categories are not necessarily reciprocal. A high mobility rate from one occupational cell to another does not imply a comparable reverse flow (Schubert et al., 2024).[7]

To overcome these limitations, we leverage worker mobility across jobs to identify relevant outside options. Our empirical strategy adapts the framework of Schubert et al. (2024), who construct an "outside-occupation option index" using transition probabilities across granular occupations to weight connected destination wages. In our baseline specification, labor markets are defined as cell-specific vectors anchored at the 3-digit occupation by municipality level, where each cell's outside option set is mapped using observed worker flows to all other cells in the economy. While Schubert et al. (2024) construct transition matrices at a finer occupational level (6-digit SOC) nationally and re-weight them by local destination employ-

[7] Schmutte (2014) measures the degree segmentation in worker flows, thereby identifying barriers to mobility that standard economic models might overlook.

Table 2: Descriptive Statistics: Workers

	Exporter Sample	Non-exporter Sample
Female	0.328	0.380
	(0.470)	(0.485)
Age	42.14	36.98
	(12.12)	(13.76)
Log Annual Earnings	12.55	11.91
	(1.10)	(1.44)
Log Daily Wages	6.935	6.47
	(0.755)	(1.023)
Firm Change	0.166	0.250
	(0.372)	(0.459)
High-skill Occupation	0.446	0.303
	(0.497)	(0.459)
Medium-skill Occupation	0.445	0.529
	(0.497)	(0.499)
Low-skill Occupation	0.109	0.168
	(0.311)	(0.374)
Unique workers	391680	856544
Unique job spells	438872	1087439
N obs	1,925,986	3,931,742

Note: This table reports means of worker-level characteristics, with standard deviations in parentheses, separately for workers employed in the two firm samples. The unit of observation is a worker-year and the period covered is 2010–2017. Firm samples are defined as in Table 1. The worker-year panel is constructed by aggregating monthly employment spells from the BFL register: we retain workers aged 18–60, drop spells with missing occupation codes, and assign each worker-year to the primary spell, defined as the spell covering the most months of employment in that year and, among spells of equal length, the one with the highest labor income. *Log Annual Earnings* and *Log Daily Wages* are expressed in logs of 2010 Danish kroner, where daily wages are earnings divided by days worked in the spell. *Firm Change* is a binary indicator equal to one in the year in which the worker moves to a different firm. Occupational skill groups are defined from the first digit of the DISCO occupation code; throughout the analysis, occupation-municipality cells are instead defined at the 3-digit DISCO level. *Unique workers* and *Unique job spells* count distinct individuals and distinct worker-firm spells, and *N obs* counts worker-year observations.

ment shares, both strategies reflect a fundamental tradeoff between market-definition flexibility and the estimation sparsity inherent in granular flow data. We compare our approach to the results based on alternative choices in Appendix Table B13. Crucially, rather than imposing closed or symmetric market boundaries, our framework captures directional and asymmetric patterns of worker mobility.

Formally, for each occupation-municipality cell, we define its labor market by computing a vector of transition probabilities to all other cells. This transition probability vector provides a direct set of weights, which we use to determine the relevance of outside options for each worker employed in a given occupation-municipality cell. Once we establish relevance weights for different cells, we link them to firm-level export shocks. Each firm is assigned a weight proportional to its share of total employment within the respective occupation-municipality cell.

3.2 Methodology

We construct our empirical labor market vectors using highly granular monthly labor market spell data from Denmark. Specifically, we exploit detailed worker-level job-to-job transitions observed monthly[8] and aggregated annually between occupation-municipality cells. For each occupation-municipality pair defined by occupation o and municipality m, we calculate the probability of transitioning to every other cell (o', m') within a year. Formally, this transition probability is defined as:

$$\pi_{(om),(o'm'),t} = \frac{f_{(om),(o'm'),t}}{\sum_{(o',m')} f_{(om),(o'm'),t}} \tag{1}$$

where $f_{(om),(o'm'),t}$ denotes the total count of employment-to-employment transitions from occupation-municipality cell (o, m) to cell (o', m'), aggregated from monthly transition data.

The leave share for each cell is defined as the ratio of the total job-to-job transitions from cell (o, m) to all other cells, excluding transitions within the same cell, to the total number of employment-to-employment transitions originating from cell (o, m):

[8]To compute valuable employment-to-employment transitions we select full-time workers with a primary employment every month.

$$ls_{o,m,t} = \frac{\sum_{(o',m') \neq (o,m)} f_{(om),(o'm'),t}}{\sum_{(o',m')} f_{(om),(o'm'),t}} \quad (2)$$

This measure thus captures the proportion of workers leaving their current occupation-municipality cell to take employment in a different cell within a given year conditional on changing employment.

Our baseline specification relies on transition probabilities derived from worker flow data in 2010. We define weights using the first year of our dataset to mitigate concerns about endogeneity in worker flows. To address potential measurement error issues arising from data sparsity, we exclude occupation-municipality cells that either do not appear in all sample years or have fewer than 10 total annual transitions in 2010. This ensures that our transition matrix is constructed from sufficiently informative and stable flows.[9] Appendix Figure A1 provides further evidence supporting this methodological choice, illustrating a significant correlation of both intensity of flows and probability of transitions between cells across years as well. This underscores both persistent mobility patterns and some inherent year-to-year noise. As robustness checks, we explore alternative thresholds, utilize flows from other sample years, perform percentile trimming, and separately analyze exporter and non-exporter firm samples. These alternative approaches are comprehensively detailed in Section 5.4.

Using our granular occupation-municipality definitions, we document notably high worker mobility across cells, highlighting substantial heterogeneity and asymmetry in the Danish labor market. The overall leave share, measured as the share of people changing their occupation-municipality cell conditional on changing employment, is approximately 0.8, indicating that a large majority of job-to-job transitions involve workers moving to a different occupation, municipality, or both within a single year. This result underscores the relevance of analyzing labor markets at this granular level rather than relying solely on broader, predefined geographic or occupational categories.

Disaggregating this mobility reveals that geographic movements dom-

[9]On average, each cell has 39.4 job changers, with a median of 21. The vast majority of cell pairs have no observed transitions between them. Among the cell pairs where at least one transition is observed, the mean number of transitions is 2.2, and the median is 1.

inate occupational transitions. Specifically, the mean leave share across municipalities is approximately 0.7, whereas the mean leave share for occupational transitions at 3 digit level is around 0.5. These findings highlight that geographic transitions play a comparatively larger role in shaping labor market dynamics than purely occupational shifts.

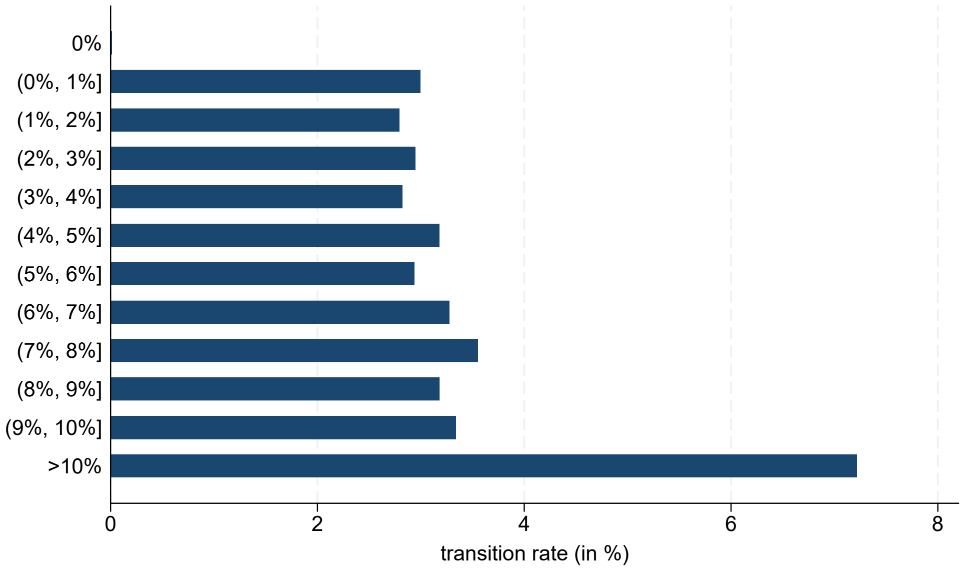
Moreover, we observe significant variation in worker mobility across occupation-municipality cells. The distribution of leave shares is notably skewed upward, with the first quartile at about 0.69 and the third quartile reaching approximately 0.94. Such considerable variation likely reflects differential integration of cells into broader labor market networks, driven by heterogeneous job availability, commuting costs, occupational specificity, and regional economic conditions.

An examination of the transition matrix reveals significant sparsity and asymmetry in worker mobility patterns. There are 2,897 occupation-municipality cells and on average a cell is connected to around 16 other cells by any flows and to 15 cells through flows of at least 1%. Figure 1 summarizes the directionality of worker flows across occupation-municipality cells. The majority of cell pairs are not connected through labor flows. On the extensive margin, there is a substantial relationship between inflows and outflows - when a transition occurs in one direction, it is likely that a transition also occurs in the opposite direction. For the small fraction of cells that experience very strong outflows (with a transition probability exceeding 10%), we also observe strong reverse flows (with a transition probability of more than 7%). In contrast, for the majority of connected cell pairs - where transition probabilities in one direction fall between 0% and 10% - there is no clear relationship between inflow and outflow intensities. This pronounced asymmetry underscores the importance of explicitly modeling directional labor flows, as labor market connections are inherently asymmetric rather than reciprocal.

4 Identifying Labor Market Competition

This paper examines how firm-level product demand shocks, induced by changes in global trade, propagate across firms through labor markets. To fully capture the transmission chain of export shocks via labor markets,

Figure 1: Asymmetry in Transition Probabilities Between Occupation-Municipality Cells



Note: This figure documents the extent to which labor market connections between occupation-municipality cells are reciprocal. The unit of observation is an ordered pair of occupation-municipality cells (k, k'), where occupations are defined at the 3-digit DISCO level and cells are restricted to those with more than 10 job changers in 2010 and present in all sample years. For each pair we compute the outward transition probability $\pi_{k,k'}$, defined as the share of all employment-to-employment transitions originating in cell k in 2010 that end in cell k', and the corresponding reverse transition probability $\pi_{k',k}$. Pairs are grouped into bins according to the outward probability $\pi_{k,k'}$ (vertical axis), and each bar reports the mean reverse transition probability $\pi_{k',k}$ within the bin (horizontal axis, in percent). Transitions are measured from monthly BFL employment spells for full-time workers with a primary employment relationship and aggregated to the year. Cell pairs with no outward flow exhibit essentially no reverse flow, and pairs with very high outward probabilities ($> 10\%$) display substantial reciprocal flows (above 7% on average). Over the intermediate range, which accounts for the large majority of connected pairs, the reverse probability is roughly flat in the outward probability, indicating that the strength of a connection in one direction is largely uninformative about its strength in the other. This asymmetry motivates the use of directional transition probabilities as weights in the construction of the competitor export measure.

it is essential not only to demonstrate how export fluctuations directly impact workers in exporting firms - a mechanism previously documented by Hummels et al. (2014) - but also to explore potential spillover effects on other firms, including both exporting and non-exporting firms.

While export fluctuations generate a complex set of general equilibrium effects on the economy (Dix-Carneiro and Kovak, 2017b), this paper focuses specifically on labor market competition as a transmission mechanism. The core mechanism operates through changes in workers' outside options.[10] Specifically, a positive export shock to a competitor of firm j can increase its attractiveness within the labor market. For a worker currently employed at firm j, this improves their outside option value, making the implicit risk or explicit threat of leaving more credible. As a result, firms may face greater wage competition, exerting upward pressure on wages across the market. Beyond wage adjustments, positive shocks to competitors can also lead to competitive worker reallocation, as employees leave firm j in favor of its expanding competitors. The strength of this transmission mechanism and the relative importance of wage and mobility adjustments depend on several factors, including firms' differential reliance on export markets, frictions that impede competitive pressures, and the prevalence of wage bargaining versus wage posting arrangements.

4.1 Empirical Framework

To explore the effects of competitors' exports on the labor market outcomes for workers employed at different firms, we propose two main regression specifications:

$$y_{ijt} = \alpha_{ij} + \beta \log(\text{Exp}_{jt}) + \gamma \, \text{CompExp}_{-j,t}(i) + \tau_{IND,t} + \tau_{MUN,t} + \varepsilon_{ijt}, \tag{3}$$

$$y_{ijt} = \alpha_{ij} + \delta_{jt} + \gamma \, \text{CompExp}_{-j,t}(i) + \varepsilon_{ijt}, \tag{4}$$

[10] Our focus is on identifying the immediate effects of export shocks, rather than capturing long-run effects such as knowledge and productivity spillovers from exporters. For instance, exporters may attract high-skilled workers and facilitate knowledge flows between firms (Helm, 2020).

where:

- y_{ijt} is an outcome variable - the log of annual earnings or daily wages of worker i at firm j in year t, or a firm change indicator. The first outcome is used to capture competitive pressure on incumbents' wages, while the second outcome to reflect the workforce reallocation triggered by the augmented labor demand from j's competitors.
- α_{ij} denotes spell fixed effects.
- δ_{jt} represents firm-year fixed effects.
- $\log(Exp_{jt})$ captures the log of exports for firm j in year t.
- $\tau_{IND,t}$ and $\tau_{MUN,t}$ control for 6-digit industry-year and municipality-year fixed effects, respectively.
- $\text{CompExp}_{-j,t}(i)$ is the leave-one-out competitor export measure for worker i at firm j in year t. Specifically,

$$\text{CompExp}_{-j,t}(i) \;=\; \sum_{k'} \pi_{k,k'}\, \overline{\log(\text{Exp})}^{-j}_{k',t},$$

  where $\pi_{k,k'}$ is the estimated transition probability weight between occupation-municipality cell k and occupation-municipality cell k' from Section 3 , and

$$\overline{\log(\text{Exp})}^{-j}_{k,t} \;=\; \sum_{h \neq j} w_{hkt}\, \log(\text{Exp}_{ht}).$$

  denotes the firm-cell specific leave-one-out average (log) export. Here, w_{hkt} denotes firm h's share of employment in cell k at time t, and the superscript $-j$ indicates that the focal firm j is excluded from the competitor-export measure. Notably, two workers employed by firm j in year t and belonging to the same occupation-municipality cell k will face the same competitors export shock.

In Eq. 3 and 4, our main coefficient of interest is γ, which represents the within-spell elasticity of wages (or the probability of a worker i at firm j leaving) to changes in exports from j's labor market competitors.

The effect of own firm export on wages and firm change are represented by β in Eq. 3. It is important to acknowledge that both a firm's own-export effect and the effect of competitors' exports depend crucially on firms' varying degrees of their export share over total revenues. Differences in the estimated competitor-export effects across labor markets thus do not solely reflect differences in labor market competition; they may also reflect heterogeneity in the extent to which competing firms depend on exports. Therefore, interpreting the magnitude of the competitor-export effect demands caution, as stronger responses could partially signal greater reliance on international markets rather than exclusively reflecting more intense labor market competition.

Holding worker flow weights fixed at their 2010 baseline constrains our estimates to an intention-to-treat type framework. In practice, the total impact of a competitor's export shock operates through two distinct channels: first, by affecting workers and firms in cells established as key outside options prior to the shock; and second, by altering mobility patterns themselves, thereby reshaping which destination cells serve as relevant outside options ex post. While we cannot separately identify these effects, we provide direct evidence that export shocks indeed reshape labor flows in the economy.

The identification in Eq. 4 comes from comparing within-spell changes in outcomes of workers who are employed in the same year at the same firm but have different outside options. This can happen when workers are in different occupations and/or at different workplace locations within a given firm. Therefore, they potentially face different export shocks to their outside options.[11]

We classify firms into two distinct categories: exporters and non-exporters.[12] In the exporter sample, we can separately identify the effects of a firm's own exports and those of its competitors by constructing WID instruments

[11] In both specifications we identify the immediate effect of export fluctuations. To measure turnover on the same timing as wage adjustment, the mobility indicator equals one in the year in which the worker leaves the focal firm, so that a separation in year t is matched to the export shock in year t, exactly as a wage observed in year t is matched to the shock in year t. Job spells are constructed accordingly, with the year of the separation as the final year of the spell.

[12] As discussed in Section 2, "non-exporters" refer to firms that were not observed exporting in 2010 or had a very low export share.

for both variables. In contrast, for non-exporters, we can only estimate the impact of competitors' export shocks, as these firms do not engage in exporting themselves. Hence, to analyze labor market spillovers of export shocks to non-exporting firms, we estimate specifications that introduce firm-year fixed effects and omit the own-export term in Eq. 4, allowing us to isolate the effects driven by labor market competition.

4.1.1 Identification Challenges

A fundamental identification concern is unobserved worker, firm, and match-specific heterogeneity. There are strong reasons to believe that the process through which workers and firms form matches may be correlated with exports at both the firm and market levels. To address this concern, both Eq. 3 and Eq. 4 include spell fixed effects. Identification therefore relies on within-spell variation in competitors' exports when estimating their effects on wages and worker reallocation.

Even after controlling for match-specific heterogeneity, the remaining variation in firms' own exports and competitors' exports may still be endogenous. In particular, the factors driving exports may simultaneously affect worker outcomes. For example, productivity shocks may affect both exports and workers' wages. This concern applies to both own-firm and competitor export effects. Firm-specific productivity shocks may bias the estimated effect of a firm's own exports, while productivity shocks shared by firms competing in the same labor market may bias the estimated effect of competitors' exports. To partially mitigate the influence of such common shocks, Eq. 3 includes six-digit industry-by-year and municipality-by-year fixed effects, while Eq. 4 includes firm-by-year fixed effects.

An ideal experiment would generate variation in export demand that is unrelated to these unobserved confounders. For example, one could imagine randomly assigning export subsidies across product-by-destination-country markets. Provided that firms competing for similar workers differ sufficiently in their export portfolios, such an experiment would generate variation that separately identifies the effects of own-firm and competitors' exports.

Our empirical strategy approximates this experiment using shift-share instruments that combine fluctuations in world import demand with predetermined firm-level product-country portfolios (Hummels et al., 2014). The design can be characterized as a shift-based approach to shift-share identification (Borusyak et al., 2025): identification is driven by changes in WID at the product-by-destination-country level. We first describe the instrument for a firm's own exports and then explain how the firm-level instruments are aggregated to construct the instrument for competitors' exports.

We construct a firm-level instrument as:

$$log(WID_{jt})^{\mathbb{IV}} = \sum_{c} \sum_{p} \overline{s}_{jcp} log(WID_{cpt}) \tag{5}$$

Here, $\overline{s}_{jcp}$ denotes the average share of firm j's export revenue associated with product p and destination country c between 2006 and 2010, while $WIDcpt$ is world import demand in country c for product p at time t (excluding Denmark). Changes in WID_{cpt} - the "shifts" are driving force of identification. They may reflect shocks to consumer tastes, the industrial use of products, and changing patterns of comparative advantage.

We next construct the instrument for the exports of firm j's labor market competitors. Identification again comes from the same product-country WID shifts. The competitors' instrument therefore aggregates the firm-level WID instruments defined above using the same transition-probability weights employed in the construction of the competitor export measure.

To avoid endogeneity arising from firm entry and exit, we fix the firm population and firm employment weights at their 2010 values. Specifically, we construct a synthetic panel of all firm-product-country combinations present in 2010 and project them through 2017 by combining firms' average 2006–2010 product-country portfolios with the time-varying WID measures. Firms thus remain in the construction of the instrument as if they continued to operate after 2010, even if they subsequently exited. This ensures that the competitors' instrument is not affected by endogenous changes in the firm population.

We apply the same leave-one-out market construction used for competitors' exports:

$$\text{CompWID}_{-j,t}(i) \;=\; \sum_{k'} \pi_{k,k'}\, \overline{\log(\text{WID})}^{-j}_{k',t} \;\cdot\; \mathbb{1}[\, j \in K_{2010}\,], \tag{6}$$

where $\text{CompWID}_{-j,t}(i)$ is the leave-one-out world-import-demand instrument for competitor exports, $\pi_{k,k'}$ is the transition probability weight from occupation-municipality cell k to cell k' from Section 3, and

$$\overline{\log(\text{WID})}^{-j}_{k',t} \;=\; \sum_{h \neq j} w_{hk'}\, \log(\text{WID}_{ht}),$$

is the firm-cell-level measure of WID that excludes the focal firm j. w_{jk} denotes 2010-fixed firm employment weights, which is pre-determined in the construction of the competitors instrument measure to avoid any endogeneity concern. The indicator $\mathbb{1}[\, j \in K_{2010}\,]$ denotes the fact that we restrict the instrument to the 2010 firm population.

We thus identify the effects of own-firm exports and competitors' exports within the same empirical framework, using the same underlying product-country WID shifts but two separately constructed endogenous variables and their corresponding instruments. Our approach differs from the recent contribution by Garin and Silverio (2024) in two respects. First, our mobility-based definition of labor markets contrasts with their reliance on narrow industry classifications as proxies for labor market boundaries. Second, when identifying the effect of own-firm exports, we explicitly account for analogous demand shocks experienced by the firm's labor market competitors. In contrast, Garin and Silverio (2024) develop a strategy that assumes that destination-specific product-demand shocks are firm-specific and orthogonal to the shocks experienced by competitors.[13] More broadly, our paper focuses on how labor market competition transmits export shocks across firms, rather than primarily on the effects of own-firm export shocks.

The validity of the design requires the product-country WID shifts to be orthogonal to unobserved determinants of Danish labor market outcomes. Our identification strategy therefore relies on the assumption that Danish exporters are small relative to the corresponding global product markets, so that changes in global import demand are not driven by Danish-specific

[13]We thank the anonymous referee for highlighting these important differences between the two approaches.

shocks that may also affect Danish labor markets. To further mitigate this concern, we exclude products for which Danish firms account for more than 25% of the global market. This restriction reduces the possibility that the identifying WID variation reflects endogenous domestic factors.

We also complement the analysis with balance tests. We conduct balance tests at three levels of aggregation: workers, firms, and occupation-municipality cells.[14] Using the 2010–2017 worker sample, we residualize the own-firm and competitors' WID shocks with respect to industry-by-year and municipality-by-year fixed effects. We then construct the following pre-period outcomes, measured in 2009: log daily wages, daily-wage growth, log annual earnings, annual-earnings growth, and an indicator for changing firms. For the firm- and cell-level tests, we aggregate these outcomes to the corresponding level and regress each pre-period outcome on the residualized WID shocks. These tests assess whether subsequent exposure to larger own-firm or competitors' WID shocks is systematically related to pre-period differences in wages, earnings, wage and earnings growth, or worker mobility. Appendix Table B7 provides little evidence of such relationships. The estimated associations are small and generally statistically insignificant, suggesting that the identifying WID variation is not systematically related to pre-period worker, firm, or cell characteristics.

A further requirement of the shift-based design is that identification should not be driven by a small number of influential shifts. Table 3 presents descriptive statistics for the product-country WID shifts. WID varies annually across products and destination countries, yielding more than 128,000 shifts in an average year. Following Borusyak et al. (2025), we also calculate the effective number of shifts for each instrument. This statistic is informative because designs with a small effective number of shifts may be disproportionately sensitive to a limited number of shocks. We assign each shift an importance weight equal to its average exposure weight across observations in a given year. The effective number of shifts is then defined as the inverse of the sum of squared importance weights. It is approximately 2,000 per year for the own-firm instrument and approximately 5,000 per year for the competitors' instrument. Thus, neither

[14] Because the shifts are defined at the product-by-destination-country level, predetermined covariates are not available at that level.

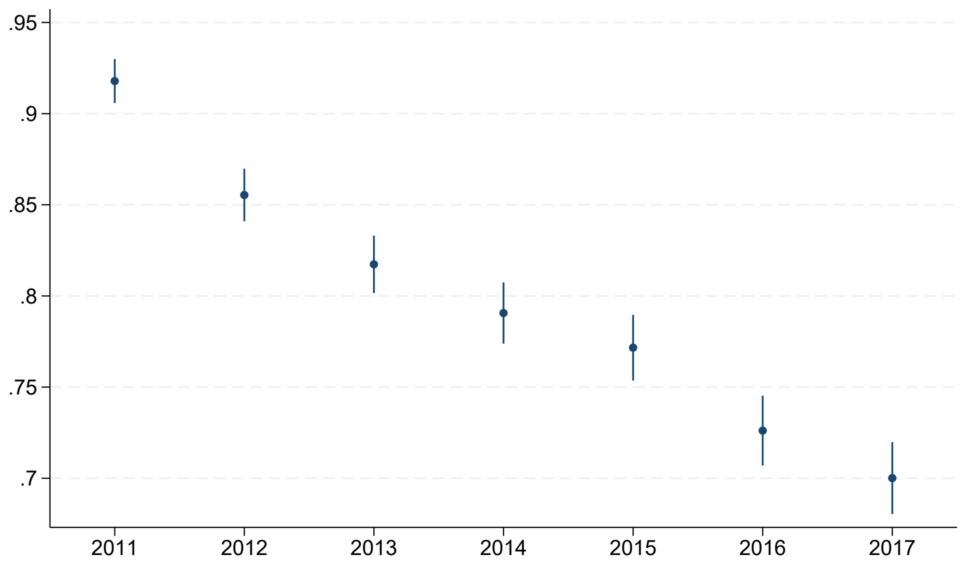
instrument is driven by only a small number of product-country shocks.

The validity of a shift-share design also requires either that the exposure shares sum to one or that appropriate controls for incomplete shares are included (Borusyak et al., 2025). In our setting, the shares do not sum to one for either instrument. For the own-firm instrument, shares may sum to less than one because we exclude product-country markets in which Danish exporters hold a global market share above 25%. Firms exporting these products to these destinations therefore have part of their export portfolio omitted from the instrument. For the competitors' instrument, incompleteness additionally arises because competing firms' WID shocks within each cell are weighted by their shares of total cell employment. We therefore control in all regressions for the sums of shares associated with both the own-firm and competitors' instruments, interacted with year indicators. Figure A2 presents the residual variation in both instruments together with the corresponding distributions of the sum-of-shares controls.

Finally, the relevance of both instruments requires firms' initial export compositions to remain sufficiently stable over time. This assumption is plausible given the substantial costs associated with changing export destinations or product portfolios. Figure 2 illustrates the stability of firms' export portfolios over time. Specifically, it reports coefficients from firm-level regressions of export portfolios in year t on their corresponding values in 2010. A coefficient close to one indicates that firms largely preserve their product composition across export markets, whereas lower coefficients indicate greater portfolio turnover. The results show strong persistence in firms' export portfolios, supporting the use of predetermined export compositions to construct subsequent export-demand instruments.

Our interpretation is that we identify the effects of competitor export shocks that are transmitted through labor market competition. However, it is clear that exporting firms can influence other firms in the economy through multiple channels beyond labor markets. For instance, firms trade with each other, and trade shocks may propagate through input-output linkages. Additionally, firms compete in domestic markets, meaning that export shifts could alter product market interactions. Finally, local housing demand fluctuations may also impact firm outcomes, particularly if labor markets are geographically concentrated. Despite these alternative

Figure 2: Stability of Product Portfolios Over Time



Note: This figure assesses the persistence of the firm-level export portfolios used to construct the shift-share instruments. For each year $t \in \{2011, \ldots, 2017\}$ we estimate a separate OLS regression of a firm's export revenue share in product-country pair (p, c) in year t on the corresponding share in 2010, pooling across all firm-product-country combinations in the *Exporter Sample*; each point reports the estimated slope coefficient from one such regression and the vertical bars are 95% confidence intervals based on standard errors clustered at the firm level. The sample consists of firms recorded as exporters in UHDI in 2010 with at least 10 full-time equivalents in 2010 and a 2010 export share above the first quartile of the 2010 export-share distribution, with the firm population held fixed at its 2010 composition. A coefficient close to one indicates that firms allocate their exports across destinations and products in year t in essentially the same proportions as in 2010; lower coefficients indicate reallocation of the export portfolio. The estimated persistence declines gradually from about 0.92 in 2011 to about 0.70 in 2017, which supports the use of pre-determined 2006–2010 average export shares as exposure weights in Equation (5).

Table 3: Shift-Level Descriptive Statistics

	Shifts	Firm IV Weights	Comp IV Weights
Panel A: Log WID			
Mean	8.60	11.51	11.27
Standard Deviation	2.45	2.36	2.51
(Eff.) Number of Shifts	128,035.25	1,991.52	5,082.62
Panel B: Change in WID			
Mean	0.02	0.01	-0.00
Standard Deviation	0.62	0.47	0.50
(Eff.) Number of Shifts	127,409.13	1,992.22	5,083.46

Note: This table reports shift-level descriptive statistics for the world import demand (WID) shocks that underlie our shift-share instruments, following the diagnostics recommended by Borusyak, Hull, and Jaravel (2025). A shift is a country-product-year cell. Panel A describes the shifts in levels (log WID); Panel B describes their year-on-year change (Δ log WID). Within each panel, the column *Shifts* reports unweighted moments of the shift distribution, while the columns *Firm IV Weights* and *Comp IV Weights* report moments weighted by the importance (exposure) weights that the shifts receive in the own-firm and competitor instruments, respectively; these importance weights are the average product-country export-revenue shares aggregated to the shift level.

transmission mechanisms, our empirical approach-specified in (3) and (4)-provides strong evidence that the effects we identify are most likely driven by labor market competition. If shocks were primarily transmitted through input-output links or product market interactions, they should affect all workers within the same firm-or at least within the same six-digit industry-in a similar manner. However, workers within the same firm may face different outside option shocks, depending on their occupation and the set of alternative job opportunities available to them. Similarly, workers within the same municipality-while subject to common local housing shocks-may still be exposed to different labor markets based on their occupations. This variation in outside options across workers is the key source of identification that allows us to isolate the labor market transmission of export shocks. Additionally, we conduct further robustness checks that reinforce the credibility of our interpretation of the results.

5 Results

5.1 Labor Market Competition among Exporters

Building on the framework outlined in Section 4, we examine how workers are affected by export shocks to their own firms as well as by similar shocks to their outside options. Our analysis focuses on two key mechanisms of labor market adjustment: (i) wage competition among firms, (ii) worker reallocation across firms.

We begin by investigating how firms adjust wages for stayers in response to their own export shocks and the export shocks experienced by their competitors. We then examine how these dynamics shape worker mobility patterns, particularly the likelihood of moving to a new employer within or outside the local labor market. Next, we explore heterogeneity in the transmission of export shocks across different worker groups and firm types. Finally, we assess whether labor market spillovers extend beyond exporting firms to impact wages and mobility in non-exporting firms. We compare our findings to alternative definitions of labor markets and conduct a series of robustness checks to validate our results.

5.1.1 Wage Competition and Worker Reallocation among Exporters

If labor markets are characterized by imperfections, fluctuations in a firm's export demand can translate into changes in incumbent workers' wages (Card et al., 2018; Garin and Silverio, 2024). When firms and workers negotiate compensation, positive product demand shocks from abroad may give workers greater bargaining power to secure wage increases. At the same time, competitive pressures in the labor market can also drive wage adjustments. When exporting firms that hire from the same labor market expand, employers may be compelled to raise wages for incumbent workers to remain competitive. The extent to which these forces shape transmission of export shocks through labor markets depends not only on the degree of labor market frictions but also on the relative importance of exports to competing firms.

Table 4 presents the effects of both a firm's own export shocks and export shocks from labor market competitors on incumbent workers' annual earnings and daily wages. While daily wage effects serve as a proxy for the price dimension of labor adjustments, annual earnings also capture quantitative effects, including worker mobility decisions. If competitive pressure increases wages but simultaneously induces workers to leave the firm, the net effect on annual earnings may be ambiguous. However, both daily wages and annual earnings respond positively to both own and competitor export shocks.

The first row of Table 4 reports the β coefficients, which reflect the impact of a firm's own export shocks. The second row presents the γ coefficients, indicating the effect of competitors' exports. The IV estimates remain significantly robust across all wage specifications. β coefficients are not identified in the model that includes firm-year fixed effects.

Importantly, these positive effects are identified in a specification that includes a set of granular fixed effects. In addition to spell fixed effects, which ensure that the effects are identified based on job stayers, our baseline specification includes six-digit industry-by-year and municipality-by-year fixed effects. As emphasized earlier, this setup ensures that the estimated competitive effects are driven by labor market competition rather than alternative mechanisms. Furthermore, the competitor effects remain robust even in the model with firm-year fixed effects. In this specification, identification comes from job stayers within the same firm and year but exposed to different labor markets, making it impossible to identify own-firm effects within this framework. Appendix Table B2 illustrates how both own and competitor effects evolve as we progressively saturate the model with fixed effects. Table B3 presents the corresponding first-stage results.

Wage adjustments for stayers, however, are not the only channel through which export shocks propagate in labor markets. On one hand, firm-specific labor demand shifts can influence worker turnover. Expanding firms may have a greater ability to retain workers, while shrinking firms may struggle to do so. On the other hand, when competitors grow, firms may lose workers to them.

Table 5 illustrates these patterns of competitive worker reallocation driven by export shocks. Importantly, we differentiate between two types of worker mobility: within-market mobility and outside-market mobility.

Table 4: Export Shocks & Wage Competition

	Annual Earnings			Daily Wages		
	OLS	IV	IV	OLS	IV	IV
Own Export	0.030***	0.032***		0.005***	0.009**	
	(0.003)	(0.009)		(0.001)	(0.004)	
Comp. Export	0.009***	0.012***	0.010***	0.005***	0.005**	0.005**
	(0.002)	(0.004)	(0.003)	(0.001)	(0.003)	(0.002)
6-digit Industry-Year FE	Yes	Yes	No	Yes	Yes	No
Municipality-Year FE	Yes	Yes	No	Yes	Yes	No
Spell FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Year FE	No	No	Yes	No	No	Yes
Observations	1,922,469	1,922,469	1,921,530	1,922,469	1,922,469	1,921,530

Note: This table reports estimates of the effect of export shocks on the wages of incumbent workers, based on Equation (3). The unit of observation is a worker-year and the estimation sample is the *Exporter Sample* over 2010–2017. The dependent variable is log annual earnings in columns (1)–(3) and log daily wages in columns (4)–(6). *Own Export* is the log of the total export value of the worker's own firm in year t. *Comp. Export* is the leave-one-out competitor export measure $\sum_{k'} \pi_{k,k'} \log(\text{Exp})^{-j}_{k',t}$: a weighted average of the log exports of all other firms employing workers in occupation-municipality cells k', where the weights $\pi_{k,k'}$ are the job-to-job transition probabilities from the worker's own cell k to cell k' estimated from observed 2010 worker flows, firms within a cell are weighted by their employment share in that cell, and the worker's own firm j is excluded. Columns (1) and (4) report OLS estimates. Columns (2)–(3) and (5)–(6) report 2SLS estimates in which *Own Export* and *Comp. Export* are instrumented by the corresponding shift-share world import demand (WID): the firm-level instrument combines 2006–2010 average product-country export revenue shares with contemporaneous world import demand for that product-country pair excluding Denmark, and the competitor instrument aggregates these firm-level instruments using the same 2010 transition probabilities and 2010-fixed firm employment weights, holding the firm population fixed at its 2010 composition. All specifications include the incomplete-shares control interacted with year fixed effects. All columns include job-spell fixed effects, so that identification comes from within-spell (job-stayer) variation. Columns (3) and (6) replace the industry-year and municipality-year fixed effects with firm-year fixed effects; *Own Export* is absorbed by these fixed effects and therefore cannot be identified, which is why the corresponding cells are left blank. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Since we do not define markets as closed sets of outside options but rather as a continuous, directional relevance measure, we distinguish between mobility to jobs with positive relevance weights and those with zero relevance weights in our framework. As we mentioned before, the transition matrix on which the weights are calculated is sparse, so this categorization appears natural. However, in Appendix Table B11 we also check the robustness of the results to alternative placements of this boundary, applied to the classification of moves as within or outside the market.

The first outcome variable captures the probability that a worker moves to a job at another firm in a municipality-occupation cell with a positive relevance weight. In other words, this measures the likelihood of a worker transitioning to a labor market competitor that is experiencing an export shock. The second outcome variable measures the probability that a worker moves to a job that our approach does not identify as a relevant outside option (i.e., a destination cell with a zero weight in our transition matrix). In this case, mobility occurs outside the segment directly affected by the export shock.

As shown in Table 5, an increase in a firm's own exports makes workers less likely to leave, both within and outside the market, with a slightly stronger response in within-market mobility. This result complements the wage effects: as firms benefiting from export expansion share these benefits with incumbent workers, workers are generally less likely to leave, especially for a competing firm.

At the same time, expanding competitors are more likely to poach workers, as evidenced by the increased probability that a worker moves within the market. This pattern reflects competitive labor reallocation. However, when competitors grow due to export-driven expansion, workers are also more likely to stay within the market, suggesting that rising job opportunities within the relevant labor market reduce exits to unrelated segments. Importantly, both competitor export effects remain robust even when including firm-year fixed effects, further reinforcing that these patterns are driven by labor market competition rather than alternative mechanisms.

Movement of workers toward expanding competitors also implies dynamic reshaping of labor markets through export shocks. As we discussed before, when defining the competitor export measure and corresponding in-

Table 5: Export Shocks & Worker Mobility

	Firm Change (Within the Market)			Firm Change (Outside the Market)		
	OLS	IV	IV	OLS	IV	IV
Own Export	-0.012***	-0.020***		-0.006***	-0.017***	
	(0.002)	(0.004)		(0.001)	(0.003)	
Comp. Export	0.005***	0.015**	0.019***	0.001	-0.026***	-0.017
	(0.001)	(0.008)	(0.007)	(0.001)	(0.010)	(0.010)
6-digit Industry-Year FE	Yes	Yes	No	Yes	Yes	No
Municipality-Year FE	Yes	Yes	No	Yes	Yes	No
Spell FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Year FE	No	No	Yes	No	No	Yes
Observations	1,885,637	1,885,637	1,884,829	1,885,637	1,885,637	1,884,829

Note: This table reports estimates of the effect of export shocks on worker mobility, based on Equations (3) and (4). The unit of observation is a worker-year and the estimation sample is the *Exporter Sample* over 2010–2017. The dependent variable is a binary indicator equal to one in the year in which the worker moves to a different firm, and job spells are constructed accordingly, so that a move in year t is matched to the export shock in year t — the same timing convention used for the wage and earnings outcomes in Table 4. In columns (1)–(3) the indicator equals one only for *within-market moves*, defined as job-to-job transitions from the worker's own occupation-municipality cell i to a cell j with a strictly positive 2010 transition probability, where cell i must have more than 10 total job changers in 2010 and both cells i and j must be present in all sample years. In columns (4)–(6) the indicator equals one only for *outside-market moves*, defined as transitions to any residual cell that does not satisfy these criteria. *Own Export* is the log of the total export value of the worker's own firm in year t, and *Comp. Export* is the leave-one-out, transition-probability-weighted average of log exports of firms employing workers in the cells connected to the worker's own cell, excluding the worker's own firm; both are defined exactly as in Table 4. Columns (1) and (4) report OLS estimates. Columns (2)–(3) and (5)–(6) report 2SLS estimates in which both export measures are instrumented by the corresponding shift-share world import demand (WID) instruments. All columns include job-spell fixed effects. All specifications include the incomplete-shares control interacted with year fixed effects. Columns (3) and (6) replace the industry-year and municipality-year fixed effects with firm-year fixed effects, which absorb *Own Export* and leave it unidentified; the corresponding cells are blank. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

strument we use transition matrix weights defined in 2010 and, hence, we do not capture this reshaping in our estimates. However, it is possible that export shocks to competitors also reinforce the importance or relevance of their jobs as outside options. Appendix Table B6 examines this directly, regressing the realized transition probability between an origin and a destination cell on export shocks to each side of the pair. Export shocks to the origin cell reduce outflows to other cells, while shocks to the destination cell raise the intensity of transitions into it, and both effects are unchanged across every fixed-effect structure we consider, including one that compares the same ordered cell pair over time. Because our weights are fixed in 2010, the estimates reported above are intention-to-treat with respect to this reshaping: they hold the map of outside options fixed while export shocks are redrawing it. The direction of the reshaping implies that a design allowing weights to respond would attribute more competitive reallocation to competitor shocks rather than less, since cells receiving positive shocks become more relevant destinations than their 2010 weights record.

5.1.2 Heterogeneity in Labor Market Transmission of Export Shocks

To better understand how export shocks propagate between firms in the labor market, Table 6 examines the heterogeneity in wage and mobility effects across different groups of workers and types of firms. Differences in both own and competitor effects reflect, in part, the relative importance of exports across firms and markets. However, they also provide insights into how labor markets competition transmit export shocks in different market segments.

First, we observe distinct patterns across skill levels. The most highly educated group of workers ("High Ed") experience a much stronger wage response to competitors' export shifts than we observed on average across workers. At the same time, wage responses for low-educated workers ("Below Tertiary Edu") are statistically insignificant. This suggests that the most educated incumbent workers benefit the most from both their own firm's export expansion and that of their labor market competitors. When looking at the effects on competitive worker reallocation, we observe the opposite pattern. Low-educated workers exhibit a stronger mobility response to both own-firm and competitor export shocks, while we do not observe statistically significant mobility responses for the most educated workers.

This pattern indicates that lower-educated workers respond more strongly through mobility to competitor firms, whereas higher-educated workers exhibit larger wage responses within their current employment. One possible interpretation is that labor market institutions generate different degrees of wage flexibility across worker groups. Although collective bargaining is widespread in Denmark, collectively negotiated wage floors, standard rates, and agreement cycles may be more binding for lower-educated workers. In contrast, higher-educated workers may have greater scope for individualized or firm-level wage adjustment. Under this interpretation, lower-educated workers adjust relatively more through mobility when outside opportunities improve, whereas higher-educated workers adjust relatively more through wages. We can not, however, unambiguously interpret these results as direct evidence of a strict distinction between different wage-setting mechanisms. Education groups differ along many other dimensions, including firm and market types. The observed heterogeneity may therefore reflect both differences in wage-setting institutions and the sorting of workers across firms and jobs. Another explanation might be that, because collective agreements shape wage bargaining, competitors' pressures might not materialize instantaneously but rather act with a lag. Moreover, this might be more pronounced for lower-educated groups of workers. Appendix Table B8 attempts to separate immediate and lagged effects and shows that while it is the case that competitors' export shock (as opposed to own wage effects) has lagged effects on average, this pattern does not explain the absence of wage effect for the least educated group.

There are significant differences in how the transmission of export shocks across firms affects workers of different demographic groups. Female workers experience stronger wage effects from competitor export shocks than male workers, whereas mobility responses are similar across gender for both own and competitor shocks.

Age heterogeneity separates the two margins. Wage responses to both own-firm and competitor export shocks are positive and concentrated among workers under 45, and neither effect is statistically significant for workers aged 45 and above. Mobility reverses this ranking for own-firm shocks. An expansion at a worker's own firm lowers the probability of leaving in both age groups, and this retention effect is larger in magnitude for workers aged 45 and above. Competitor shocks raise the probability of moving, but only among younger workers; the mobility of older workers does not respond to shocks at competing firms. Younger workers therefore respond to changes in their outside options on both margins.

As is well known and also shown in Table 1, exporting firms tend to be larger, more productive, and pay higher wages. However, it remains unclear how these characteristics influence the effect of export shocks on labor markets - not only in terms of whether these firms exhibit a higher pass-through of their own export shocks to workers, but also whether their workforce is more sensitive to export shifts among competing firms.

Table 6 examines whether these characteristics are associated with differences in the transmission of export shocks, both from firms' own export performance and from export shifts among their labor market competitors.

Competitor-induced wage spillovers are concentrated among workers at

Table 6: Effects of Export Shocks by Subsample

		Daily Wages		Mobility (Within Market)	
		Own Export	Comp Export	Own Export	Comp Export
Education	Below Tertiary	0.009**	0.001	-0.019***	0.028***
		(0.004)	(0.002)	(0.004)	(0.010)
	Tertiary	0.009	0.010***	-0.018***	0.012*
		(0.008)	(0.003)	(0.004)	(0.007)
	High Edu	0.026*	0.019***	-0.001	0.002
		(0.015)	(0.006)	(0.008)	(0.014)
Gender	Male	0.010**	0.004*	-0.020***	0.018**
		(0.004)	(0.002)	(0.004)	(0.009)
	Female	0.007	0.008**	-0.017***	0.020**
		(0.006)	(0.004)	(0.004)	(0.009)
Age	Under 45	0.012**	0.009***	-0.018***	0.042***
		(0.006)	(0.003)	(0.004)	(0.010)
	45 and Over	0.006	-0.002	-0.023***	0.007
		(0.004)	(0.002)	(0.004)	(0.008)
Firm Characteristics	Small–Medium	0.008**	0.002	-0.011***	0.015**
		(0.004)	(0.002)	(0.002)	(0.007)
	Large	0.028**	0.008**	-0.010***	0.014
		(0.013)	(0.004)	(0.004)	(0.010)
	Low Prod.	0.002	0.002	-0.012***	0.005
		(0.006)	(0.002)	(0.003)	(0.007)
	High Prod.	0.010**	0.008**	-0.019***	0.031***
		(0.004)	(0.003)	(0.005)	(0.011)
	Low Wages	0.009***	-0.001	-0.008**	0.026
		(0.004)	(0.003)	(0.004)	(0.016)
	High Wages	0.007	0.009***	-0.024***	0.015**
		(0.007)	(0.003)	(0.004)	(0.006)
	Low Export Int.	0.011***	0.001	-0.008***	0.003***
		(0.004)	(0.002)	(0.002)	(0.001)
	High Export Int.	0.010*	0.006**	-0.023***	-0.001
		(0.006)	(0.003)	(0.005)	(0.001)

Note: This table reports 2SLS estimates of own-firm and competitor export shocks by worker and firm characteristics. Each row is a separate regression on the indicated subsample of the *Exporter Sample* over 2010–2017 (see Table 1); the unit of observation is a worker-year, and *Own Export* and *Comp Export* are the two coefficients of interest from that single regression, estimated jointly. The dependent variable is log daily wages in columns (1)–(2) and a binary indicator equal to one in the year in which the worker moves to a different firm in columns (3)–(4), defined as a move to a cell carrying a strictly positive 2010 transition probability, with the cell-inclusion rules of Table 5. Both regressors are instrumented by the corresponding shift-share world import demand (WID) instruments, and the incomplete-shares control reconstructed within each subsample. All specifications include job-spell, municipality-year, and 6-digit industry-year fixed effects, so that identification comes from within-spell variation among job stayers. Education groups are *Below Tertiary* (at most upper-secondary or vocational), *Tertiary* (short- and medium-cycle), and *High Edu* (long-cycle and above). Firm characteristics are fixed at 2010 values: *Large* firms employ more than 250 FTE; *High Prod.*, *High Wages*, and *High Export Int.* firms lie above the median of 2010 sales per worker, wage bill per worker, and export share of sales respectively; complementary groups are defined analogously. Standard errors, in parentheses, are clustered at the firm level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

large, high-productivity, high-paying, and export-intensive firms. The corresponding estimates for small and medium-sized firms, low-productivity firms, low-paying firms, and firms with low export intensity are statistically indistinguishable from zero. Own-firm wage pass-through follows a similar pattern along the size and productivity dimensions: the estimate is substantially larger at large firms and is not precisely estimated at low-productivity firms. The pattern is less clear across the firm-wage distribution, as the own-export effect is precisely estimated at low-paying firms but not at high-paying firms, despite similar point estimates.

The mobility results distinguish between firms' retention responses to their own export expansion and workers' movements toward expanding competitors. The retention effect is largest at high-paying, export-intensive, and high-productivity firms, while it varies little by firm size. Mobility responses to competitor shocks are strongest at high-productivity firms and are also present at high-paying and small and medium-sized firms. The estimate for large firms is similar in magnitude but less precise.

The contrast by export intensity is particularly informative. Workers at export-intensive firms receive wage increases when competitors expand but show no corresponding mobility response. At firms with low export intensity, competitor shocks instead increase mobility without affecting wages. This pattern reinforces the caution raised in Section 4: the magnitude and margin of adjustment to competitor shocks may reflect not only the strength of labor market competition, but also the focal firm's capacity to respond using export-generated revenues.

5.2 Labor Market Competition among Non-Exporters

A crucial piece of evidence on how labor markets mediate the transmission of export shocks throughout the economy comes from their effects on non-exporting firms. Although these firms are not directly exposed to changes in global demand, they compete for workers with exporting firms that are affected by such shocks. The strength of this transmission may, however, depend on differences in mobility patterns across labor market segments and on frictions that limit competition between firms.

Table 7 presents the results for workers employed at non-exporting firms, using two alternative definitions of the transition probabilities that determine competitor weights.[15] In both panels, the focal firms are non-exporters, while the competitor export shocks are generated by firms belonging to the 2010 exporter sample. Thus, the source of the shocks is the same across the two panels; only the transition matrix used to weight exporting competitors differs.

In Panel A, competitor weights are constructed using all job-to-job transitions observed in 2010, irrespective of whether the origin and destination firms are exporters or non-exporters. This is the same weighting procedure used in the baseline analysis of exporting firms. In Panel B, the weights are instead constructed using only transitions between non-exporting firms. The second approach allows the structure of workers' outside options to reflect mobility patterns specific to the non-exporting segment. Importantly, it does not redefine the firms generating the export shocks: in both panels, workers at non-exporting firms are exposed to shocks originating from exporting competitors.

The effects of competitor export shocks on workers at non-exporting firms are estimated using our most restrictive specification, which includes firm-year fixed effects. Intuitively, the specification compares workers employed by the same firm in the same year who are differentially exposed to export-driven changes in their outside options. This design isolates variation operating through worker-specific labor market exposure rather than mechanisms common to the firm, municipality, or industry.

Turning first to wage spillovers, Panel A provides no evidence that competitor export shocks affect wages or earnings at non-exporting firms when competitor weights are constructed using all job-to-job transitions. In Panel B, where the weights reflect mobility patterns among non-exporting firms, the estimated wage spillovers are of the similar magnitude to the ones observed between exporters. This difference suggests that the relevant structure of outside options may differ between workers employed at exporting and non-exporting firms.

Both panels also show significant reallocation from non-exporters toward exporters within the relevant labor market. The magnitudes should

[15] Table B4 presents the build-up of the first stage for non-exporters as the model is progressively saturated with fixed effects.

not, however, be compared directly across panels. We define within-market reallocation as a move to a destination cell with a positive transition probability in the corresponding 2010 transition matrix. Because the matrix based on all job-to-job transitions is mechanically less sparse than the matrix based only on transitions among non-exporters, the set of moves classified as within-market differs between the two specifications.

Overall, the results show that export shocks affect not only directly exposed firms and other exporters, but also workers in the non-exporting sector. The precise magnitude and form of these spillovers depend on how the outside options of workers at non-exporting firms are represented, suggesting that mobility patterns and labor market segmentation shape the transmission of export shocks across firms.

5.3 Standard Proxies

Table 8 examines the results under alternative labor market definitions. Geography-, industry-, and occupation-based classifications are widely used to delineate labor markets because they are transparent and straightforward to implement (e.g., Autor and Dorn, 2013; Kline and Moretti, 2014). Their main limitation is that they impose discrete boundaries that are not directly informed by observed worker mobility, thereby treating all jobs within a market as relevant outside options and all jobs outside it as irrelevant.[16]

Table 8 considers six alternative proxies: two- and six-digit industries, two- and six-digit occupations, commuting zones, and municipalities. Panel A reports effects on daily wages, while Panel B reports effects on within-market mobility. Under each definition, a cell is treated as a self-contained labor market, and competitor exports and instruments are constructed as employment-weighted averages across firms in that cell. Thus, all firms within a cell are treated as equally relevant competitors, conditional on their employment shares.

The available fixed-effect structure differs across proxies. Occupational definitions allow us to retain both six-digit industry-by-year and municipality-

[16]Related concerns arise in the literatures on local labor market spillovers (Moretti, 2011) and employment transitions (Amior and Manning, 2018), where standard classifications may not capture the actual structure of worker mobility.

Table 7: The Effects of Export Shocks on Workers in Non-exporting Firms

	Earnings	Daily Wages	Mobility (Within)	Mobility (Outside)
A: All Flows				
Comp Export	-0.003	-0.001	0.079***	0.012
	(0.003)	(0.001)	(0.013)	(0.011)
Observations	3,925,430	3,925,430	3,780,619	3,780,619
B: Only Flows Across Non-exporters				
Comp Export	0.007*	0.005***	0.002***	0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Observations	3,527,004	3,527,004	3,726,334	3,726,334
Spell FE	Yes	Yes	Yes	Yes
Firm-Year FE	Yes	Yes	Yes	Yes

Note: This table reports 2SLS estimates of the effect of competitor export shocks on workers employed at non-exporting firms. The unit of observation is a worker-year and the estimation sample is the *Non-Exporter Sample* over 2010–2017. The dependent variables are log annual earnings (column 1), log daily wages (column 2), and binary indicators for a within-market (column 3) and an outside-market (column 4) employer change. A *within-market move* is a job-to-job transition from the worker's own occupation-municipality cell i to a cell j with a strictly positive 2010 transition probability, where cell i must have more than 10 total job changers in 2010 and both cells i and j must be present in all sample years; an *outside-market move* is a transition to any residual cell that does not meet these criteria. Because non-exporting firms have no exports of their own, no own-export term enters the specification and *Comp Export* is a market-level rather than a strictly leave-one-out average: it is the transition-probability-weighted average of the log exports of firms employing workers in the cells connected to the worker's own cell. *Comp Export* is instrumented by the corresponding shift-share world import demand (WID) instrument. In Panel A (*All Flows*) the transition probabilities $\pi_{k,k'}$ are estimated from all observed 2010 job-to-job flows, imposing that labor markets are defined identically for workers at exporting and non-exporting firms. In Panel B (*Only Flows Across Non-exporters*) they are estimated from 2010 flows between non-exporting firms only, allowing the pattern of outside options to differ for non-exporters. Both panels include job-spell and firm-year fixed effects, so that identification comes from comparing job stayers within the same firm and year who are exposed to different shocks to their outside options. All specifications include the incomplete-shares control interacted with year fixed effects. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

by-year fixed effects. Industry-based definitions do not permit industry-by-year fixed effects at the same level, while spatial definitions do not permit municipality-by-year fixed effects. These omissions may matter because the estimated competitor effects can then also capture industry-level mechanisms, such as product-market competition or input-output linkages, or spatial shocks, such as local housing-market conditions.

Different labor market proxies may produce different competitor-effect estimates for several reasons. Larger coefficients could indicate that a proxy includes more relevant outside options and excludes more irrelevant ones. However, the estimates also depend on the composition of the firms and shocks captured by each definition. Firms differ in export intensity, and labor market segments differ in the prevalence and economic importance of exporters. Moreover, shocks to firms outside workers' actual option sets may still predict shocks to relevant competitors when export shocks are correlated across cells. Differences in estimates therefore reflect both how well a proxy captures outside options and the composition of the firms and shocks included in the resulting market.

Among the three groups of proxies, spatial definitions yield no statistically detectable competitor effects on either wages or mobility. Industry-based definitions similarly produce little evidence of wage spillovers, although the broad two-digit industry definition yields a modest positive mobility response. Occupational definitions, by contrast, produce positive and statistically significant competitor effects on both wages and within-market mobility, with broader occupational markets generally yielding larger estimates. We interpret this pattern as evidence that the occupational dimension contains particularly relevant information for tracing the labor market transmission of export shocks. At the same time, differences in the available fixed effects and in market composition caution against interpreting coefficient magnitudes as a direct ranking of proxy quality.

5.4 Robustness

Our approach to defining workers' outside options frees us from several decisions that are arbitrary and not informed by the data, but it still requires a number of methodological choices. Table 9 reports the sensitivity of our

Table 8: Standard Labor Market Proxies

	(1) 2d Industry	(2) 6d Industry	(3) 2d Occupation	(4) 6d Occupation	(5) Commuting Zone	(6) Municipality
Panel A: Daily Wages						
Own Export	0.006*	0.004	0.009**	0.008**	0.007*	0.007*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Comp Export	0.000	0.003	0.025***	0.008**	0.009	0.006
	(0.005)	(0.011)	(0.007)	(0.004)	(0.021)	(0.015)
Observations	2,694,428	2,680,756	2,706,286	2,705,872	2,706,817	2,706,817
Panel B: Mobility (Within Market)						
Own Export	-0.020**	-0.019*	-0.019***	-0.016***	-0.016	-0.020***
	(0.010)	(0.010)	(0.005)	(0.005)	(0.039)	(0.007)
Comp Export	0.007**	0.003	0.097***	0.008**	1.321	0.032
	(0.003)	(0.002)	(0.033)	(0.004)	(3.509)	(0.034)
Observations	2,618,666	2,604,992	2,629,647	2,629,214	2,629,649	2,629,649
Spell FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FE	Yes	Yes	Yes	Yes	No	No
6d Industry-Year FE	No	No	Yes	Yes	Yes	Yes

Note: This table presents IV estimates of the effects of own and competitor export shocks under alternative labor market proxies. Each column treats the indicated cell as a self-contained labor market: 2-digit industry (column 1), 6-digit industry (column 2), 2-digit occupation (column 3), 6-digit occupation (column 4), commuting zone (column 5), and municipality (column 6). Competitor export and instrument values are computed using firms' employment shares within the market, so that all outside options within the market receive equal weight. Panel A reports effects on log daily wages; Panel B reports effects on within-market mobility, defined as a job-to-job transition to another firm within the same proxy market. Industry-based proxies (columns 1–2) omit industry-year fixed effects and geography-based proxies (columns 5–6) omit municipality-year fixed effects, as these would absorb the competitor export term. All specifications include the incomplete-shares control interacted with year fixed effects. Exports are instrumented with world import demand shocks. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

results to these choices.[17]

Our ability to identify which jobs are relevant outside options for a given worker depends on the estimation of the transition matrix used to construct weights. Noise in these weights blurs the distinction between competing and non-competing firms and attenuates the estimated effect of competitors' export shocks. Panel A reports three ways of reducing it. On wages, trimming transition probabilities at the 1% and 5% levels raises the competitor elasticity, and estimating weights from all available years of flows rather than from 2010 alone raises it further. Both own and competitors' effects appear to be robust to these choices. Using all years to define transition matrix weights leads to higher competitors' export effects. This might be interpreted in different ways. It could reflect reduction in measurement error of weights. But it could reflect bias from the fact that weights themselves in this exercise are endogenous to export flows. We have illustrated this before (Appendix Table B6).[18]

[17] Appendix Table B10 shows the same tests but for the within market mobility results. The mobility outcome is defined relative to each market definition, so a destination counts as within-market only if it carries positive relevance under the construction used in that column. Hence, if market definition is changed, point estimates are not directly comparable to our baseline estimate.

[18] Dividing the sample between years used to define the weights and years used to estimate the effects involves a trade-off. Table B5 constructs markets from 2010–2012

Panel B investigates alternative ways in which worker transitions can be used to identify labor markets. One key methodological choice we make is determining the level at which initial cells are defined. Defining cells at the industry-by-municipality level leaves the competitor effect indistinguishable from zero on both margins, while leaving the own-export effects intact. Once again that shows that, occupational information, and the transitions between occupations, is therefore essential to identifying relevant outside options. Adding industry as a third dimension to occupation-municipality cells leaves a wage effect significant only at the 10% level and removes the mobility effect. That might be explained by the fact that a finer partition produces a sparser transition matrix in which many weights rest on a handful of transitions, and the noise this introduces costs more than the additional dimension recovers.

The complementary question is what is lost by dropping a dimension rather than adding one. Schubert et al. (2024) measure transitions across occupations and impose spatial relevance separately, by scaling occupational transition rates by local occupational employment shares within commuting zones. This construction yields a far smaller transition matrix than ours, and Table B13 asks what it costs. At the 3-digit occupation level, which matches the occupational granularity of our baseline cells, neither the wage nor the mobility competitor effect is distinguishable from zero, whether or not transition rates are scaled by local employment shares. At the 6-digit level a competitor wage effect emerges, at least as large as our baseline, in both the scaled and the unscaled construction. The mobility estimates are uninformative throughout and they do not point to competitive reallocation at any granularity we examine. Allowing occupational transition probabilities to vary across space is in this sense not redundant.

An important feature of our approach is its ability to capture the directional nature of outside option relevance: if job A is a relevant outside option for a worker in job B, the reverse is not necessarily true. One way to test whether this quality is crucial is to reverse the transition rate used for weighting. Instead of using the share of workers moving from A to B

flows and estimates effects on 2013–2017, avoiding overlap between market construction and estimation. The competitor wage effect remains significant, while the own-export wage effect becomes smaller and imprecise.

between t and $t+1$ as a fraction of all job changers in A at t, we instead use the share of workers moving from B to A as a fraction of all job changers in A at $t+1$. Reversing the weights leads to a slightly attenuated estimate (the change is more substantial for reallocation effects - Appendix Table B10).[19]

Another key characteristic of our approach is that outside option relevance is treated as a degree rather than a binary inclusion. To test the importance of this feature, we explore an alternative approach in which a market is defined as all cells to which a given cell has significant transitions, using a 1% threshold. This method, originally applied in a different context by Schubert et al. (2024), allows us to retain directionality while imposing a discrete boundary on labor markets. The competitor effect is at or above its baseline value on both margins. Table B9 raises the threshold further, to 2%, 3%, and 5%. The own-export elasticity is unchanged at every threshold and the competitor elasticity varies within a narrow band, on samples that are effectively identical, since raising the threshold removes weak links from the market definition rather than observations from the sample. Identifying the relevant set of outside options matters more for the magnitude of the competitors' effect than assigning an exact continuous measure of relevance within that set, and the boundary of that set can be drawn anywhere between 1% and 5% without consequence for the wage results.

Panel C examines the importance of the type of firm-to-firm transitions used to estimate weights. We first exclude flows between firms within the same four-digit industry. This allows us to assess the role of the industry dimension, which is not explicitly accounted for in our definition of labor markets, and such flows are also more likely to be driven by product market competition than by labor market competition. Dropping them leaves the estimate close to baseline.

We then exclude flows between industries that are tightly connected by input-output links, which Cardoza et al. (2024) document as a notable feature of worker transitions. The wage effect is unchanged. The stability

[19] Figure 1 illustrates the relationship between inflows and outflows. While there is a strong correlation on the extensive margin-meaning that the presence of any inflows is predictive of the presence of any outflows-for the majority of cell pairs (those with a transition rate below 10%), inflow and outflow rates are not systematically related.

of the wage effect indicates that the competitive pressure we measure does not run through supply-chain relationships between firms.

As shown in Table 7, restricting flows to non-exporters helps account for differences in outside option relevance between the exporter and non-exporter segments of the economy. Applying the two sets of weights to the exporter sample separates the margins. Exporter-only weights deliver stronger wage competition effects than our baseline or non-exporter only weights. This points again that pattern of outside options might be specific to firm exporting status.

Panel D varies the outcome and the sample restrictions. The first column of Panel D, in Table 9, replaces daily wages with weekly earnings, which respond to the number of days worked within the week. Both elasticities are identical to the daily wage baseline, so neither shock moves incumbent workers along the intensive margin of labor supply, and the gap between the annual earnings and daily wage responses in Table 4 reflects mobility rather than hours. The remaining columns retain all employment spells, restrict to firms with more than 50 full-time equivalents, and restrict to firms exporting more than 2.5 million DKK. Wage estimates are stable in magnitude across all three, and the competitor elasticity is unchanged.

The last methodological choice we examine is the threshold that separates exporters from non-exporters. Our baseline retains firms whose 2010 export share exceeds the first quartile of the distribution among 2010 exporters, and Table B12 replaces this quartile with explicit cutoffs. The own-export elasticity is stable across cutoffs. The competitor elasticity rises with the cutoff, and it is distinguishable from zero at every cutoff except the lowest, which retains every firm with any positive 2010 export and is also the specification with the largest estimation sample. Including firms for which exporting is a negligible source of revenue therefore weakens the measured competitor effect rather than strengthening it.

6 Conclusion

This paper examines how export-driven shifts in product demand propagate through labor markets, focusing on whether these impacts remain limited to directly affected firms or generate broader labor market spillovers.

Table 9: Robustness Checks: Daily Wages

Panel A				
	Baseline	π trimmed 1%	π trimmed 5%	All years flows
	(1)	(2)	(3)	(4)
Own Export	0.009**	0.009**	0.009**	0.008**
	(0.004)	(0.004)	(0.004)	(0.004)
Comp Export	0.005**	0.006***	0.006**	0.007***
	(0.003)	(0.002)	(0.002)	(0.002)
Observations	1,922,469	1,921,402	1,916,878	2,713,016
Panel B				
	Ind-Mun	Ind-Occ-Mun	Reverse Flows	1% threshold
	(1)	(2)	(3)	(4)
Own Export	0.009**	0.008	0.008**	0.009**
	(0.004)	(0.005)	(0.004)	(0.004)
Comp Export	0.003	0.004*	0.004**	0.006**
	(0.003)	(0.002)	(0.002)	(0.002)
Observations	2,285,367	1,331,814	1,911,577	1,922,469
Panel C:				
	Drop Within Industry	Drop IO Connected	Non-exporter Flows	Exporter Flows
	(1)	(2)	(3)	(4)
Own Export	0.008**	0.009**	0.008**	0.004
	(0.004)	(0.004)	(0.004)	(0.005)
Comp Export	0.004**	0.005**	0.004*	0.010***
	(0.002)	(0.002)	(0.002)	(0.003)
Observations	1,876,805	1,822,348	1,351,841	988,860
Panel D				
	Weekly Wages	All Spells	Size > 50 FTEs	Exports > 2.5M
	(1)	(2)	(3)	(4)
Own Export	0.009**	0.010**	0.008	0.009*
	(0.004)	(0.004)	(0.006)	(0.004)
Comp Export	0.005**	0.006**	0.006**	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	1,922,469	1,947,953	1,575,732	1,783,832

Note: This table presents robustness checks for the effect of export shocks on log daily wages, estimated in the exporter sample. All estimates are IV, with own and competitor exports instrumented by world import demand (WID) shocks. Reported coefficients are the own-export elasticity (β) and the competitor-export elasticity (γ). Panel A addresses potential measurement error in the transition weights by trimming transition probabilities at the 1–99% and 5–95% levels (columns 2–3) and by using all available years of flows instead of only 2010 (column 4). Panel B explores alternative cell definitions and weighting approaches: industry-by-municipality cells (column 1), adding industry to occupation-municipality cells (column 2), reversing flow direction (column 3), and applying a discrete 1% threshold (column 4). Panel C tests the exclusion of specific types of flows when estimating weights: within-industry transitions (column 1), transitions among input-output connected industries (column 2), only non-exporter flows (column 3), and only exporter flows (column 4). Panel D, except the first entry, modifies sample restrictions: log weekly wages as outcome (column 1), all employment spells (column 2), firms with more than 50 full-time equivalents (column 3), and firms with exports exceeding 2.5 million DKK (column 4). Unless otherwise indicated, all specifications include spell, municipality-year, and 6-digit industry-year fixed effects, and the incomplete-shares control interacted with year fixed effects. In Panel B column 1, 4-digit industry-by-municipality cells are used to identify competitors, so only spell and municipality-year fixed effects are included. Standard errors in parentheses are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Leveraging a flow-based definition of workers' outside option sets derived from observed job-to-job transitions, we demonstrate that increased global demand not only raises wages and reduces turnover within exporting firms but also exerts significant competitive pressures across the broader labor market. Specifically, expansions at competing exporters compel other firms to increase wages to retain their workforce.

Importantly, these competitive dynamics are not confined to the exporting sector. Non-exporting firms competing for labor within the same labor markets exhibit significant wage and labor reallocation responses, highlighting that labor demand spillovers from export shocks influence a wide segment of the economy.

Our heterogeneity results emphasize the role of outside options in wage setting. We document important distinctions across worker demographics and firm characteristics: blue-collar workers are more likely to adjust on the extensive margin, by switching employers, while white-collar workers experience larger wage gains. Moreover, our empirical strategy isolates labor-demand spillovers from product-market or input-output effects, reinforcing the central role of labor market competition in shaping the diffusion of trade shocks.

Overall, this study bridges important gaps between the branches of literature on trade-induced labor market spillovers, firm-level responses to trade shocks, and empirical definitions of labor market boundaries. Our findings suggest that the total effect of foreign demand fluctuations on workers is substantially larger than what direct firm-level exposure alone would imply. By explicitly accounting for labor market dynamics through observed worker flows, we uncover extensive spillovers that shape wage dynamics and employment reallocation across a wide cross-section of the economy.

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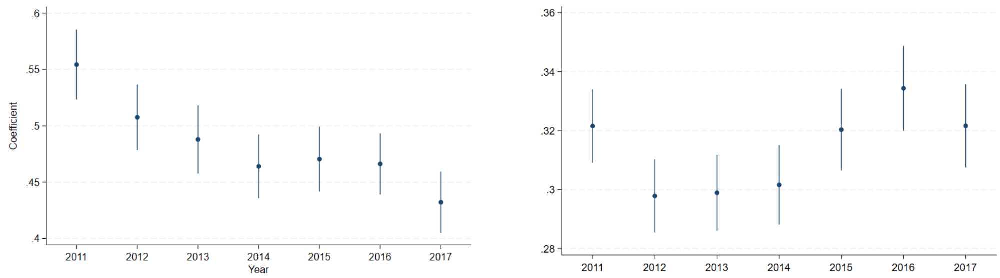
A Appendix: Figures



(a) 2010 Transition Rate (b) Probability of Any Flow

Figure A1: Stability of Flows Between Municipality-Occupation Cells Over Time

Note: This figure assesses the persistence of transition patterns between occupation–municipality cells. Panel (a) reports coefficients from a linear regression of the transition rate between each ordered cell pair in 2010 on the corresponding transition rate in each year t. Transition rates are defined as the share of all job-to-job transitions from the origin cell that end in the destination cell. Thus, a coefficient of 0.55 means that a 10 percentage-point higher transition rate in 2010 is associated with a 5.5 percentage-point higher rate in 2011; it does not refer to transition counts. Panel (b) reports analogous regressions for an indicator of whether any transition is observed between the two cells. All ordered cell pairs are retained, including zero-flow pairs, and the models are estimated by OLS. Vertical bars show 95% confidence intervals based on standard errors clustered by origin cell. The R^2 is 0.31 in Panel (a) and 0.11 in Panel (b).

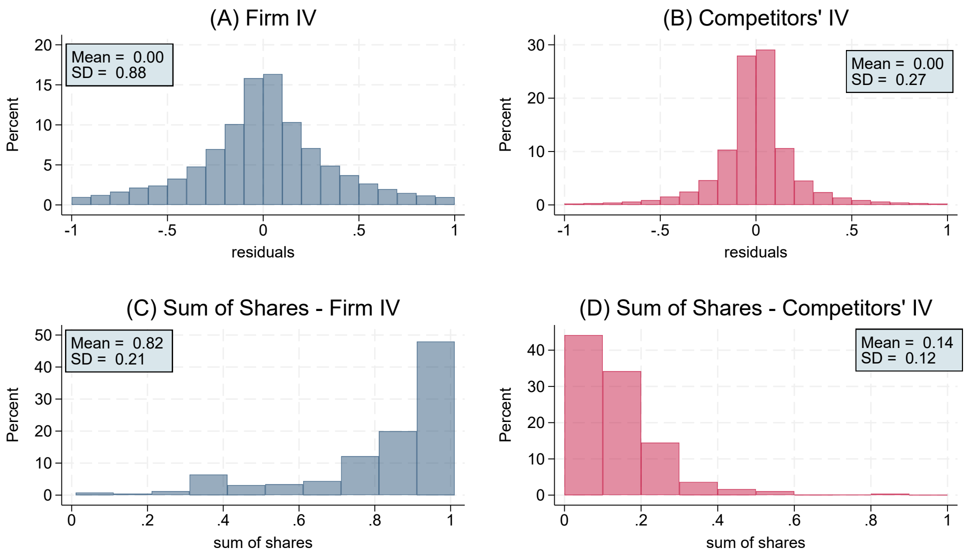


Figure A2: Residualized Variation in Instruments and Incomplete Share Controls

Note: This figure characterizes the identifying variation in the two shift-share instruments and the incompleteness of their underlying exposure shares. The unit of observation throughout is a job spell-year in the *Exporter Sample*. Panels (a) and (b) plot the distribution of the firm-level and competitor-level world import demand (WID) instruments after partialling out the full set of fixed effects used in the baseline specification (job-spell, municipality-year, and 6-digit industry-year fixed effects) together with the incomplete-shares controls. Panels (c) and (d) plot the distribution of the corresponding sums of exposure shares: for the firm instrument, the sum over product-country pairs of firm j's 2006–2010 average export revenue shares; for the competitor instrument, the sum $S_{k,t} = \sum_{k'} \pi_{k,k'} \sum_{h \neq j} w_{hk'} \sum_{c,p} s_{hcp}$ of the transition- and employment-weighted exposure shares entering the worker's competitor instrument.

B Appendix: Tables

Table B1: Descriptive Statistics: Workers

	Population	Sample
Female	0.506	0.363
	(0.500)	(0.481)
Age	38.32	38.68
	(12.26)	(13.46)
Log Annual Earnings	11.65	12.13
	(1.67)	(1.377)
Log Daily Wages	6.385	6.627
	(1.055)	(0.968)
Firm Change	0.275	0.222
	(0.446)	(0.416)
High-skill Occupation	0.413	0.350
	(0.492)	(0.477)
Medium-skill Occupation	0.460	0.501
	(0.498)	(0.500)
Low-skill Occupation	0.126	0.149
	(0.332)	(0.356)
# Unique Workers	2,969,651	1,154,738
# Unique Job Spells	8,621,369	1,526,311
Observations	23,624,482	5,857,728

Note: This table compares the pooled analysis sample with the broader worker population, as a check on representativeness. The unit of observation is a worker-year over 2010–2017. *Population* covers workers aged 18–60 satisfying the baseline age restrictions; *Sample* covers the worker-year observations actually used in the analysis (exporter and non-exporter firms combined). Means are reported with standard deviations in parentheses. *Log Annual Earnings* and *Log Daily Wages* are in logs of 2010 Danish kroner; *Firm Change* is a binary indicator equal to one in the year in which the worker moves to a different firm, so its mean is the annual probability of leaving the current firm. Skill groups are defined from the first digit of the DISCO occupation code; occupation-municipality cells elsewhere in the paper use the 3-digit level. The final block reports unique workers, unique job spells, and worker-year observations.

Table B2: Daily Wage Effects: Model Build-Up

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Own Export Only							
Own Export	0.094*	0.006	0.007**	0.011***	0.011***	0.009**	
	(0.049)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	
Observations	2,085,095	1,922,495	1,922,495	1,922,491	1,922,471	1,922,469	
Panel B: Own and Competitors' Export							
Own Export	0.077*	0.006*	0.008**	0.011***	0.011***	0.009**	
	(0.042)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	
Comp. Export	0.060***	0.007**	0.006**	0.005**	0.006**	0.005**	0.005**
	(0.012)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	2,085,095	1,922,495	1,922,495	1,922,491	1,922,471	1,922,469	1,921,530
Year FE	Yes	Yes	No	No	No	No	No
Spell FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FE	No	No	Yes	Yes	Yes	Yes	No
2d Industry-Year FE	No	No	No	Yes	No	No	No
4d Industry-Year FE	No	No	No	No	Yes	No	No
6d Industry-Year FE	No	No	No	No	No	Yes	No
Firm-Year FE	No	No	No	No	No	No	Yes

Note: This table reports 2SLS estimates of the effect of own-firm and competitor export shocks on log daily wages as the fixed-effects structure is progressively saturated. The unit of observation is a worker-year and the sample is the exporter sample over 2010–2017 (see Table 1). *Own Export* is log firm exports; *Comp. Export* is the leave-one-out, transition-probability-weighted average of log exports of firms employing workers in the occupation-municipality cells connected to the worker's own cell, excluding the worker's own firm (see Table 4). Both are instrumented with the corresponding shift-share world import demand (WID) instruments. Columns move from year fixed effects only (column 1) to firm-by-year fixed effects (column 7); the fixed effects in each column are listed in the lower block, and observation counts differ slightly across columns because singleton groups are dropped. Column 1 does not include spell fixed effects, so it is identified from between-spell as well as within-spell variation. All specifications include the incomplete-shares control interacted with year fixed effects. Panel A includes only the own-export term; Panel B includes both terms. Own exports are absorbed by firm-by-year fixed effects and hence unidentified in column 7, leaving only the competitor term, which varies across workers within the same firm and year. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: The First Stage: Model Buildup

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Own Export							
Own WID	0.230***	0.203***	0.202***	0.156***	0.152***	0.153***	
	(0.054)	(0.045)	(0.041)	(0.015)	(0.013)	(0.013)	
Comp WID	0.065	-0.079*	-0.072*	-0.043*	-0.023**	-0.020**	
	(0.115)	(0.047)	(0.041)	(0.024)	(0.010)	(0.010)	
Observations	2,085,095	1,922,495	1,922,495	1,922,491	1,922,471	1,922,469	
F-stat	12.07	10.71	12.39	55.89	68.50	67.58	
Panel B: Competitors' Export							
Own WID	0.017	0.008	0.009	0.002	0.008**	0.008**	
	(0.019)	(0.007)	(0.006)	(0.003)	(0.003)	(0.003)	
Comp WID	1.685***	0.920***	0.956***	0.983***	1.061***	1.065***	1.191***
	(0.055)	(0.073)	(0.068)	(0.065)	(0.061)	(0.061)	(0.062)
Observations	2,085,095	1,922,495	1,922,495	1,922,491	1,922,471	1,922,469	1,921,530
F-stat	464.5	81.03	101.8	119.6	155.8	157	368.1
Year FE	Yes	Yes	No	No	No	No	No
Spell FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FE	No	No	Yes	Yes	Yes	Yes	No
2d Industry-Year FE	No	No	No	Yes	No	No	No
4d Industry-Year FE	No	No	No	No	Yes	No	No
6d Industry-Year FE	No	No	No	No	No	Yes	No
Firm-Year FE	No	No	No	No	No	No	Yes

Note: This table reports the first stages corresponding to the build-up in Table B2. The unit of observation is a worker-year and the sample is the exporter sample over 2010–2017 (see Table 1). The dependent variable is log own-firm exports in Panel A and the leave-one-out competitor export measure in Panel B. *Own WID* and *Comp WID* are the two excluded instruments: the firm-level shift-share world import demand instrument, built from 2006–2010 average product-country export revenue shares interacted with world import demand excluding Denmark, and its transition-probability-weighted competitor counterpart, constructed with 2010-fixed firm employment weights and a firm population fixed at its 2010 composition. Fixed effects are listed in the lower block.All specifications include the incomplete-shares control interacted with year fixed effects. Own exports are absorbed by firm-by-year fixed effects, so Panel A is not estimated in column 7. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: The First Stage: Model Buildup. Non-exporters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Comp WID	2.321***	1.554***	1.589***	1.658***	1.672***	1.673***	1.791***
	(0.050)	(0.063)	(0.063)	(0.058)	(0.057)	(0.057)	(0.036)
Observations	4,577,855	3,929,649	3,929,648	3,929,641	3,929,594	3,929,584	3,925,430
F-stat	2144	607.9	640.1	820.3	856.2	854.6	2482
Year FE	Yes	Yes	No	No	No	No	No
Spell FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FE	No	No	Yes	Yes	Yes	Yes	No
2d Industry-Year FE	No	No	No	Yes	No	No	No
4d Industry-Year FE	No	No	No	No	Yes	No	No
6d Industry-Year FE	No	No	No	No	No	Yes	No
Firm-Year FE	No	No	No	No	No	No	Yes

Note: This table reports the first stage for the non-exporter sample as the fixed-effects structure is progressively saturated. The unit of observation is a worker-year over 2010–2017 and the sample consists of workers at private-sector firms outside the exporter sample, i.e. firms not recorded as exporters in UHDI in 2010, firms with a 2010 export share below the first quartile, firms that begin exporting after 2010, and firms whose transactions are not recorded in UHDI (see Table 1). Because these firms do not export, there is a single endogenous regressor—the market-level competitor export measure, defined as the transition-probability-weighted average of log exports of firms employing workers in the cells connected to the worker's own cell—and a single excluded instrument, *Comp WID*, its shift-share world import demand counterpart. Fixed effects are listed in the lower block; all specifications include the incomplete-shares control interacted with year fixed effects. observation counts differ across columns because singleton groups are dropped. Standard errors, reported in parentheses, are clustered at the occupation-municipality level, as in the corresponding second-stage estimates in Table 7. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Pre-Period Market Definition: Weights Measured 2010–2012, Effects Estimated 2013–2017

	(1) Annual Earnings	(2) Daily Wages
Own Export	0.023**	0.007
	(0.010)	(0.004)
Comp Export	0.013***	0.006**
	(0.004)	(0.003)
Spell FE	Yes	Yes
Municipality-Year FE	Yes	Yes
6d Industry-Year FE	Yes	Yes
Num. of Obs.	1,284,165	1,284,165

Note: This table is the counterpart to the all-years specification reported in Panel A of Table 9. That specification pools job-to-job flows across the whole sample period to reduce noise in the transition matrix, but in doing so constructs labor markets from flows observed during the estimation window, so any effect of export shocks on the flows themselves enters the weights. Here the two periods are separated: every predetermined input is measured over 2010–2012 and the model is estimated over 2013–2017 only. Pooling therefore still reduces sampling noise relative to the single-year baseline, in which transition probabilities, sample selection, and employment weights are all fixed in 2010, while no flow used to define a market is drawn from the estimation window. Transition probabilities $\pi_{k,k'}$ are computed from job-to-job flows pooled over 2010–2012, firm product-country export revenue shares are averaged over 2010–2012, and firm employment weights within occupation-municipality cells are fixed in 2012. The exporter sample is re-selected on the same base year: it comprises firms recorded as exporters in UHDI in 2012, employing at least 10 full-time equivalents in 2012, whose 2012 export share exceeds the first quartile of the 2012 export-share distribution among 2012 exporters, and the firm population underlying the competitor instrument is fixed at its 2012 composition. Cells enter the transition matrix if they record more than 10 job changers over the pooled 2010–2012 flows and are present in every year of the estimation window, so that a cell defining an outside option is one in which employment is observed throughout 2013–2017. The unit of observation is a worker-year and the sample is the exporter sample (see Table 1). Occupation-municipality cells are defined at the 3-digit DISCO occupation level. The dependent variable is log annual earnings in column (1) and log daily wages in column (2). Within each column, *Own Export* and *Comp Export* are the two coefficients of interest from a single 2SLS regression estimated jointly, with both instrumented by the corresponding shift-share world import demand (WID) instruments and the incomplete-shares control interacted with year fixed effects. The estimation sample is smaller than in Table 9 because the panel is restricted to 2013–2017. The fixed effects included in every specification are indicated in the lower block. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B6: Cell-to-Cell Mobility: Response of Worker Flows to Export Shocks

	(1) π: Both	(2) π: Pair	(3) π: Destination	(4) π: Origin
Export Shock - Origin	-0.000021***	-0.000021***		-0.000021***
	(0.000007)	(0.000007)		(0.000007)
Export Shock - Destination	0.000011***	0.000011***	0.000011***	
	(0.000004)	(0.000004)	(0.000004)	
F-stat	27.57	27.57	72.76	55.14
Origin FE	Yes	No	No	Yes
Destination FE	Yes	No	Yes	No
Year FE	Yes	Yes	No	No
Origin × Destination (Pair) FE	No	Yes	No	No
Origin × Year FE	No	No	Yes	No
Destination × Year FE	No	No	No	Yes
Observations	18,750,114	18,750,114	18,750,114	18,750,114

Note: This table asks whether export shocks themselves move the job-to-job transition weights used to construct the competitor export measure. The unit of observation is an origin cell × destination cell × year triple, where cells are occupation-municipality pairs defined at the 3-digit DISCO level. The panel is rectangularized over the grid of ordered cell pairs and years, excluding self-reflexive (own-cell) pairs, so that pairs with no observed transition enter with a flow of zero, giving 18,750,114 observations. The dependent variable is the realized transition probability $\pi_{k,k',t}$, that is, the flow from origin k to destination k' as a share of all employment-to-employment transitions originating in k in year t. *Export Shock – Origin* is the employment-weighted average log export of firms in the origin cell k, and *Export Shock – Destination* is the same measure computed over firms in the destination cell k'; both are instrumented with the corresponding shift-share world import demand (WID) instruments. The four columns differ in the fixed effects, listed in the lower block: additive origin, destination and year effects (column 1); origin-by-destination pair effects with year effects (column 2); origin-by-year effects with destination effects (column 3); and destination-by-year effects with origin effects (column 4). A blank entry indicates that the regressor is absorbed by the fixed effects of that column rather than dropped from the specification: origin-by-year effects absorb any origin-level regressor in column (3), and destination-by-year effects absorb any destination-level regressor in column (4). Because the dependent variable is a share of the transitions originating in k, the flows retained in a given origin-year sum to one minus the origin cell's own retention probability. Excluding the self-reflexive pairs therefore breaks the exact adding-up constraint that would otherwise hold the origin-year mean of the dependent variable fixed at a constant, leaving genuine origin-level variation in that mean. Origin-level regressors are nonetheless absorbed mechanically by the origin-by-year fixed effects in column (3), and destination-level regressors by the destination-by-year effects in column (4). The reported F-statistic is the first-stage F-statistic. Standard errors, reported in parentheses, are clustered at the origin-cell level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: Balancing Tests

	(1) Daily Wage Growth	(2) Daily Wage	(3) Earnings Growth	(4) Earnings	(5) Mobility
Panel A: Worker Level					
Res. Firm IV	-0.000	-0.003	0.000	-0.003	-0.001
	(0.000)	(0.002)	(0.001)	(0.003)	(0.001)
	[0.384]	[0.214]	[0.581]	[0.356]	[0.281]
Res. Comp. IV	-0.001	-0.003	0.001	-0.004	0.000
	(0.001)	(0.004)	(0.001)	(0.005)	(0.002)
	[0.342]	[0.476]	[0.331]	[0.435]	[0.774]
Num. of Obs.	1,304,310	1,304,310	1,304,310	1,304,310	1,304,310
Panel B: Firm Level					
Res. Firm IV	-0.002*	-0.003	-0.004	0.000	-0.002
	(0.001)	(0.004)	(0.003)	(0.005)	(0.001)
	[0.079]	[0.483]	[0.122]	[0.960]	[0.108]
Res. Comp. IV	-0.001*	0.001	-0.000	0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)
	[0.093]	[0.755]	[0.886]	[0.796]	[0.540]
Num. of Obs.	1,476,918	1,476,918	1,476,918	1,476,918	1,476,918
Panel C: Cell Level					
Res. Firm IV	0.000	-0.002	-0.000	-0.002	-0.000
	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)
	[0.736]	[0.592]	[0.995]	[0.658]	[0.838]
Res. Comp. IV	0.000	-0.010	0.004	-0.011	0.001
	(0.003)	(0.006)	(0.005)	(0.009)	(0.001)
	[0.939]	[0.124]	[0.430]	[0.226]	[0.663]
Num. of Obs.	1,260,719	1,260,719	1,260,719	1,260,719	1,260,719

Note: This table reports balancing tests for the two shift-share instruments. Each column reports a regression of the indicated 2009 labor market characteristic on the residualized own-firm and competitor world import demand (WID) instruments. The instruments are residualized with respect to the baseline fixed effects and the incomplete-shares control. The outcomes are daily wage growth, daily wages, earnings growth, earnings, and job mobility. Panel A reports worker-level outcomes; Panels B and C average the variables to the firm and occupation–municipality-cell levels, respectively. The sample is based on workers in the 2010–2017 exporter sample, while all outcomes are measured before the estimation period. Standard errors are reported in parentheses and p-values in brackets. Standard errors are clustered at the firm level. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table B8: Dynamic Wage Adjustment by Education Group

	(1) All	(2) Below Tertiary Edu	(3) Tertiary Edu	(4) High Edu
Own Export	0.007**	0.009***	-0.001	0.031
	(0.003)	(0.003)	(0.006)	(0.021)
	[0.029]	[0.005]	[0.829]	[0.144]
Lagged Own Export	-0.000	-0.002	0.008	0.000
	(0.003)	(0.003)	(0.007)	(0.016)
	[0.916]	[0.465]	[0.237]	[0.990]
Comp Export	0.003*	0.001	0.006**	0.014***
	(0.002)	(0.002)	(0.002)	(0.003)
	[0.096]	[0.435]	[0.011]	[0.000]
Lagged Comp Export	0.002**	0.001	0.003**	0.005**
	(0.001)	(0.001)	(0.002)	(0.003)
	[0.040]	[0.144]	[0.049]	[0.044]
Spell FE	Yes	Yes	Yes	Yes
Municipality-Year FE	Yes	Yes	Yes	Yes
6d Industry-Year FE	Yes	Yes	Yes	Yes
Num. of Obs.	939,167	655,723	263,389	48,041

Note: This table reports estimates from a dynamic specification that augments the baseline wage regression with one-year-lagged own-firm and competitor export shocks, allowing wage responses to materialize with a delay.The unit of observation is a worker-year, the sample is the exporter sample (see Table 1) restricted to 2011–2017 by the inclusion of lags, and the dependent variable is log daily wages. *Own Export* and *Comp Export* are the contemporaneous own-firm and competitor export shocks, and *Lagged Own Export* and *Lagged Comp Export* are their one-year lags; all four are instrumented with the corresponding contemporaneous and lagged world import demand (WID) shocks. Column (1) reports estimates for the full sample; columns (2)–(4) split the sample by education, into workers below tertiary education, workers with tertiary education, and workers with the highest education level, respectively. The fixed effects included in every specification are indicated in the lower block. This exercise addresses whether wage adjustment to export shocks is delayed for some worker groups—for example, if wages of lower-skilled workers respond only after collective agreements are renegotiated—by comparing the contemporaneous and lagged responses across education groups. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Alternative Transition-Probability Thresholds for Market Definition

	Transition-Probability Threshold			
	(1) 1%	(2) 2%	(3) 3%	(4) 5%
Own Export	0.009**	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)
Comp Export	0.006**	0.007***	0.006***	0.006***
	(0.003)	(0.002)	(0.002)	(0.002)
Spell FE	Yes	Yes	Yes	Yes
Municipality-Year FE	Yes	Yes	Yes	Yes
6d Industry-Year FE	Yes	Yes	Yes	Yes
Num. of Obs.	1,922,469	1,922,469	1,922,461	1,921,822

Note: This table reports a robustness check on how the labor market of each occupation-municipality cell is defined. Following the discrete-threshold approach (as in Schubert et al. (2024) and the 1% threshold specification of the main robustness table), a cell's labor market is defined as the set of destination cells whose 2010 transition probability from that cell exceeds a given threshold, while cells below the threshold are excluded. The four columns vary this threshold: 1% (column 1), 2% (column 2), 3% (column 3), and 5% (column 4). Raising the threshold retains only the more strongly connected—and hence potentially more relevant—outside options. The unit of observation is a worker-year, the sample is the exporter sample over 2010–2017 (see Table 1), and the dependent variable is log daily wages. Each column reports IV estimates of the effect of own-firm and competitor export shocks on log daily wages, with own and competitor exports instrumented by world import demand (WID) shocks. The fixed effects included in every specification are indicated in the lower block. All specifications include the incomplete-shares control interacted with year fixed effects. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B10: Robustness Checks: Within-Market Mobility

Panel A				
	Baseline	π trimmed 1%	π trimmed 5%	All years flows
	(1)	(2)	(3)	(4)
Own Export	-0.019***	-0.012***	-0.007***	-0.018***
	(0.004)	(0.002)	(0.001)	(0.003)
Comp Export	0.015**	0.008*	-0.003	0.015***
	(0.008)	(0.005)	(0.003)	(0.005)
Observations	1,885,637	1,884,639	1,878,438	2,649,553
Panel B				
	Ind-Mun	Ind-Occ-Mun	Reverse Flows	1% threshold
	(1)	(2)	(3)	(4)
Own Export	-0.015***	-0.007***	-0.018***	-0.020***
	(0.002)	(0.002)	(0.004)	(0.003)
Comp Export	-0.001	0.000	0.008*	0.017***
	(0.004)	(0.002)	(0.005)	(0.006)
Observations	2,235,042	1,315,962	1,875,111	1,885,637
Panel C				
	Drop Within Industry	Drop IO Connected	Non-exporter Flows	Exporter Flows
	(1)	(2)	(3)	(4)
Own Export	-0.018***	-0.018***	-0.014***	-0.027***
	(0.003)	(0.003)	(0.003)	(0.005)
Comp Export	0.011**	0.003	0.019***	0.005
	(0.005)	(0.006)	(0.004)	(0.008)
Observations	1,844,159	1,792,077	1,324,554	984,283
Panel D				
		All Spells	Size > 50 FTEs	Exports > 2.5M
		(2)	(3)	(4)
Own Export		-0.020***	-0.023***	-0.022***
		(0.003)	(0.005)	(0.004)
Comp Export		0.013*	0.017*	0.014*
		(0.008)	(0.008)	(0.008)
Observations		1,901,714	1,543,421	1,749,324

Note: This table presents robustness checks for the effect of export shocks on within-market mobility, estimated in the exporter sample. The dependent variable is a binary indicator equal to one in the year in which the worker moves to a different firm in an occupation-municipality cell with a positive 2010 transition-probability weight. All estimates are IV, with own and competitor exports instrumented by world import demand (WID) shocks. Reported coefficients are the own-export effect (β) and the competitor-export effect (γ). Panel A addresses potential measurement error in the transition weights by trimming transition probabilities at the 1–99% and 5–95% levels (columns 2–3) and by using all available years of flows instead of only 2010 (column 4). Panel B explores alternative cell definitions and weighting approaches: industry-by-municipality cells (column 1), adding industry to occupation-municipality cells (column 2), reversing flow direction (column 3), and applying a discrete 1% threshold (column 4). Panel C tests the exclusion of specific types of flows when estimating weights: within-industry transitions (column 1), transitions among input-output connected industries (column 2), only non-exporter flows (column 3), and only exporter flows (column 4). Panel D modifies sample restrictions: all employment spells (column 2), firms with more than 50 full-time equivalents (column 3), and firms with exports exceeding 2.5 million DKK (column 4). Unless otherwise indicated, all specifications include spell, municipality-year, and 6-digit industry-year fixed effects, and the incomplete-shares control interacted with year fixed effects. In Panel B column 1, 4-digit industry-by-municipality cells are used to identify competitors, so only spell and municipality-year fixed effects are included. Standard errors in parentheses are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: Within- and Outside-Market Mobility under Alternative Market Boundaries

	(1) Within Move	(2) Outside Move
Panel A: Baseline	($\pi > 0$ vs. $\pi = 0$)	
Own Export	-0.020***	-0.017***
	(0.004)	(0.003)
Comp Export	0.015**	-0.026***
	(0.008)	(0.010)
Panel B: First-Quartile Threshold		($\pi > Q_1$ vs. $\pi \leq Q_1$)
Own Export	-0.020***	-0.017***
	(0.003)	(0.003)
Comp Export	0.012*	-0.024**
	(0.007)	(0.009)
Panel C: Median Threshold	($\pi >$ med vs. $\pi \leq$ med)	
Own Export	-0.018***	-0.018***
	(0.003)	(0.003)
Comp Export	0.010	-0.021**
	(0.007)	(0.009)
Spell FE	Yes	Yes
Municipality-Year FE	Yes	Yes
6d Industry-Year FE	Yes	Yes
Num. of Obs.	1,885,637	1,885,637

Note: This table varies where the boundary between a worker's labor market and the rest of the economy is drawn, and reports the effect of export shocks on mobility on each side of that boundary. The unit of observation is a worker-year and the sample is the exporter sample over 2010–2017 (see Table 1). In every cell the dependent variable is a binary indicator equal to one in the year in which the worker moves to a different firm and the destination occupation-municipality cell falls on the relevant side of the boundary; each cell reports the coefficient from a separate regression. Panels differ only in where that boundary is set, using the 2010 transition probability π from the worker's own cell to the destination cell: Panel A applies the baseline definition, under which a move is *within* the market whenever $\pi > 0$ and *outside* it when $\pi = 0$; Panel B raises the boundary to the first quartile of the distribution of strictly positive transition probabilities, so that destinations with $0 < \pi \leq Q_1$ are reclassified as outside moves; and Panel C raises it to the median of the same distribution. Within each panel the two outcomes are mutually exclusive and exhaustive, so the coefficients in columns (1) and (2) sum to the effect on the overall probability of changing employer, which is common across panels. Moving down the panels therefore reallocates weakly connected destinations from column (1) to column (2), and tests whether export-driven competitive reallocation is concentrated among the most strongly connected outside options. Own and competitor exports are defined as in Table 4 and instrumented with the corresponding shift-share world import demand (WID) instruments. The estimation sample is identical across panels, since only the coding of the outcome changes. Fixed effects are listed in the lower block. All specifications include the incomplete-shares control interacted with year fixed effects. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B12: Alternative Export-Share Thresholds for the Exporter Sample

	Export-Share Threshold			
	(1) Any Positive	(2) 1%	(3) 2%	(4) 5%
Own Export	0.010**	0.009**	0.010**	0.009*
	(0.005)	(0.004)	(0.004)	(0.005)
Comp Export	0.005	0.006**	0.006**	0.007**
	(0.004)	(0.003)	(0.002)	(0.003)
Spell FE	Yes	Yes	Yes	Yes
Municipality-Year FE	Yes	Yes	Yes	Yes
6d Industry-Year FE	Yes	Yes	Yes	Yes
Num. of Obs.	3,113,940	1,868,216	1,702,450	1,484,844

Note: This table reports the sensitivity of the main daily-wage results to the threshold used to define the exporter sample. In the baseline, exporters are firms whose 2010 export share exceeds the first quartile of the 2010 export-share distribution (which roughly coincides with a 1% export share). Here the threshold is varied explicitly: column (1) includes all firms with any positive 2010 export share; columns (2)–(4) restrict the sample to firms with a 2010 export share of at least 1%, 2%, and 5%, respectively. Each column re-selects the exporter sample according to the corresponding threshold and re-estimates the model, so the estimation sample differs across columns. The dependent variable is log daily wages, and each column reports IV estimates of the effect of own-firm and competitor export shocks, with own and competitor exports instrumented by world import demand (WID) shocks. Progressively raising the threshold excludes marginal exporters, addressing the concern that firms with negligible export activity might drive the estimated effects. The fixed effects included in every specification are indicated in the lower block. All specifications include the incomplete-shares control interacted with year fixed effects. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B13: Labor Markets Based on Occupation-Transitions

	Unscaled		Scaled to CZ	
	(1) Daily Wages	(2) Mobility (Within)	(3) Daily Wages	(4) Mobility (Within)
Panel A: 3-digit Occupation				
Own Export	0.009**	-0.044***	0.009**	-0.058***
	(0.004)	(0.005)	(0.004)	(0.006)
Comp Export	0.003	-0.005	0.004	-0.009
	(0.004)	(0.023)	(0.004)	(0.021)
Num. of Obs.	2,754,838	2,662,326	2,749,981	1,724,814
Panel B: 6-digit Occupation				
Own Export	0.008**	-0.038***	0.008**	-0.049***
	(0.004)	(0.005)	(0.004)	(0.005)
Comp Export	0.009***	0.010	0.006***	0.010
	(0.003)	(0.028)	(0.002)	(0.013)
Num. of Obs.	2,712,230	2,662,326	2,707,382	1,691,575
Fixed effects (common to both panels)				
Spell FE	Yes	Yes	Yes	Yes
Municipality-Year FE	Yes	Yes	Yes	Yes
6d Industry-Year FE	Yes	Yes	Yes	Yes

Note: This table contrasts our baseline occupation-by-municipality market construction with the approach of Schubert et al. (2024), in which labor markets are defined across occupations and spatial relevance is imposed separately. Transitions are measured across occupations only, at the 3-digit DISCO level (Panel A) and, as a variant, at the 6-digit DISCO level (Panel B). Within each panel, columns (1)–(2) use the unscaled occupational transition rates within commuting zones, while columns (3)–(4) scale the occupational transition rates by local occupational employment shares within commuting zones, following Schubert et al. (2024). The dependent variable is log daily wages in columns (1) and (3), and an indicator for within-market mobility (a job-to-job move to a destination cell with positive relevance) in columns (2) and (4). All estimates are IV, with own and competitor exports instrumented by world import demand (WID) shocks. The fixed effects, common to both panels, are indicated in the lower block. All specifications include the incomplete-shares control interacted with year fixed effects. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance is denoted as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.