



LAJOS TAMÁS SZABÓ, ÁLMOS TELEGDY, ANDRÁS BORSOS

# **UPSTREAM AND DOWNSTREAM WORKER MOBILITY ALONG THE SUPPLY CHAIN**

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MNB Working Papers 2026/4

### **Upstream and Downstream Worker Mobility Along the Supply Chain\***

(Munkavállalói áramlás a beszállítói és vevői kapcsolatok mentén)

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# Abstract

This paper examines how interfirm trade relationships shape job-to-job mobility along domestic supply chains. Using Hungarian administrative data of firm-to-firm transactions and matched employer–employee records from 2015–2022, we estimate event-study models with dyadic fixed effects and matched non-trading controls. Trade roughly doubles worker mobility between partners, with larger effects for pairs characterized by high trade shares, and when the partner has a high wage, is a superstar, or growing. Downstream mobility always exceeds upstream mobility. We find no consistent differences between high- and low-skill mobility, and mobility along supply chains does not generate systematically higher wage growth.

JEL Classification: F16, J60, F14

Keywords: Production Network, Interfirm Worker Mobility, Hungary

## Összefoglaló

Tanulmányunkban azt vizsgáljuk, hogy a vállalatok közötti kereskedelmi kapcsolatok milyen szerepet játszanak a munkavállalók cégek közötti áramlásában a hazai ellátási láncok mentén. A vizsgálatunkat a magyar vállalatok közötti tranzakciós adatokra és a kapcsolt munkáltató-munkavállaló adatbázisra támaszkodva végeztük a 2015–2022 közötti időszakra egymással nem kereskedő kontrollvállalat-párok és vállalatpár-fixhatások segítségével. Eredményeink szerint a kereskedelmi kapcsolat közel kétszeresére növeli a munkavállalók áramlását a partnercégek között. A hatás különösen erős azoknál a vállalatpároknál, amelyek között jelentős a kereskedelmi forgalom, illetve amikor a partner magasabb bérszinttel rendelkezik, szupersztár vállalatnak minősül vagy dinamikusan növekszik. A vevői irányba történő munkahelyváltás mértéke minden esetben meghaladja a beszállítói irányba történő áramlást. Nem találunk ugyanakkor szisztematikus különbséget a magas és alacsony képzettségű munkavállalók munkahelyváltási mintázatai között, és az ellátási láncokon belüli munkaerő-áramlás nem jár együtt következetesen magasabb bérnövekedéssel.

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# 1 Introduction

Interfirm trade is essential for most firms, providing an important source of revenue and access to the intermediate goods. Beyond these direct benefits, it also serves as an information conduit between firms, with implications not only for the flow of trade but also for labor market outcomes, potentially increasing mobility between trading firms. Until recently, this issue received little attention, mainly because comprehensive data on interfirm trade were not available.<sup>1</sup>

The main aim of this paper is to estimate the effect of trade relations on interfirm worker mobility. To reduce the bias originating from the simultaneity of trade and mobility, we use panel data methods on dyadic data and add dyadic fixed effects to the regression.<sup>2</sup> First, we examine the average effect of trade on mobility, and find substantial effects. Then we separate worker flows from suppliers to buyers ( $S - B$  mobility) and from buyers to suppliers ( $B - S$  mobility), to identify potential asymmetries between the two directions of mobility, and find that downstream ( $S - B$ ) mobility is substantially larger than upstream ( $B - S$ ) mobility (a regularity which we find in all type of specifications and robustness checks). Then we look into the heterogeneity of interfirm mobility by several partner characteristics: the importance of the trading partner (measured by the share of trade in the firm's sales), the average wage of the partner, whether it is a superstar firm (defined as exporting or large or foreign-owned based on Amiti et al. (2024)) and whether it is on a growing employment trajectory. All these partner types induce stronger mobility which is always larger downstream than upstream. Another type of heterogeneity we analyze is the skill of the worker, and do not find differences between them. Finally, we estimate the wage differential obtained if the worker moves along the supply chain as opposed to switching to a job at an unrelated company, and we find no effect.

From the firms' perspective, it is not irrelevant whether an employee moves to a random firm or to a trading partner. For example, when a firm becomes a supplier to a larger company, it may face an increased risk of employee departures to that customer. In such cases, the firm's own commercial relationship has contributed to creating the conditions under which the worker leaves. By contrast, when an employee moves to an unrelated firm, the separation is not directly linked to any specific action or business relationship of the employer.

In the analysis we use several Hungarian administrative datasets. The interfirm trade data contain information on domestic trade linkages and trade values for the period 2015–2022, covering annual transactions with a VAT value of at least HUF 1 million.<sup>3</sup> We merge this dataset with worker flows and firm balance sheets. To estimate the relation between trade and interfirm mobility, we run regressions where the observation is a supplier – buyer dyad and the dependent variable is the number of workers changing jobs.

Interfirm trade may foster information exchange between the firm and the worker. Most interfirm trade takes place through relational contracting, which materializes in regular exchange of information between the two firms (Macchiavello and Morjaria (2021); Van Biesebroeck and Schmitt (2022)). For example, the sale of critical inputs often require frequent meetings about the use of technology, quality of the product, and the organization of the production process. As a result, employees of trading firms often acquire inside information about the partner's job openings, working conditions and promotion possibilities, and employers also learn about workers' abilities. The interaction between partner firms and workers reduces both the job search costs and the uncertainty associated with transitioning to a new workplace. The information asymmetry can further be reduced if the trade is important for the supplier of the buyer, as in this case information exchange will probably be more intense. Furthermore, if the partner is a superstar firm, information exchange may reveal more about job opportunities and promotion possibilities. We also expect that high-wage partners may poach workers more efficiently from their trade partners. Finally, if a partner is growing, it will need workers and trade may further increase the mobility between the two firms.

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<sup>1</sup> We found two papers which analyze worker mobility along the value chains: Cardoza et al. (2025) in the Dominican Republic and Komatsu and Dhyne (2023) in Belgium.

<sup>2</sup> We do not know any paper using this estimation method to answer this question.

<sup>3</sup> This threshold is rather low and is equivalent to a net transaction value of EUR 11.5 thousand (0.2% of the average yearly firm-level sales).

Estimating the relation between interfirm trade and worker mobility faces several identification challenges. First, the data do not have information on *potential* trade links: we observe only firm dyads that engage in trade, while dyads that could potentially trade but do not are unobserved. Thus, in our dyad level data, we lack a control group that naturally exists at the firm level.

The solution to this problem in the literature is to construct a counterfactual mobility based on the assumption that workers were allocated randomly to available job openings, and compare the expected and the observed share of mobility occurring between trade partners (Cardoza et al. (2025); Hellerstein and Neumark (2008)). We build on this approach and integrate it with the configuration model methodology (Chung and Lu (2002)), which provides a closed-form expression for the probability of workers moving between two given firms (“naive mobility”). This enables us to compare these expected flows with the actual mobility observed along supply-chain links in the data (we refer to this method as the “*configuration model*”). We find that worker flows are much larger along the supply chain than between unrelated firms of the same location, industry, and the skill of the hired worker, with an odds ratio of about 6. We find similar odds ratios for S – B and B – S mobility.

This method creates a comparable control group and thus reduces the potential simultaneity issues between trading and mobility, which is likely to exist. Trading firms are often located near each other and tend to operate either within the same economic sector or in sectors connected through upstream or downstream linkages. In such cases, substantial worker mobility between these firms may occur even in the absence of interfirm trade. By choosing a suitable control group, it becomes possible to account for differences in geographical distance and skill needs, but unobservable drivers of mobility may still contaminate the estimation. Moreover, the estimation can also be biased due to reverse causality: not only trade connections can influence worker mobility, but worker mobility can also drive the establishment of trade links. For instance, if key employees – a sales manager or chief engineer – move from one firm to another, they may help fostering new trade relationships between the firms (Patault and Lenoir (2024)). Our preferred method is an alternative strategy that also creates a control group and further diminishes the simultaneity bias while it provides a partial test for reverse causality (we refer to this as the “*regression method*”).

We extend the dyad-level data longitudinally, and also create a control group by matching buyers with firms of similar characteristics with the following procedure. First we take firm dyads that have a trade relation in our data and add the years without trade to the sample. With this method we circumvent the problem of deciding which firms may trade in reality by using only those which did experience interfirm trade at some time.<sup>4</sup> To form a control group, match look for a match for every buyer within the same year, county, 2-digit industry, and size category while keep the actual supplier.<sup>5</sup> Then we append this sample to the original.

We perform our regressions on this sample. We add interactions between the supplier’s and buyer’s industry with years to the regression to control for industry-level time-varying shocks which may affect product demand (and the need for interfirm trade) and labor demand (which influences worker mobility) at the same time. We also control for dyadic fixed effects: this approach exploits within-dyad time variation and measures the effect of trade on mobility relative to mobility between the same dyad but without trade (e.g., before and after the trade relation existed). Dyadic fixed effects diminish simultaneity bias: as we estimate from the time variation of the same supplier-buyer relation, we partial out biases arising from interfirm mobility which arises naturally between two firms.

Reverse causality remains a potential concern even with dyadic fixed effects (Patault and Lenoir (2024)). While our ability to mitigate this bias is limited, we gauge the robustness of our results by restricting the sample to dyads with no prior mobility before the trade relation began, effectively shutting down the reverse causality channel. However, this selection method leads to a mechanical upward bias in the coefficient: by design, there is no worker mobility before the trade relationship forms, but afterwards, mobility can arise both due to the trade connection itself and due to random factors. To address this problem, we construct a pseudo-trade variable in the matched sample of non-trading dyads that mirrors the distribution of trade in the restricted sample. Running the same regression on this variable, we find that actual trade is followed by much higher mobility than pseudo-trade.

<sup>4</sup> We allow either one switch from “no trade” to “trade” or from “trade” to “no trade”. Firms that switch multiple times are excluded from the sample (less than 10% of all dyads). This restriction does not change the results qualitatively, but it allows the estimation of clear event-time patterns.

<sup>5</sup> The regressions can be performed without this control group, when not-yet-treated dyad-years will serve as controls. The estimated coefficients based on this sample are similar to the ones with the matched group.

Our results based on the configuration model find that the actual mobility along the supply chain is much larger than the expected mobility. Compared to the expected baseline mobility of 1.7%, the actual share of movers between trading firms is 9.6%, which corresponds to an odds ratio of 5.6. We find approximately the same odds ratios of about 6 between actual and expected mobility for S – B and B – S mobility.

The regression method estimates the number of workers moving between firms as a result of trade relative to the mobility for the same partners without trade. Trade relations increase the total worker mobility between trading partners by 0.019 workers. This number is very small, but the average unconditional mobility between two firms without trade is also very small (0.024).<sup>6</sup> Relative to this number, interfirm trade increases mobility by 79%. We find similar effects for S – B and B – S mobility (0.013 and 0.007 workers, respectively). In proportional terms, S – B mobility increases by 93% and B-S mobility by 70% when there is a trade relation between two firms.

To assess the difference between the configuration model and the regression method, we compute odds ratios from the regression results in the following way. We divide the mobility between trading firms (which equals the mobility without trade plus the estimated trade effect) with the mobility without trade. The odds ratios range from 1.8 to 2.4. These are substantial in magnitude, but considerably smaller than those obtained using the configuration model. We attribute the smaller odds ratios to a more conservative estimation method, when we compare mobility under trade with mobility of the same dyadic firm relation without trade.

Event-study results show that mobility between two firms jumps up when a trade link is formed and falls persistently when it is terminated, with limited evidence of pre-trends and stronger persistence when the trade relation terminates. When we limit the sample to those supplier-buyer pairs which did not experience mobility before treatment (to limit the possibility of reverse causality), the estimated effects do not change (and they are more persistent in time), suggesting that reverse causality probably does not play an important role in the estimation of the trade effect on mobility.

To assess the importance trade relations play in overall worker mobility, we compute the total number of workers moving from one firm to the other as a result of trade in the following way. We multiply the estimated effect (which is the additional number of workers moving along the supply chain in presence of interfirm trade) with the average number of trade links to obtain firm-level mobility. This number is further multiplied with the number of firms which have trade relation, which equals the total number of workers moving along the supply chain as a result of trade. This number can be compared to the total business-to-business (B2B) mobility of the firms which engage in trade to obtain the proportion of total B2B mobility originating from trade relations.

We find that total mobility due to trade amounts to 3.4% of total B2B mobility of the firms which have a trade relation. In the case of S – B and B – S mobility, trade induced mobility make up 1.9% and 1% of total B2B mobility of suppliers and buyers.<sup>7</sup>

Regarding various partner types, we find that they all increase worker mobility relative to all other types of partners. To start with important partners, they increase S – B mobility by about 150% and B – S mobility by 80% relative to the average worker mobility without trade. Trade partners with higher average wages than the firm, have a larger S – B mobility (145%), but lower B – S mobility (48%). Superstars attract more workers from suppliers to buyers (155%) and also from buyers to suppliers (110%). Finally, growing buyers increase S – B mobility by 116% and growing suppliers by 100%. These results show that these peculiar partners indeed raise the interfirm mobility of workers, and they also show that this is always larger for worker separations from suppliers than from buyers.

High- and low-skilled workers both exhibit large proportional increases in mobility along supply chains when firms trade, with no robust evidence that high-skilled workers systematically respond more. Wage regressions that compare within-dyad changes in movers' wages before versus after trade formation/termination show coefficients essentially indistinguishable from zero, suggesting that moving along supply chain does not, on average, yield higher wage gains than comparable moves between non-trading firms.

Related literature. Our paper is the third after Cardoza et al. (2025) and Komatsu and Dhyne (2023), which analyses worker movements along the supply chain. While we build on these studies, we also extend and complement them in several ways.

<sup>6</sup> The low mobility level is due to the fact that our dyadic time series incorporates all years, including those in which no mobility occurred between firms, rather than restricting the sample to years with mobility only.

<sup>7</sup> Total mobility does not add up to S-B and B-S mobility, as many firms are both suppliers and buyers at the same time.

Closest to our approach is Cardoza et al. (2025). They compute the difference between the actual mobility between trade partners and a simulated mobility between similar firms which are not trading. While we build on this paper (for example, our first method is very similar to the one they use), our paper differs in several important aspects.<sup>8</sup> Our dataset has both advantages and limitations compared to the one used by Cardoza et al. (2025). We consistently observe only trade transactions above approximately 11.5 thousand euros, whereas they capture all transactions. However, we have access to worker occupation data, which they lack. Our preferred method is a fixed effects regression applied to firm dyads and we look into the heterogeneity of the mobility effect by the direction of the mobility ( $S - B$  versus  $B - S$ ). We also explore the heterogeneity of these effects based on firm-level wage differentials, the importance of the partner, whether the partner is on a growing trajectory, and whether it is a superstar firm. Additionally, we analyze differences in labor mobility by worker skill level as well. To the best of our knowledge, this nuanced exploration is novel in the literature.

Komatsu and Dhyne (2023) has a different approach to ours. They develop an equilibrium model that integrates firm-to-firm production networks with on-the-job search to explain worker mobility along the supply chain. They identify two search channels: a market search channel and a network search channel, where workers receive additional opportunities from firms directly connected to their employer in the production network. They demonstrate that B2B trade concentrates workers' labor market options within their positions in the supply network, thereby increasing their vulnerability to productivity shocks within the production network. In contrast, our approach is a reduced form estimation, which tries to decrease biases as much as the data allows.

More generally, our work contributes to the literature on the role of networks in search and matching processes within the labor market. While traditional studies have predominantly focused on social networks as a means of reducing search frictions (Montgomery (1991); Dustmann et al. (2016); Glitz (2017); Arbex et al. (2019); Caldwell and Harmon (2019); Lester et al. (2021); Carrillo-Tudela et al. (2023)), recent research has increasingly shifted attention to the interplays between firm-level trade and labor flow networks. Demir, Fieler, et al. (2024) using similar data from Turkey, reveal positive assortative matching of skills between buyers and suppliers. Huneus et al. (2021) provide evidence that labor mobility is more common within business groups than between unaffiliated firms, with top workers frequently reallocated and receiving wage increases, facilitating the transfer of intangible inputs. Adão et al. (2022) demonstrates that international trade has a detrimental impact on earnings inequality among Ecuadorean workers, and finally, Patault and Lenoir (2024) uses an event study analysis to measure the effect of sales managers' job transitions on the formation on trade connections.

Our analysis also contributes to the literature on the consequences of joining the supply chains of various types of companies. On the one hand, several studies highlight the positive outcomes of establishing economic ties with large multinationals. For example, Alfaro-Urena et al. (2022) demonstrate that firms in Costa Rica experience substantial and sustained performance improvements after becoming suppliers to major multinational corporations, while Amity et al. (2024) find that joining the supplier networks of *superstars* – large domestic firms, exporters, or multinationals – boosts suppliers' productivity by 8% within three years. On the other hand, the impact of such large firms on labor markets is more contested. Moscarini and Postel-Vinay (2012) reveal that large companies contribute negatively to net employment during periods of above-trend unemployment, and Balsvik et al. (2023) show that the presence of multinationals generates mixed effects, including overall lower wages but higher profits for domestic firms. Additionally, Haltiwanger et al. (2018) show evidence that small, young firms inhibit the mechanisms of the firm size ladder by actively poaching workers from all other businesses.

Lastly, our study is connected to the broader literature on the role of trade networks, leveraging granular B2B trade information. This novel data source has facilitated groundbreaking research on various topics, including modeling shock propagation along supply chains (Barrot and Sauvagnat (2016); Carvalho et al. (2021); Demir, Javorcik, et al. (2024); Diem et al. (2022)), understanding the origins of aggregate fluctuations (e.g., Acemoglu et al. (2012); Di Giovanni and Levchenko (2012)), and identifying the drivers of firm performance (Bernard et al. (2019); Alfaro-Ureña et al. (2021); Bernard et al. (2022); Dhyne et al. (2022)).

The remainder of the paper proceeds as follows. Section 2 discusses the theoretical reasoning behind worker mobility along the supply chain. Section 3 describes our data and provides descriptive statistics on trade and labor mobility. Section 4 presents our empirical methodology, while the results of the analyses are described in Section 5. Section 6 concludes.

<sup>8</sup> Cardoza et al. (2025) use data from the Dominican Republic, whereas we analyze Hungary. Both are small open economies with similar populations of around 10 million. Hungary is part of the European Union and primarily trades with developed European Union countries. Dominica's main trading partner is the United States.

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## 2 Interfirm Trade and Worker Mobility

In a Mortensen–Pissarides type model of job search, workers switch jobs if the discounted net benefit of the new job exceeds that of the current job (Mortensen and Pissarides (1999)). The most important benefit is often the wage differential between the new and old job, but other job attributes also matter. These attributes can be numerous, including promotion opportunities, potential for acquiring new skills, or an inclusive workplace culture.

Besides its benefits, mobility also entails costs. Forming a productive worker–firm match requires both parties to engage in search, a process that is time-consuming and costly. Search is further complicated by substantial uncertainty about match quality. Labor is an experience good: a worker’s true productivity and the quality of the match with a firm are difficult to assess *ex ante*. Similarly, many workplace attributes are hard to observe for workers before joining, leading to additional informational frictions.

Interfirm trade can reduce both the cost of search and the uncertainty inherent in the process of matching firms and workers. Unlike a simple neoclassical model where trade occurs through arm’s-length contracts, most interfirm exchanges involve complex products. Producing these products requires relational contracting, which involves exchanging information about the production process, quality assurance, and procedures that ensure delivery even under unfavourable conditions.<sup>9</sup> This relational contracting involves information exchange between trade partners (Antras (2020)), enabling workers and firms to learn about each other’s unobservable characteristics which cannot be revealed during a standard job interview. Thus, relational contracting reduces search costs by allowing workers and firms to learn about each other while workers stay in their current job. It also reduces uncertainty about match quality since workers gain insight into workplace characteristics and firms learn about workers’ abilities. The lower search costs and reduced uncertainty will lower the total costs associated with job search, and we expect higher levels of worker mobility between two trading firms than between unrelated corporations. Because the productivity of high-skilled workers is harder to observe (Eeckhout (2018)), theory suggests that high-skilled workers are more likely to move to firms connected to their old job by trading relations.

Several partner characteristics may alter the effect of trade on worker mobility. First, trade involving substantial value (relative to firm size) should induce more mobility. Connections between such important trade partners are likely to involve regular meetings between the employees of the supplier and buyer, facilitating information exchange between workers about their skills and also about the job opportunities, and the workplace attributes in the partner firm. Second, high-quality partners (which we call superstar firms after Amiti et al. (2024)) may poach more workers than typical partners, as they offer better salaries, working conditions, and promotion opportunities. Third, the average wage level of the company may also matter. Since workers do not observe wages directly, trade relations can reveal the wage level of the partner firm more efficiently than a simple job interview. Finally, expanding firms generate demand for additional hiring, thereby further increasing labor mobility.

We argue that the magnitude of the wage differential of workers between the new and the old job is theoretically ambiguous. Wages may be larger for movers between trading firms as the higher level of information exchange can foster the creation of better matched which creates higher joint productivity of firms and workers, resulting in higher wages. On the other hand, the lower search costs can be covered with relatively low wage differentials, which can result in lower wage differentials for movers along the supply chain. Lower uncertainty about the new workplace can also induce workers to switch even if they could gain higher wages in another company, which is less certain to be a good match for them.

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<sup>9</sup> Békés et al. (2021) measure the type contracting between firms in Hungary, Romania and Slovakia, and find that over half of the interfirm relations are based on relational contracting.

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# 3 Data, Working Sample and Descriptive Statistics

## 3.1 DATA DESCRIPTION

We assemble the data from three proprietary administrative datasets: (i) firm-to-firm transaction network data, (ii) linked employer-employee data and (iii) data on firms' balance sheets and income statements. All datasets are anonymized, but can be linked with the help of an identifier. These data have information of the whole population of firms and workers.<sup>10</sup>

**B2B transactions data.** We constructed the firm-to-firm transaction network using value-added tax (VAT) reports collected by the National Tax and Customs Administration of Hungary. The dataset has information on only domestic trade relations. While the data collection began in 2014, we exclude the first year due to poor data quality caused by initial challenges in implementing the new reporting rules.

The transaction value threshold for including suppliers and buyers in VAT reports has changed multiple times during the period studied. Between 2015 and 2017 only trade connections with VAT content over HUF 1 million were recorded. This corresponds to a net transaction value of HUF 3.7 million or approximately EUR 11.5 thousand. Reporting rules changed in the middle of 2018, when the reporting threshold was decreased to one-tenth (VAT content of HUF 100 thousand), but the unit of observation was a transaction instead of the total value of transactions between two firms in a year. This means that firms with many small transactions may fall out of the sample.<sup>11</sup> Since the middle of 2020, all transactions have to be reported.<sup>12</sup> We aggregate all transactions to the yearly level.

To keep a consistent sample across years, we use a version of the dataset designed to ensure consistency across years. Our approach involves aggregating the data annually at the level of trade links between firms and applying filters based on the strictest reporting rules enforced since the data collection began. Specifically, we retain links which involve transactions with yearly VAT content exceeding HUF 1 million. Thus, our main variable of interest – *TRADE* – is a dummy variable that equals one in the years when two firms exchange goods or services in a value of at least HUF 3.7 million.<sup>13</sup>

**Worker mobility data.** To compute B2B worker mobility, we rely on the linked employer–employee database of the Hungarian State Treasury, which contains all employees whose contracts require social security contribution. We keep only the employees of corporations (so we drop non-working individuals, the self-employed and public employees). An individual can have multiple employment contracts at the same time and we keep the one which has the highest wage. If two spells have the same wage we keep the one with the longer length of time spent in the given employer.

We compute worker mobility on a yearly basis. For each year, we construct a snapshot of the employee records as of June 20. We identify a mobility event from firm A to firm B whenever a worker's firm identifier changes from A in year  $t$  to B in year  $t + 1$ . This allows us to track job-to-job transitions consistently across the entire observation period. Based on this, we define our worker mobility measure as the total number of workers moving between two firms (total mobility), from the supplier to the buyer (S - B mobility) and from the buyer to the supplier (B - S mobility).

Firm boundary changes (mergers or acquisitions) show up in the data as large movements of workers from one firm to the other. To attenuate this problem, we drop those firm-to-firm flows where the number workers moving from one firm to the other is larger than 50.

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<sup>10</sup> The version of the data we use is maintained by the Central Bank of Hungary.

<sup>11</sup> As the reporting threshold is very low, this affects the sample very little.

<sup>12</sup> For further details and in-depth descriptive analyses of the Hungarian trade network, see Borsos and Stancsics (2020) and Bacilieri et al. (2023).

<sup>13</sup> Note that firms engaged in trade relations under the threshold appear as if they have no trade. This misclassification works against us in our regressions: when we compute the within-firm difference of the dependent variable between no-trade and trade states, some firm-years are incorrectly classified as no-trade despite actual trade. Therefore, our estimates should be considered a lower bound of the true effect.

Firm-level data. The host of the firm-level data is the National Tax and Customs Administration. The data include all firms with double-entry book-keeping, meaning that all firms are present except small partnerships. The data has information on firms' balance sheets and income statements as well as information on employment, sector of activity and ownership (state, domestic, foreign).

## 3.2 WORKING SAMPLE AND VARIABLE DEFINITION

Our goal is to use panel data econometrics to reduce the bias of the estimation of interfirm trade on worker mobility. For this, we need a within-firm change of the explanatory variable in treated dyads and a group without changes to serve as a control group.

We construct the treated group by augmenting the trading dyads' time series with years when both the supplier and the buyer operate, and set the trade variable to 0. To obtain a clear identification and arrange the data in a format which allows the execution of event-time regressions, we keep only those dyads which have one change in the trade variable. This can either be the birth of the trade relation (trade switches from 0 to 1) or its death (trade switches from 1 to 0).<sup>14</sup> We refer to these data as the *trade data*.

We construct a control group by a matching procedure. We take each buyer in the data, and identify the group of firms in the same county, sector and number of employees in the first year of the buyer's appearance in the data. From this group, we randomly select one company and create a new firm dyad with the original supplier and the selected company. We refer to this group of dyads as the *matched data*. The balanced tables for matching are in the Appendix (Table 12 for sectors, Table 13 for the headquarters of firms, Table 14 for firm size and Table 15 for other variables).

Our final data is the trade data augmented with the matched data, to which we merge the variables of worker mobility and several firm level variables (sales, export volume, sector of activity, and a dummy indicating foreign ownership). From the worker data we merge employment (the number of workers associated with the firm identifier in a given year) and average wages (computed as the sum of individual wages over the number of workers).

We define the following four dummy variables which describe partner types: *important partner*, which equals one if the value of trade is larger than 10% of the firm's total sales; *high wage partner*, which equals one if the partner's average wage is higher than that of the firm's; and *superstar partner*, which equals one if the partner is foreign owned or exports at least 10% of its sales and has more than 100 employees, or it has at least 500 employees (we exclude state-owned firms from superstars).

The final dataset comprises 61 – 70 thousand suppliers and 80 – 100 thousand buyers depending on the year. The total number of B2B transactions is 1,291,198 (161,400 on average each year), out of which 509,458 dyads experience a trade link formation and 137,691 a termination of trade relation.<sup>15</sup>

## 3.3 CHARACTERISTICS OF SUPPLIERS, BUYERS AND THE TRADE RELATION

Table 1 summarizes several firm-level characteristics of the trading data. The number of buyers and suppliers is around 100,000, and they are very similar across several attributes. Their average employment size is about 20 (suppliers are somewhat larger). Labor productivity (measured as sales per employee) is also very similar at HUF 65 – 67 million. The number of superstar partners is low (just above 2000) and there are about 10% more superstar buyers than suppliers.

The distribution of the number suppliers and buyers is presented in Figure 1. The number of partners is rather similar: approximately half of firms have only one buyer and 45% one supplier. 20% have two buyers/suppliers, and about 10% of the firms have 10 or more partners.

<sup>14</sup> Multiple trade switches within dyad make up 8 percent of the trading data. If we include these firms, we obtain very similar results.

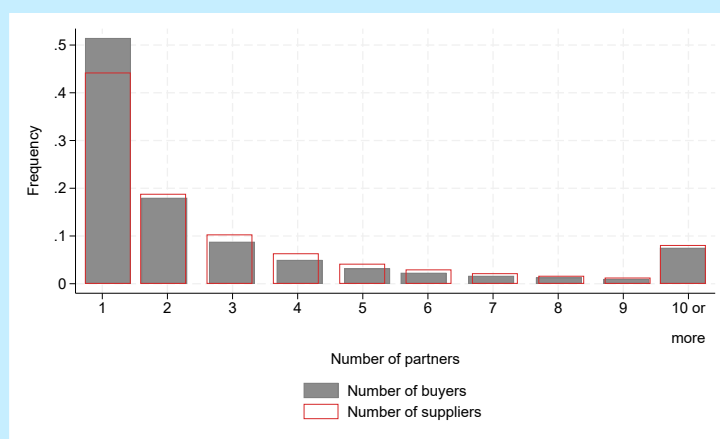
<sup>15</sup> The total number of observations in the trade sample is 3,844,737 (this includes the years where there were no trade between two firms). The matched data consist of 3,036,991 observations. The sample size is less than the original because in some cases we do not find any similar buyer during the matching process.

**Table 1**  
**Buyers and Suppliers: Main Characteristics**

|          | N      | Employment      | Labor<br>Productivity | Superstar<br>Firm |
|----------|--------|-----------------|-----------------------|-------------------|
| Buyer    | 102144 | 19.9<br>(170.3) | 67.1<br>(433.8)       | 2219              |
| Supplier | 96846  | 23.2<br>(245.7) | 65.4<br>(462.3)       | 2028              |

Notes: The table presents the characteristics of suppliers and buyers for the working sample. Employment = yearly average number of employees; Labor productivity = yearly average sales per employee (in million HUF). Superstar firm: Exporter or Foreign owned and employment  $\geq 100$  or employment  $\geq 500$ . Number of partners = yearly average number of suppliers for buyers, and buyers for suppliers (only for those firms which have a partner in a given year). Standard deviation in parentheses.

**Figure 1**  
**Number of Suppliers and Buyers**



Notes: The figure presents the distribution of the average yearly number of suppliers and buyers of a firm.

Figure 2 shows the distribution of the observed length of the trade relation. We compute this by computing the length of the relation in years, and weighting it with the number of years. This statistic shows the frequency of the maximum duration of the trade relation in the data.<sup>16</sup> The figure presents a linearly decreasing pattern, with 27% of trade relations lasting for only one year and about 5% for 7 years (the maximum length in the data).

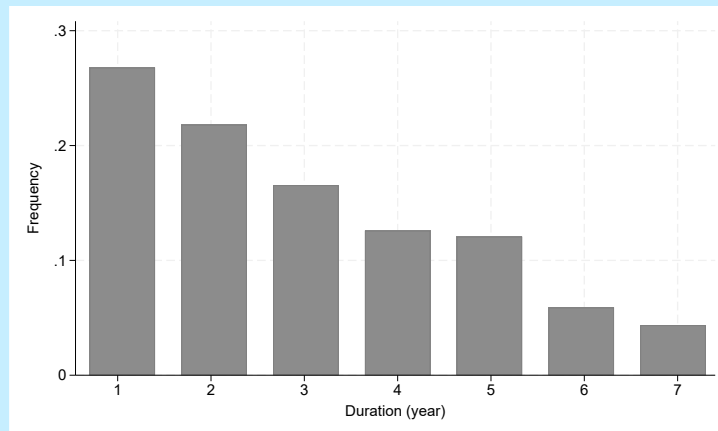
Figure 3 presents the share of trade value between two firms relative to their suppliers' and buyers' total value of sales. A large fraction of interfirm trade is small relative to the size of the firm (under 1%), but there 12-15% of the suppliers and buyers exchange goods and services which amount to over 10% of their sales.

Table 2 presents the share prevalence of the four partner types (important, high-wage, superstar, growing partner), and also of unimportant partners (which indicates that the partner is in neither of the four categories). Suppliers and buyers have similar shares of these peculiar types: 12-14% of them are important, 21-23% are superstars and 37-38% are growing. The only difference between buyers and suppliers is the high wage type, which has a 61% share among suppliers and only 44% among buyers.

Are partner types distinct, or can a supplier or a buyer fall into more than one category? To answer this question, Appendix Table 17 reports the share of each special partner type that also belongs to another special category. The table shows, for each row partner type, the proportion of firms that are simultaneously classified into the column type.<sup>17</sup> Some types tend to

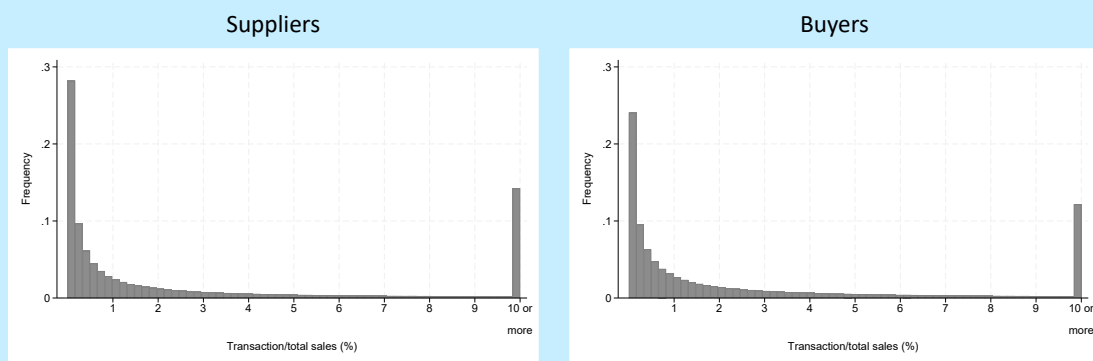
<sup>16</sup> For example, relations which last one year are given a weight of 1, those lasting 2 years a weight of 2 and so on. Have we not weighted this statistic, short-term relations would be mechanically overrepresented.

**Figure 2**  
**Duration of the Trade Relation**



Notes: The figure presents the distribution of the duration of the trade relation between firm dyads. The duration is measured by the number of years in trade relation. The histogram is weighted by the number of years spent in the relation.

**Figure 3**  
**Share of Interfirm Trade in Sales**



Notes: The figure presents the distribution of the value of trade between two dyads relative to the supplier's or buyer's value of total sales.

**Table 2**  
**Buyer and Supplier Types**

|          | Important | High Wage | Superstar | Growing | Other |
|----------|-----------|-----------|-----------|---------|-------|
| Supplier | 0.12      | 0.61      | 0.21      | 0.38    | 0.32  |
| Buyer    | 0.14      | 0.44      | 0.23      | 0.37    | 0.29  |

Notes: N = 3,843,122. The table presents the average share of supplier and buyer types. Important partner = 1 if the share of sales in total sales to a given buyer  $\geq 10\%$ . High-wage partner = 1 if the partner's average wage is above the firm's average wage. Superstar partner = 1 if the partner is an exporter or foreign owned and employment  $\geq 100$ , or employment  $\geq 500$ . Growing partner = 1 if employment increased during the trade relation. Other partner = none of the above.

be present together, but none of these proportions are very high (the maximum is 43%). Thus, there is enough variation in the data to estimate regressions with these types.

Table 3 reports measures of B2B mobility, distinguishing between total mobility and mobility along the supply chain. In the first panel we show the proportion of B2B movers relative to total employment in the population of all Hungarian corporations.

<sup>17</sup> For example, the first row presents the share of important partners that are also high-wage, superstar or growing partners relative to the number of important partners.

9.6% of workers change employer from one year to the next, which is very similar to the same statistic in the working sample (12.4%). 8.2% of the employees of suppliers and 8.4% of buyers move to another firm. Relative to total B2B mobility, only a small proportion – 0.88% – of workers switch to trading partners. Thus, the share of mobility along the supply chain is 9% of the total B2B mobility.<sup>18</sup> This number, however, does not take into account the baseline probability of switching between two firms, which is much lower for trading firms, as we show below.

**Table 3**  
**Interfirm Worker Mobility**

|                | B2B Mobility<br>Total | B2B Mobility<br>Trading Partners |
|----------------|-----------------------|----------------------------------|
| Population (%) |                       |                                  |
| Total          | 9.56                  | 0.88                             |
| Sample (%)     |                       |                                  |
| Total          | 12.4                  | 0.88                             |
| Supplier       | 8.20                  | 0.74                             |
| Buyer          | 8.40                  | 0.46                             |

Notes: The table presents the percentage of workers moving to another firm and to trading partners in the population (top panel) and in the trading sample (firm-level, bottom panel). The results for the sample are presented separately for mobility from suppliers and from buyers, relative to total supplier and buyer employment, respectively.

Worker switches from buyers to suppliers is 60% lower than from suppliers to buyers (0.46% and 0.75% of total B2B worker mobility of suppliers and buyers, respectively). This translates to a yearly average of 0.03 workers moving from a given supplier to a given buyer and 0.02 from a given buyers to a given supplier.

<sup>18</sup> In the Dominican Republic this statistic is 19% (Cardoza et al. (2025)) and in Belgium 40% (Komatsu and Dhyne (2023)). Besides differences between the countries, the reason for this large discrepancy is that they observe all trade relations, while we have in our data only those which pass a value threshold of VAT content of HUF 1 million.

## 4 Methodology

One fundamental difficulty of the estimation of mobility along the supply chain is the lack of a control group, because the data have information only on the materialization of interfirm trade and not the potential trade relations. Thus, the researcher has to construct firm dyads without trade, to compare the mobility of workers between these firms with the mobility between firms which have interfirm trade relation.

We employ two approaches to address our question. The first method builds on the framework proposed by Cardoza et al. (2025), which compares the observed mobility along the supply chain with mobility between firms with similar characteristics to trading partners, but without a trading relationship. Instead of relying on repeated random generation of potential samples, we implement a deterministic formula, which we borrow from network science. The second method is our preferred specification, which draws from the event study literature, and generates difference-in-difference estimates of worker mobility around changes of interfirm trade status while using a properly constructed control group.

### 4.1 MOBILITY ALONG THE VALUE CHAIN RELATIVE TO NAIVE EXPECTATIONS

Cardoza et al. (2025) demonstrated that worker mobility is significantly more frequent toward firms' trade partners than between randomly chosen firms. To quantify this increased mobility, they calculated the proportion of workers transitioning to buyers and suppliers under a hypothetical scenario in which workers are randomly allocated to firms within their likely labor market. Firms were defined as having  $N$  job openings if they hired  $N$  workers from other firms. Workers were then randomly assigned to these openings, and the proportion moving to buyers and suppliers was recorded. This randomization process was repeated 100 times to compute the average share of worker flows to trade partners across iterations. This approach effectively controls for the fact that larger firms typically have more trade relationships, and hence, buyers or suppliers are likely to be larger than the average firm. Additionally, their method accounted for several factors that might influence both trade link formation and labor flows, including firms' locations and sector of activity.

This computational procedure aligns with the logic of standard null models in network science, which randomize the edges of a graph while keeping other network characteristics, such as the density and the degree distribution intact<sup>19</sup>. However, the method used in Cardoza et al. (2025) provides only a numerical approximation of the true odds ratios instead of comparing the observed number of trade connections along labor flows to the exact expected number of such links computed with a closed form formula. We describe this latter approach below and apply it to our data in Section 5.

For a given firm  $i$ , let  $k_{ij,t}^{out}$  denote the number of workers transitioning from firm  $i$  to firm  $j$  in period  $t$ . For these workers, we can count the number of relevant alternative job openings, considering factors like location and industry similarity, that might have served as potential destinations.<sup>20</sup> Thus, the number of elements, i.e. the cardinality of the set of relevant openings,  $\theta_{i,j}$  is defined as the number of workers flowing into the potential destination firms during period  $t$ :

$$|\theta_{i,j,t}| = \sum_{l=1}^L (k_{l,t}^{in}), \quad (1)$$

where  $L$  is the number of firms with the relevant job openings, and  $k^{in}$  represents the in-degree of a firm in the labor mobility network.

Similarly, we can define the cardinality of the subset of relevant job openings of firms which also maintain trade relations with firm  $i$  in time  $t$ :

<sup>19</sup> See the *configuration model* Chung and Lu (2002), the *stochastic block model*, or the *balls and bins model* Bernard and Zi (2022).

<sup>20</sup> Following Cardoza et al. (2025), we assume that all job openings across firms are filled, hence, observable in the labor mobility data.

$$|\Phi_{i,j,t}| = \sum_{n=1}^N (k_{n,t}^{in}), \quad (2)$$

where  $N$  denotes the number of firm  $i$ 's trade partners with relevant job openings. Using these definitions, the expected number of workers transitioning from firm  $i$  to its trade partners can be expressed as:

$$\mathbb{E}[W_{i,t}^{\tau}] = \sum_{j=1}^M (k_{ij,t}^{out} \times \frac{|\Phi_{i,j,t}|}{|\theta_{i,j,t}|}). \quad (3)$$

where  $M$  denotes the number of firms to which workers of  $i$  moved in time  $t$ . This formula is analogous to the expected edge weight in the Chung-Lu configuration model (Chung and Lu (2002)).

The odds ratios can be directly calculated by comparing the empirical worker mobility observations to these expected values, providing a more precise alternative to simulation-based approximations.

## 4.2 REGRESSION METHOD

We employ panel data methods to estimate the effect of interfirm trade on worker mobility. We run the regressions on the trade and matched data together.

On these data, we run the following equation:

$$M_{sbt} = \beta + \gamma \cdot TRADE_{sbt} + \alpha_{sb} + \theta_t \times IND_s + \theta_t \times IND_b + \epsilon_{sbt}, \quad (4)$$

where  $s$  and  $b$  indexes supplier and buyer firms and  $t$  indexes years. The dependent variable,  $M_{sbt}$  is the number of workers moving from the supplier to the buyer.  $TRADE_{sbt}$  is a dummy variable which equals one if there was trade between the buyer and the supplier in year  $t$ .  $\alpha_{sb}$  is the buyer-supplier pair (dyadic) fixed effect,  $\theta_t$  stands for year fixed effects and  $IND_s$  and  $IND_b$  are two digit sectoral dummy variables.

This regression controls for a number of biases which are likely to exist in the data. First and most important, two firms may exchange workers not only because of bilateral trade, but also for other reasons, such as geographical proximity or similar composition of workforce. Biases arising from this regularity are taken care in the data, as our dyad fixed effects regression compares the same supplier-buyer pair in presence of trade and without it. The carefully chosen control group makes sure that control dyads are similar to those in trade relation: in fact, in our matched control group the suppliers are the same as treated suppliers and the buyers are also similar by their observable characteristics.

One source of bias not solved by the regression method is that of reverse causality. If the underlying mechanism is such that workers who move from one firm to another cause bilateral trade (and not vice versa), the regression will be biased. To test for this possibility, we restrict the sample to those dyads which experience a new trade relation (as opposed to the cessation of one) and did not have any mobility before the trade took place. This restriction decreases the possibility of reverse causality. But this regression can produce a spurious correlation between mobility and trade by construction. As we do not allow any mobility before trade, random mobility during the trade relation will result in a positive coefficient. To control for this, we perform a pseudo outcome test. We estimate a time varying hazard of beginning of trade relations among the set of dyads with new trade relations without mobility. We apply this hazard on the matched dyads (which do not have trade by construction), and create a pseudo trade variable. With this variable, we run our baseline regression. The coefficient resulting from the pseudo trade regression is the random part of the mobility. By comparing this with the coefficient on the actual trade, we can measure the true effect of trade on mobility, when no mobility took place before trade.

A final source of bias may arise because we measure trade with a systematic error as we do not observe trade relations under a certain value threshold. This may cause a bias as in some years which we code as without trade the two firms are actually

having some small trade. We argue, however, that this biases our fixed effects results downwards: in the years without trade some have trade, so trade may already have some effect on mobility before/after years of trade. Thus, our estimated coefficient should be interpreted as a lower bound or the true effect.<sup>21</sup>

Finally, macroeconomic shocks affecting firm output, may be correlated with both the formation or destruction of trade relations and also with firms' need for workers. Thus, shocks may drive our results. To control for such problems, we add 2-digit industry – year controls to the data for both the suppliers and buyers.

Having established the average effect of trade on mobility, we turn our attention to trade and partner characteristics: important high wage, superstar and growing partners (which we defined in Section 3.2). When we estimate these heterogeneous treatment effects, we augment Equation 4 with a dummy variable indicating an intense trade relation, high wage differential of superstar partner:

$$M_{sbt} = \beta + \gamma_1 \cdot TRADE_{sbt} + \gamma_2 \cdot TRADE_{sbt} \cdot HET_{sb} + \alpha_{sb} + \theta_t \cdot IND_s + \theta_t \cdot IND_b + \epsilon_{sbt}, \quad (5)$$

where  $HET_{sb}$  is a dummy for indicating either strong supplier-buyer relationship or the partner having higher average wages or the partner being a superstar firm or a growing partner.

Finally, we analyze the wage differential between the old and new workplace. For this, we compute the difference in the log average wages of the movers between the subsequent and precedent two years relative to the year of move. Then we run the same regression as before, but only on a two time period panel.<sup>22</sup>

<sup>21</sup> We show below that the intensity of trade is proportional with the mobility effect.

<sup>22</sup> In this setup we can only use those dyads where there was actual labor flow.

# 5 Results

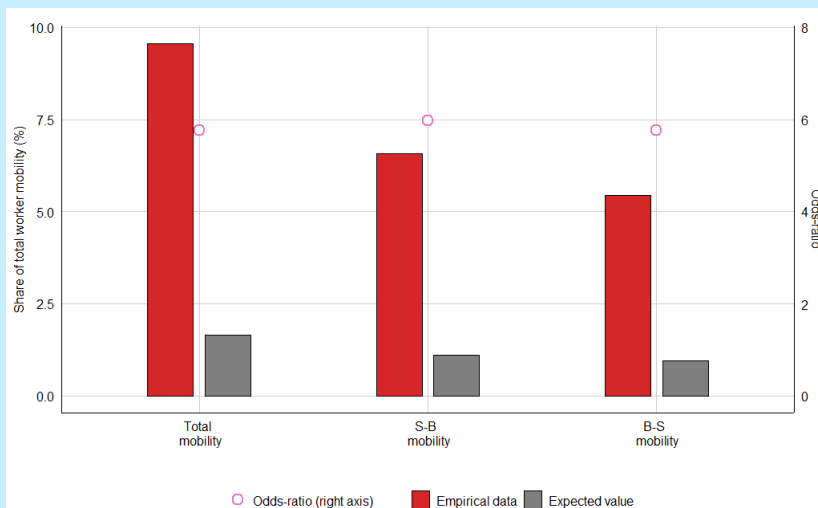
## 5.1 EFFECT OF TRADE ON INTERFIRM WORKER MOBILITY (CONFIGURATION MODEL)

We start the investigation of mobility along the supply chain by comparing the actual worker mobility with the expected mobility between firms in trade relation, as described in Section 4.1. We performed this analysis by calculating  $|\theta|$  (the number of workers flowing into the potential destination firms) and  $|\Phi|$  (the number of workers flowing into the potential destination firms which also maintain trade relations with the focal firm) with restrictions on the location, industry and skill requirements of the potential openings to match the characteristics of the empirically observed worker flow destinations.<sup>23</sup> We evaluate the total share of workers moving between trade partners, as well as flows from suppliers to buyers and from buyers to suppliers.

The actual mobility is much larger than the mobility resulting from the null model (Figure 4). Total mobility between trade partners is 9.6% of B2B mobility in the data, while the null model predicts only 1.7%. The corresponding odds ratio is 5.8. This value is higher than the result obtained by Cardoza et al. (2025) in the Dominican Republic, who find that trade boosts mobility by about 12% and Komatsu and Dhyne (2023) in Belgium who find an even larger effect of 22-25%. One reason for this can be the different definition of trade links: these papers can detect all the trade linkages while in the Hungarian data we have a value threshold.

**Figure 4**

**Worker Mobility Between Trade Partners: Observed and Computed from the Null Model**



Notes: The Empirical data (red bars) denotes the shares of movers to trade partners in the empirical data, while the Expected value (gray bars) denotes the expected value of these shares calculated using the location, the industry, and skill requirements of the potential openings as control variables. The pink dots show the corresponding odds ratio. We evaluate the share of workers moving to trade partners when we consider (i) all trade links (total mobility), (ii) links to buyers only (S – B mobility), and (iii) links to suppliers only (B – S mobility).

When we distinguish between trade links directed toward buyers and suppliers, we find similar effects. The difference between the measured and the expected mobility for S – B mobility is 6.6 - 1.1 = 5.4% and for B – S mobility 5.4 - 0.9 = 4.5%. The direction of the trade link does not affect considerably the odds ratio as they are close to 6 and 5.8.

<sup>23</sup> Firm locations are considered at the NUTS 3 level. Industries are classified using 2-digit NACE categories, with manufacturing industries grouped together. Job skill requirements are categorized as either high (ISCO code 1-3) or low (ISCO code 4-9).

## 5.2 EFFECT OF TRADE ON INTERFIRM WORKER MOBILITY (REGRESSION METHOD)

Table 4 presents the results of our baseline regression specifications for three distinct dependent variables: the number of workers moving between partners (total mobility) from supplier to buyer (S – B mobility), and from the buyer to supplier (B – S mobility). Trade relations increase worker mobility along the supply chain: the estimated coefficient of bilateral mobility equals 0.019 and it is highly significant. The estimated coefficient for S – B mobility is 0.013 and of B – S mobility 0.007.<sup>24</sup> The small size of the coefficients is due to the small baseline worker mobility between any specific pair of firms: without trade the unconditional mobility in our data is 0.024, 0.014 and 0.010 for the three types of mobility. To assess the magnitude of the effect, we translate it to proportional terms: relative to the mean mobility in absence of trade, total mobility increases by 79%, S – B mobility by 93% and B – S mobility by 70%.<sup>25</sup>

**Table 4**  
**Effect of Interfirm Trade on Worker Mobility**

|                         | Mobility<br>Total   | Mobility<br>Supplier - Buyer | Mobility<br>Buyer - Supplier |
|-------------------------|---------------------|------------------------------|------------------------------|
| Trade                   | 0.019***<br>(0.001) | 0.013***<br>(0.001)          | 0.007***<br>(0.000)          |
| Proportional Effect (%) | 79                  | 93                           | 70                           |
| Mean Mobility           | 0.024               | 0.014                        | 0.010                        |

Notes: N = 6,793,752. Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Mean mobility = average unconditional mobility in the years when Trade = 0. Proportional effect = estimated coefficient divided by mean mobility (%). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

While our preferred specification accounts for time-invariant heterogeneity across firm pairs, it cannot fully rule out the possibility that the estimated effect is driven by factors that simultaneously influence both trade relationships and B2B mobility. To address this concern, we estimate alternative specifications in which dyadic fixed effects are replaced with supplier-year or buyer-year fixed effects, as well as a specification that includes both sets of fixed effects simultaneously. Although these approaches do not control for time-invariant firm- or dyad-level characteristics that may be jointly correlated with both the explanatory variable and the outcome, they identify the effect of trade on mobility by exploiting variation in worker mobility between trading partners and firms that are not engaged in a trade relationship. Under this framework, product demand shocks that affect mobility flows from both trading and non-trading partners should not bias the estimated coefficients.

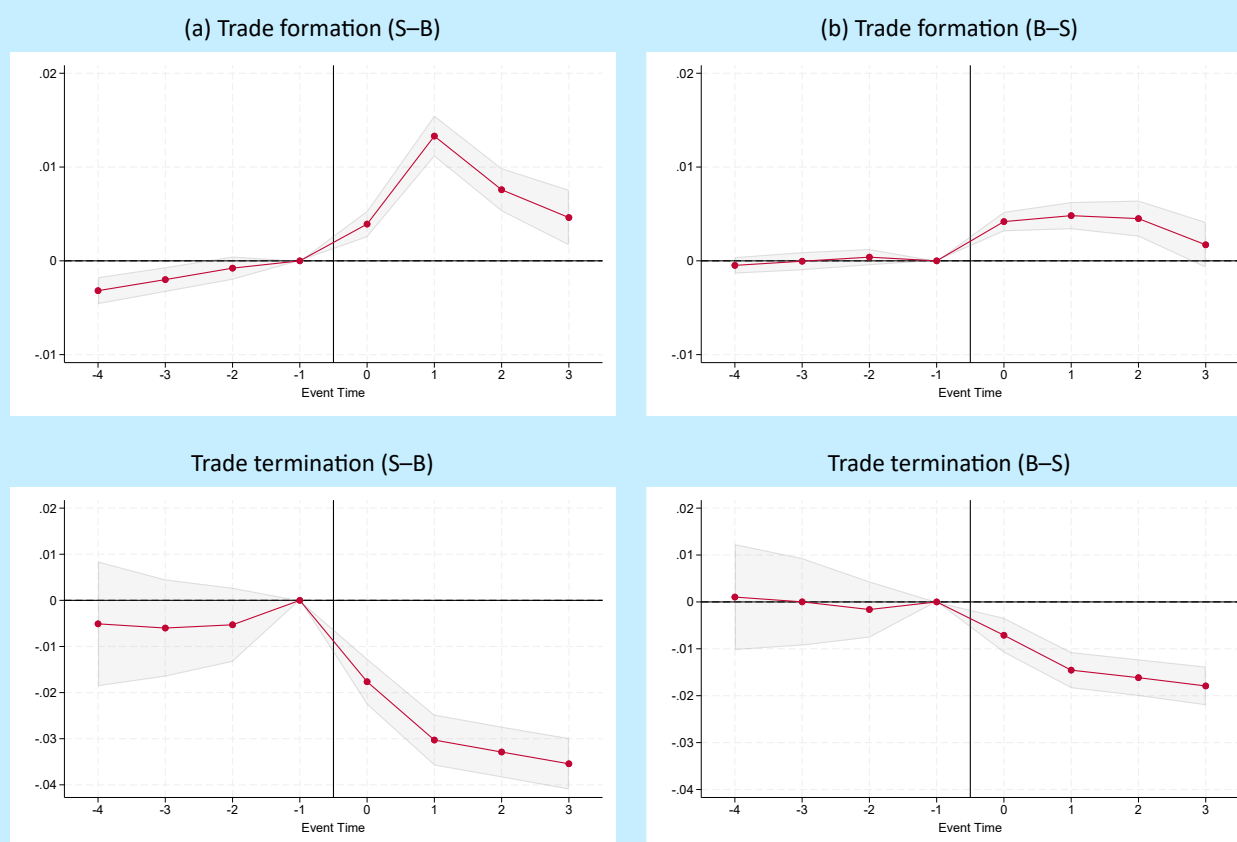
The results, reported in Appendix Table 16, remain positive and statistically significant. The coefficients are substantially larger than those obtained from our preferred specification, both in absolute terms and relative to the mean level of B2B mobility. This pattern suggests that dyadic fixed effects play an important role in removing confounding variation, thereby yielding more conservative estimates of the effect of trade on worker mobility.

To compare the regression estimates with the results from the configuration model, we compute odds ratios in the following way. We compute the worker mobility when there is trade between firms, which equals to the mean mobility without trade ( $m_0$ ) and the estimated coefficient  $\gamma$ . The odds ratio is the ratio between the mean mobility with and without trade,  $(m_0 + \gamma)/m_0$ . The odds ratios for total, S – B and B – S mobility are 1.8, 1.9, and 2.4, which are sizeable, but much smaller than what we computed from configuration model (5.8 – 6.0).

<sup>24</sup> These results are robust to several changes. Dropping the matched control dyads do not affect the estimated coefficients qualitatively. Replacing supplier sector - year and buyer sector - year fixed effects with simple year fixed effects also provides similar estimates. Adding to the data dyads which switch their trade relation multiple times also provides similar estimates.

<sup>25</sup> To test the robustness of the estimation method, we run Poisson Pseudo-Maximum Likelihood (PPML) regressions with dyadic fixed effects. The results are in Appendix Table 18 and estimate coefficients that are highly statistically significant and comparable in magnitude to those obtained in our preferred specifications: total mobility increases by 66%, S – B mobility by 84% and B – S mobility by 48%. (The proportional effect equals  $e^{\beta} - 1$ , where  $\beta$  is the estimated effect.)

**Figure 5**  
**Dynamic Effects of Trade Link Formation and Termination**



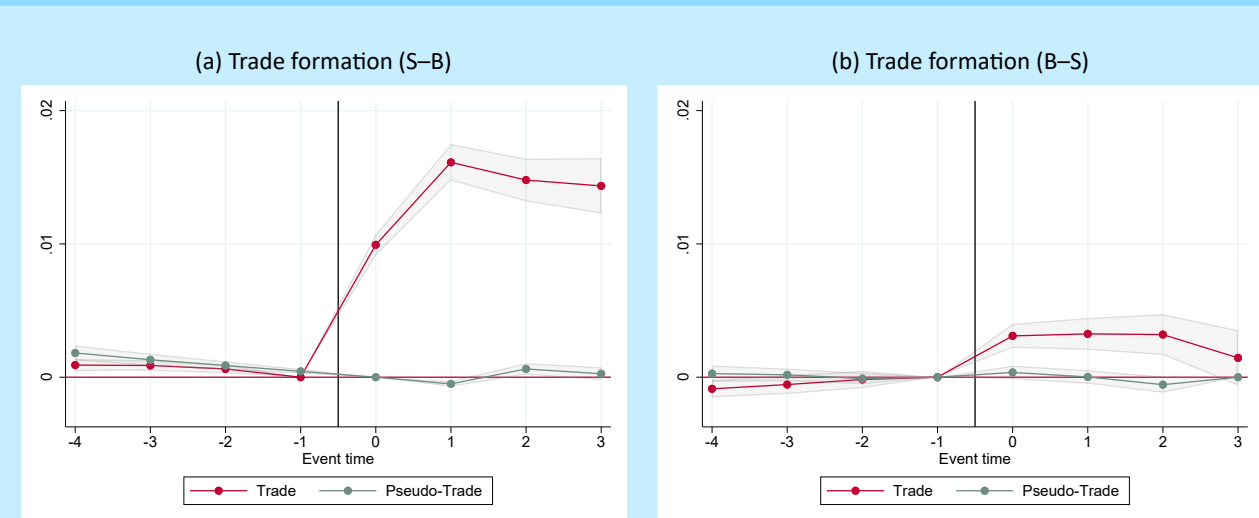
Notes:  $N=5,535,106$  for trade formation and  $N=3,564,250$  for trade termination. The figures present the estimated coefficients and 95-percent confidence intervals of event-time regressions around trade formation (top figures) and trade termination (bottom figures). Comparison year: one year before the event.

Our data consists of suppliers and buyers which experienced trade formation and others which underwent trade termination. We look at these two processes separately in Figure 5, where we present the estimated coefficients and 95-percent confidence intervals for event-time analyses of trade formation and termination separately. The figure also serves for testing pre-trends to assess potential endogeneity problems. For example, it is possible that worker mobility between two firms is already increasing when trade takes place (or declining when trade terminates), and our estimates pick up this spurious trend.

Panels A and B show  $S - B$  and  $B - S$  mobility around trade formation while panels C and D around trade termination. New trade between two firms induces a quick increase in both  $S - B$  and  $B - S$  mobility, which is reduced after 2 – 3 years. Trade termination has a more permanent effect: mobility declines for about two years and remains at a low level thereafter. The figures indicate that reverse causality is unlikely to be a major concern in the data. The drop in mobility associated with trade termination is not preceded by any pre-trends, and this is also true for trade formation for the  $B - S$  mobility.  $S - B$  mobility for new trade connections has a pre-trend, but it is small relative to the estimated increase in mobility after trade was established.

To mitigate the possibility of reverse causality for trade formation, we run the dynamic regressions only on those firms which did not experience any mobility before trade formation (we cannot do this for trade termination). An inherent difficulty with this specification is that the estimated coefficient is biased by construction. Mobility may occur for many reasons, and when we restrict the sample to dyads that do not experience mobility prior to trade, any mobility observed after trade will be attributed to trade itself. To correct for this bias, we construct a pseudo-trade variable in the matched sample, which has the same distribution as the actual trade variable. Then we select those dyads from the matched sample which did not have any mobility in the three years before pseudo-trade variable takes the value of 1. On this sample we run our regression with the pseudo-

**Figure 6**  
**Dynamic Effects of Trade Link Formation (No Mobility Before Trade Formation)**



Notes:  $N = 5,500,785$  when there is no mobility before and  $1,337,609$  for the pseudo sample. The figures present the estimated coefficients and 95-percent confidence intervals of event-time regressions around trade formation, when mobility before the event is zero (red line), and for pseudo trade formation on the matched sample when mobility is zero before the pseudo-trade formation year (green line). Comparison year: one year before the event.

trade variable as the variable of interest. The results of these regressions are presented in Figure 6, and demonstrate that trade induces a strong mobility effect in this sample. On the contrary, mobility on the pseudo trade sample changes very little.

To assess the overall magnitude of trade on worker mobility, it is useful to compute the proportion of workers moving between partners relative to the total B2B mobility, which we do in the following way. First we multiply the number of workers moving due to trade (that is, the estimated coefficient) with the average number of trade links to obtain the number of workers moving to trade partners at the firm level. Then we multiply the resulting number by the average yearly number of firms engaged in trade, to obtain the total number of mobile workers due to trade. We compare this number to the total average B2B mobility of the firms which engage in trade.

The results of this computation are in Table 5. For example, the first line of the table shows the numbers for total mobility. The estimated additional mobility due to trade is 0.019. We multiply this number by the average number of trade links (4.2), to obtain the additional number of workers at the firm level (0.081). We multiply this number by the number of firms in trade relation (55,139) to obtain the total number of workers moving between trade partners (4,455). The total average yearly mobility of trading firms is 130 thousand. Thus, trade relations increase mobility between partners by 3.4%. The same calculation for S – B and B – S mobility results in 1.9% and 1%.<sup>26</sup>

The proportion of mobility resulting from trade relative to total B2B mobility is based on our fixed effects estimations. It is useful to compare these proportions with the results presented in the previous subsection, when the actual mobility is compared with an expected mobility without trade (see Figure 4). The comparable number to our proportions of these estimations is the difference between the actual and expected mobility. This method estimates that total mobility due to trade makes up 7.9% of total B2B mobility, which is 2.3 times larger than our estimate of 3.4%. For S – B and B – S mobility, the estimates from the previous method are 5.5% and 4.5%, which are 2.9 and 4.5 times larger than our estimates. One explanation for this discrepancy is the presence of dyadic fixed effects in our estimations, which estimates the change of mobility of the same firm pairs with and without a trade relation.

<sup>26</sup> The proportional effects of S – B and B – S mobility do not add up to total mobility as many firms engage in interfirm trade as both suppliers and buyers.

**Table 5**  
**Share of Trade Induced Worker Mobility in Total Business-to-Business Mobility**

|                | Effect | No. Links | Firm level Mobility | No. Firms | Total Mobility | Total B2B Mobility | % B2B Mobility |
|----------------|--------|-----------|---------------------|-----------|----------------|--------------------|----------------|
| Total Mobility | 0.019  | 4.2       | 0.081               | 55139     | 4455           | 130299             | 3.4            |
| S – B Mobility | 0.013  | 4.0       | 0.052               | 37214     | 1935           | 101115             | 1.9            |
| B – S Mobility | 0.007  | 4.4       | 0.031               | 39380     | 1213           | 115877             | 1.0            |

Notes: N = 6,793,752. The table presents the share of trade-induced worker mobility relative to total B2B mobility. Effect = estimated number of workers moving along the supply chain. Firm-Level Mobility = Effect  $\times$  No. Links; No. Firms = average number of firms with a given trade relation; Total Mobility = Firm-Level Mobility  $\times$  No. Firms; Total B2B Mobility = average number of workers moving to another business for the given group of firms; % B2B Mobility = Total Mobility  $\div$  Total B2B Mobility (%).

### 5.3 HETEROGENEITY BY PARTNER TYPE, WORKER SKILL AND EFFECT ON WAGES (REGRESSION METHOD)

Trade effect by partner type. We start our heterogeneity analysis by estimating the effect of trade on mobility for various partner types. In particular, we measure the importance of the trade relation with a dummy variable which equals 1 if the interfirm trade with a given partner accounts for at least 10% of the firm's sales. We also investigate mobility when the partner's average wage is higher than of the firm's, whether it is a superstar firm (defined in Section 3.2) and if its employment is growing during the trade relation. Beyond revealing how partner characteristics influence the trade effect on mobility, this exercise also allows us to test whether the asymmetry observed between S – B and B – S mobility is driven by the composition of buyers and suppliers or by a more fundamental underlying mechanism.

The first panel of Table 6 presents the results for S – B mobility. As above, in addition to reporting the estimated coefficients, we also present the proportional change in mobility in the table. This measure equals the sum of the coefficients on trade and on the interaction between trade and the partner characteristic, divided by the unconditional mean mobility in the absence of trade. A trade relationship with an important partner increase mobility by 148% relative to the baseline mobility of suppliers.<sup>27</sup> A partner with a higher average wage also increases mobility by about the same proportion, while superstar partners raise mobility by 155%. Relation with a growing buyer also raise mobility by 116%. In the last column, we add all the interaction terms together to the regression. The estimated coefficients do not change to a large extent. The sole exception is the variable indicating a high wage partner, which declines by 60%. Taken together trade and its interactions with various partners raises mobility almost 4 times.

The estimated effects of various partner types on B – S mobility are presented in the lower panel of Table 6. These also indicate increased mobility from buyers to suppliers for each partner types, except for high-wage suppliers, when the effect is smaller than the baseline trade effect. If the trade relation is important for the buyer, mobility increases by 81%; for high wage suppliers it grows by only 48%; for superstar suppliers by 110%, and for growing suppliers by 100%. When all the interaction terms are jointly estimated, the effect doubles for important suppliers and declines by 50% for high wage suppliers. In total, they double mobility. These effects are large in magnitude, but all of them are smaller than what we measure for S – B mobility. In fact, the asymmetry in the effects is larger for these partners than for the baseline trade effect (see Table 4).

Our results show that downstream mobility along the supply chain is stronger than upstream mobility, and this asymmetry persists even after accounting for the type of trading partner. This finding suggests that there is an inherent difference in the upstream and downstream mobility of workers along the supply chain. This is underlined by the fact that S – B mobility is larger in our data than B – S mobility even in absence of trade. In the years in which no trade occurred between future of past trading partners, S – B mobility accounts for 4.4% of all B2B mobility, while B – S mobility accounts for only 2.8%. Thus, even in the absence of trade, suppliers experience a higher share of workers moving to buyers than buyers to suppliers.

Trade effect by worker skill. Next, we group workers by skill, and run our regressions for the high- (ISCO 1 – 3) and low-skilled (ISCO 4 – 9) workers separately. The framework discussed in Section 2 suggested that trade should induce more high skilled

<sup>27</sup> As a robustness check, we also examined how alternative definitions of important partners (5%, 1% or 0.1% thresholds) affects the results. The higher the threshold, the more workers switch jobs along the supply chain (see Table 19 in Appendix).

**Table 6**  
**The Effect of Interfirm Trade on Worker Mobility by Partner Type**

|                         | Mobility            | Mobility             | Mobility            | Mobility            | Mobility             |
|-------------------------|---------------------|----------------------|---------------------|---------------------|----------------------|
| <b>S – B Mobility</b>   |                     |                      |                     |                     |                      |
| Trade                   | 0.010***<br>(0.001) | 0.010***<br>(0.001)  | 0.009***<br>(0.001) | 0.009***<br>(0.001) | 0.002**<br>(0.001)   |
| Important Partner       | 0.021***<br>(0.003) |                      |                     |                     | 0.019***<br>(0.001)  |
| High wage Partner       |                     | 0.010***<br>(0.001)  |                     |                     | 0.004*<br>(0.001)    |
| Superstar Partner       |                     |                      | 0.019***<br>(0.002) |                     | 0.015***<br>(0.002)  |
| Growing Partner         |                     |                      |                     | 0.012***<br>(0.001) | 0.011***<br>(0.001)  |
| Proportional Effect (%) | 148                 | 145                  | 155                 | 116                 | 395                  |
| Mean Mobility           | 0.021               | 0.017                | 0.018               | 0.024               | 0.013                |
| <b>B – S Mobility</b>   |                     |                      |                     |                     |                      |
| Trade                   | 0.005***<br>(0.001) | 0.009***<br>(0.001)  | 0.005***<br>(0.000) | 0.005***<br>(0.001) | 0.006***<br>(0.001)  |
| Important Partner       | 0.004***<br>(0.001) |                      |                     |                     | 0.008***<br>(0.002)  |
| High Wage Partner       |                     | -0.004***<br>(0.001) |                     |                     | -0.006***<br>(0.001) |
| Superstar Partner       |                     |                      | 0.006***<br>(0.001) |                     | 0.007***<br>(0.001)  |
| Growing Partner         |                     |                      |                     | 0.006***<br>(0.001) | 0.005<br>(0.001)     |
| Proportional Effect (%) | 81                  | 48                   | 110                 | 100                 | 203                  |
| Mean Mobility           | 0.011               | 0.009                | 0.010               | 0.011               | 0.010                |

Notes: N = 6,831,519. Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Mean mobility = average unconditional mobility in the years when Trade = 0. Proportional effect = estimated coefficient divided by mean mobility (%). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

mobility, because their skills are harder to reveal. This is not the case in our data: relative to their own unconditional baseline mobility low- and high-skilled workers' mobility increases by a similar proportion when their employers trade. Table 7 shows S – B mobility increases by 94% and 105% for low- and high-skilled workers, while B – S mobility by 62% and 66%.<sup>28</sup>

Trade effects by industry. We further investigate whether the proportional effects vary across sectors. To this end, we re-estimate the baseline specification (Table 4) conditional on both buyer and supplier industries. We report only those proportional effects for which the estimated coefficient is statistically significant and the average annual labor flow between 2015 and 2022 is at least 20.

In Table 8, the number in the cells indicate proportional trade changes from one industry to the other.<sup>29</sup> In the SB direction, the largest number of significant coefficients is observed in suppliers (who are the previous employers in this case) from man-

<sup>28</sup> We also ran the heterogeneity analysis for low- and high-skilled workers separately, and the results confirm that their mobility changes in by similar magnitudes (see Appendix Tables 20 and 21).

**Table 7**  
**Effect of Interfirm Trade on Worker Mobility: Low- and High-Skilled Workers**

|                         | Mobility S – B      |                     | Mobility B – S      |                     |
|-------------------------|---------------------|---------------------|---------------------|---------------------|
|                         | High Skilled        | Low Skilled         | High Skilled        | Low Skilled         |
| Trade                   | 0.003***<br>(0.000) | 0.009***<br>(0.000) | 0.002***<br>(0.000) | 0.004***<br>(0.000) |
| Proportional Effect (%) | 94                  | 105                 | 62                  | 66                  |
| Mean Mobility           | 0.003               | 0.008               | 0.003               | 0.006               |

Notes: N = 6,793,752. Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Mean mobility = average unconditional mobility in the regression sample when Trade = 0. Proportional effect = estimated coefficient divided by mean mobility (%). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

ufacturing (C), construction (F), and wholesale and retail trade (G), with 6, 4, and 4 cases, respectively. From the perspective of current employers (buyers), wholesale and retail trade (G), professional, scientific and technical activities (M), and manufacturing (C) exhibit the highest counts of significant coefficients, with 5, 5, and 4 cases, respectively.

**Table 8**  
**Proportional Effects for SB Direction by Sectors**

|                   |   | Buyer industry |   |     |     |     |     |     |     |     |     |     |     |   |
|-------------------|---|----------------|---|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|---|
|                   |   | A              | B | C   | D   | E   | F   | G   | H   | I   | J   | L   | M   | R |
| Supplier industry | A | 1.9            |   | 2.8 |     |     |     |     |     |     |     |     |     |   |
|                   | B |                |   |     |     |     |     |     |     |     |     |     |     |   |
|                   | C |                |   | 0.7 |     |     |     | 1.0 | 0.8 |     | 0.7 | 2.3 | 1.2 |   |
|                   | D |                |   |     |     |     |     |     |     |     |     |     |     |   |
|                   | E |                |   |     |     | 2.6 |     |     |     |     |     |     |     |   |
|                   | F |                |   | 1.2 | 1.4 |     | 0.9 | 1.1 |     |     |     |     |     |   |
|                   | G |                |   |     |     |     | 1.7 | 1.1 |     |     | 1.0 |     | 0.9 |   |
|                   | H |                |   |     |     |     | 1.3 |     | 0.8 |     |     |     | 1.8 |   |
|                   | I |                |   |     |     |     |     |     |     | 2.0 |     |     |     |   |
|                   | J |                |   |     |     |     |     | 1.3 |     |     | 0.7 |     | 0.9 |   |
|                   | L |                |   |     |     |     |     | 2.0 |     |     |     |     |     |   |
|                   | M |                |   | 0.8 |     |     |     |     |     |     |     |     | 0.5 |   |
|                   | R |                |   |     |     |     |     |     |     |     |     |     |     | 1 |

Notes: Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Proportional effect = estimated coefficient divided by mean mobility (%). Only those cells are reported for which the estimated coefficient is statistically significant and the annual average labor flow between the given sectors is at least 20 employees.

In the BS direction (Table 9), considering supplier industries (which correspond to current employers in this case), the highest number of significant coefficients again occur in wholesale and retail trade (G) and manufacturing (C), with 5 and 4 cases, respectively. From the perspective of previous employers (buyers in the BS direction), manufacturing (C), wholesale and retail trade (G), and professional, scientific and technical activities (M) each display the largest number of significant coefficients, with 3 cases per sector.

<sup>29</sup> For example, cell (1,3) indicates that trade increases worker flows from agriculture (supplier and previous employer) to manufacturing (buyer and current employer) by 280%

**Table 9**  
**Proportional Effects for BS Direction by Sectors**

|                   |   | Buyer industry |   |     |   |     |     |     |     |   |     |   |     |   |
|-------------------|---|----------------|---|-----|---|-----|-----|-----|-----|---|-----|---|-----|---|
|                   |   | A              | B | C   | D | E   | F   | G   | H   | I | J   | L | M   | R |
| Supplier industry | A | 1.5            |   | 1.0 |   |     |     | 0.9 |     |   |     |   |     |   |
|                   | B |                |   |     |   |     |     |     |     |   |     |   |     |   |
|                   | C |                |   | 0.3 |   |     |     | 0.8 | 1.1 |   |     |   | 0.9 |   |
|                   | D |                |   |     |   |     |     |     |     |   |     |   |     |   |
|                   | E |                |   |     |   | 1.4 |     |     |     |   |     |   |     |   |
|                   | F |                |   |     |   |     |     |     |     |   |     |   |     |   |
|                   | G | 1.4            |   | 0.8 |   |     | 1.7 | 0.5 |     |   |     |   | 1.0 |   |
|                   | H |                |   |     |   |     |     |     | 0.7 |   |     |   |     |   |
|                   | I |                |   |     |   |     |     |     |     |   |     |   |     |   |
|                   | J |                |   |     |   |     |     |     |     |   | 0.8 |   | 0.7 |   |
|                   | L |                |   |     |   |     |     |     |     |   |     |   |     |   |
|                   | M |                |   |     |   |     |     |     |     |   |     |   |     |   |
|                   | R |                |   |     |   |     |     |     |     |   |     |   |     |   |

Notes: Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Proportional effect = estimated coefficient divided by mean mobility (%). Only those cells are reported for which the estimated coefficient is statistically significant and the annual average labor flow between the given sectors is at least 20 employees.

Comparing the SB and BS directions reveals several notable differences. The SB matrix contains 28 significant elements, whereas the BS matrix contains only 16. This finding is consistent with our results suggesting stronger mobility effects in the SB direction. Moreover, the SB direction exhibits not only more frequent but also generally larger proportional effects than the BS direction, implying that supplier-to-buyer trade relationships are more strongly associated with labor mobility. The proportional effect is larger in the SB direction in 24 cases and larger in the BS direction in only 7 cases.<sup>30</sup>

Manufacturing-related linkages are particularly prominent in both directions, highlighting the central role of manufacturing in production networks and inter-firm labor mobility. Overall, the matrices are relatively sparse: most sector pairs are either insignificant or associated with low labor flows, while only a limited subset exhibits strong effects. Significant coefficients tend to cluster among economically interconnected sectors, especially manufacturing, wholesale and retail trade, and professional and technical activities. This suggests the importance of production complementarities and transferable firm-specific knowledge. Notably, cross-sector effects often exceed within-sector effects (i.e., the diagonal elements of the matrices), indicating that trade relationships may facilitate labor mobility across technologically or commercially related industries rather than solely within narrowly defined sectors.

The average number of inter-firm transactions across sectors (Table 10) further supports the interpretation that labor mobility follows production-network linkages. The transaction matrix shows that the most intensive trade relationships are concentrated among manufacturing (C), wholesale and retail trade (G), construction (F), transportation and storage (H), and professional and business services (M). These sectors also exhibit the largest and most frequent proportional labor-flow effects. In particular, manufacturing and wholesale and retail trade display especially strong transaction linkages, reflecting high levels of inter-firm interaction.

The overlap between dense transaction networks and significant labor-flow effects suggests that worker mobility is not randomly distributed across industries but instead follows economically meaningful production structures. Sectors with more intensive buyer–supplier interactions may enhance information flows, skill transferability, and the accumulation of relationship-specific knowledge, thereby increasing the likelihood of worker mobility between trade-connected firms.

<sup>30</sup> This comparison is restricted to sector pairs with significant effects and sufficient labor flows. If the effect is not significant or the labour flow is below 20 we consider as the proportional effect is 0.

**Table 10**  
**Average Number of Transactions Between Sectors**

|                   |   | Buyer industry |     |       |     |     |      |       |      |      |      |      |      |     |
|-------------------|---|----------------|-----|-------|-----|-----|------|-------|------|------|------|------|------|-----|
|                   |   | A              | B   | C     | D   | E   | F    | G     | H    | I    | J    | L    | M    | R   |
| Supplier industry | A | 1254           | 7   | 875   | 36  | 17  | 92   | 1040  | 66   | 28   | 9    | 29   | 48   | 17  |
|                   | B | 8              | 20  | 98    | 4   | 10  | 149  | 102   | 66   | 0    | 0    | 8    | 15   | 1   |
|                   | C | 1430           | 70  | 10520 | 236 | 317 | 3063 | 10005 | 864  | 617  | 281  | 479  | 827  | 91  |
|                   | D | 557            | 29  | 2052  | 277 | 113 | 200  | 729   | 154  | 403  | 73   | 491  | 121  | 111 |
|                   | E | 111            | 2   | 583   | 46  | 383 | 185  | 237   | 85   | 87   | 27   | 100  | 63   | 29  |
|                   | F | 265            | 36  | 1970  | 197 | 220 | 4277 | 1301  | 369  | 202  | 179  | 700  | 601  | 94  |
|                   | G | 3814           | 139 | 14173 | 343 | 685 | 7896 | 27099 | 2912 | 3214 | 1103 | 1301 | 1838 | 271 |
|                   | H | 229            | 60  | 2593  | 56  | 157 | 637  | 3064  | 3250 | 66   | 104  | 101  | 201  | 31  |
|                   | I | 18             | 0   | 133   | 5   | 7   | 47   | 179   | 26   | 199  | 41   | 36   | 59   | 29  |
|                   | J | 47             | 4   | 929   | 139 | 85  | 215  | 1238  | 273  | 104  | 1671 | 132  | 883  | 88  |
|                   | L | 38             | 7   | 620   | 40  | 50  | 357  | 1289  | 171  | 218  | 259  | 294  | 326  | 61  |
|                   | M | 352            | 29  | 2499  | 255 | 193 | 1054 | 2218  | 363  | 225  | 859  | 434  | 2141 | 141 |
|                   | R | 9              | 2   | 90    | 13  | 9   | 83   | 152   | 28   | 42   | 90   | 25   | 76   | 172 |

Notes: The table shows the annual average number of transactions between 2015 and 2022. Only transactions with VAT content above HUF 1 million are included. Sectors K, N, O, P, Q, S, T, and U are excluded.

In the main specification and in all robustness checks we concluded that the SB direction is stronger than the BS. The following explanation is necessarily speculative, but it may help account for the observed asymmetry. Firms operating at different stages of the supply chain may have different tendencies to expand vertically in either the downstream (SB) or upstream (BS) direction. For example, firms located further upstream are often more likely to undertake upstream-oriented expansion. A mining company rarely enters automobile manufacturing, whereas an automobile manufacturer may acquire mining operations. Conversely, firms located further downstream may be more inclined towards downstream expansion. An online retailer is unlikely to begin assembling final products, whereas a manufacturer of final goods may choose to establish its own online retail platform.

If the sectoral composition of the economy is such that one type of vertical expansion is more prevalent than the other, this may also generate asymmetries in labour flows between suppliers and buyers. A rigorous empirical investigation of this mechanism is beyond the scope of the present study. Moreover, such an analysis would ideally require product-level data, which are not available in our dataset.

**Wage effects.** Our last inquiry regards the wage change of workers who move along the supply chain. As information exchange between trade partners may help learning about the qualities future employees, we expect that wage changes would be higher of movers along the supply chain. To test this hypothesis, we restrict the sample to firm dyads that exchange workers both before and after the formation or termination of a trade relationship, thereby allowing us to compare wage changes before and after the treatment. Using this subsample, we estimate the same specification as before, but with the average wage change of movers between a given pair of firms as the dependent variable. Specifically, we measure the change between the worker's last wage at the previous employer and the first wage at the new employer and examine whether this differs in the presence of a trade relationship. As Table 11 shows, wages do not differ systematically with the presence of trade: the estimated coefficients are essentially zero.

The unconditional mean of the dependent variable is 1.1708 in the absence of a trade relationship between two firms and 1.1714 when the firms trade with each other.<sup>31</sup> These figures likewise indicate that, within this subsample, trade relationships are not associated with larger wage gains for workers moving between firm pairs. One possible explanation is that both firms and workers have already accumulated relationship-specific information prior to the establishment of a trade relationship.

<sup>31</sup> The dependent variable is the wage ratio between the worker's first wage at the new employer and the last wage at the previous employer. Accordingly, a value of 1.1708 implies that movers earn, on average, 17.08% more at their new employer than at their previous employer.

Firms may have learned about the quality of potential hires through earlier labour mobility, while prospective movers may have become better informed about working conditions, career opportunities, and wages at the partner firm through information shared by workers who had previously moved between the two firms. Consequently, the formation of the trade link does not substantially reduce information asymmetries further and therefore does not lead to additional wage gains.

**Table 11**  
**Wages of Workers Moving to Trading Partners**

|       | Mobility S – B   |                   |                  | Mobility B – S   |                  |                   |
|-------|------------------|-------------------|------------------|------------------|------------------|-------------------|
|       | All              | High Skilled      | Low Skilled      | All              | High Skilled     | Low Skilled       |
| Trade | 0.004<br>(0.007) | -0.001<br>(0.015) | 0.014<br>(0.009) | 0.001<br>(0.008) | 0.014<br>(0.016) | -0.004<br>(0.009) |
| N     | 16,964           | 4,882             | 11,256           | 14,834           | 4,003            | 9,749             |

Notes: Regression sample: dyads with worker mobility before and after the change in trading relation. The dependent variable is the ratio of the average wage in the new job to the average wage in the old job among workers who switch between the two given firms. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

This result is consistent with the findings of Komatsu and Dhyne (2023), who analyse Belgian data and find that workers who move to their employer's buyers or suppliers experience slightly lower wage gains than those who move to unaffiliated firms. They argue that employees can search for jobs at lower cost within the supply chain, reducing the wage premium associated with job changes and resulting in zero or even negative wage gains compared to those who find a job outside the trading network. In contrast, Cardoza et al. (2025) find that, in the Dominican Republic, workers who move along the supply chain experience wage gains that are 2.2 percentage points higher, four years after the job transition, than those of workers who move to unaffiliated firms. The difference between our findings and theirs may reflect the different time horizons considered: while our analysis focuses on the immediate wage change following a job transition, Cardoza et al. examine wage growth over a four-year period, during which the benefits of improved worker–firm matching may gradually materialise.

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## 6 Conclusions

This paper applied a difference-in-differences framework to dyadic supplier – buyer data to estimate how the formation and termination of interfirm trade relationships shape job-to-job mobility along domestic supply chains. By augmenting trading dyads with pre- and post-trade periods and constructing a matched control group of non-trading dyads, we are able to identify within-dyad changes in mobility that occur when firms start or stop trading.

The results show that interfirm trade relationships generate substantial increases in worker mobility, though the magnitudes obtained from the DiD regressions are considerably smaller than those produced by the configuration model approach. Whereas the configuration method yields odds ratios around six, our preferred within-dyad estimates imply increases in mobility of roughly 70–100 percent relative to unconditional baseline flows. These effects are economically meaningful but more conservative, reflecting the fact that the DiD strategy compares the same pair of firms in trading and non-trading years, thereby sweeping out persistent dyad-specific factors that inflate mobility gaps in cross-sectional comparisons.

A consistent feature of the results is the asymmetry of mobility along the supply chain: worker flows from suppliers to buyers rise more strongly than flows in the opposite direction. This pattern persists even after conditioning on partner characteristics, and even in periods without trade, suggesting that the asymmetry is at least partly inherent in the structure of upstream–downstream labor market linkages.

Partner characteristics also play an important role. Mobility responses are systematically larger when firms trade with important partners, with superstar firms, with growing firms, or with higher-wage partners. These relationships amplify worker flows beyond the average trade effect, reinforcing the idea that relational contracting and information exchange are more intensive, and therefore more consequential for labor mobility, within economically stronger or strategically central partnerships.

We do not find differences between low- and high-skilled workers in their responsiveness to trade links. Nor we find evidence that moving along supply chains generates systematically higher wage growth.

Taken together, these findings show that interfirm trade relationships reshape worker mobility patterns in meaningful and asymmetric ways, with partner characteristics strongly amplifying these effects.

Our findings demonstrate that labor reallocation across firms and industries is shaped not only by product demand, but also through the relational channels created by interfirm trade. Firms connected through supply chains exhibit substantially higher worker mobility, implying that buyer–supplier relationships reduce labor-market frictions by facilitating the exchange of information about workers and job opportunities. This suggests that policies aimed at strengthening production networks – such as supplier-development programs, cluster policies, or industrial coordination initiatives – may also affect labor-market matching and worker reallocation.

The results also point to important asymmetries within production networks. Worker mobility is consistently stronger from suppliers to buyers, particularly when the buyer is a high-wage, superstar, or rapidly growing firm. This suggests that dominant downstream firms may attract workers from their suppliers and thereby concentrate human capital within the production network. Such dynamics may reinforce productivity differences and bargaining asymmetries between large firms and smaller suppliers. From a policy perspective, this highlights that supply-chain relationships can influence the distribution of labor and capabilities across firms, which may matter for competition policy, SME support, and regional industrial development.

Although interfirm trade substantially increases worker mobility between trading partners, the aggregate magnitude of trade-induced mobility remains limited relative to overall job-to-job mobility. Also, while mobility is larger for downstream industries, we measure similar magnitudes – albeit smaller effects – for upstream industries. This results in a small net effect. Fears associated with the entry of large, multinational firms poaching workers from local SMEs may be well founded based on multinationals paying higher wages and thus generating stronger labor demand, but direct connections to such firm probably does more good to trading partners through productivity increases and higher levels of production, than harm through poaching workers.

Moreover, we do not find systematically higher wage gains for workers moving along supply chains. These findings suggest that production networks facilitate labor reallocation primarily through improved matching and information flows, rather than through aggressive labor poaching or disruptive competition for workers. As a result, stronger supply-chain integration may enhance labor-market connectivity without generating substantial instability for partner firms.

From the employees' perspective, transitions to trading partners may lead to better job matches. Investigating this mechanism precisely would require constructing an employee-level database and identifying job-to-job transitions that occur between trading partners. If workers who move to trading partners remain employed at their new firms for longer than comparable workers who move elsewhere, this would provide evidence of higher-quality matching. However, analysing match quality is beyond the scope of the present paper and is left for future research.

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# Additional tables and figures

| Table 12                 |          |         |
|--------------------------|----------|---------|
| Balance Table for Sector |          |         |
|                          | Original | Matched |
| A                        | 4847     | 4847    |
| B                        | 169      | 169     |
| C                        | 12884    | 12884   |
| D                        | 309      | 309     |
| E                        | 705      | 705     |
| F                        | 14342    | 14342   |
| G                        | 30229    | 30229   |
| H                        | 5469     | 5469    |
| I                        | 6096     | 6096    |
| J                        | 3667     | 3667    |
| L                        | 4669     | 4669    |
| M                        | 7838     | 7838    |
| N                        | 3924     | 3924    |
| R                        | 1301     | 1301    |
| S                        | 1161     | 1161    |
| Total                    | 97610    | 97610   |

Notes: Sector was one of the characteristics used in the matching procedure.

**Table 13****Balance Table for Firm Headquarters (County)**

|                        | Original | Matched |
|------------------------|----------|---------|
| Budapest               | 31180    | 31180   |
| Baranya                | 2889     | 2889    |
| Bács-Kiskun            | 4900     | 4900    |
| Békés                  | 2088     | 2088    |
| Borsod-Abaúj-Zemplén   | 3893     | 3893    |
| Csongrád-Csanád        | 3578     | 3578    |
| Fejér                  | 3581     | 3581    |
| Győr-Moson-Sopron      | 4336     | 4336    |
| Hajdú-Bihar            | 4443     | 4443    |
| Heves                  | 1945     | 1945    |
| Komárom-Esztergom      | 2726     | 2726    |
| Nógrád                 | 929      | 929     |
| Pest                   | 14352    | 14352   |
| Somogy                 | 2203     | 2203    |
| Szabolcs-Szatmár-Bereg | 3540     | 3540    |
| Jász-Nagykun-Szolnok   | 2315     | 2315    |
| Tolna                  | 1579     | 1579    |
| Vas                    | 1931     | 1931    |
| Veszprém               | 2920     | 2920    |
| Zala                   | 2282     | 2282    |
| Total                  | 97610    | 97610   |

Notes: County (megye) was one of the characteristics used in the matching procedure.

**Table 14****Balance Table for Firm Size**

|        | Original | Matched |
|--------|----------|---------|
| micro  | 69.0     | 70.1    |
| small  | 24.2     | 23.0    |
| middle | 5.4      | 5.6     |
| large  | 1.3      | 1.4     |
| Total  | 100.0    | 100.0   |

Notes: Firm size was one of the characteristics used in the matching procedure.

**Table 15**  
**Balance Table for Other Variables**

|               | Original | Matched | Standardized<br>Difference |
|---------------|----------|---------|----------------------------|
| Employment    | 19.2     | 21.3    | -0.006                     |
| Sales         | 782.8    | 832.5   | -0.003                     |
| Foreign share | 0.08     | 0.08    | -0.006                     |
| Average wage  | 175.8    | 195.2   | -0.079                     |
| N             | 97,610   | 97,610  |                            |

Notes: Standardized Difference:  $SD = \frac{\bar{X}_{orig} - \bar{X}_{matched}}{\sqrt{SD_{orig}^2 + SD_{matched}^2}}$ .

**Table 16**  
**Effect of Interfirm Trade on Worker Mobility: Supplier-Year and Buyer-Year Fixed Effects**

|                                    | (1)<br>Mobility<br>Total | (2)<br>Mobility<br>Supplier - Buyer | (3)<br>Mobility<br>Buyer - Supplier |
|------------------------------------|--------------------------|-------------------------------------|-------------------------------------|
| Supplier – Year FE                 |                          |                                     |                                     |
| Trade                              | 0.043***<br>(0.001)      | 0.026***<br>(0.001)                 | 0.017***<br>(0.001)                 |
| Proportional Effect (%)            | 192                      | 203                                 | 178                                 |
| Buyer – Year FE                    |                          |                                     |                                     |
| Trade                              | 0.039***<br>(0.001)      | 0.022***<br>(0.001)                 | 0.017***<br>(0.001)                 |
| Proportional Effect (%)            | 175                      | 172                                 | 179                                 |
| Supplier – Year + Buyer – Year FE) |                          |                                     |                                     |
| Trade                              | 0.040***<br>(0.001)      | 0.023***<br>(0.001)                 | 0.017***<br>(0.001)                 |
| Proportional Effect (%)            | 181                      | 182                                 | 179                                 |
| Mean Mobility                      | 0.022                    | 0.013                               | 0.009                               |

Notes: N = 6,842,757 (supplier-year FE), 6,705,281 (buyer-year FE), and 6,660,899 (supplier-year + buyer-year FE). The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. All regressions control for industry-year fixed effects. Regressions in the first row control for supplier-year fixed effects, those in the second row for buyer-year fixed effects, and those in the third row for supplier-year and buyer-year fixed effects. Standard errors are clustered at the dyad level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 17**  
**Proportion of Partners That Are of Multiple Types**

|                   | Important Partner | High Wage Partner | Superstar Partner | Growing Partner |
|-------------------|-------------------|-------------------|-------------------|-----------------|
| <b>SUPPLIERS</b>  |                   |                   |                   |                 |
| Important Partner | –                 | 0.04              | 0.22              | 0.40            |
| High Wage Partner | 0.16              | –                 | 0.28              | 0.40            |
| Superstar Partner | 0.05              | 0.29              | –                 | 0.38            |
| Growing Partner   | 0.07              | 0.22              | 0.30              | –               |
| <b>BUYERS</b>     |                   |                   |                   |                 |
| Important Partner | –                 | 0.05              | 0.29              | 0.40            |
| High Wage Partner | 0.24              | –                 | 0.31              | 0.43            |
| Superstar Partner | 0.06              | 0.22              | –                 | 0.38            |
| Growing Partner   | 0.06              | 0.33              | 0.27              | –               |

Notes: N = 3,843,122. The table shows the proportion of partners that belong to two types, relative to the number of partnerships of the type indicated in the corresponding row.

**Table 18**  
**Effect of Interfirm Trade on Worker Mobility: PPML Estimations**

|       | Mobility Total      | Mobility Supplier – Buyer | Mobility Buyer – Supplier |
|-------|---------------------|---------------------------|---------------------------|
| Trade | 0.509***<br>(0.020) | 0.611***<br>(0.027)       | 0.388***<br>(0.026)       |
| N     | 248,349             | 157,707                   | 152,825                   |

Notes: The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry–year and dyad fixed effects. Standard errors are clustered at the dyad level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 19**  
**Effect of Important Partners on Mobility: Alternative Thresholds**

|                          | Mobility            | Mobility            | Mobility            | Mobility            |
|--------------------------|---------------------|---------------------|---------------------|---------------------|
| <b>MOBILITY S – B</b>    |                     |                     |                     |                     |
| Trade                    | 0.010***<br>(0.001) | 0.010***<br>(0.001) | 0.008***<br>(0.001) | 0.007***<br>(0.001) |
| Important partner (10%)  | 0.021***<br>(0.003) |                     |                     |                     |
| Important partner (5%)   |                     | 0.015***<br>(0.002) |                     |                     |
| Important partner (1%)   |                     |                     | 0.011***<br>(0.001) |                     |
| Important partner (0.1%) |                     |                     |                     | 0.008***<br>(0.001) |
| Proportional effect      | 1.48                | 1.31                | 0.79                | 0.59                |
| Mean Mobility            | 0.021               | 0.019               | 0.024               | 0.022               |
| <b>MOBILITY B – S</b>    |                     |                     |                     |                     |
| Trade                    | 0.006***<br>(0.000) | 0.005***<br>(0.000) | 0.005***<br>(0.001) | 0.005***<br>(0.001) |
| Important partner (10%)  | 0.007***<br>(0.002) |                     |                     |                     |
| Important partner (5%)   |                     | 0.006***<br>(0.001) |                     |                     |
| Important partner (1%)   |                     |                     | 0.003***<br>(0.001) |                     |
| Important partner (0.1%) |                     |                     |                     | 0.002***<br>(0.001) |
| Proportional effect      | 0.93                | 0.82                | 0.60                | 0.51                |
| Mean Mobility            | 0.013               | 0.013               | 0.013               | 0.013               |

Notes: N = 6,831,519. Important partner dummies = 1 if the sales of the supplier to a given buyer or the intermediate input purchases of a buyer from a given supplier account for at least 10%, 5%, 1%, or 0.1% of its total sales. Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Mean mobility = average unconditional mobility in the regression sample. Proportional effect = estimated coefficient divided by mean mobility (%). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 20**  
**The Effect of Interfirm Trade on Worker Mobility by Partner Type:**  
**High and Low Skilled Workers (S--B Mobility)**

| Panel A: High Skilled |                     |                     |                     |                     |
|-----------------------|---------------------|---------------------|---------------------|---------------------|
| Trade                 | 0.002***<br>(0.000) | 0.002***<br>(0.000) | 0.003***<br>(0.000) | 0.002***<br>(0.000) |
| Important Partner     | 0.004***<br>(0.001) |                     |                     |                     |
| High Wage Partner     |                     | 0.002***<br>(0.000) |                     |                     |
| Superstar Partner     |                     |                     | 0.002***<br>(0.001) |                     |
| Growing Partner       |                     |                     |                     | 0.002***<br>(0.000) |
| Proportional Effect   | 215                 | 132                 | 151                 | 146                 |
| Panel B: Low Skilled  |                     |                     |                     |                     |
| Trade                 | 0.007***<br>(0.000) | 0.007***<br>(0.001) | 0.006***<br>(0.000) | 0.006***<br>(0.001) |
| Important Partner     | 0.014***<br>(0.002) |                     |                     |                     |
| High Wage Partner     |                     | 0.004***<br>(0.001) |                     |                     |
| Superstar Partner     |                     |                     | 0.012***<br>(0.001) |                     |
| Growing Partner       |                     |                     |                     | 0.008***<br>(0.001) |
| Proportional Effect   | 257                 | 130                 | 221                 | 171                 |

Notes: N = 6,831,519. Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Mean mobility = average unconditional mobility in the regression sample. Proportional effect = estimated coefficient divided by mean mobility (%). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 21**  
**The Effect of Interfirm Trade on Worker Mobility by Partner Type:**  
**High and Low Skilled Workers (B→S Mobility)**

| Panel A: High Skilled |                     |                      |                     |                     |
|-----------------------|---------------------|----------------------|---------------------|---------------------|
| Trade                 | 0.002***<br>(0.000) | 0.002***<br>(0.000)  | 0.002***<br>(0.000) | 0.001***<br>(0.000) |
| Important Partner     | 0.001*<br>(0.000)   |                      |                     |                     |
| High Wage Partner     |                     | -0.000<br>(0.000)    |                     |                     |
| Superstar Partner     |                     |                      | 0.001<br>(0.001)    |                     |
| Growing Partner       |                     |                      |                     | 0.001***<br>(0.000) |
| Proportional Effect   | 104                 | 0.56                 | 0.80                | 0.90                |
| Panel B: Low Skilled  |                     |                      |                     |                     |
| Trade                 | 0.003***<br>(0.000) | 0.005***<br>(0.000)  | 0.003***<br>(0.000) | 0.001***<br>(0.000) |
| Important Partner     | 0.004***<br>(0.001) |                      |                     |                     |
| High Wage Partner     |                     | -0.002***<br>(0.001) |                     |                     |
| Superstar Partner     |                     |                      | 0.003***<br>(0.001) |                     |
| Growing Partner       |                     |                      |                     | 0.004***<br>(0.001) |
| Proportional Effect   | 135                 | 0.52                 | 107                 | 116                 |

Notes: N = 6,831,519. Regression sample: dyads where the trade connection changed at most once (from trading to non-trading or vice versa), augmented with the matched sample. The dependent variable is the number of workers changing jobs between a pair of firms in a given year. Trade = 1 if there is a trade relation between two firms in a given year, 0 otherwise. The regression controls for industry-year and dyad fixed effects. Standard errors are clustered at the dyad level. Mean mobility = average unconditional mobility in the regression sample. Proportional effect = estimated coefficient divided by mean mobility (%). \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.





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