Virág Ilyés

Social Networks and Individual Labor Market Outcomes

Evidence from Linked Employer-Employee Panel Data

Doctoral School of Sociology and Communication Science

Sociology Doctoral Program

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Abbreviations

AKM	**A**bowd, **K**ramarz and **M**argolis (1999)
CEE	**C**entral and **E**astern **E**urope
FE	**F**ixed **E**ffects
HE	**H**igher **E**ducation
LEEP	**L**inked **E**mployer-**E**mployee **P**anel Data
LPM	**L**inear **P**robability **M**odel
SE	**S**tandard **E**rror
TWFE	**T**wo-**W**ay **F**ixed **E**ffects
OLS	**O**rdinary **L**east **S**quares
OR	**O**dds **R**atio

1. Introduction

The role of social networks in labor markets has been widely recognized for decades. Social ties can be essential assets in reducing information and search costs and have proven to be useful intermediaries in matching supply and demand. They can positively affect individual labor market outcomes: they may increase the chance of finding suitable vacancies (Dustmann *et al.*, 2016), enhance the quality of the newly acquired jobs or promote career advancement (Podolny and Baron, 1997; Lutter, 2015). At the same time, they might influence firm-level outcomes as well: informal ties can facilitate knowledge sharing within teams (Wei, Zheng and Zhang, 2011; Tortoriello, Reagans and McEvily, 2012), accelerate the creation and diffusion of innovation (Rost, 2011), enhance the productivity within teams (Fernandez, Castilla and Moore, 2000; Afridi *et al.*, 2020), or even contribute to firm-level (Boschma, Eriksson and Lindgren, 2008) and regional productivity growth (Lengyel and Eriksson, 2016; Eriksson and Lengyel, 2019; Lengyel *et al.*, 2021; Lőrincz, 2021).

The dissertation focuses primarily on the perspective of individuals and aims to give a deeper understanding of how social networks shape the economic and employment opportunities of individuals. It investigates the direct and indirect effects of professional ties on a wide range of labor market outcomes, such as job finding probabilities, the level of wages, job stability and the chances for upward mobility in terms of employment outcomes. Following recent trends in the measurement of network effects, the empirical work takes a relatively novel approach to quantifying the economic benefits of social ties by exploiting the potential of a large Hungarian linked administrative employer-employee panel dataset. Although direct information on social ties is not available in the dataset, the availability of anonymous firm and individual identifiers makes it possible to identify various segments of networks, such as former co-workers and university peers. Building on studies using similar data (Dustmann *et al.*, 2016; Eliason *et al.*, 2019), elaborating on the empirical approaches already applied for measuring network-related effects (Kramarz and Skans, 2014; Glitz and Vejlin, 2021; Saygin, Weber and Weynandt, 2021) and incorporating recent developments in panel data methods (Woodcock, 2008; Cardoso, Guimarães and Portugal, 2016), this dissertation presents 3 studies on the role of social ties in the labor market.

We begin with assessing sources of wage gains attributable to former co-workers and their magnitude. Studies using administrative data and different network segments indicated that social ties may channel individuals into high-paying firms (Schmutte, 2015), increase the selections of good quality workers into firms (Hensvik and Skans, 2016) and can contribute to the creation of better employer–employee matches through referrals (Dustmann *et al.*, 2016). Drawing on this research and by focusing on professional relationships (whose help seems the most vital in the labor market), we investigate jointly the presence of all the above channels and quantify their relative importance with respect to wage benefits. By augmenting and applying the decomposition method proposed by Woodcock (2008), we show that there are non-negligible differences in all empirically separable wage elements, namely, in the individual-specific, firm-specific, and match-specific components as well. Regarding the first two, we also reveal that they are mainly driven by their respective within-firm and within-person components. Individuals who start at their co-workers' workplace get access to higher premium firms compared to where they usually work, and firms can increase the quality of their worker pool with referral hires. This way, the research highlights the beneficial role of former co-workers for both individuals and firms, and in general the society, by enhancing the creation of better employer-employer matches. The results also shed light on gender differences in the magnitude of wage gains and the importance of particular selection channels. They revealed that investigating the heterogeneity of network effects by gender is an essential area worth exploring, even beyond wage outcomes.

Therefore, in Chapter 4, as a continuation of the previous study, we assess the heterogeneity of co-worker effects by gender on job search opportunities and career development through inter-firm mobility. Although considerable research has been devoted uncovering the gendered effect of social ties on job finding (Moore, 1990; Ibarra, 1993; Greguletz, Diehl and Kreutzer, 2019; Blommaert *et al.*, 2020), the results of this study can still provide interesting and relevant findings. Namely, that (1) in line with the theoretical literature (Lin, 1999) men benefit more from the help of former co-workers, (2) contrary to intuitive expectations and some empirical indications (such as Brown, Setren and Topa, 2016), gender homophily in network effects is only present due to already established gender segregation patterns, and (3) the hiring benefits of women are mostly driven by the help of those contacts higher up in the occupational ladder. To investigate the differential role of contacts on career improvement by gender, in the second part of the analysis we focus on the cases of inter-firm mobility. The results unveil that while informal ties are coupled with increased chances for career advancement (measured by better position in the overall or within-firm employee wage distribution, or by higher firm premia), the benefits are unevenly distributed both across and within genders. In general, the returns to social ties are greater for men, who tend to realize meaningful benefits regardless of their prior job and firm quality. On the contrary, among women, only those with average or worse labor market positions receive such gains. The results reflect a duality in network effects: besides enhancing the (otherwise) limited opportunities of women in worse positions, social ties contribute to the preservation of existing gender differences at the top segments of the labor market. By demonstrating the role of social relations in the reproduction and maintenance of the glass ceiling phenomena, and by presenting solid proofs on the inequality aspects of networks in general, the research promotes discussions about the less traditional sources of gender inequalities in the labor market.

Finally, in Chapter 5, we focus on another specific sub-group of the labor market for whom the utilization of social ties can be crucial, namely university graduates starting their careers. Although career tracking surveys from a wide range of countries revealed that social ties can shape the economic opportunities for university graduates (Bartus, 2001; Franzen, 2006; Kogan, 2011; Kogan, Matković and Gebel, 2013), information on the role of different network segments is in modest supply. This study aims to partly fill this gap by focusing on one particular, professionally relevant subgroup of the graduates' social network, most notably former university fellows. We investigate the role of such ties on two types of labor market outcomes for master's graduates: job finding chances and post-hiring outcomes (such as wages, job stability, the prestige and status of the acquired jobs). Our results provide evidence that former university ties can be essential sources of job opportunities and informal help: their presence at given firms meaningfully increase the hiring chances of the individuals at these companies. However, the measured benefits are primarily attributable to those ties from bachelor's studies, while the effect of master's peers is mostly driven by the selection of individuals alongside prevalent study track-firm pathways. The results contribute to the discussions about the role of tie similarity, by showing that the overly similar education and labor market paths of peers, early in their careers, may limit their chances of helping each other. In addition, we also show that those who start working at their peers' firms tend to earn higher wages, acquire jobs with better prestige and status, and have longer employment spells. However, part of these gains can be attributed primarily to the selection of linked graduates to firms that provide increased benefits to career entrants in general. The study, thus, contributes to the literature as it provides useful insights on the role of professional relationships for early career workers, addresses the issues related to causality, and reflects on the role of tie similarity as an important determinant of contact effects.

The results of the studies, taken together, advance our understanding on the relationship between networks and individual economic opportunities and provide essential insights for the disciplines of sociology, economics and social policy as well. One the one hand, we draw attention to the importance of professional ties as essential intermediaries facilitating and directing worker mobility. Such mobility is typically associated with desirable benefits for both individuals and firms, and in general may enhance the overall productivity in the labor market as well. These effects can be considered vital from an economics point of view. On the other hand, the dissertation also provides indications on the downside of networks. The obtainable benefits depend strongly on the available social ties and their characteristics. Based on Chapter 3 and 4, the gender and occupation of contacts can be such traits, but as Chapter 5 shows the similarity of educational and career paths might also have a strong effect on the amount of help received – and it is not necessarily positive. The lack of good quality social networks, restricted access to useful social ties can be detrimental for future career prospects of individuals and may even result in gaps in opportunities between different groups of the society. The traces of such inequalities are reflected in the findings of Chapter 4. In this way, the thesis draws attention to the fact that assessing the direct and indirect effects of social ties on maintaining or even increasing labor market inequalities is essential to fully understand the impact of social relations on the labor market.

The dissertation can also be seen as a good complement to existing international research, as it presents the first estimates from the CEE countries on the labor market role of different kind of professional ties by using administrative data. By demonstrating that contact effects are present in a former state-socialist country, such as Hungary as well, the research also strengthens the idea that the function and relevance of social ties on the labor market is universal across different countries, despite of structural differences.

After this introduction, the thesis is structured as follows. Chapter 2 introduces shortly the wider context of the research and reflects on the prevailing methodological dilemmas in the field. In Chapter 3, we address the presence, magnitude, and composition of wage gains related to former co-workers and discuss the mechanisms that could explain their existence. Then, in Chapter 4 we assess the heterogeneous labor market returns to former co-workers by gender, in terms of job finding chances and career improvement. In Chapter 5, we examine the role of former university peers in structuring the labor market outcomes of university graduates at the beginning of their career. Finally, in Chapter 6, after outlining the main findings, I address the possible limitations of the research and close the discussion with suggestions for future research.

2. Background

This chapter provides an overview of the research on the role of social networks in the labor market, by incorporating core ideas from both sociology and economics. The first part of the chapter begins with the brief introduction of the main theories related to the utilization of informal ties in the labor market and the summary of the theorized benefits for workers and firms. It also introduces the factors that may explain the heterogeneity of benefits associated with networks. Then, the second part of the chapter gives a short overview of the empirical results based on survey data and presents recent advances relying on administrative LEEP datasets in the literature.

2.1 Theoretical background

2.1.1 The role of networks in matching workers to firms

Over the past decades, the role of social networks in the labor market has received increased attention by both sociologists and economists, and has been addressed by considerable theoretical and empirical research. Large and growing evidence suggests that social interactions play an essential role in matching workers to jobs, and that informal ties can help to mitigate labor market imperfections such as search and matching frictions and asymmetric information (Granovetter, 2019; Topa, 2019).

Irrespective of choosing the perspective of the firms or the individuals, when investigating the role of networks in facilitating matches between employers and workers we are essentially after the same social interactions: information transmission (directed from contacts to job seekers about jobs, flowing toward firms about job seekers or both) and referral (Montgomery, 1991; Afridi *et al.*, 2020; Vacchiano, 2021). According to Topa (2019) these can be considered two ends of a broad spectrum of social interactions, which might entail more or less involvement from our informal ties. Recommendation, either it happens informally, within the framework of a formal recommendation system or realized in a form of a letter, requires a higher amount of information flow between the referrers and the employers/hiring managers, and demands taking more responsibility from the referrers' side. In contrast, providing tips about open vacancies to promising candidates, or asking around in one's network about opportunities entails lower reputations costs and less involvement from the contacts' side. Despite the differences, according to the growing body of literature, both mechanisms are theorized to provide significant benefits for companies using informal hiring and individuals who utilize informal search.

2.1.2 Drivers of network-based hiring and job search

The typical motivations for utilizing informal networks in the labor market can be categorized into three main groups of explanations. The use of social ties can help in reducing job search and recruitment costs, it may provide information benefits for firms and job seekers, and can even bring on-the-job benefits as well.

By relying on their employee networks to disseminate information about job openings, firms can expand their pool of applicants without allocating extra monetary resources on advertising and on external recruitment (Rees, 1966; Rees and Shultz, 1970; Fernandez and Weinberg, 1997; Fernandez, Castilla and Moore, 2000). In addition to the reduction of recruitment costs, such networks can also play an important role in reducing the firms' screening costs during hiring events. On the one hand, incumbent employees can provide direct information about the job candidates they know. On the other hand, they can indirectly signal the unobserved productivity of their acquaintances with their own (Ullman, 1966; Miller and Rosenbaum, 1997; Munshi, 2003; Hensvik and Skans, 2016). Once hired, existing relationships between candidates and the referrers (or other employees) may provide additional benefits. According to the social enrichment theory, the existence of such relationships can enhance the entrants' sense of belonging and integration (Manwaring, 1984; Fernandez, Castilla and Moore, 2000; Castilla, 2005). Also, they can ensure smoother knowledge sharing in the workplace (Manwaring, 1984; Fernandez, Castilla and Moore, 2000), positively affect worker productivity and mitigate moral hazard problems (Kugler, 2003; Bandiera, Barankay and Rasul, 2009; Heath, 2018).

Job-seekers, by utilizing not only formal, but also informal search can accumulate a higher amount of job offers, providing a wider set of options to choose from.[1] By relying on informal contacts, they may get a more detailed picture on the actual content and requirements of given vacancies (Breaugh, 1981; Phillips, 1998; Zottoli and Wanous, 2000; Granovetter, 2019).[2] This way, by gaining more accurate and realistic previews on

[1] They can even receive information on jobs that were not announced formally.
[2] Based on the meta-analysis of (Schlachter and Pieper, 2019) the topic is addressed by multiple, conceptually similar/identical theories in the referral literature, including, but not limited to, the realistic information hypotheses (Breaugh, 1981), the realistic preview theory (Linnehan and Blau, 2003), the

given jobs (Rees, 1966; Ullman, 1966; Rees and Shultz, 1970; Wanous, 1980; Quaglieri, 1982; Breaugh and Mann, 1984) they can develop more reasonable job expectations (Schlachter and Pieper, 2019).

Based on the mechanisms described above, the quantifiable labor market gains of firms and individuals can be summarized as follows. From the side of the firms, informal hiring is expected to facilitate the quicker and wider spreading of information about open positions, contribute to the collection of more extensive and better candidate pools, and might entail better opportunities for finding the most appropriate workers for given vacancies. Also, such networks are expected to reduce worker turnover and enhance worker productivity. From the perspective of individuals, the mechanisms presented proposed to translate into higher job finding chances and better post-hiring outcomes. The use of social connections is expected to contribute to obtaining jobs with higher wages, greater authority, better status and prestige. Finally, informal intermediaries can even contribute to the creation of better employer-employee matches as well (Brown, Setren and Topa, 2016; Dustmann *et al.*, 2016; Matsuda and Nomura, 2017), which can be reflected in higher job stability and better wages. In the following empirical chapters, we focus on the labor market outcomes listed above, and aim to identify some of the underlying mechanisms responsible for individuals' potential gains.

2.1.3 Network-related determinants of individual labor market outcomes

The presence and magnitude of the individuals' economic returns, however, depend on a variety of factors. According to Ioannides and Loury (2004) the related sociological work is centered on three main strands of research focusing on employer heterogeneity, contact heterogeneity and relational heterogeneity in network effects. Besides reviewing these, the core notions on the role of individual heterogeneity will be also discussed.

Relational heterogeneity

A large body of empirical and theoretical work proposes that the magnitude of labor market returns to informal help is highly determined by the characteristics of social relationships. One of the central debates is related to the importance of tie strength, which can be defined as the combination of the frequency of interaction, the emotional intensity, and the degree of reciprocity and intimacy that characterize a relationship (Granovetter,

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differential information hypothesis (Williams, Labig and Stone, 1993), the better match theory (Fernandez, Castilla and Moore, 2000; Elliott, 2001) and the realistic expectations theory (Vecchio, 1995).

1973). According to Coleman (1988, 1990), greater investment in social ties ('bonding') and higher involvement in cohesive network structures can enhance the creation of social capital through closure mechanisms. Therefore, strong ties that are usually characterized by trust, shared norms and cooperation, can serve as a good basis for reciprocity in help. The use of such contacts are proposed to be associated with greater amount of direct help (such as referral) and with more personal and emotional involvement. On the other hand, another line of research posits that individuals benefit more from loosely connected network structures, either by utilizing weaker ('bridging') ties or by being in network positions that entails information control and brokerage benefits (Granovetter, 1973, 1983; Burt, 1992). Based on this work, weak ties are the best potential sources of novel, non-redundant information as they have access to different communities of people. Such information benefits may be translated into additional gains on the labor market, as the wider access to job opportunities and work-related information is generally associated with better chances of finding more fitting jobs and higher quality employment.

The similarity of contacts can be another relational feature that may have a significant impact on the labor market benefits by networks. Homophily between acquaintances in terms of professional interest, labor market history, skills and productivity may be associated with higher quality and more useful help (Montgomery, 1991). Homophilic relationships can provide information about a wider range of suitable jobs, give direct or indirect cues about the skills and productivity of acquaintances, and facilitate better employer-employee matches (Ullman, 1966; Miller and Rosenbaum, 1997; Munshi, 2003). However, the degree of similarity is also a crucial factor that can even negatively affect the chances of individuals helping each other. Too much professional similarity may actually lead to crowding out mechanisms, or to the lack of help due to peers trying to avoid competition within workplaces. Conversely, too much diversity may be associated with less information about the relevant jobs.

Based on the intersection of the two dimensions presented, we may propose that as the strength of ties increases, acquaintances will be more inclined to make greater efforts to help each other. However, their help can be most beneficial in the labor market if they share similarities to a reasonable extent. In Chapter 5 we provide some indications in favor of this assumption.

Contact, individual and employer heterogeneity

The characteristics of contacts are also key determinants of the presence and magnitude of labor market returns by social ties. One of the most influential theory is Lin's (1982, 1999) social resource theory. It describes the society as a well-structured system: individuals are embedded in a pyramidal-shape, hierarchical social structure, where positions are associated with varying amount of resources (in terms of wealth, status, and power). Higher positions in this hierarchy are more difficult to access, are typically associated with more resources and also provide a better view on the overall structure of the system (Lin, 1999). Thus, the position of individuals within this structure will be an important determinant of the structural constraints and opportunities and of the available social resources that the individuals have. According to Lin, individuals can improve on their outcomes through the mobilization of their social resources, most notably by performing instrumental actions. Actions, which aim to obtain initially not owned resources – for instance income, wealth, job and status attainment. However, since individuals are more likely to have relationships with similar others in terms of occupation and social position[3] (e.g. "strength of position hypothesis"),[4] they need to deliberately mobilize those type of their contacts who are endowed with and have access to more abundant resources or higher up in the social hierarchy (Ryan, 2011; Lin, 2017). Upward mobility in terms of income, status, prestige, therefore requires the help of dissimilar others, such as weaker ties, who are more likely to reach out vertically (Lin, 1999). To assess the implications of this theory, we will investigate whether those types of ties endowed with more abundant economic resources, status and power (even in terms of bargaining power) provide greater benefits to individuals.

The "strength of position hypothesis", by proposing that the individuals' social position affect the amount of their social resources, provides a potential explanation for the presence of individual-specific heterogeneity in terms of networks effects. As some social groups in the society are excluded from top tiers of occupations and are over-represented in less advantageous structural positions, they may have limited access to contacts with more abundant resources. The relative lack of such ties could lower the chances of individuals for receiving network-related instrumental benefits and acquiring better labor

[3] Or even other types of dimensions such as race, ethnicity or gender.
[4] Network homophily (McPherson, Smith-Lovin and Cook, 2001) also proposes the sorting of individuals alongside observable or unobservable characteristics or group memberships as a key mechanism of network building.

market outcomes (McGuire, 1999; Lalanne and Seabright, 2016). Building on these notions, in one of the following empirical studies we will focus on a factor that may be associated with lower quality network capital, namely gender. Due to the prevailing structural barriers of women in the labor markets and the presence of gender-specific network building strategies, women are more likely to lack influential work-related contacts (Trimble O'Connor, 2013; Blommaert *et al.*, 2020)**,** have stronger and more expressive ties (Moore, 1990; Ibarra, 1993) and lower network diversity. Also, they may be more disadvantaged by the limited amount of help (job offers and referral) received from their contacts and the lower willingness of their ties to help (Lindenlaub and Prummer, 2016; Beaman, Keleher and Magruder, 2018; Zeltzer, 2020). Thus, we expect that women realize less benefits by networks in terms of labor market outcomes.

Finally, heterogeneity in employer characteristics with respect to contact effects has also received attention in the sociological research. The related studies typically investigated firm-level and job-specific characteristics and proposed stylized facts that could motivate the existing differences in the use of referrals. According to such studies the use of informal hiring (mostly referrals) will be more prevalent in the high-tech industry, when flexible staffing strategy is applied, where informal ties are actively used in eliciting performance, and in occupations where the necessary skills are hardly observable or potential selection errors would be too costly (Marsden and Gorman, 2001). Accordingly, we will investigate whether firms where informal hiring is present are themselves high-premium firms and, if so, to what extent this characteristic of firms is responsible for the wage advantages of those who used informal contacts in their job search.

2.2 Empirical evidence

2.2.1 Survey-based methods

Using survey-based methods, several studies aimed to explore the causal relationship between the utilization of contacts and individual labor market outcomes. Such techniques are typically referred to in the literature as "single name" or "important matters" generators (Marsden, 1987; Marin, 2012), and unlike other methods (e.g. name, position and resource generators) they are designed to find the most important contacts in given situations (e.g. in job search).

By using such approach, initial research focused on the prevalence of using social ties during job search. Among others, Corcoran, Datcher and Duncan (1980), Granovetter

(2019), Holzer (1987, 1988), Myers and Shultz (1951), Rees and Shultz (1970) provided solid evidence on the widespread use of informal ties in job search. According to their results approximately 50% of the job seekers found their new jobs with the help of their contacts. Over the years, the scope of the investigated labor market outcomes has been expanded: many studies started to explore the effect of informal ties on wages, job stability and quality of the created employer-employee matches (for example Simon and Warner (1992), Coverdill (1998), Marmaros and Sacerdote (2002)). However, the acquired results were mixed, especially on match quality and wages. Some papers argued that social ties might increase entry wages (Holzer, 1988; Simon and Warner, 1992; Rosenbaum *et al.*, 1999; Bian, Huang and Zhang, 2015; Brown, Setren and Topa, 2016), while others could not detect such wage premium (Campbell and Rosenfeld, 1985; Bridges and Villemez, 1986; Marsden and Hurlbert, 1988; Antoninis, 2006; Loury, 2006) or even showed the negative effect of contacts on wages (Bentolila, Michelacci and Suarez, 2010; Greenberg and Fernandez, 2016; Goel and Lang, 2019). The variation in the results is reasonable since the research population, the collection of available covariates, and the contact type of interest (for instance family, friends, co-workers) were quite different in these papers. Also, there could be multiple, simultaneously existing underlying mechanisms in play with distinct potential outcomes, which might add up differently on the aggregate level.

Considerable research focused on the heterogeneity of effects by tie-related or individual characteristics as well. Regarding the former, many studies provided evidence on the beneficial effect of weak ties on job finding chances (Yakubovich, 2005; Granovetter, 2019), occupational status attainment (Lin, Ensel and Vaughn, 1981; Moroşanu, 2016) and migration decision (Dolfin and Genicot, 2010; Giulietti, Wahba and Zenou, 2018). With respect to wages, however, the related studies yielded mixed results (Bridges and Villemez, 1986; Marsden and Hurlbert, 1988; Montgomery, 1992).

Concerning individual characteristics, a number of papers have highlighted the role of (among others) gender, ethnicity or lower socioeconomic status as key determinants of the magnitude of network effects. As for women, the main findings suggest that they typically obtain fewer job opportunities through their networks compared to men (Corcoran, Datcher and Duncan, 1980), and that both the composition of their social networks (Moore, 1990; Marmaros and Sacerdote, 2002) and their chances of exploiting social connections is worse (Lalanne and Seabright, 2016; Lindenlaub and Prummer, 2016; Friebel *et al.*, 2017; Beaman, Keleher and Magruder, 2018). Regarding ethnicity, several studies have shown that members of ethnic minorities are more likely to be embedded in segregated networks (Braddock and McPartland, 1987), have lower chances for mobilizing those of their contacts endowed with more abundant resources (Smith, 2000) and more likely to rely on their contacts from the same ethnic background (Klinthäll and Urban, 2016). Furthermore, some studies even revealed that co-ethnic networks have essential role in preserving and increasing the segregation of individuals to lower wage jobs or occupations with low prestige (Falcón, 1995; Tegegne, 2015; Kim, 2018).

2.2.2 Shortcoming of survey-based methods

Survey-based methods have proved useful in examining how social networks affect individual labor market outcomes. However, they have some shortcomings. Such data gathering methods have a limited capacity to collect comprehensive information simultaneously on individual, firm-level, network-specific and tie-related characteristics. As only a limited number of questions can be asked in a survey, it is difficult to get a comprehensive picture of, for example, respondents' past work history or career development. Information obtainable on social contacts is typically also limited (e.g. we may know the gender of the contacts, but not their employment history or productivity). Besides, the use of retrospective survey questions could also introduce some memory bias (as the recalling accuracy of given events or details might be worse over time), which could affect the credibility of the recalled network-related and labor market information.[5] Also, higher non-response rate might be present when measuring labor market outcomes through survey-questions (e.g. wages), affecting the resulting estimates. Finally, when using cross-sectional survey data for explaining the labor market outcomes of individuals it is typically harder, if not even infeasible to capture individual heterogeneity in terms of unobserved skills and productivity. Such characteristics, however, could affect network quality, the general features of personal networks and individual outcomes as well. Therefore, their omission may lead to biased estimates for instance when measuring effects on wage outcomes. Panel data, in this sense, might provide some advantages, as it

[5] Such as which contacts provided tips on vacancies or what was the exact length of unemployment.

gives us the opportunity to indirectly control for unobserved, time-invariant characteristics by using various methods (for example fixed effects).

2.2.3 Analysis of linked employer-employee administrative data

In the last decades, many promising endeavors appeared in the field of economics which utilized either new research designs (such as experiments, for instance Pallais and Sands (2016)) or other type of data sources (firm-level hiring panel data, call center data) to overcome at least part of the proposed issues (Cappellari and Tatsiramos, 2015; Brown, Setren and Topa, 2016). The use of administrative linked employer-employee panel datasets presents another, viable option.

Administrative LEEP datasets offer great opportunities to understand individual outcomes, as they contain extensive information on both the individuals' labor market history and the characteristics of the observed employment spells. The available information typically covers wages, unemployment spells, employment status, occupation, and educational attainment among other things, while we can also track the flow of workers between occupations and firms over time. By utilizing the panel structure in such data, we can also overcome the issues related to unobserved worker heterogeneity, and control for firm-specific unobserved characteristics as well (e.g. capture the wage setting strategies of firms). As we are having information on a considerable amount of the society (the Hungarian sample covers 50% of the population), the generalizability of the results might be better and we can even investigate phenomena that occur with a lower incidental rate.

However, despite the vast amount of utilizable information in such datasets, there is typically a lack of direct information on the help of social ties and the list of acquaintances.[6] Nevertheless, by using alternative strategies, different network segments can be identified from such data as well, based on the co-occurrences of individuals at the same institutions or organizations (e.g. firms or universities), or on the basis of common group-level characteristics (e.g. same ethnic background). The identified network segments, then, can be further used to generate either aggregate measures (e.g. the share of given type of ties in general, or at specific firms) or variables that proxy the help of

[6] In some countries (for example Germany) we can see examples that for a smaller sample of the administrative records additional survey information is attached (for example data on job finding methods). In other countries, mostly in the Nordic ones, information on family ties is directly available.

social ties (e.g. captured by the re-union of acquaintances at given firms). Individual and labor market data of the identified contacts is also available. By utilizing such approach, several studies focused on the role of school acquaintances (Eliason *et al.*, 2019), co-workers (Cingano and Rosolia, 2012; Hensvik and Skans, 2016; Glitz, 2017; Glitz and Vejlin, 2021; Saygin, Weber and Weynandt, 2021), neighborhood (Bayer, Ross and Topa, 2008; Hellerstein, McInerney and Neumark, 2011; Schmutte, 2016) or co-ethnic ties (Damm, 2009; Dustmann *et al.*, 2016), family relationships (Kramarz and Skans, 2014), or even former military comrades (Laschever, 2009) in structuring individual labor market opportunities.

Apart from the lack of direct information on job search methods and contacts, there are some further issues with administrative datasets that may be of concern. The first one is related to the data generation process of administrative registers. Since these datasets combine information from different government institutions, only legally documented employment contracts and the corresponding wage and job characteristics are included. Thus, undeclared wages or bonuses cannot be traced in such data. Second, while information on employment is rich, other types of variables related to the individuals' subjective opinions and preferences (e.g. job satisfaction) or motivations (e.g. motivations to change jobs) are typically missing. This makes it harder to identify the driving mechanisms of network-related effects, or to distinguish between them. As various theories can affect more than one outcome and any outcome can be affected by more than one mechanism, we can find only a limited number of papers that tried to match the outcomes with the potential (causal) explanations.

2.2.4 Investigating the role of networks by using administrative LEEP

In recent years a wide range of studies relying on administrative LEEP datasets appeared, focusing on individual labor market outcomes and using different type of proxies for social ties. Kramarz and Skans (2014), by using direct information on parent-child triads investigated the importance of family ties in shaping the labor market outcomes of graduate students. By using Swedish register data they showed that career entrants are more likely to get their first stable jobs after graduation at their parents' workplace compared to other options (e.g. the workplace of their peers' parents). The effects are found to be the most substantial for lower educated individuals, and also if the parents' position is strong (e.g. longer tenure) or their firm is more productive. Their results also revealed that the use of strong ties, on the average, can reduce the duration of unemployment and ensures better perspectives on the long run (higher wages, for instance). However, positions acquired through parental help are typically less suited for the graduates' former training tracks and are accompanied by lower entry wages. We extend upon these results in Chapter 5, by focusing on the role of university ties on labor market outcomes.

Damm (2009) and Dustmann et al. (2016) both used co-ethnicity identified from administrative datasets based on direct information on minority affiliation or the country of origin. The former study focused on the role of residential segregation in determining the individuals' economic success. The authors showed that refugees with less favorable unobserved qualities tend to self-select (spatially) into ethnic enclaves, which can affect their employment and earnings outcomes. Dustmann et al. (2016), by using German administrative data analyzed the effect of referrals from individuals who share the same ethnic background. They demonstrated that individuals are more likely to acquire better wage-related outcomes and less likely to leave their firms if they were hired by firms where minority workers with the same ethnic origin are present with a larger share. In order to account for the non-random sorting of workers across firms, they included both person and firm fixed effects in their estimates. They also showed that the difference between referred and non-referred workers disappear over time and reinforced their finding by using survey data on referrals. In the next chapter, when estimating the wage benefits by former co-worker ties, we will use a similar two-way fixed effect approach to eliminate any potential bias.

A group of studies focused on neighborhood networks and showed that those who live at the same residences or neighborhood blocks are more likely to work at the same establishments as well. The studies found larger effects for immigrants and minorities (Hellerstein, McInerney and Neumark, 2011), and in those cases when acquaintances shared similar sociodemographic characteristics (Bayer, Ross and Topa, 2008). Schmutte (2016) also showed that referral networks can affect the labor market outcomes of individuals through various channels, for example by funneling individuals to high-wage firms. In the following empirical chapters, we will also test whether and to what extent this channel is responsible for the gains measured by professional contacts.

Finally, a branch of studies identified co-worker networks based on shared employment spells at the same firms. Cingano and Rosolia (2012), Saygin, Weber and Weynandt (2021) and Glitz (2017) investigated former co-workers' capability of generating job offers on Italian, Austrian and German data. They used the information transmission model of Calvó-Armengol and Jackson (2004, 2007), which suggests a clear, positive relationship between the share of our employed contacts and reemployment outcomes (if the former raise so as the latter). All three papers utilized establishment closures (as exogenous shocks) for measuring network effects and found positive relationship between the share of employed former co-workers and the individuals' reemployment outcomes.[7] In addition, Saygin, Weber and Weynandt (2021) examined the direct help of former co-workers on job finding using a "potential option" framework. In their empirical setting, they investigated whether individuals are more likely to get their new jobs at their former co-workers' workplace than at other firms. To account for the non-random sorting of individuals, they used sending firm-target firm fixed effects. When estimating the role of professional ties on hiring chances (in Chapter 4 and 5), we will use slightly modified versions of this empirical design.

Hensvik and Skans (2016) and Glitz and Vejlin (2021) by focusing on cases when former co-workers reunited at new (relatively small) firms demonstrated that the presence of contacts is typically associated with wage gains (on Swedish and Danish data). According to their results the size of the wage premium depends largely on the contacts' unobserved quality (Hensvik and Skans, 2016), last approximately 4-5 years and the created individual-firm matches proven to better in terms of stability (Glitz and Vejlin, 2021). By focusing on among others former co-worker links, Eliason et al. (2019) investigated the role of social ties in inducing labor market sorting. By drawing on this line of research, in Chapter 3, we will investigate the presence and magnitude of all those sorting channels jointly that may contribute to wage benefits by co-workers.

The upcoming empirical chapters can be considered as a continuation of this line of research. They build heavily on the empirical designs presented by these studies and use similar strategies for identifying professional acquaintances. However, by investigating previously less studied or yet unexplored questions or by further developing the

[7] Focusing specifically on vocational school graduates, Hensvik and Skans (2014) also shown that former co-workers from the internships can significantly help graduates to find their first stable job.

methodological approaches used so far, the empirical chapters aim to extend the current literature and contribute to a better understanding of the role of social networks in the labor market.

3. Decomposition of co-worker wage gains[8]

3.1 Introduction

The group of former co-workers forms an essential part of our social networks. As shown by early survey-based evidence, one's co-worker acquaintances can be essential sources of job-related information and they may also play an important role in the job-acquiring process (Corcoran, Datcher and Duncan, 1980; Holzer, 1988; Granovetter, 2019). Besides this type of studies, which typically exploited self-reported information about the individuals' job search process, in recent years, several studies used administrative registers to address the labor market effects of co-worker networks. Although having their limitations, such as the lack of direct information on social links or hiring methods, these datasets contain precise and reliable information about employment and wages that can be utilized to bypass these shortcomings. Using various techniques, recent studies showed that former co-workers can positively affect different individual labor market outcomes such as hiring probabilities (Cingano and Rosolia, 2012; Glitz, 2017; Saygin, Weber and Weynandt, 2021), tenure length and turnover (Glitz and Vejlin, 2021), and quite notably, wages (Hensvik and Skans, 2016; Glitz and Vejlin, 2021). The explanations for the existence of these beneficial effects mostly highlighted the role of two mechanisms: information transmission and employee referral.

In this chapter, we address the presence and magnitude of wage gains related to former co-workers and discuss the mechanisms that could potentially drive them. In our empirical estimates, we rely on administrative data from Hungary and use former co-workership as a proxy for actual social connections. Using a wage-decomposition technique, we document not only an overall wage gain of those job-switchers who have a former co-worker present in the receiving firm upon entry but also show that there are non-negligible differences in all empirically separable wage elements, namely, in the individual-specific, firm-specific, and match-specific components as well.

Studies that utilized a similar approach to assess the wage effects of former co-workers documented that gains can be mainly attributed to referral activity (Dustmann *et al.*, 2016; Glitz and Vejlin, 2021). However, a few other papers revealed additional channels through which gains are generated. Hensvik and Skans (2016) showed that homophily in

[8] This chapter is based on the article "Decomposition of co-worker wage gains" that has been published in the IZA Journal of Labour Economics. The project is a joint work with István Boza, and the work was supported by the Széchenyi 2020 program (EFOP-3.6.1-16-2016-00013).

co-worker networks can lead to the selection of better individuals into firms. Schmutte (2015), on the other hand, established that selection to high-wage firms is also prevalent. Furthermore, Eliason et al. (2019) found that referral is more likely to happen when the applicants are of better quality and their social contacts' firm pays higher wages.

We contribute to the literature of co-workers, employee referral, and wage differences in three ways. First, by being the first to document the presence of wage gains commonly attributed to the referral activity of former co-workers through the estimation of a two-way fixed effects wage equation on starting wages. We also claim that the gain estimated this way consists of two distinct factors: the presence effect of referral—which assumes the continuous presence of a referrer — and the selection of individuals into better matches. Although these mechanisms are empirically indistinguishable with our proposed methodology and data, the distinction is important for theoretical clarity. Second, to assess the presence and relative importance of selection channels in overall wage gains in detail, we augment and apply the decomposition method proposed by Woodcock (2008). To interpret our findings, we link differences in wage components to the established theories in the referral and co-worker literature. Finally, to reinforce our arguments, we provide additional empirical evidence by focusing on scenarios where referral activity is expected to be more prevalent, or conversely, where it is considered less probable.

To identify the effects of co-workers, ideally, we would compare hiring events to counterfactual observations of the same worker entering the same firm, but without/with a connection at the firm. As such variation is not present in the data, we control for observed and unobserved firm and individual heterogeneity by using a two-way fixed effects approach. We find a 2.1% wage gain for male workers, which could either reflect productivity sorting or other aspects of referral. This gain is accompanied by a 1.7% and 0.9% wage advantage attributable to better worker and average firm quality, respectively, that is high-quality employees are sorted into firms where co-workers are present and workers with former co-worker links are sorted into high-wage firms. These better firms, however, tend to hire high-quality workforce even without the co-worker links. The superior skills of new hires will be responsible only for a 1.3% wage advantage relative to market hires. The remaining 0.4% difference in worker effects is coming from an already established assortativeness among the involved firms and high-quality workers. Selection into better firms is more substantial when it is compared to the individuals' own work history, which typically consists of a somewhat inferior firm pool. The latter difference dampens the 1.2% within-individual gain by 0.3%. Considering female workers, most of the gains are attributable only to the selection of high-quality workers both in absolute and relative terms. Regarding occupational heterogeneity, we observe that two-way fixed effects parameters are generally stronger and individual selection is weaker in higher occupations. Moreover, the presence of firm selection is stronger in skilled occupations with stronger educational requirements. When relying on mass layoffs as exogenous sources of variation, we found similar results. Based on the implications of the theoretical literature and some reasonable assumptions, we interpret these figures as a result of referral and information transmission.

We supplement these arguments by showing that referral-related wage gains are stronger when the contact is of relatively higher occupation, had a longer tenure at the receiving firm, or if the length of the previous co-working spell with the job entrant was longer. We try to identify the referrer-dependent (presence) effects from separations of referrers and the prevalence of various occupation-specific skills. We find only small and insignificant differences, which may suggest that match-specific selection accounts for a substantial portion of referral-related gains.

3.2 Background

3.2.1 Mechanisms and possible explanations of wage gains

The literature identifies two mechanisms through which former co-workers (and in some cases, other social contacts) might shape the individuals' labor market outcomes: information transmission and employee referral. The former refers to the phenomenon that former co-workers might have access to relevant work-related information, which they can pass on to job seekers. Employee referral, on the other hand, covers those cases when employees of certain firms (referrers) bring together their acquaintances (applicants) and the vacancies at their companies. The main difference rests in the direction of information flows. In the former case, only job seekers receive information about the quality of some potential employers. However, in the latter case, information about worker type based on the shared co-working experience is also revealed to the employer in the form of recommendation.[9] To this distinction, we would add an additional layer of cases, when, upon hiring a new applicant, the referrer continues to act as a

[9] Referral without informing the applicant may happen, but is rather unlikely.

provider of information, either about the applicant's behavior to the employer or about firm-specific knowledge to the new co-worker. While keeping the above distinction in mind, we collect and systematically review various potential components of wage gains generated by former co-workers and aim to map the theories that might explain their existence.

The first component of co-worker wage gains consists of those elements, which essentially depend on the presence of a referrer. The related theories typically utilize the relationship between referrers and applicants. One group of such explanations is related to the mitigation of the employers' monitoring costs (Bartus, 2001; Kugler, 2003). Referrers can affect the performance of the newly hired workers both directly – by voluntarily monitoring their effort (Saloner, 1985; Smith, 2005; Ekinci, 2016) – and indirectly, if the applicants increase their productivity to compensate the referrers' favor (Smith, 2005). Also, referrers might have an important role in the integration of the workforce, as their presence might support smooth knowledge sharing and better cooperation at work (Fernandez, Castilla and Moore, 2000; Castilla, 2005). The enhanced productivity of workers and lower monitoring costs could increase the firm's profits, but it is not trivial whether the firm shares the emerging rent with the applicant. If the firm does so, we will observe a wage advantage of referred workers. For the sake of brevity, we refer to everything that is dependent on the active presence of a referrer and is perceived, valued, and compensated by the firm as presence effects.[10]

Besides the monetary benefits attributable to the above mechanisms, wage gains might originate from three types of selections as well: those based on match-specific productivity, worker-specific general skills, and firm-specific wage levels. Gains attributable to these selections, which capture previously existent productivity differences, are essentially different from referrer-dependent effects, as those actually increase the worker's productivity. In understanding the detailed role of co-workers in the labor market, we believe that the description of these selections is equally important as focusing only on causal channels.

First, referral activity might facilitate the sorting of workers into better employer–employee matches. The presence of such synergy implies a higher wage relative to both

[10] Favoritism can be also considered a source of these gains as the applicants only acquire wage gains if a particular referrer resides at their new company (Bian, Huang and Zhang, 2015).

the firm's wage level and the individual's outside options.[11] Dustmann et al. (2016) showed that the wage prospects of nonreferred workers are more uncertain as their match-specific productivity is not revealed in the hiring process. Therefore, they will potentially turn down job offers that would be good matches, leading to a higher expected match element for referral hires. However, the emergence of better matches could happen even without the active participation of a referrer if employees pass information to only those who would be a good fit for a given vacancy at their firms.

The use of employee referrals might also promote the selection of those workers who generally have better skills and would earn more at any firm compared to someone with similar observable characteristics.[12] As referrers can decrease screening costs either by providing information about their former co-workers or by signaling worker quality with their own productivity based on the assumption of network homophily in productivity (Montgomery, 1991; Munshi, 2003; Hensvik and Skans, 2016), they can contribute to the reduction of information asymmetry about the general characteristics of applicants.[13] This way firms may avoid low-quality workers and, on average, hire better-quality applicants, even if they are not better-matched ones (Ullman, 1966; Saloner, 1985).[14]

Selection into high-wage firms, on the other hand, is mainly driven by information transmission. Former co-workers can be good sources of job offers (Calvó-Armengol and Jackson, 2004, 2007; Granovetter, 2019), and their information might mitigate the job seekers' uncertainties about the possible employers (Wanous, 1980; Tate, 1994). By choosing from a larger set of vacancies, the expected quality of one's new firm could be

[11] Employers could, however, withhold the gains from these productivity improvements. A firm mitigating a moral hazard problem with efficient wages may prefer to hire workers through referral, as social factors already incentivize them to work hard. Thus, the wages of such applicants could be lowered (Dhillon, Iversen and Torsvik, 2021).

[12] We suppose that information transmission in itself cannot be accountable for such selection. When their contribution remains hidden to the firm, workers rather share work-related information either to all of their relevant acquaintances or to only those who would be a good fit for the specific opening.

[13] An employer could also assume that homophily is present not only regarding general skills but also match-specific ones. Wage premium paid based on this assumption would enhance the previously discussed match selection.

[14] While employers could share gains from the reduced screening costs with the applicants through higher wages, this scenario is rather unlikely, as firms usually only incentivize their referrers, by one-time bonuses. Based on industry interviews we conducted, even these practices were not yet commonly utilized during the time frame of the research.

higher. However, we note that positive firm selection could be also observed if, on average, higher-wage firms rely on the use of referrals.

We suppose that the above selections and the role of presence effects relate to information transmission and referral mechanisms in the following way. Firm selection is mainly driven by information transmission, but employee referral might also account for such gains if it dominantly happens in high-wage firms. Individual selection, we believe, is only present if employee referral happens either through direct (recommendation) or through indirect signals (homophily). Match selection could be a product of both mechanisms but is probably much more prominent in cases of active referral (Dustmann *et al.*, 2016). Finally, presence effects emerge only when the referral is followed by other, continuous actions on the referrer's side as well. When decomposing the wage gains attributable to former co-workers, we will rely on the above framework to interpret the results.

3.2.2 Empirical evidence

In this section, we survey recent empirical evidence from papers that are based on matched employer–employee administrative data and focus on wage effects of various social contacts.[15] While some papers aim to estimate the direct effects of employee referral or provide evidence on information transmission through networks, others are especially after the selections in the labor market produced by referral and job information networks. This analysis is related to both lines of research, both in theoretical approach and the utilized methods as well.

To study the role of employee referrals, Glitz and Vejlin (2021) constructed an indicator of events when former co-workers have reunited at a new firm with one of them arriving earlier. After showing that the number of such events in Denmark is higher than what random network forming would suggest, they interpreted these instances as potential cases of referral. They found a 4.6% wage advantage attributable to the presence of former co-workers after controlling for firm fixed effects, but not accounting for individual heterogeneity.[16] Besides, they also demonstrated that the initial wage gains of

[15] The related survey-based literature is summarized in Chapter 2.
[16] This difference also includes gains related to the superior unobserved quality of workers hired this way.

the referred workers decline over time, and in the long run, they eventually end up with lower wages than those who were hired through the external market.

Earlier, Hensvik and Skans (2016) provided similar evidence on former co-workers' effects on wages and assessed the role of homophily in terms of abilities of workers as a potential driver of individual selection. Using Swedish administrative data, including military test scores as a proxy for individual productivity, they showed that linked workers can earn 3.6% more compared to other new hire in the same establishment. Additionally, they demonstrated that the wage premium of the connected employees increases as the incumbent workers' abilities improve. This indicates that from the firms' perspective, current employees' productivity might unintentionally signal the quality of their acquaintances. The results also support the idea that network inbreeding might contribute to the generation of wage inequalities.

Dustmann et al. (2016) investigated the effects of referral on wages and turnover rates by using German data. They used the share of workers with the same ethnicity at the firms at the time of hiring as a proxy and also a direct indicator of referral coming from survey data. Their model of wages incorporated both individual and firm fixed effects, which account for the nonrandom sorting patterns of workers to firms alongside unobserved worker and firm characteristics. Their findings suggest a 3.3% wage gain by directly measured referral, potentially generated by the better matches among employers and linked hires.

Focusing more on the role of information transmission, Cingano and Rosolia (2012), Glitz (2017) and Saygin, Weber and Weynandt (2021) investigated the co-worker network's capability of generating job offers and its impact on the reemployment outcomes of displaced workers based on the model of Calvó-Armengol and Jackson (2004, 2007). Their results demonstrated that an increase in the share of employed former co-workers comes with a higher re-employment rate of displaced workers, suggesting information transmission through the co-worker networks. Furthermore, Saygin, Weber and Weynandt (2021) also found a significant difference between the displaced workers' pre- and post-displacement wage outcomes when the share of employed former co-workers in high-wage firms was high. This result is in line with our notion about information transmission's effect on firm selectivity.

Additionally, some papers provided evidence for the presence of individual and firms selections. Using US data, Schmutte (2015) showed that job seekers are more likely to become co-workers of their neighbors from the same block as the individual than those from their broader neighborhood. After estimating an AKM (after Abowd, Kramarz and Margolis (1999)) decomposition of wages, he also demonstrated that referrals are more likely to happen when the applicants have better skills or when the referrers work at high-wage firms. He also argued that employee referral in itself cannot explain this set of results, and that information transmission over the job information network also has to play a critical role.

Besides additional evidence on selection patterns and homophily, inequality consequences are also documented in the study of Eliason et al. (2019). The authors constructed a proxy of the local labor market for displaced workers by linking their closing firm to workplaces where the former co-workers of displaced workers were employed at the time of the plant closure. Comparing the role of social links in increasing hiring probabilities by levels of previously obtained AKM-style individual and firm fixed effects, they found that social ties might induce positive sorting. High-wage job seekers tend to have links with high-wage workers who more likely to work at high-wage firms. The combination of homophily and positive assortative matching could then increase inequalities. However, they also showed that the causal impact of ties on hiring probability is the strongest for low-wage firms, which eventually leads to a lower level of sorting inequality. As directly assessing assortativeness is out of the scope of this chapter, our main takeaway from their work is that referral may be more prominent in low-wage firms, attenuating the firm selection patterns generated by information transmission.

In this study, we focus on former co-worker contacts' effect on entry wages by relying on a proxy like Hensvik and Skans (2016) and Glitz and Vejlin (2021), and using multiway fixed effects approach similar to Dustmann et al. (2016). However, we utilize a framework that can also capture selections induced by co-workers. To do this, we improve upon and use the decomposition of Woodcock (2008) to assess selection mechanisms both in absolute and relative terms. In the process, we rely on AKM firm and person effects as measures of employer and worker quality, similarly to Schmutte (2015) and Eliason et al. (2019). Therefore, our proposed framework attempts to assess the direct and indirect consequences of co-worker networks at the same time. We find evidence for both wage gains after controlling for individual and firm heterogeneity like Dustmann et al. (2016) — which, we add, could still incorporate match selection and presence effects as well — and also for the presence of individual and firm selections as Hensvik and Skans (2016) and Schmutte (2015), respectively. Furthermore, we show that selections are mainly driven by their respective within components: linked workers get access to higher premium firms compared to where they usually work, and firms can increase the quality of their worker pool with reerral hires.

3.3 Model and Empirical Strategy

To investigate the mechanisms discussed in Section 3.2.1, we estimate differences in specific wage components. We start by introducing an AKM model of wage-setting (Abowd, Kramarz and Margolis, 1999), augmented with match effects similar to Woodcock (2008). Our wage equation also includes the effect of the presence of a referrer, θ, as a wage-determining factor.

$$w_{ijt} = \alpha + \theta T_{ijt} + \beta_X X_{it} + \beta_Y Y_{jt} + \beta_Z Z_{ijt} + \delta_i + \gamma_j + \mu_{ij} + \pi_t + \varepsilon_{ijt}\,, \qquad (1)$$

In Eq. (1), w_{ijt} denotes the starting wage earned by person i at firm j at calendar year t. X_{it} contains the observable characteristics of the individual, such as age and education. Y_{jt} comprises the properties of the firm, such as sector and ownership. Finally, Z_{ijt} includes variables corresponding to the actual employment spell of individual i at firm j, among other occupation and form of contract. One such factor is an indicator of whether the given worker has obtained the job through a social contact: T_{ijt}. However, this latter variable is rarely observed directly and is usually substituted by a proxy, which indicates whether an individual has a co-worker at a new firm upon entry with whom they had worked together earlier.

Besides these observable characteristics, many unobservable factors can alter an employee's starting wage at a new job. We suppose that these features, namely, the latent quality of the individual (δ_i)[17], the quality of the employer–employee match (μ_{ij}), and the wage levels of firms (γ_j) are constant over time. Seasonal and trend effects (π_t) may also affect wages over a longer period. All other factors make up the independent error term with zero expected value (ε_{ijt}).

[17] Time invariant unobservable individual characteristics essentially refer to the innate abilities of individuals (e.g. resourcefulness, talent or latent productivity).

3.3.1 Identification of match and presence effects

The proper estimation of the full model is, however, infeasible. To obtain the match effects, we would have to compare multiple entries to the same firm by the same person. Although such a scenario occurs sometimes, gains estimated from comparing these observations could also reflect, for instance, the presence of firm-specific knowledge. Therefore, we prefer to omit these cases from the estimation sample. This way, and by focusing only on entry wages, we have only one observation for each employer–employee match. Besides, as in every match, someone either has a contact or not, there is no variation in T_{ijt} within the ij groups. These limitations induce that there will be no way to distinguish the match effects, μ_{ij}, from the idiosyncratic residual terms, ε_{ijt}, and to identify the parameter on presence effects, θ, which could reflect lowered monitoring costs, knowledge transfer, or favoritism.

Therefore, we have to rely on a second-best estimator in which we cannot control for the match effects. To present the resulting implications, let us introduce the following matrix notation, based on Woodcock (2008), as an alternative for Eq. (1).

$$w = \theta T + \beta X + D\delta + F\gamma + G\mu + \varepsilon \qquad (2)$$

In this form, w is the vector of wages, X is the matrix of observables, with T being the indicator for the presence of a co-worker link, and D, F, and G the design matrices of individual, firm, and match fixed effects, respectively. Without accounting for match effects, the two-way fixed effects estimator would be biased in the following way.

$$E[\theta_{TWFE}] = \theta + (T'M_{XDF}T)^{-1}T'M_{XDF}G\mu \qquad (3)$$

The matrix M_{XDF} is a projection matrix, taking out the within-firm (F), within-individual (D), and observables-specific (X) variation from both the indicator (T) and match effects ($G\mu$).[18] Therefore, by omitting the match fixed effects and controlling only for separable and additive person and firm effects, the estimator will also incorporate the average difference of match effects among the two groups, controlled for firm and person effects and observables. $\hat{\theta}_{TWFE}$ would estimate θ without bias only if the match effects were, conditionally on X, F, and D, independent of the presence of contacts.[19]

[18] The whole formula $(A'M_{abc}A)^{-1}A'M_{abc}B$ is the OLS estimator of A's effect on B, controlling for factors a, b and c. If A is a dummy variable, it reflects the conditional expectation of the difference in B between the two groups defined by A.

[19] We note that the omission of match effects will lead to a biased estimation of both individual and firm effects. We discuss the implications later in the chapter.

While the independence of the idiosyncratic error term from the match effects seems to be a plausible assumption, the use of social contacts and match effects might be related. According to the literature, the selection into or creation of superior matches is one of the main mechanisms of referral activity. The second term in Eq. (3), $(T'M_{XDF}T)^{-1}T'M_{XDF}G\mu$, which, in the above setting, is an omitted variable bias, actually captures the magnitude of this selection. Therefore, by using two-way fixed effects regressions, we can only estimate the total of the match selection term and gains related to referrer presence, but we cannot separate them.

Naturally, it would have been nice to empirically demonstrate the presence and magnitude of both types of gains to get a more accurate picture of the overall composition of wage effects. However, since our general interest is in exploring all kinds of gains associated with wages, and the aim of the research is not solely to look for causal effects by referral, having such a composite term is not a major problem. Especially, as the parameter still can, under certain assumptions, give some intuition on direction of the given effects (e.g. a strong positive $\hat{\theta}_{TWFE}$ most likely imply the presence of positive match selection based on the theoretical explanations). Nevertheless, in Section 3.5.3, we attempt to bypass this limitation by providing further specifications.

3.3.2 Individual and firm selections

Besides the presence effects and the match selection induced by contacts, we are interested in the individual and firm sorting patterns related to co-worker networks as well. To pursue this goal, we rely on the following decomposition, based on Woodcock (2008), to compare the overall gain, θ_{OLS} with θ_{TWFE}.

$$E[\theta_{OLS}] = E[\theta_{TWFE}] + \underbrace{(T'M_XT)^{-1}T'M_XD\delta}_{\psi_{ind}} + \underbrace{(T'M_XT)^{-1}T'M_XF\gamma}_{\psi_{firm}} \qquad (4)$$

This decomposition suggests that by not accounting for person and firm effects, we introduce two additional, distinct omitted variable biases. The first, ψ_{ind}, is the controlled difference between the unobserved skills among linked and nonlinked employees measured in (nominal) wage terms. That is, how much wage difference is implied by the linked employees' different latent qualities. A positive bias term suggests that good quality employees are more liable to be referred for jobs or more prone to applying to firms with their acquaintances present.

The bias arising from the omission of firm effects (ψ_{firm}) is the difference between the premium paid by firms where linked hires or referral activity occur and where they are not present, implicitly weighted by the number of new hires. A positive value suggests that linked employees can, on average, end up receiving higher wages as they can enter better quality firms, which pay higher (starting) wages for the same job relative to similar firms.

These average selection terms, however, do not capture whether the differences can be experienced within or between workers/firms. It is possible, for example, that while the linked workers of a firm are not especially high-wage ones, they are still better relative to the worker pool of the given firm. To account for the possibility that such patterns are present on the aggregate level as well, we further decompose the above-introduced selection terms.[20]

$$\underbrace{(T'M_XT)^{-1}T'M_XD\delta}_{\psi_{ind}} = \underbrace{(T'M_{XF}T)^{-1}T'M_{XF}D\delta}_{\xi_{ind}} + \underbrace{(T'M_XT)^{-1}T'M_XF\gamma^S}_{\omega_{ind}} \quad (5)$$

$$\underbrace{(T'M_XT)^{-1}T'M_XF\gamma}_{\psi_{firm}} = \underbrace{(T'M_{XF}T)^{-1}T'M_{XD}F\gamma}_{\xi_{firm}} + \underbrace{(T'M_XT)^{-1}T'M_XD\delta^S}_{\omega_{firm}} \quad (6)$$

In this decomposition, γ^S denotes the vector of firm effects obtained from a second-stage fixed effects regression on the estimated individual effects from the original two-way fixed effects wage equation. A firm that tends to hire individuals with high worker effects will have a high γ^S, regardless of the value of its firm effects. Similarly, ${\delta_i}^S$ reflects the average premium of firms a given individual ever works at. If there would be no systematic differences among firms or individuals in these parameters, as in case of the total absence of assortative matching, within and average differences in estimated effects would be the same due to the lack of correlation between individual and firm effects. Hence, this decomposition would be redundant.

Equation (5), therefore, shows that the average difference in the worker effects between linked and nonlinked hires is the sum of the average difference within firms (ξ_{ind}) where

[20] For the sake of brevity, let us assume, for now, that estimated individual and firm effects are estimated without bias.

linked hires present and the difference in the average level of worker effects between firms with and without any linked hires (ω_{ind}). The first term could signal whether given firms benefit from accessing relatively better-skilled individuals through linked hires, while the second term describes how is the average worker pool of firms with linked hires compared to firms without such. Similarly, ξ_{firm} will reflect whether firms, where hiring linked workers is prevalent, are better compared to the work history of the linked hires. That is, whether they benefit by moving to firms of their former colleagues. Finally, the parameter ω_{firm} will characterize the firms that are generally accessed by these workers even when they are hired without links. Table 1 briefly summarizes all the introduced parameters.

Table 1. Summary of parameters

Parameter			Interpretation
0.		$\hat{\theta}_{OLS}$	The wage differential between linked and nonlinked hires, controlling for only observed worker and firm characteristics.
1.		$\hat{\theta}_{TWFE}$	The wage differential between linked and nonlinked hires, controlling for unobserved firm and worker heterogeneity (except match heterogeneity).
	1a.	θ	The pure 'presence effects' of having a potential referrer at the firm.
	1b.		Bias arising from the possibility that linked workers are better matched with firms (match selection).
2.		$\hat{\psi}_{ind}$	The average worker effect differential between linked and nonlinked hires.
	2a.	$\hat{\xi}_{ind}$	The average worker effect differentials between linked and nonlinked hires within firms.
	2b.	$\hat{\omega}_{ind}$	The average worker effect differential between firms that tend to make linked hires and those that tend to make nonlinked hires.
3.		$\hat{\psi}_{firm}$	The average firm effect differential between linked and nonlinked hires.
	3a.	$\hat{\xi}_{firm}$	The average firm effect differentials between linked and nonlinked hires within worker careers.
	3b.	$\hat{\omega}_{firm}$	The average firm effect differential between workers that tend to be hired with and without links.

Note: $\hat{\theta}_{TWFE}$ also contains the expected difference in error terms from Eq. (1), controlling for observables and person and firm effects. Our identifying assumption is that this term is zero. Also $\hat{\theta}_{OLS}$ could contain additional differences in the error terms due to misspecification or proxy issues that are only relevant if one does not control for two-way fixed effects. This is also assumed to be zero. This way $\hat{\theta}_{OLS} = \hat{\theta}_{TWFE} + \hat{\psi}_{ind} + \hat{\psi}_{firm}$. Also $\hat{\psi}_{ind} = \hat{\xi}_{ind} + \hat{\omega}_{ind}$ and $\hat{\psi}_{firm} = \hat{\xi}_{firm} + \hat{\omega}_{firm}$.

3.3.3 Estimation of decompositions

To obtain the parameters of the proposed decompositions, we estimate the following set of equations. First, we estimate the wage equation introduced in Eq. (1), but without match effects.[21]

$$w_{ijt} = \alpha + \theta_{TWFE} T_{ijt} + \beta_X X_{it} + \beta_Y Y_{jt} + \beta_Z Z_{ijt} + \delta_i + \gamma_j + \pi_t + \varepsilon_{ijt} \quad (7)$$

Then using the estimated person and firm effects $\hat{\delta}_i$ and $\hat{\gamma}_j$ we estimate the following equations to get the decompositions from Eqs (4–6).

$$\hat{\delta}_i = \alpha_2 + \psi_{ind} T_{ijt} + \beta_{2X} X_{it} + \beta_{2Y} Y_{jt} + \beta_{2Z} Z_{ijt} + \pi_{2t} + \varepsilon_{2ijt} \quad (8)$$

$$\hat{\gamma}_j = \alpha_3 + \psi_{firm} T_{ijt} + \beta_{3X} X_{it} + \beta_{3Y} Y_{jt} + \beta_{3Z} Z_{ijt} + \pi_{3t} + \varepsilon_{3ijt} \quad (9)$$

$$\hat{\delta}_i = \alpha_4 + \xi_{ind} T_{ijt} + \beta_{4X} X_{it} + \beta_{4Y} Y_{jt} + \beta_{4Z} Z_{ijt} + \gamma_j{}^S + \pi_{4t} + \varepsilon_{4ijt} \quad (10)$$

$$\hat{\gamma}_j = \alpha_5 + \xi_{firm} T_{ijt} + \beta_{5X} X_{it} + \beta_{5Y} Y_{jt} + \beta_{5Z} Z_{ijt} + \delta_i{}^S + \pi_{5t} + \varepsilon_{5ijt} \quad (11)$$

$$\hat{\gamma}_j{}^S = \alpha_6 + \omega_{ind} T_{ijt} + \beta_{6X} X_{it} + \beta_{6Y} Y_{jt} + \beta_{6Z} Z_{ijt} + \pi_{6t} + \varepsilon_{6ijt} \quad (12)$$

$$\hat{\delta}_j{}^S = \alpha_7 + \omega_{firm} T_{ijt} + \beta_{7X} X_{it} + \beta_{7Y} Y_{jt} + \beta_{7Z} Z_{ijt} + \pi_{7t} + \varepsilon_{7ijt} \quad (13)$$

We note that the omission of match effects may bias the estimated values of $\hat{\gamma}_j$ and $\hat{\delta}_i$. Firm effects will contain whether the firm makes good matches on average, and individual effects will contain if someone is prone to create good (or bad) matches. These bias terms are, however, independent of observables, including our proxy, T.[22] Thus, the controlled differences in fixed effects introduced above ($\hat{\psi}$, $\hat{\xi}$ and $\hat{\omega}$) are not affected by such biases.

Another concern could be the bias arising from identifying firm effects (and therefore person effects) only from a limited number of moves between establishments. As our panel is only a 50% sample (and we have to apply further restrictions to our sample), limited mobility bias (Andrews *et al.*, 2008) could not be neglected. On the other hand, we can use six years of data and observe within-year movements as well, which may somewhat counterbalance the potential lack of identifying mobility. The most commonly discussed consequence of this bias is the overestimation of the variation in firm effects, and the underestimation of the correlation between firm effects and worker effects, a

[21] Models with multiple fixed effects are estimated based on the method of Correia (2017). Models with one or no fixed effects also use the Stata routine of Correia (2017), as it allows for two-way clustering of standard errors.

[22] The estimated effects will capture conditional average differences in match effects in the following way: $E[\delta_{TWFE}] = \delta + (D'M_{TXF}D)^{-1}D'M_{TXF}G\mu$ and $E[\gamma_{TWFE}] = \gamma + (F'M_{TXF}F)^{-1}F'M_{TXF}G\mu$ (Woodcock, 2008).

measure of assortative matching. While there are established methods for correcting the bias in these moments (Andrews *et al.*, 2012; Gaure, 2014; Bonhomme, Lamadon and Manresa, 2019; Bonhomme *et al.*, 2020), we face a different problem.

According to Kline, Saggio and Sølvsten (2020), limited mobility bias can also affect standard errors when someone projects the estimated firm (or person) effects of an AKM model on a set of observables, as we do in our decompositions with the proxy for social links. For instance, as we estimate biased firm effects with a higher variation, we will be seemingly able to explain this variation well with observable factors and get smaller standard errors and therefore biased inference in the second-stage estimates. In our example, for instance, we could overstate the role of the firm component in the overall wage difference. The authors propose a correction method for standard errors, which accounts for this possibility, and correct inference. However, we lack the computational infrastructure required for this exercise. Hence, standard errors in our decompositions may be somewhat underestimated, and measures of statistical significance are less reliable. Results should be treated accordingly, focusing on the relative magnitude of components of the decompositions rather than their statistical significance.[23]

3.3.4 Identification, proxy quality, and generalizability

The regression with two separable fixed effects, if estimated, yields a parameter, which measures the additional wage individuals could earn due to being hired with a link compared to the amount implied by their latent and observed qualities, the firm's wage setting-strategy, and other characteristics. This parameter is identified from both a comparison of employees at mixed firms and the comparisons of employment spells in the working history of individuals who were linked at least once.[24]

Based on the above, it is important to note that we cannot predict what would happen in those sectors where hiring through links is not prevalent or in population sub-samples

[23] As a robustness check, we estimated an AKM model on a much larger set of data, which included mostly all spells in mostly all firms available, to acquire better estimates of firm and person effects. Then, instead of using conventional fixed effects methods (within transformation), we conditioned on these "pre-estimated" fixed effects in estimating the wage equation. The correlation between the pre-estimated firm effects and those from our main equations were 0.84, while for individual effects, it was only 0.66. Parameters estimated this way were similar in magnitude; however, standard errors have increased for firm selection and decreased for individual selection terms.

[24] More precisely, firms whose linked workers are always linked and whose nonlinked workers are never linked do not contribute to the parameter estimates. People who are linked in firms where everyone is linked and nonlinked at firms where no one is linked are also omitted from the comparisons.

where no such events are observed.[25] Therefore, the results may not be generalized to the whole population. However, this is not a problem as we are interested in the effects of co-worker connections where they are actually relevant. Also, it is important to note that the estimated individual and firm effects are comparable only within connected sets of workers and firms. As common in such datasets, we have a giant component in the paired graph of employers and employees, consisting of 92.7% of observations. We will estimate all models on this subset.

As the actual job-finding method is not observed in the data, another issue of our approach is the reliability of the proxy variable used. Namely, the proxy, T_{ijt}, may capture different variation depending on the controls. That is, the variation of T_{ijt} on average (OLS) or around a person's or a firm's mean (one-way fixed effects regressions) may not proxy the same phenomenon. Hence, while the variation of the proxy when using both firm and person fixed effects probably captures referral activity (Dustmann *et al.*, 2016), the selection terms let in other aspects from a broader set of phenomena. As we discussed previously, the sorting of high-wage workers to firms, or passing information about high-wage vacancies are aspects that we consider as part of the relevant mechanisms. However, some unintended variation may still be present in the proxy, so we have to interpret the selection terms with caution. For instance, in the case of hiring constantly from the same firm, we would systematically observe the arrival of linked workers and may falsely interpret these hires as referred ones, while wage gains may be related to the familiarity with the sending firm. We also have to account for the fact that workers getting into the same firm randomly is more common in sectors with high fluctuation and for people who switch workplaces often. If wages are high in these sectors or skilled persons tend to move a lot or have a limited number of options fit for their skills, we would face some unintended biases. While the two-way fixed effects regression controls for these issues, in the less-controlled regressions, we aim to avoid them by some sample restrictions and the inclusion of specific control variables.[26]

[25] Or in sectors where everyone is linked or with persons who are always linked.
[26] We are aware of one confounding factor that we cannot capture without person fixed effects: the personal preference for working with acquaintances. We can only assume that it is independent of average wage level or general skills; therefore, it will not lead to a higher wage among those who favor working in firms with social links.

After discussing our main results, we present additional evidence that may further suggest that the observed individual and firm selections are mostly driven by referral and information transmission-related mechanisms, instead of empirical artifacts or unintended variation in our proxy.

3.4 Data and co-workers

Our empirical analysis uses the Panel of Administrative Data from the Databank of the Centre for Economic and Regional Studies (formerly part of the Hungarian Academy of Sciences). It is a large administrative, linked employer–employee dataset, covering a random 50% of the working-age Hungarian population followed from January 2003 to December 2011. The dataset combines data from the official records of the Pension Directorate, the Tax Office, the Health Insurance Fund, the Office of Education, and the Public Employment Service. The raw register data were compiled and restructured by the Databank into a monthly level panel, in which all observations refer to the employment status of individuals on the 15th day of the given month.[27] For each observation belonging to an employment spell, the dataset has anonymous individual and employer identifiers, monthly earnings data, featuring the number of days in employment, information about employment type, occupation, and balance sheet data of the employer. Variables on health expenditures and social transfers received by the individuals are also available. Using the linked nature of the dataset, we could extract all those co-worker pairs who worked at the same company in any given month.

3.4.1 Co-worker definition and sample restrictions

By adding additional constraints, we selected those former colleague relationships that have the potential to serve as a basis for referral activity and/or information transmission. We defined former co-workers as those pairs of employees who had worked together at the same company, which had a maximum of 50 observed employees, before their reunion at another firm. Setting a limit on the company size of the first encounter was an essential and necessary step. Not using such a restriction would have led to the overestimation of the number of real social connections among former colleagues for two reasons. First, because at medium and large companies, not everyone knows each other. Second, among these companies having multiple sites is more typical, which would have further

[27] While the source data contain all legal employment spells, which generate social contribution obligations, in the final structure, we do not observe employment spells that are shorter than 1 month and are not present on the 15th day of any month.

increased the probability of misclassification since the data contains only firm-level, but not establishment-level information. To enhance the probability that co-workers actually knew each other, we applied further conditions. Co-worker pairs were considered valid only if they had worked together for at least 12 months in the past, they had reunited at a firm with a maximum of 250 observed employees[28], and the incumbent employees had arrived at least 1 month before their former co-workers did.[29] Also, as weaker social connections usually erode over time, we restricted the time that could pass between the two encounters to 5 years.

Our variable of interest then would be a proxy indicating whether upon entering a new firm, the entrant had at least one former co-worker who met the above criteria. Among those who had no such relations, we differentiated three groups. Regarding two of these, we cannot observe any link by definition: the first group consists of the first observed employment spell of each worker, while the second one comprises those workers who had worked only in larger firms (more than 50 observed employees). The remaining observations where former co-workers could be but are not present form the most comparable control group. While this latter is the one we will compare observations to, the former two groups are also included in the sample for the proper estimation of firm fixed effects.

In our estimates, we included only those 15–65-years-old private-sector employees who had no more than 15 distinct employment spells over 9 years and were not receiving social transfers. To avoid the confounding effects of social benefits on reservation wages, we focused only on job-to-job transitions and hires after unemployment spells no longer than 12 months. Artificial changes in firm identifiers, like those resulting from mergers, could have resulted in the overestimation of the referred employees' wage premium as we would see (re-)entries with high wages during someone's real employment spell. We removed from the data all identifiable cases of such artifacts. Observations, when more than three linked newcomers arrived together at a company from the same firm, were excluded, as comobility in itself can provide a substantial wage premium (Marx and

[28] We restricted the possible size of the receiving firm, as our data do not comprise plant-level information. Also, in such large firms, there is a higher chance that contacts will be unaware of the application of their former co-workers.

[29] This restriction only enforces that one worker arrived definitely earlier. By observing, instead of detailed employment spells, the registered workforce of the firms only on the 15th of each month this 1-month gap will reflect a 2–60-day difference between the starting dates of the two workers.

Timmermans, 2017). We removed entries where the simple majority of the receiving firm's hires in the past year came from the same sending firm, which would potentially reflect the presence of a sending firm premium. Finally, all cases of workers returning to one of their former employers were omitted to avoid capturing the effects of firm-specific knowledge. Based on the process of defining peers, we note that in the early years of the observation period, there were artificially fewer former co-worker pairs than in later years (Figure 1). Hence, we used the first three years of data as the connection-forming period and only the later years (2006–2011) for the estimates.

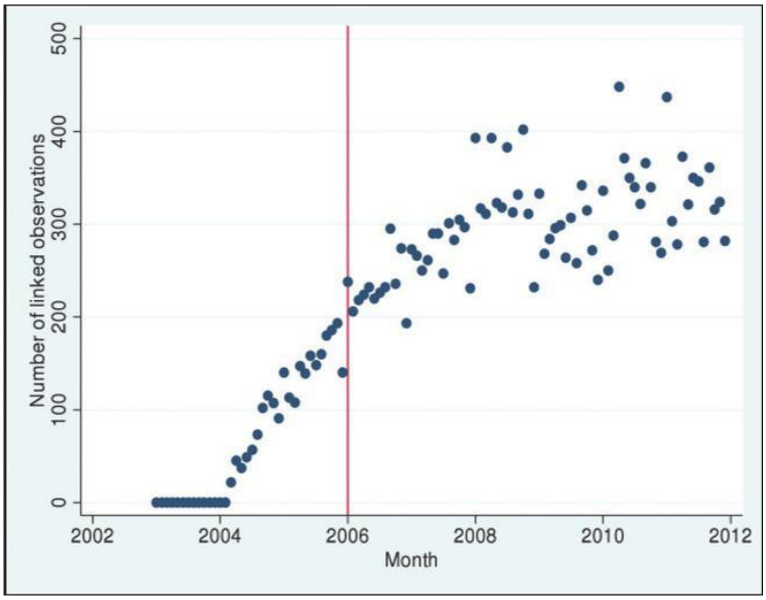

Figure 1. Number of linked workers over time

Note: The figure displays the number of hires with co-worker links present in each calendar month from 2003 until 2011. Naturally, in the first year of the observation period we cannot observe any linked hires since our co-worker definition demands at least 12 months overlap in the employment spells of individuals. From 2004 January onwards, the number of linked hires starts to rise and becomes fairly stable around 2006. The subsample used is the same as in Table 2.

As we focus on linked entries only in small and medium firms, we dropped entries from the nonlinked groups at firms with more than 250 observed employees as well. To get comparable estimates of firm effects, we used only the largest connected mobility group, which consists of 92.7% of the sample with the above restrictions.

Despite these restrictions, there is still a chance of misclassification. If employees do not get to know all of their co-workers within a year or if the former co-worker relationships erode in less than 5 years, employees may have been incorrectly labeled as linked ones. The reverse may also occur due to database-related issues since we could not identify former colleagues who were not part of the 50% sample.[30] Furthermore, as opposed to our definition, some connections may form in large companies or may not erode even after 4 years. Either allocating high-wage, linked workers to the low-wage, nonlinked group, or vice versa results in a lower observable wage difference between the two groups. Therefore, both types of misclassification have the same effect on our estimates: the measured difference between the linked and nonlinked groups will be lower than their true values and the estimated effects will be biased toward zero.

3.4.2 Definition of variables

To estimate the parameters of the model defined in Eqs (7–13), we use the first months of employment spells as observation units. We define the independent wage variable, w_{ijt}, as the logarithm of daily earnings over the national average of daily earnings to standardize over time. We prefer to use starting wages as they are determined by different processes than, for instance, wages in subsequent years in a working spell. When defining starting wages, employers usually cannot rely on actually observed performance of the workers. Hence, a referrer's contribution might be essential in the assessment of hiring risks. Inside information about the firm could also alter the initial wage expectations of new applicants. In the subsequent months of employment, wages can be adjusted according to employers' experiences with newcomers' performance.

Individual controls consist of the interaction of (quadratic) age and imputed education[31], residence[32], and gender, with the latter two only included in regressions without individual fixed effects. We also control for previous work experience with the number of former workplaces in an elastic form, as subsequent employment spells have an increasing probability to be linked even in the absence of referral. We included the indicator of work experience in the two-digit occupation category of the new job. Time-variant firm-specific characteristics include ownership (foreign or domestic private) and a two-digit industry code. To control for any possible remaining time trends, we include

[30] With not being able to observe 50% of the population, we lose 75% of all possible connections and, based on simulations, around 66% of the linked observations. In our sample, around 10% of the nonlinked hires would actually be linked, given this sampling issue.

[31] Unfortunately, we can observe the actual level of education only in special cases. Therefore, in our empirical specifications we include an approximate measure of education which is defined based on the occupation in the individual's overall work history which demands the highest level of education.

[32] The database contains only details about individuals' residence in 2003. However, supplementary investigations show that changing residence is not common in the sample, as it affects only 5% of individuals.

year dummies. To get the effect of unemployment on reservation wages, we include dummies for the length of the unemployment spell, measured in months, preceding entering the firm.

We also include a full set of controls interacted with an indicator of the observation being the first observed employment spell of an individual and another dummy indicating that the individual could not obtain proper co-worker ties due to the lack of experience at small firms. This allows us to estimate firm fixed effects properly and also to have a larger connected mobility group (Glitz and Vejlin, 2021).

Our main variable of interest is the dummy indicating the presence of any former co-worker, interacted with gender categories to capture heterogeneous effects. In regressions without individual fixed effects, we include the set of the gender-occupation category dummies, where occupation can take on five categories: manager, skilled white-collar, unskilled white-collar, skilled blue-collar, and unskilled blue-collar.

3.4.3 Baseline differences

We define the control group, to which linked hires could be reliably compared, as those nonlinked workers who previously worked at a small firm. The groups of workers in their first employment spells and those who previously only worked at large companies were also distinguished. Table 2 contains the mean values and distributions of some key variables in the sample by these observation groups.

When comparing raw means of outcomes, we can observe a significant wage advantage of linked hires over the control group. In nominal monthly earnings, the difference is more than 17%. However, when we use a finer measure, in which we normalize by the number of days worked and the national average wage, we see only a 0.1 log point difference, suggesting a 10% wage advantage of linked hires over market ones. It is also worth to note that the wage level of firms the linked group works at is 6% higher.

However, the mean difference in wages might only reflect differences in regional, occupational, or sectoral composition. Although the distribution of these observables is similar in the two groups, we control for them in our estimates. Differences in a few specific factors especially have to be accounted for, as they may be structurally connected to how links are generated in the data. For instance, if social contacts would have no effect on job search, we would expect that people who change jobs more often, are older, or work at larger firms have a higher chance of ending up in the same firm as a former co-worker. We observe significant age differences, as linked workers are on average 4 years older. However, we find that there is no difference in firm size and actually linked hires are the ones who have fewer employers in the observation period. This latter may suggest another beneficial effect of links, longer expected tenure.

Table 2. Summary statistics: job entrants, freshly acquired jobs, and receiving firms

	Subsample			**Nonlinked subgroups**		**Control group**	
	Linked	**Non-linked**	**First spell**	**W/o small firm experience**	**Control group**	**Always nonlinked**	**Nonlinked and linked**
No. of observations	20 227	*944 579*	*135 818*	*147 600*	*661 161*	*645 253*	*15 908*
Log of relative daily earnings	−0.470	*−0.580*	*−0.745*	*−0.468*	*−0.571*	*−0.572*	*−0.552*
Monthly earnings (HUF)	128 511	*108 053*	*80 897*	127 616	*109 264*	*109 245*	*110 046*
Age	38.0	*32.7*	*25.6*	*32.3*	*34.2*	*34.2*	*36.2*
Elementary education	12%	*11%*	21%	*14%*	*9%*	*9%*	13%
Secondary education	63%	*66%*	*63%*	*65%*	*67%*	*67%*	*64%*
Tertiary education	25%	*23%*	*16%*	*22%*	*25%*	25%	*24%*
Central Hungary	32%	*34%*	*34%*	*29%*	*35%*	*35%*	*34%*
Central Transdanubia	12%	12%	*10%*	*15%*	12%	12%	*13%*
Western Transdanubia	9%	*10%*	*8%*	*12%*	*10%*	*10%*	9%
Southern Transdanubia	8%	*7%*	*6%*	*8%*	*8%*	*8%*	8%
Northern Hungary	11%	*9%*	*8%*	10%	*9%*	*9%*	11%
Northern Great Plain	13%	*11%*	*11%*	13%	*11%*	*11%*	12%
Southern Great Plain	12%	*11%*	*10%*	*10%*	*11%*	*11%*	*11%*
Max. number of workplaces	5.39	*5.57*	*2.58*	*4.77*	*6.37*	*6.35*	*7.27*
Occup.-specific experience	70%	*47%*	–	*37%*	*59%*	*59%*	*69%*
Length of prev. unemployment	1.5	*2.2*	–	*2.4*	*2.1*	*2.2*	*1.6*
Manager	7%	*3%*	*2%*	*4%*	*4%*	*4%*	*5%*
White-collar work	6%	6%	6%	*8%*	6%	6%	*5%*
Other white-collar work	16%	*19%*	*24%*	*20%*	*18%*	*18%*	*14%*
Skilled blue-collar work	43%	*40%*	*38%*	*34%*	*41%*	*41%*	*44%*
Unskilled blue-collar work	28%	*31%*	*30%*	*35%*	*30%*	*30%*	*33%*
Relative wage level of firm	0.907	*0.877*	*0.816*	*1.028*	*0.856*	*0.857*	*0.815*
Sector: Agriculture	3%	*2%*	3%	*2%*	*2%*	*2%*	*2%*
Sector: Industry	41%	*34%*	*32%*	*38%*	*34%*	*34%*	*36%*
Sector: Trade and services	56%	*64%*	*65%*	*60%*	*64%*	*64%*	*62%*
Foreign firm	18%	*20%*	*20%*	*27%*	19%	19%	*16%*
Domestic firm	82%	*80%*	*80%*	*73%*	81%	81%	*84%*
Number of employees	39.2	*42.9*	*44.4*	*60.0*	38.8	39.0	*30.8*

Note: The estimation sample consists of starting months of worker employment spells, between 2006 and 2011, which follow a maximum 12-month long job search period. It includes those 15–65 years old, private sector employees, who had less than 15 distinct employment spells in the observation period and did not receive social transfers. The table comprises the average wage outcomes of individuals upon entry to a new firm, demonstrates the personal traits of workers, and contains the characteristics of the workers' new jobs and firms. Indented figures reflect statistically significant differences ($p < 0.05$) from the linked group, according to t-tests.

The descriptive comparison also suggests that contacts reduce the average length of job search by around a half month.[33] In addition, we can observe an essential difference between the linked and nonlinked groups in terms of occupation-specific experience and the ownership of the individuals' firms. In our equations we will control for these properties to account for differences between the two samples.

Regarding the other nonlinked groups, we see that those who spend their first working spell in the estimation period are typically younger, earn less compared to linked hires, a higher share of them works in trade and services, and a lower share is working in the industrial sector. Workers without small firm experience on average earn approximately the same amount as linked workers while having somewhat fewer employment spells in the period and being on average younger than linked workers. Their wage advantage compared to the other nonlinked groups might originate from working at large firms who, especially multinational employers, pay significantly higher wages in Hungary than their domestic counterparts (Köllő, Boza and Balázsi, 2021).

3.5 Results

3.5.1 Main results

To understand the wage gains related to co-worker networks, we start by estimating the model described in Eq. (7), and then we decompose the gains according to Eqs (8–13). Additionally, we calculate a pooled OLS panel regression (Eq. (7) without any fixed effects) as well. The main results are presented in Table 3 and Table 4, of which the former shows the results of the specification in which the variable of interest is interacted with gender.

[33] Workers in the identifying sample for person effects, that is, those who have variation in the proxy for contact presence, make up almost 2% of the estimation sample. On average, they earn 5% more, are 3 years older, and work at 1.5 more workplaces than workers in our estimation sample. These differences mostly come from the requirement of observing more hiring events for these workers.

Table 3. Decomposition of co-worker gains by gender

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Male$	0.0465***	0.0213***	0.0167***	0.0086*	0.0125***	0.0118*	0.0041	−0.0032
	(0.0055)	(0.0051)	(0.0038)	(0.0041)	(0.0034)	(0.0049)	(0.0023)	(0.0034)
$Female$	0.0313***	−0.0024	0.0254***	0.0083	0.0265***	0.0148	−0.0010	−0.0065
	(0.0082)	(0.0096)	(0.0063)	(0.0064)	(0.0055)	(0.0080)	(0.0038)	(0.0057)
N	964 806	501 200	964 806	964 806	943 643	571 441	964 806	964 806
N_i	616 386	197 435	616 386	616 386	616 365	223 021	616 386	616 386
N_j	105 818	61 121	105 818	105 818	84 655	105 778	105 818	105 818
R^2	0.327	0.860	0.204	0.200	0.453	0.612	0.052	0.087

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variable of interest, the proxy for links, is interacted with two gender categories. Additional controls (if the corresponding fixed effects are not included) consist of gender, quadratic age interacted with imputed education, residence, the number of workplaces and job search length in an elastic form, five levels of occupation, two-digit industry codes, firm ownership, a dummy for occupation-specific experience, and dummies for calendar years. All controls are interacted with the indicators for first employment spells and for nonlinked workers without small firm experience. These observations contribute only to the proper estimation of firm effects. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

While the descriptive statistics (Table 2) demonstrate that there is a significant difference in raw earnings between linked and nonlinked entrants, the OLS results indicate that even after controlling for observable characteristics, the difference between the two groups is still present.[34] We can observe a 4.65% wage gain for linked male workers and 3.13% for linked female workers compared to their nonlinked counterparts.

Even if the parameters were not underestimated, the magnitude of such overall wage gains can still be considered meaningful. Studies on the perception of pay increases within firms and the associated emotional reactions have already shown that a change in earnings of around 5% makes a perceived difference to individuals and elicits a positive reaction (Mitra, Tenhiälä and Shaw, 2016). In addition, the net benefits realized naturally vary within the wage distribution: while the overall gain by former co-workers results in 5.000-6.000 HUF per month for those in the lowest wage deciles, it can be much higher in the middle deciles, up to 25.000-35.000 HUF (KSH, 2022d) per month. Such benefits can be of help to those at either the top or the bottom of the wage deciles and may improve the sense of the quality of life. Finally, as we will see later, the magnitude of the wage gains

[34] We ran the OLS specification on the sample used for the TWFE estimates to assess whether sample distortions could account for differences in parameters. We found reasonably similar OLS parameters. The parameter on males turned out to be 0.051 (t = 7.7), and for female workers, we observed a small decrease to 0.028 (t = 2.4). It seems that sample differences account for only limited part of the differences between the OLS and other models, which control for unobserved heterogeneity.

highly differ by occupations and the measured effects can be more substantial for some groups of individuals.

This gross premium is, however, composed of various elements. By estimating the two-way fixed effects model from Eq. (7), we get the wage premium which is attributable to either match selection or referrer-dependent explanations. The $\hat{\theta}_{TWFE}$ parameter is only significant for male workers. Among them, those who have co-worker links upon their arrival at a new workplace earn 2.13% more compared to similar workers, even considering the workers' employment history and other hires of the same firm. As established in Section 3.3.1, due to the lack of variability of the proxy within worker-firm pairs, the above two elements are empirically indistinguishable using the present methodology and data. Therefore, we cannot tell whether this gain is driven by selection into better matches or by effects related to the presence of a referrer and the rent sharing of the firm. However, we know that for male workers, the sum of the two gains result in a positive wage advantage. The magnitude of this estimate is in line with the literature, especially with Dustmann et al. (2016), who measured a 3.3% gain in a model with two-way fixed effects and direct information on referral.

We use the first decomposition to account for the average selection of high-wage individuals and high-wage firms into linked hire events. Based on the parameter $\hat{\psi}_{ind}$, linked male workers earn 1.67% more than nonlinked workers due to their higher individual effects. Accordingly, more than one-third of the overall wage gains originates in linked workers having better unobservable qualities. As a result of the decreased screening costs, due to direct or indirect signaling, the firm may be able to hire better quality workers whose skills would be appreciated by other firms as well in terms of higher wages. Moreover, approximately one-sixth of the male wage difference (0.86%) is explained by the higher premium of firms linked individuals work at when they are linked.[35] This may suggest a certain level of information transmission through the co-worker network or employees obtaining access to better-quality firms that would not be accessible to them in the absence of their connections. For women, this channel, and the parameter $\hat{\psi}_{firm}$ is of the same absolute magnitude, although it is not statistically

[35] Due to limited mobility bias, this parameter might not be significant. In our robustness check, using pre-estimated firm effects, the standard error of $\hat{\psi}_{firm}$ was somewhat higher.

significant.[36] The most dominant element of their overall wage difference is the individual selection term.

Although $\widehat{\psi}_{ind}$ and $\widehat{\psi}_{firm}$ provide some insight into the average difference between linked and nonlinked workers and employers in unobservable wage components, we are interested in how the latent qualities of linked hires compare to their peers or competing firms. To achieve this, we further decompose the average differences in worker and firm effects into within ($\hat{\xi}$) and between ($\widehat{\omega}$) unit components.

The $\widehat{\omega}_{firm}$ parameter shows that those male individuals who ever become linked have somewhat ordinary firm pools. They typically work at firms that provide average or slightly below-average wages. However, if these workers start their new job at companies where they have links, they can easily get into higher premium firms compared to their own work history as the positive parameter $\hat{\xi}_{firm}$ suggests. Concerning linked women, even though they can get into better premium firms compared to their employment histories, on average, this gain is dampened by the fact that they usually work in inferior establishments, resulting in a nonsignificant overall difference.

Parameter $\widehat{\omega}_{ind}$ demonstrates that linked male workers are typically admitted to companies where the worker pool is on the average slightly better than in similar firms without links. However, even compared to this slightly better pool, they are still better in terms of their unobserved qualities. As $\hat{\xi}_{ind}$ suggests, there is a 1.25% advantage in starting wages, attributable to higher person effects of linked male workers compared to the firms' other employees. Similarly, for women, only this within term is dominant, with the between-firm difference being very close to zero. These results are comparable with findings by Hensvik and Skans (2016) and Glitz and Vejlin (2021), who relied on controlling for firm fixed effects, hence capturing the total of presence effects, match selection, and the within-firm selection of individuals. They found 3.6% and 4.6% wage gains, respectively.[37]

[36] That is, we cannot reject the possibility that this component is zero (and hence other components are more dominant). Increased standard errors due to the modest number of within (firm or individual) comparisons certainly contribute to the results.

[37] These parameters should be compared to the sum of $\hat{\theta}_{TWFE}$ and $\hat{\xi}_{ind}$ the overall within person gain in our model, which is around 3.38%.

All things considered, it seems that both male workers and employers profit from co-worker networks. Workers can get into high-wage firms (both on average and in relative terms) through their contacts' information, while firms can find and select better-quality workers (averagely and compared to their own workforce) through relying on referrers. In addition, the creation of better matches and/or the referrer-related gains might benefit both parties. Regarding female workers, the only relevant channels are the selection of better workers into firms, and some, weak sorting into better firms relative to the working history of these women.

Table 4. Decomposition of co-worker gains by occupations — male results

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Manager$	−0.0988***	−0.0030	−0.0775***	−0.0183	−0.0699***	0.0226	−0.0076	−0.0409**
	(0.0259)	(0.0310)	(0.0211)	(0.0142)	(0.0196)	(0.0241)	(0.0092)	(0.0127)
$Skilled_W$	0.0924***	0.0551*	−0.0006	0.0378*	−0.0049	0.0094	0.0044	0.0285
	(0.0274)	(0.0235)	(0.0187)	(0.0179)	(0.0169)	(0.0250)	(0.0101)	(0.0156)
$Unskilled_W$	0.0627***	0.0409*	0.0167	0.0051	0.0153	−0.0113	0.0013	0.0164
	(0.0182)	(0.0183)	(0.0134)	(0.0129)	(0.0120)	(0.0172)	(0.0076)	(0.0108)
$Skilled_B$	0.0584***	0.0228**	0.0217***	0.0140*	0.0128**	0.0123	0.0089**	0.0016
	(0.0070)	(0.0077)	(0.0050)	(0.0055)	(0.0044)	(0.0065)	(0.0034)	(0.0047)
$Unskilled_B$	0.0475***	0.0118	0.0340***	0.0017	0.0326***	0.0161	0.0014	−0.0144*
	(0.0081)	(0.0075)	(0.0048)	(0.0069)	(0.0045)	(0.0085)	(0.0030)	(0.0056)
N	964 806	501 200	964 806	964 806	943 643	571 442	964 806	964 806
N_i	616 386	197 435	616 386	616 386	616 365	223 021	616 386	616 386
N_j	105 818	61 121	105 818	105 818	84 655	105 778	105 818	105 818
R^2	0.327	0.860	0.190	0.200	0.443	0.612	0.052	0.086

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8–13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variable of interest, the proxy for links, is interacted with 10 categories based on gender and five occupational categories: managers, skilled white-collar, unskilled white-collar, skilled blue-collar, and unskilled blue-collar workers. Only the parameters for male workers are presented. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Next, we investigated the effect of links in interaction with gender and occupation. Table 4 comprises the parameters for male workers.[38] Ignoring, for the moment, the managerial category, we observe that both the OLS and the two-way fixed effects parameters are smaller in less prestigious occupations. For the unskilled blue-collar workers, the $\hat{\theta}_{TWFE}$ parameter is not even significant.[39] Regarding this latter group, individual selection is the

[38] Parameters for the female occupation categories coming from the same regression are in Appendix Table A1 while Appendix Table A2 presents the model with only occupation categories not differentiated by gender.

[39] We note that we lose a lot of statistical power when we work with these categories, as the identification of the parameters rely on within-firm and within-person comparisons of workers of a given occupation-gender category only.

most relevant: the differences in worker effects, coming mostly from the within term, account for 72% of the observed average gap. This channel is also important for skilled blue-collar workers, and no other groups, where individual differences (both within and between firms) contribute to almost half of the difference between OLS and two-way fixed effects results. The results suggest that match or presence-related gains are high in occupations where firm-specific or job-specific knowledge is more essential, and, therefore, the match-specific component is a more important determinant of wages. Accordingly, in less demanding categories, we observe selection with respect to general skills and productivity of workers ($\hat{\psi}_{ind}$), which is presumably more important in these occupations.

Selection into higher wage firms seems to be a dominant factor only in the two skilled occupational categories that demand specific qualifications. For skilled blue-collar jobs, the within element of firm selection is dominant, while for skilled white-collar positions, the better firm pool of linked workers drives the results. It also looks like that in these skilled occupations, linked workers get into firms with generally high-wage worker pools. Compared to these pools, skilled blue-collar workers can be somewhat better, while skilled white-collar workers are slightly worse. Finally, managers who get into firms where their former co-workers (mostly subordinates) work are usually employed in firms with lower wages. However, relative to their worse firm pool, they still get into better firms when they are hired with links, but initially earn less than other nonlinked managers. Added together, these elements result in a lower expected wage for linked managers.[40]

The patterns we observed are consistent with the predictions about how employee referral and information transmission should affect the different wage components of linked workers. The observed strong match-specific wage differences (especially for more specialized occupations) and individual selection of better workers (more in general occupations) suggest a strong role of the signaling power of employee referral. On the other hand, selection into higher-wage firms, even if weak, suggests a better opportunity pool provided by contacts through information transmission. In Section 3.5.3, we aim to

[40] The identifying sample for this parameter is quite specific and small as firms need to hire both linked and nonlinked managers.

reinforce this interpretation through alternative specifications focusing on scenarios where one or more of the mechanisms are expected to exert stronger effects.

3.5.2 Exogenous job mobility

A concern that scholars often face in this literature is that employee movements are most often endogenous, especially job-to-job transitions. Since we typically observe job switches among those who had both the opportunity and the intention to move[41], and we cannot see failed attempts to change jobs, we may overestimate the impact of social contacts. Papers focusing on re-employment outcomes through contacts naturally focus on exogenous job loss (e.g., plant closures, mass layoffs[42]), while the ones about wages typically do not make this restriction as (multiple) fixed effects are ought to take care of selection issues. However, as we interpret the selection terms as well, it is worth assessing whether the selection patterns we document may be different when switching jobs is just an option for workers and when they have to find work due to job loss. To do so, we labeled cases where more than one-third of a firm's workforce left within a 3-month long period as exogenous job losses.[43] Then, we interacted our original proxy variable with mobility type (and gender). The results are presented in Table 5.

Table 5. Endogenous and exogenous job mobility

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Endog.$	0.0436*** (0.0058)	0.0162** (0.0055)	0.0188*** (0.0041)	0.0086* (0.0044)	0.0161*** (0.0037)	0.0071 (0.0054)	0.0027 (0.0025)	0.0015 (0.0037)
$Exog.$	0.0620*** (0.0119)	0.0423*** (0.0117)	0.0103 (0.0080)	0.0095 (0.0087)	−0.0003 (0.0072)	0.0366*** (0.0107)	0.0106** (0.0041)	−0.0272*** (0.0073)
N	964 806	501 200	964 806	964 806	943 643	571 442	964 806	964 806
N_i	616 386	197 435	616 386	616 386	616 365	223 021	616 386	616 386
N_j	105 818	61 121	105 818	105 818	84 655	105 778	105 818	105 818
R^2	0.327	0.860	0.203	0.200	0.453	0.612	0.052	0.087

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variable of interest, the proxy for links, is interacted with four categories based on gender and whether the hire was preceded by an exogenous job loss event (Exog.). Only the parameters for male workers are presented. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

[41] Who are more productive, have better skills and characteristics, or for instance more connected.

[42] Such events ensure that workers are forced to switch their jobs, irrespective of their individual characteristics, skills.

[43] We applied this definition only to firms with at least 15 observed employees, and cases when the majority laid off workers did not appear again under the same firm identifier.

The parameters are fairly similar to the ones we have seen before, although the relative importance of some patterns changed. The overall gains of linked male workers are even higher after exogenous job losses compared to conventional movements, with the main difference coming from a substantial and significant increase in $\hat{\theta}_{TWFE}$. This may suggest that referring someone after a job loss happens either when referrers are willing to take more responsibility (e.g., in voluntary monitoring) or when better signals can be provided. Signals, however, seem to be match-specific, instead of those of general skills, as the individual selection term is rather small, with its within component being virtually zero. The creation of better matches is consistent with the finding of Eliason et al. (2017), who show that companies often create new positions to acquire good workers experiencing layoffs. The composite effect $\hat{\psi}_{firm}$ is driven by linked workers getting into higher-wage companies compared to their averagely lower-wage firm pool. The strong within component could suggest the importance of information transmission. However, the inferior pool of the linked workers is puzzling. The overall wage gain of linked workers, nevertheless, may mitigate the long-term disadvantages of displaced individuals (Eliason and Storrie, 2006).

3.5.3 Supplementary specifications

In this section, we aim to provide further suggestive evidence that reinforces our claim that the wage gains we observed are mostly driven by information transmission and/or referral activity — as opposed to, for instance, the random reunion of former co-workers at given firms. To do so, we focus on scenarios where one or more of the (sub-)mechanisms are anticipated to exert stronger effects on wages, for instance, when referrers have larger bargaining power at their employer and expect to observe an increase in the corresponding wage gain components. First, we focus on such cases where referral-related gains should be larger, but information transmission is not necessarily more prevalent. Then, we present two exercises aimed at distinguishing between the presumably small referral-related presence effects and gains originating in match selection. Finally, we focus on job entries where information transmission in itself could be a dominant factor in generating high-paying job opportunities.

First, we are interested in whether the relative position of the former co-worker in the entry firm affects the estimated wage effects. We differentiate three broad levels of occupations: managers, occupations with either vocational or general higher education requirement and those without such prerequisites. We then refine the proxy from the main estimates and create three new ones, showing whether a former colleague is present at the firm in a more demanding, a similar, or a lower requirement occupation. We expect that better peers, that is managers for everyone and skilled positions for unskilled entrants, will have larger bargaining power at the firm and hence may have a larger effect on referral-related wage gains upon entry. Inferior peers may not be able to recommend the applicants at all and moving to places with such contacts are more probably random reunions.[44] Information flows, on the other hand, may be actually less common between different occupational levels.

Table 6 comprises the results of the regression, in which we used the alternative proxies.[45] If the occupation of the links is similar compared to the job entrants, we find gains of a similar magnitude as in our main equations. Firm selection, especially compared to the entrants' work history, is somewhat stronger, suggesting that relevant information could be passed about vacancies that the incumbent worker has experience with. This channel seems negligible for superior peers, as $\hat{\psi}_{firm}$ suggests. However, the individual selection parameter, $\hat{\psi}_{ind}$ is twice as large in the latter scenario than in the baseline case, with the point estimate of $\hat{\theta}_{TWFE}$ being roughly similar. This may reflect the fact that higher-position peers may provide better quality, more reliable signals about the match-specific and general productivity of applicants, enhancing the corresponding aspects of the selection. The effect of inferior peers is insignificant regarding all wage components, being mostly near zero or slightly negative.

[44] At the same time, we do not expect homophily in worker quality to be a stronger factor in the superior or inferior cases.

[45] In the upcoming specifications, similarly as before, we interact our key variables with gender, but report the parameters for only male workers.

Table 6. Heterogeneity of co-worker gains by relative position of contact

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Superior$	0.0572***	0.0210*	0.0331***	0.0030	0.0444***	−0.0073	−0.0113*	0.0104
	(0.0110)	(0.0106)	(0.0075)	(0.0084)	(0.0070)	(0.0110)	(0.0051)	(0.0069)
$Similar$	0.0463***	0.0163**	0.0167***	0.0133**	0.0096**	0.0177**	0.0071**	−0.0044
	(0.0058)	(0.0058)	(0.0040)	(0.0044)	(0.0036)	(0.0055)	(0.0024)	(0.0037)
$Inferior$	−0.0125	−0.0002	−0.0177	0.0054	−0.0142	0.0115	−0.0035	−0.0061
	(0.0127)	(0.0130)	(0.0100)	(0.0083)	(0.0088)	(0.0109)	(0.0054)	(0.0074)
N	964 806	501 200	964 806	964 806	943 643	571 441	964 806	964 806
N_i	616 386	197 435	616 386	616 386	616 365	223 021	616 386	616 386
N_j	105 818	61 121	105 818	105 818	84 655	105 778	105 818	105 818
R^2	0.327	0.860	0.204	0.200	0.453	0.612	0.052	0.087

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variables of interests reflect the presence of contacts in occupational positions that are superior, similar, or inferior compared to the job entrant's occupation in terms of skill requirements. The indicators are interacted with gender, the table presents the coefficients for male workers. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Next, we check whether the tenure of contacts can also affect the wage gains of newcomers similarly to the (relative) occupation of the links at the firm. It seems reasonable that as the working experience of the potential referrers increases, they will establish more trust and bargaining power, so they can generate more reliable signals about the productivity of newcomers. Therefore, it is more likely that they can meaningfully affect the hiring probabilities and wages of the applicants. We also investigate heterogeneity by tie-specific characteristics as well, such as the length of the common working spell and the time that has elapsed between the two encounters of the worker pair. We assume that while a longer co-working spell could enhance the creation of stronger links, the elapsed time between the co-working spells might weaken those links. Therefore, changes in these features can strengthen or moderate the probability of referral and information transmission and might affect the observable wage gains. To estimate the effect of the introduced features, we interacted them with the referral proxy and included these interactions in the same regression.[46]

In line with our expectations, both the tenure of the links and the length of the common working experience enhance the individual and the firm selections, although we see no effect on the $\hat{\theta}_{TWFE}$ parameter (see Table 7). This implies more intense information

[46] We demeaned the three characteristics by their sample means in order to estimate their slopes in their usual range.

transmission to both applicants and firms, but only about general qualities. It also seems that the age of the tie is not a relevant factor regarding the presence of such selections. Nevertheless, the fact that some of the gains are larger when social links tend to be stronger suggests that the selection terms are driven by the participation of peers.

Table 7. Heterogeneity of co-worker gains by link and tie characteristics

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Linked$	0.0382***	0.0202***	0.0173***	0.0008	0.0142***	−0.0002	0.0031	0.0009
	(0.0054)	(0.0054)	(0.0038)	(0.0041)	(0.0034)	(0.0052)	(0.0023)	(0.0034)
$Seniority$	0.0020***	−0.0005	0.0012***	0.0013***	0.0013***	0.0009	−0.0001	0.0004
	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0005)	(0.0002)	(0.0003)
$Since$	−0.0003	−0.0001	−0.0000	−0.0001	−0.0003	0.0001	0.0002	−0.0002
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0004)	(0.0002)	(0.0002)
$Common$	0.0013***	0.0000	0.0006*	0.0007**	0.0005*	0.0005	0.0001	0.0003
	(0.0003)	(0.0004)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0001)	(0.0002)
N	964 806	501 200	964 806	964 806	943 643	571 441	964 806	964 806
N_i	616 386	197 435	616 386	616 386	616 365	223 021	616 386	616 386
N_j	105 818	61 121	105 818	105 818	84 655	105 778	105 818	105 818
R^2	0.345	0.860	0.203	0.232	0.453	0.628	0.069	0.059

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variable of interest is the proxy for links, which is interacted with both gender and contact or tie-related characteristics. Seniority refers to the tenure of the links with the longest working spell at the entry firm. Variable Since indicates the time elapsed since the latest common working spell with the link(s). Common denotes the length of the longest common co-working spell in the past. The coefficients show the effect of positive deviation in months of all three characteristics from their mean value among linked male workers. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

In an attempt to capture the relevance of referral gains that depend on the continuous presence of the referrer, we leverage that peers may leave the firm earlier than their new, referred colleagues. Although we believe that expected voluntary monitoring of the peer and knowledge sharing are already evaluated in starting wages, the separation of the referrer will probably weaken the bargaining position of the worker in the firm due to the loss of productivity-enhancing features, reducing the wage advantage in the long run. Even if this does not lead to a decrease in wages, it may impede further wage increase and dissolve the advantage of referred workers over market hires. We estimate and plot the two-way fixed effects wage gains over the first three years at the firm for those who at the given time still have their former referrer at the firm and those whose peers have left by the time.[47]

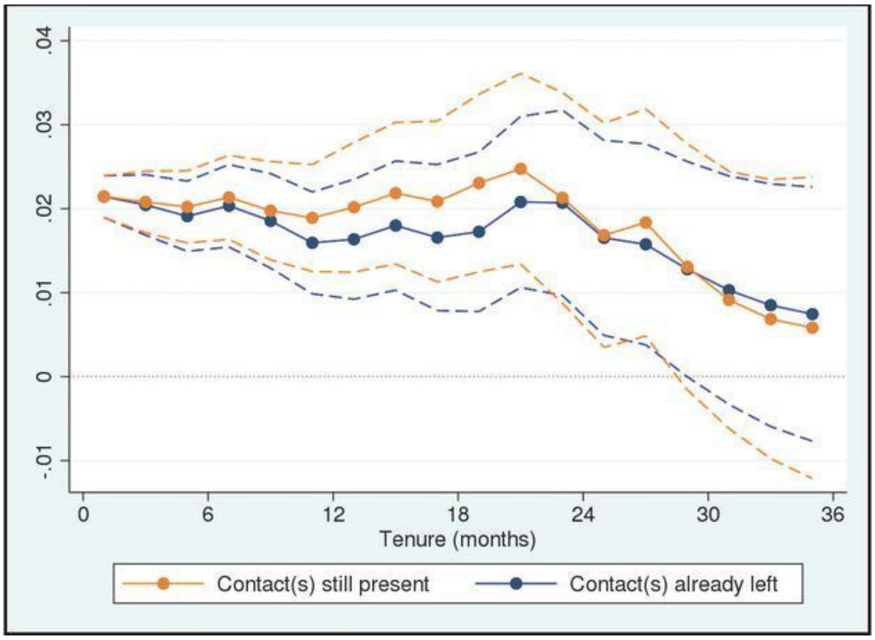

Figure 2. Wage-tenure profiles and referrer presence

Note: The figure displays point estimates and 95% confidence intervals for two sets of specifications. The first set uses the proxy for contact presence upon entry, while in the other one the same indicator is set to zero if the original contact(s) left the firm by the given month. Both graphs present the parameter of eighteen separate regressions on the logarithm of daily earnings of male workers in the given month, for all odd months of the first three year of the employment spells. Female workers are included in the specifications, with a constant gender difference being assumed. Controlling for fixed effects is achieved by including pre-estimated individual and firm effects from the entry month equation. Additional controls are the same as listed in Table 3. Standard errors used for the confidence intervals are clustered at both firm level and individual level.

We observe that referral gains, proposed by and documented in (among others) Jovanovic (1979), Simon and Warner (1992) and Dustmann et al. (2016), disappear over time as actual productivity of all workers gets revealed, and workers of inferior quality leave the firm (Figure 2). However, there is a modest, although statistically insignificant difference in the point estimates of the gain-tenure path depending on the presence of the original contact(s). For those workers who do not have their peers present anymore, gains start to dissolve earlier, but even this difference disappears over time.

As an additional endeavour to separate the gains related to referrer presence from the match-specific ones, we interacted occupation-specific skill variables with the proxy of links. We assumed that regarding certain occupations, the role of monitoring, knowledge

[47] We match on the pre-estimated individual and firm effects from the equations to enforce comparing similar individuals and firms, while also maintaining the feasibility.

sharing and various off-cv elements will be more valuable. Therefore, larger referral gains could be observed in occupations where such related skills are dominant. For instance, knowledge sharing may have a larger role and be more valuated by the employer in jobs requiring more independence. We obtained various skill and ability measures from the O*NET 24.2 Database by the U.S. Department of Labor, Employment and Training Administration.[48] However, we could not find any skill requirement that would significantly alter the $\hat{\theta}_{TWFE}$ parameter in those occupations, which demand certain unobserved skills (see Table A3 in Appendix). This may suggest that the gains we would like to measure are rather modest or cannot be effectively captured by occupation-related skills. The signs of the parameters, however, have a pattern similar to what we observed in the specification with occupational categories (Table 4). The interaction terms are positive for job traits that reflect the need for specific knowledge (like innovation or analytical thinking), and negative for those skills which can be considered more generally applicable (like stamina and stress tolerance). While these exercises are not conclusive, they suggest match selection as the main driver of $\hat{\theta}_{TWFE}$.

In our final exercise, in contrast with the previously introduced cases, we look at specific scenarios where the presence of actual recommendation is unlikely. To do so, we incorporate two additional indicators in the general model from Eq. (7). The first one indicates the presence of those nonlinked individuals who have at least one former firm in common with the applicant but did not share a common working spell together at that firm, hence did not have the chance to make actual personal contact. The other indicator marks the presence of second links in the co-worker network. These individuals are former co-workers of the applicants' previous peers. For this dummy, we considered only those second links who did not share a former, common firm with the applicant.[49] While information transmission about vacancies across this network is rather reasonable, actual recommendation is unlikely due to the lack of these links' personal experience and knowledge about the applicant. We expect to observe negligible recommendation-related

[48] Although the database is based on US occupation surveys, the scores could provide some insights for Hungary as well. Among others, Handel (2012) that US and European survey-based occupation measures typically lead to comparable results. Using Hungarian job descriptions made by experts Életpálya (2020) yielded similar results.
[49] Workers whom one shared a common workplace with, but at a different time tend to mechanically become second links.

gains from the presence of both second links and of those whose firm histories overlap—but not their employment spells.[50]

Table 8. Gains from links, second links, and similar workers

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Linked$	0.0484***	0.0194***	0.0188***	0.0102*	0.0152***	0.0141**	0.0036	−0.0039
	(0.0056)	(0.0055)	(0.0038)	(0.0042)	(0.0035)	(0.0052)	(0.0023)	(0.0035)
$Similar$	0.0131**	0.0072	0.0150***	−0.0091*	0.0194***	0.0013	−0.0044*	−0.0104**
	(0.0050)	(0.0050)	(0.0035)	(0.0041)	(0.0032)	(0.0047)	(0.0023)	(0.0034)
$Second$	0.0489**	0.0125	0.0062	0.0302**	−0.0074	0.0238	0.0136*	0.0064
	(0.0157)	(0.0173)	(0.0104)	(0.0111)	(0.0098)	(0.0142)	(0.0056)	(0.0091)
N	938 791	479 919	938 791	938 791	917 835	550 362	938 791	938 791
N_i	603 975	189 756	603 975	603 975	603 955	215 546	603 975	603 975
N_j	105 061	60 367	105 061	105 061	84 105	105 022	105 061	105 061
R^2	0.326	0.860	0.199	0.198	0.452	0.611	0.050	0.088

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our parameters of interest are estimated with distinct indicators for the presence of former co-worker links, workers with similar working histories (those who share a common, former workplace with applicants), and second links (the former peers of the job-entrants' former co-workers who did not fall into the similar working history group). The indicators marked in the table as Linked, Similar and Second respectively and were interacted with gender. Results for male workers are presented. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

The results presented in Table 8 are only partially in line with our expectations. Concerning those who got workers at their new firms with similar working histories, we cannot observe a significant $\hat{\theta}_{TWFE}$, which is reassuring, as this parameter is ought to capture mostly referral-related wage gains. However, we see individual selection which is almost as strong as the one in our baseline case. This is somewhat unexpected, but not unreasonable. The similarity in working history might function as an indirect signal for the productivity of the entrant worker, as the employers might assume homophily in terms of skills between those workers who have similar working histories. A more puzzling finding is the presence of a significant negative firm selection, which as $\hat{\omega}_{firm}$ suggests, can be attributed to the fact that these individuals typically work at low-paying firms. Regarding those individuals who have only second links upon entry, we observe more consistent patterns. As expected, we see no recommendation-related individual or match

[50] As we cannot see all contacts (due to having a 50% sample), we cannot make sure that there are no first-order contacts at the new workplace. This will lead to one-sided misclassification between the groups with first links and only second links, attenuating the difference between the two set of estimated parameters, as effects estimated for second-order contacts would be contaminated by the effect of unobserved first contacts.

selections. On the other hand, a rather strong selection into high-wage firms is associated with these weak ties. This might suggest that there is indeed actual information transmission about high-paying jobs through the extended networks of co-workers.

The introduced specifications aimed to provide additional evidence that our parameters are driven by nonrandom sorting of workers and capture the effects of information transmission and referral mechanisms. When we utilized scenarios that would theoretically imply the increase of referral-related gains (such as the better position of peers at the applicants' new firm) or the dominance of information transmission-related gains (e.g., the presence of second links), our results followed the patterns we anticipated. However, we failed to infer a conclusion about the relative importance of gains strictly dependent on the presence of the referrer versus match selections already present at hiring. This could be the focus of future research.

3.6 Discussion

Taken together, our findings suggest that the reliance on links is beneficial for both firms and workers. Regardless of whether it is driven by referral or just information transmission, the use of contacts induces the selection of better workers into firms and selection of workers into better firms. What we deem important to highlight is the fact that these aggregate selections predominantly happen within units. That is, on the one hand, people get into superior firms compared to their working history. This way these mechanisms might contribute to the individuals' upward mobility. On the other hand, firms can enhance the quality of their worker pools through referral as referred hires are generally better workers compared to the firm's own average worker pool. In addition to these one-sided advantages, the effect on the average match quality is beneficial for both parties. By increasing the overall productivity in the labor market, referral can be socially desirable.

Nevertheless, the effect on individuals who cannot rely on social links should be considered as well. If workers with worse career prospects also have inferior co-worker networks, their initial disadvantages will be magnified by being crowded out from high-paying firms. Being trapped in inferior workplaces may hinder the development of network quality, reinforcing the path dependence in career paths. Referral may also lead to the increase of sorting inequality if it helps allocating the best workers to the best firms as shown by Eliason et al. (2019). While the direct assessment of assortativity was beyond the scope of this study, the between terms of our detailed decomposition suggest a weak sorting pattern: firms relying on referral generally employ slightly better than average quality workers, while on average they themselves are high-paying firms. Thus, the presence of productivity gains from the generation of better matches could be counterbalanced by the crowding-out effect of disadvantaged workers and the effect on sorting inequality, resulting in unclear implications about overall welfare.

4. Inter-firm mobility and gendered co-worker effects[51]

4.1 Introduction

In the last decades, a major change has been observed regarding the role of women in the economy. Female employment has steadily rose, accompanied by closing gender gaps in terms of wages as well. Due to the improvements in human capital accumulation of women, the introduction of antidiscrimination legislation, changing firm policies and hiring practices, entry barriers of women are considerably reduced in high-paying jobs and male-dominated occupations. However, these convergence processes have recently shown signs of a slowdown, even though persistent differences are still present between men and women with respect to labor market outcomes. Barriers of inter- and intra-firm occupational/wage mobility related to the phenomena of glass door (Hassink and Russo, 2010), glass ceiling and sticky floor (Arulampalam, Booth and Bryan, 2007) are still key contributors to the uneven opportunities of women alongside the prevailing social norms, gender stereotypes, discrimination or the challenges of work-life balance (Cortes and Pan, 2018; Das and Kotikula, 2019). However, gender differences in network composition and the utilization of social ties may also have a central role in aggravating inequality (Blommaert et al., 2020, Lindenlaub and Prummer, 2016; Zeltzer, 2020), as social ties proven to be essential in determining individual labor market outcomes (Holzer, 1988; Granovetter, 2019).

The objective of this chapter is to investigate the contribution of social networks to gender inequalities, by focusing on a specific group of social ties, namely former co-workers. To do so, the research presents estimates that address the differential effects of networks by gender on job finding probabilities and on the chance of promoting upward mobility through job switches. The analysis relies on a matched employer-employee panel from Hungary covering 50% of the entire population and use overlapping employment spells at the same companies as a proxy for social ties.

Similar studies, with respect to data and approach, have already demonstrated that workers are more likely to get their new jobs at firms where their acquaintances are employed (Saygin, Weber and Weynandt, 2021), and also that co-workers affect wages to a meaningful extent (Boza and Ilyés, 2020; Glitz and Vejlin, 2021). Some of the results

[51] This chapter is based on the article “Inter-firm mobility and gendered co-worker effects” that is currently under review at the European Sociological Review. It was a solo project, which was supported by the Hungarian Scientific Research Fund (OTKA) [K-135195].

even shed light on gender differences: women benefit less from their former co-workers in terms of wages (Boza and Ilyés, 2020) and they receive a lower amount of work-related information from their employed former co-workers (Saygin, Weber and Weynandt, 2021). However, none of the previous studies focused exclusively on or addressed comprehensively the differences in the measured effects by gender.

In an attempt to fill the above gap in the literature, by linking insights on gender inequality and the gendered aspects of social networks, this research makes two major contributions. First, besides showing that social ties affect the hiring chances of individuals by utilizing firm closures as exogenous variations, the chapter underlines the influential effect of the characteristics of the job seekers and contacts, most notably gender and occupation, in getting a job. We demonstrate that men benefit more from the help of their former co-worker ties, and also, that gender homophily in network effects is only present due to already established gender segregation patterns. Second, by utilizing the cases of inter-firm mobility the study provides evidence that while informal ties are coupled with increased chances for career advancement (measured by better position in the general or within-firm employee wage distribution, better firm premia), the benefits are unequally distributed both across and within genders. The results reflect a duality in network effects: besides enhancing the (otherwise) limited opportunities of women in worse positions, social ties contribute to the preservation of existing gender differences at the top segments of the labor market.

When assessing the impact of former co-worker ties on finding a new job, we focus on the universe of those workers who lost their job due to firm closures. Doing so, the potential sources of endogeneity related to the selectivity of workers according to mobility decisions can be eliminated. As information on job applications is not available in our dataset, similarly to Saygin, Weber and Weynandt (2021) we define separate sets of potential hiring firms (i.e. target firms) to the workers of each closing firm. The set of target firms is identified as the collection of those workplaces, where the co-displaced workers' former co-workers work, at the time of the closure event. We then, by using fixed effect linear regression models investigate whether individuals are more likely to be hired by their former peers' firms after their job loss (relative to other companies within the elaborated set of firms). The covariate of interest will be an indicator variable showing if job seekers have any former co-workers at given target firms. As mobility alongside given firm routes may be more probable (due to for instance, the regional proximity of firms or their similar skill requirements), we apply sending firm-target firm fixed effects. This way, we can avoid overestimating our parameters, by capturing the increased number of movements along given firm pathways (and thus, the higher chance of worker reunions). Thus, when identifying the parameter, we will essentially compare those individuals who lost their jobs in the same closure events for getting into the same target firms, with or without contacts there.

The results, on the one hand, reinforce the findings of the existing literature, namely that former co-workers affect the hiring chances of individuals to a substantial extent. The benefits are more substantial for male workers, even after controlling for closing firm-target firm mobility routes, suggesting the presence of less network-related help for women. On the other hand, we also demonstrate that the effects are influenced by both the gender of the job seekers and of the contacts – although not necessarily directly. In general, we can observe traces of homophily in network effects: same-gender contacts provide higher benefits for both men and women. Such patterns, however, disappear once we control for typical worker flows between (closing and targets) firms, suggesting that the observed homophily is essentially a byproduct of gender segregation. Investigating the heterogeneity of results by the occupational position of contacts, we also show that while for men essentially all types of contacts increase the hiring chances, for women contacts in managerial or higher occupational positions matter the most. This implies that contacts in better occupations (both in absolute and relative terms) can be essential assets for women if they are present, and contrary to the theoretical literature (Lin, 1999), women can exploit such ties in the job search at least as effectively as men.

The second part of the analysis focuses on job entries to new firms (either preceded by closure events or not) and demonstrates that career advancement through job mobility is also affected by the help of informal ties. Our outcome variables aim to capture different aspects of upward mobility, by contrasting the firm- and job-related outcomes of individuals at their new firm to the ones of their previous workplace. According to our results, workers with links are more likely to acquire better positions in the general and within-firm wage distribution, and their chance of getting into higher premium firms is also better. The presence of contacts also associates with increased chance for getting into firms with lower within-firm gender wage differences. However, the returns to social ties are of different magnitude for men and women, and are unequally distributed within both genders. Men benefit from contacts irrespective of their former labor market situation (in terms of wages and firm quality), while women in the best positions are excluded from such gains. By pointing out the relevance of informal ties in compensating the limited opportunities of women at the bottom segments of the labor market, and maintaining the existing gender differences at the top, the research contributes to the current discussions about gender inequalities and labor market networks.

4.2 Background

4.2.1 Gender segregation in labor market networks

A great body of literature addresses the importance of social networks in structuring individual labor market opportunities. Informal ties, through information delivery, referral and signaling, can enhance the individuals' job finding chances (Holzer, 1988; Cingano and Rosolia, 2012; Saygin, Weber and Weynandt, 2021). Moreover, they can contribute to the creation of better employer-employee matches (Dustmann *et al.,* 2016; Boza and Ilyés, 2020) which typically couples with longer tenure, lower turnover and higher wages (Hensvik and Skans, 2016; Glitz and Vejlin, 2021). The benefits acquired through informal search, however, are highly dependent on the characteristics of the individuals and their network quality. Gender can be an essential factor, as it influences both network composition and the amount of resources available through social ties (Woehler *et al.,* 2021).

The related literature reveals that women (compared to men) are more likely to have less diverse and less powerful social networks (Moore, 1990; Ibarra, 1993; Greguletz, Diehl and Kreutzer, 2019; Blommaert *et al.*, 2020), which include stronger and expressive ties to a higher extent (Marsden, 1987; Ibarra, 1993). One set of explanations for these findings is related to the prevailing gender segregation patterns and the structural barriers of women. As women are over-represented in low-wage jobs and they have limited access to the top tiers of occupations, their professional networks may comprise less high-status contacts with greater authority (Ibarra, 1993; McGuire, 2000; McDonald, 2011; Blommaert *et al.*, 2020). The relative lack of such contacts, who are typically endowed with more and a greater variety of social resources (Lin, 1999; Yakubovich, 2005), might lower the chances of women for receiving network-related instrumental benefits and acquiring better labor market outcomes (McGuire, 1999; Lalanne and Seabright, 2016). In addition, the composition and diversity of the professional networks of women can be also negatively affected by their career interruptions and heavy involvement in domestic responsibilities and child-care duties (Moore, 1990).

Another set of explanations, which accounts for gender differences in network composition, is related to the general characteristics of network building and the pre-existing gender-specific preferences toward the accumulation of social contacts. Homophily – the tendency of people to link up with similar others (McPherson, Smith-Lovin and Cook, 2001) – is a widely acknowledged principle in network formation, which through the increased creation of same-sex or status-similar relationships might hinder network diversity. Additionally, gender-specific contact selection strategies might also account for such disparities. According to Friebel et al. (2017) women are pickier when it comes to establishing new relationships ("differential selectivity") and less responsive to the likely payoffs of their partnership investments ("differential opportunism"). As a result, they might allocate more of their resources to already existing links and invest less in new ties. Therefore, the fraction of weak ties could be lower in their network. The scarcity of such links, however, entails more dense and cohesive network structures, which offer restricted opportunities for channeling in new information (Granovetter, 1973; Yakubovich, 2005) and are detrimental for career advancement (Lutter, 2015; Erickson, 2017).

Finally, gender differences are also present in the amount of help received by informal contacts and its likely benefits. According to Obukhova and Lan (2013) and Trimble O'Connor (2013), access to network resources *per se* does not guarantee informal advantages, unless if these resources are mobilized. However, as women are somewhat worse in the activation of social capital (Lin, 1999) they might attain lower gains compared to men. Another stream of research shows the other side of the coin by emphasizing the role of contacts in determining who they are willing to help (if they do) and highlighting the function of ascriptive characteristics. The status characteristics theory (Berger *et al.*, 1977) proposes that group membership based on salient ascriptive features might associate with status beliefs, which are unconsciously involved in the overall evaluation of individuals. As women tend to occupy worse positions in the labor market, they might be considered less resourceful and subordinate compared to men, resulting in a lower amount of help received (McGuire, 2002).

While differences in ascriptive features might raise discrimination, similarity might serve as a basis for help as it is suggested by homophily and the theory of social closure (Weber, 2018; Tilly, 2020). Similarity can inbreed trust, affinity and moral obligations, which might stimulate individuals to engage in opportunity hoarding and reserve their help for those people who are alike (McDonald, 2011; Zhou, 2019).[52] However, the support of same-sex or status-similar contacts is not necessarily beneficial equally for both genders: such ties can further increase the segregation of women into lower-paying, female-dominated occupations and sectors (Hanson and Pratt, 1991; Mencken and Winfield, 2000). In contrast, opposite-sex links might offer better opportunities as men are more likely to work in influential positions, occupy central places in inter-organizational networks, or be involved in old boy networks (Ibarra, 1993; Durbin, 2011).[53]

4.2.2 Empirical evidence

Empirical research on job information flows and referrals supports the proposed implications of the introduced theories. A great number of studies suggested that men receive more and better job offers (McDonald, Lin and Ao, 2009; Glitz, 2017; Saygin, Weber and Weynandt, 2021). While survey-based studies typically used the number of job leads to measure information flow through networks, research using administrative data needed to utilize alternative strategies due to the lack of information on job search methods and social networks. Saygin, Weber and Weynandt (2021) and Glitz (2017), by using Austrian and German administrative data, investigated the impact of former co-workers on the reemployment probabilities of displaced workers. They identified contacts based on overlapping employment spells at the same firms, and used the share of employed former co-workers to measure the capacity of co-worker networks to provide job information. Both studies indicated that a given increase in the co-worker share is associated with higher re-employment rates. Saygin, Weber and Weynandt (2021) also showed that men receive greater benefits, which is mostly attributable to differences in network properties by gender. Regarding the gender of contacts, they found that female ties are slightly less important for male job seekers, while women benefit equally from all their contacts. In contrast, the results of Belliveau (2005) suggested that male ties (advisors) can provide greater benefits for women.[54]

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[52] It can also be the case that individuals in similar situation better know how help one another (Trimble O'Connor, 2013).
[53] Some papers of the literature on scientific success provided evidence on the role of mentors in the early career development of young researchers (Blau *et al.*, 2010). However, such studies yielded mixed results regarding the effect of the mentors' gender. Some studies did not find difference between same- and opposite-sex mentors (Lin *et al.*, 2021), while others found better returns to opposite-gender mentors (AlShebli, Makovi and Rahwan, 2020).
[54] However, females often lack sponsorship and powerful mentors in general (Linehan and Scullion, 2008).

Referral is also influenced by the gender of the individuals and their contacts. Using longitudinal US firm-level data, Brown, Setren and Topa (2016) showed that gender homophily is substantial in referral networks (similar results were found in Obukhova & Kleinbaum (2020)). Beaman, Keleher and Magruder (2018), by using field experiment in Malawi found that men tend to refer fewer female candidates, even if eligible women candidates are present in their network. In contrast, women refer both genders almost equally, but their female candidates typically do not qualify for the jobs. A laboratory experiment by Beugnot and Peterlé (2020) presented somewhat different results, which indicated that women tend to favor women in their referral choice, while men do not pay much significance to gender when providing referral.

The most similar study in terms of data and approach is Saygin, Weber and Weynandt (2021). They investigated the effect of co-worker referrals on job finding, by focusing on firm closures and identifying former co-worker relationships with overlapping work histories. As they lack information on the job applications of workers, they used the set of those firms as a proxy for local labor markets (and employment opportunities) where the co-displaced workers' former co-workers worked at the time of closure. Their results showed that displaced workers of both genders are more likely to get their new jobs at firms where their acquaintances work, with their benefits being of the same extent. In our empirical application we will utilize the same strategy as they do, with minor modifications.

This aim of this chapter is to investigate the contribution of social ties to labor market gender inequalities. First, by utilizing firm closures and a proxy for former co-workers, the results reinforce the findings that job seekers are more likely find their new jobs at the workplaces of their former co-workers. Besides, we provide evidence that the measured effects are stronger for men, and show that any observed heterogeneity in network effects by the gender of contacts may only reflect patterns of gender segregation. Then, in the second part of the study we address gender differences in contact effects regarding upward-mobility by comparing job-entries to new firms with and without co-workers. As invisible barriers still hinder the within-firm career advances of women and their external hiring to better positions, it may be assumed that the contribution of contacts (for instance their referral) might be of great help in removing such obstacles. However, considering the introduced gender differences in network composition and network use, it seems more likely that such benefits will be moderate, if not even insignificant for women. The results show that both genders have a higher chance for getting into better firms and jobs through their contacts, but the gains are unevenly distributed both within and across genders. By exhibiting the presence of such differences, the research provides useful insights on both sides of the same coin: reflecting on the negative and positive aspects of networks in the labor market.

4.3 Data

The analysis is based on a Hungarian matched employer-employee dataset from the Databank of the Centre for Economic and Regional Studies. The longitudinal data comprises the work histories of a randomly drew 50 percent of the population on a monthly basis, between 2003 and 2011. Raw data (information on individual-employer work spells) were collected and combined from the Pension Directorate, the Tax Office, the Health Insurance Fund, the Office of Education, and the Public Employment Service. Then, it was restructured into a monthly panel by the Databank based on the employment status of individuals on every 15th day a given month. The data provides information on monthly earnings and working days, employer-specific and occupation-related variables for the two highest-paying working spells ("identities") of the individuals in each month, and it features health expenditures and social transfers as well. As the panel was constituted from administrative records, it does not contain information on neither job finding methods nor personal networks. However, the unique individual and employer identifiers in the dataset enables us to find employees who have worked for the same company at the same time, which provides a good basis for identifying co-worker networks.

4.3.1 Definitions of co-workers, firm closures and target firms

Former co-workers were defined as those individuals who shared common work experience at relatively small companies[55] (with max. 50 employees) for minimum 6 month. Restrictions on firm size and minimum length of service were necessary to increase the chance that given pairs of workers have knew each other. As in the earlier years of the dataset the chance of having former co-workers is lower, the first four years were used only as a connection forming period, and not for estimation.

Similarly to the literature (Fink, Kalkbrenner and Weber, 2014; Saygin, Weber and Weynandt, 2021), firm closures were identified by the observed disappearance of the total

[55] Both identities of the individuals could be sources of connections.

workforce before the last month of the data. Only sufficiently large firms (which never had less than 10 employees) were defined as closing firms to enhance reliability. Job mobility related to mergers and administrative changes in firm identifiers were separated and excluded. To rule out the effects of potential supply and demand shocks generated by firm closures on job finding chances, we excluded cases where more than 40% of the jointly dismissed workers started a new job at the same firm. As the process of closure usually takes time and displacements happens gradually, the last 6 observed months of the firms were considered as *closure periods*.

For the job finding analysis, the set of *firm alternatives* (or *option pool of target firms*) was identified on the basis of closing firm level as the union of those workplaces where any of the laid-off workers' former co-workers worked during the closure period. From these alternatives, we linked to the displaced workers only those firms that employed any contacts in the month of their displacement. The average number of firm alternatives was 134, which is fairly similar to Saygin, Weber and Weynandt (2021) in magnitude.[56] The detailed description of this companies can be found in Appendix Table B1.

4.3.2 Sub-sample and estimation datasets

The estimates use job changes of non-disabled workers aged 15-65 with at least 6 months of previous small firm experience.[57] To avoid capturing the effects of firm-specific knowledge on the investigated outcomes, cases of re-entries to former workplaces were excluded from the list of job entries. Those instances were also removed, when the individuals held multiple jobs during the last month of their closing firm spell.

Based on this sample, two estimation datasets have been generated: one for the estimation of hiring probabilities, and another for investigating post-hiring outcomes. The hiring estimates focus on those workers who lost their jobs during the formerly identified closure periods between 2007 and 2011[58], and were already employed before the closure periods

[56] Since the main objective is to measure the effects of contacts, the estimates include only those job seekers who had at least 2 available firm alternatives, one of them with a former co-worker present and one without it.

[57] Since the definition of co-worker links demands these criteria, it had to be adapted to the whole sample as well.

[58] By definition, those who experience displacement in 2011 will have a shorter time to find a job. If the gender composition or the composition of having contacts is different in this subgroup compared to those who lost their jobs earlier, the OLS parameters would be contaminated with these selection effects. In the OLS estimates, we aim to capture these effects with year dummies. While in models with fixed effects, such issues are non-relevant as comparisons are only made within cohorts of similar composition w.r.t gender and contacts. Nevertheless, Appendix Table B2 and B3 contains the results, where we omit firm

started. In the estimation dataset, each of the selected individual observations will appear as much times, as many target firms were identified for the individuals. Thus, the observations will be individual-sending firm-target firm triads. The co-variate of interest will be a dummy indicating whether individuals have at least one former co-worker (with a min. 1 month tenure) at a given target firm, at the month of displacement.[59]

The dataset used for upward mobility estimates covers all job entries between 2007 and 2011: both entries preceded by closures and job-to-job transitions. Although we can only allow for the non-random sorting of individuals to given firms if we focus on closures, it is job-to-job movements that typically characterize the labor market mobility of individuals. Since labor market outcomes in the new firms are clearly influenced by the type of mobility prior to entry, it is vital to examine both cases. Especially, as they associate with different life situations and affect the use of social ties differently. Of all the mobility events, only those were considered that originated in and were directed towards employment in the private sector. Co-mobility events were excluded from the sample as they are typically associated with wage benefits (Marx and Timmermans, 2017). Thus, their inclusion could have affected wage-related outcomes. The proxy of informal help marks those cases, when the job seekers have found their new job at their former co-workers' workplace, with the co-workers having started there at least 1 month earlier.

4.3.3 Descriptive statistics

Table 9 comprises the details of the estimation dataset used for the hiring analysis. In this sample, which covers only those workers who lost their jobs due to firm closures, we cannot see much difference by gender in terms of age, daily wages, and the position of individuals in the wage distribution (captured by the general and within-firm average wage decile of individuals). However, the ratio of women with secondary education is lower than the one of men, and the share of those women with tertiary education is higher.

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closures of 2011. While we lose 1/5 of our observations, these results are qualitatively the same as those presented in the main text.

[59] Connections could form at the closing firms as well, but only those co-worker pairs were considered valid connections whose members have started to work together before the start of the closure period.

Table 9. Characteristics of the hiring sample

	All	Female	Male
Individual characteristics			
Elementary education	0.11	0.11	0.11
Secondary education	0.73	*0.70*	*0.74*
Tertiary education	0.16	*0.20*	*0.15*
Av. age	38.98	39.01	38.96
Av. person FE	-0.13	*-0.19*	*-0.10*
Former firms			
Agriculture	0.01	0.01	0.01
Industry	0.24	*0.19*	*0.26*
Trade and Services	0.42	*0.54*	*0.38*
Education, Social, Other	0.33	*0.26*	*0.35*
Av. firm FE	-0.29	*-0.27*	*-0.30*
Controlled wage gap (f-m)	0.03	0.02	0.03
Former jobs			
Manager	0.06	*0.08*	*0.06*
White-collar worker	0.10	*0.23*	*0.05*
Blue-collar worker	0.83	*0.70*	*0.89*
Daily wage (HUF)	3385	3389	3384
Av. population wage decile	4.13	4.10	4.14
Av. within-firm wage decile	5.04	*4.94*	*5.08*
Job finding outcome			
Did not find a job	0.11	*0.13*	*0.11*
Job finding outside the defined firm set	0.69	0.69	0.69
Job finding in the defined firm set	0.20	0.19	0.20
Job finding without co-workers	0.11	0.11	0.11
Job finding with co-worker(s)	0.09	0.08	0.09
Job finding with female contacts	0.02	*0.04*	*0.01*
Job finding with male contacts	0.05	*0.02*	*0.06*
Job finding with both types of contacts	0.02	0.02	0.01
Observation number	10 315	2 971	7 344

Note: The table comprises the features of the sample generated for the job finding analysis. Statistics related to the former jobs of individuals were calculated based on the workers' last month at their sending firms. Person and firm effects were retrieved from the model described in Eq. (B1) in the Appendix. The controlled within-firm gender difference was calculated as the difference in residuals (coming from Eq. (B1)) within firms among genders. The average wage decile was computed based on the wages of the whole sample at a given month. Indented figures reflect statistically significant differences ($p < 0.05$) between men and women, according to two-sided t-tests.

When focusing on the upward mobility sample and comparing the job entries of those with former co-workers present and those without, we can observe essential differences (Table 10). Individuals with links are slightly older, their unobserved individual skills are somewhat better and they typically have better firm-related and job-related characteristics at both the sending and target firms. The ratio of those workers who acquired better outcomes after job mobility is higher among linked workers (according to all the investigated aspects of upward mobility). These patterns are present for both genders, although, the outcomes of women and their uncontrolled benefits through social ties are on the average worse.

Table 10. Characteristics of the upward mobility sample

	Total sample		Women		Men	
	Without link	With link	Without link	With link	Without link	With link
Individual characteristics						
Elementary education	*0.08*	*0.12*	*0.04*	*0.07*	*0.10*	*0.14*
Secondary education	*0.64*	*0.63*	*0.64*	*0.62*	0.65	0.64
Tertiary education	*0.21*	*0.18*	*0.22*	*0.21*	*0.19*	*0.17*
Unknown education	*0.07*	*0.07*	0.10	0.10	*0.06*	*0.05*
Av. age	*35.83*	*37.62*	*35.58*	*38.35*	*35.98*	*37.24*
Av. person FE	*-0.14*	*-0.12*	-0.19	-0.19	*-0.11*	*-0.08*
Former firms and jobs						
Daily wage	*3648*	*3780*	*3499*	*3641*	*3740*	*3854*
Av. wage decile	*4.37*	*4.56*	*4.20*	*4.37*	*4.46*	*4.66*
Av. within-firm wage decile	*4.08*	*4.35*	*3.79*	*4.04*	*4.26*	*4.51*
Av. firm FE	*-0.17*	*-0.15*	*-0.17*	*-0.15*	*-0.17*	*-0.16*
Av. firm size	1156	1137	1 295	1241	1073	1083
Controlled wage gap (f-m)	*0.01*	*0.02*	*0.01*	*0.03*	*0.01*	*0.02*
Av. spell number	*4.57*	*4.24*	*4.30*	*3.95*	*4.73*	*4.40*
Subsequent firms and jobs						
Daily wage	*3631*	*3948*	*3511*	*3756*	*3704*	*4051*
Av. wage decile	*4.39*	*4.75*	*4.26*	*4.53*	*4.47*	*4.86*
Av. within-firm wage decile	*3.92*	*4.49*	*3.65*	*4.17*	*4.09*	*4.66*
Av. firm FE	*-0.17*	*-0.12*	*-0.16*	*-0.12*	*-0.17*	*-0.13*
Av. firm size	*1236*	*2647*	*1420*	*3612*	*1125*	*2138*
Controlled wage gap (f-m)	*0.01*	*0.00*	*0.01*	*0.00*	*0.01*	*0.00*
Change						
Better wage	*0.52*	*0.54*	*0.53*	*0.54*	*0.52*	*0.54*
Higher firm FE	*0.35*	*0.38*	*0.34*	*0.38*	*0.35*	*0.39*
Higher wage decile	*0.40*	*0.41*	0.40	0.40	*0.40*	*0.42*
Higher within-firm wage decile	*0.39*	*0.44*	*0.38*	*0.43*	*0.39*	*0.44*
Higher wage-gap (f-m)	*0.19*	*0.21*	*0.21*	*0.25*	*0.17*	*0.19*
Av. job search time	*5.22*	*2.98*	*5.30*	*3.31*	*5.18*	*2.80*
Type of job entries						
Preceded firm closures	*0.99*	*0.99*	*0.99*	*0.99*	*0.99*	*0.99*
Job-to-job transitions	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
Observation number	576 356	42 811	218 180	14 796	358 176	28 015

Note: The sample covers all job entries between 2007 and 2011, when worker movements both originated from and was directed into employment in the private sector. Indented figures reflect statistically significant differences ($p < 0.05$) between the linked and nonlinked groups, according to two-sided t-tests.

To quantify differences in the average individual and firm quality, we estimated a 3-way AKM-style (Abowd, Kramarz and Margolis, 1999; Cardoso, Guimarães and Portugal, 2016) wage regression on the entire dataset with separable person, firm and occupation effects. A detailed description of this model can be found in Appendix B. According to the retrieved fixed effects, the average firm quality of female workers is slightly better, however, their worker quality is somewhat worse compared to men's. Using the same model, we calculated the controlled within-firm gender difference as well, as the difference in residuals within firms among genders. However, we could not find meaningful differences by gender.

Regarding job finding outcomes, there are only minor differences by gender. The probability that one will find a job, either within the presented firm option pool or outside of it, is relatively similar for both men and women. Also, there is no meaningful difference by gender in the probability of getting into firms with or without former co-workers. However, both men and women are more likely to find their new jobs at firms with same-sex contacts than opposite-sex ties, and the chance of women to remain unemployed after closures is higher.

4.4 Estimation Strategy

4.4.1 Job finding probabilities

To assess the effect of former co-workers on finding a job, we will utilize instances of job mobility induced by firm closures as exogenous variations. However, as administrative records do not include all job applications initiated by the individuals after job loss, the measurement of the effects requires taking an alternative approach. We have created multiple individual-firm pairs using companies that may provide relevant employment opportunities for the individuals. The available target firm alternatives, basically an approximation of the local labor market of individuals, will be defined as the list of those workplaces, where the former co-workers of those individuals were employed, who lost their jobs in the same firm closures.[60] Hence, our estimates will address that how the presence of links, employed at given firms, will affect the job seekers' chance of being hired by these particular workplaces. The following linear regression model[61] is estimated:

$$P_{isrt} = \beta_0 + \beta_1 X_{it} + \beta_2 Z_{st} + \beta_2 V_{ist} + \beta_3 LINK_{irt} + \delta_{sr} + \pi_t + \varepsilon_{isrt}\ , \quad (14)$$

[60] As we will see later, this option pool covers only those firms, which are part of the identification sample of the main model with two-way fixed effects. When estimating Eq. (14) with one or no fixed effects this sample will be used to avoid differences in parameters coming from changes in sample size and composition.

[61] Even though logistic regression or probit models might be more suited to the dependent variable than linear probability models, the Stata implementation of their fixed effects version has some shortcomings such as being computationally costly, not providing estimates of the marginal effects and fixed effects, or providing biased estimates under certain conditions (see Stammann, Heiss and McFadden (2016)). Therefore, and also to ensure comparability with the literature (most notably with Saygin, Weber and Weynandt (2021)), the paper focuses on and discusses the results of the fixed effect linear probability models, and will only present the results of the fixed effect logistic regression models in Appendix Table B4 and Table B5. The results of these regressions are qualitatively the same as the ones in the main tables.

where P_{isrt} refers to the probability that individual i finds her new job at firm r any time after being displaced from firm s in time t. X_i refers individual-specific controls, such as such as gender, age, education, region of residence, work experience and individual skills (captured by pre-estimated person fixed effects), while Z_{st} comprises characteristics related to the sending firms (e.g. ownership, sectors) in the month of displacement. V_{ist} includes variables corresponding to the individuals' closing firm spell (such as occupation and tenure length). To rule out the chance that some firms are more prone to hire the former workers of given companies (as they have better skills or skill sets that matches the hiring firm better) or that some worker routes are inherently more typical, the model accounts sending firm-target firm pair fixed effects (δ_{sr}). Seasonal effects (π_t) are included through year dummies to capture the changes in hiring probabilities over time.

The covariate of interest $Link_{irt}$ is a dummy, indicating the presence of former co-workers (with a min. 1 month tenure) at a given target firm in the month of displacement. β_3 reflects whether the chance for the creation of a given worker-firm match is higher if a former co-worker works there. When identifying the parameter, we will essentially compare those individuals who lost their jobs in the same closure events for getting into the same target firms, with or without contacts there. Thus, only those closing firm-target firm pairs contribute to the identification of co-worker effects, where at least one job seeker coming from firm s has former co-workers at firm r, and one hasn't. In some specifications, modified link definitions are utilized (such as whether co-workers with a specific gender are present at a given company), which are interacted with the gender of job seekers. In such cases, the covariate of interest $\beta_3 LINK_{irt}$ is substituted by the terms $\beta_3 LINK_{irt} * Female$ and $\beta_3 LINK_{irt} * Male$. Finally, ε_{isrt} is the independent error term with zero expected value.

4.4.2 Upward mobility

The second set of the estimates will address the role of contacts in structuring the individuals' chances for upward mobility. Specifically, they will test whether contacts facilitate the selection of individuals into better firms and jobs or more equal workplaces in terms of gender wage difference, with the main focus being on the heterogeneity of effects by the job seekers' gender. The analysis will utilize entries to new firms, and as opposed to the estimation of hiring chances, the data won't be restricted exclusively to job entries preceded by closures. As job-to-job transitions are more typical in the labor market and such movements might be characterized by the different use of networks, the assessment of both types of mobility might be vital.[62] To pursue the proposed goals, the following general linear regression model[63] is introduced:

$$Y_{isrt} = \beta_0 + \beta_1 X_{it} + \beta_2 Z_{st} + \beta_3 V_{ist} + \beta_4 LINK_{irt} + \pi_t + \varepsilon_{isrt}\,, \quad (15)$$

where $LINK_{irt}$ is an indicator variable, marking those cases when the job seekers have found their new job at their former co-workers' workplace (with the co-workers having started there at least 1 month earlier). The rest of the additional controls can be interpreted in the same way as in Eq. (14). Similarly as in the hiring estimates, the co-variate of interest is further interacted with gender (or other variables such as the type of mobility) in some specifications. Y_{isrt} denotes indicators of specific events, which aims to capture the aspects of upward mobility by contrasting the firm-related and job-related outcomes of individuals at their new firms to the ones at their sending firms. The first month of the new, while the last month of the previous employment spells are used to calculate the measures.

$UP_{w-f\ wage\ decile}$ indicates if the within-firm wage decile of individuals is higher at the receiving firms compared to the ones at the sending firms and $UP_{wage\ decile}$ captures if the individuals fall into higher wage deciles constructed on the whole population, at a given month. Such measures aim to capture the change in the position of workers in the job ladder and their overall labor market situation. $UP_{Firm\ quality}$ indicates if the job seekers get into better firms compared to their previous one, based on pre-estimated firm effects (δ_r) resulting from a 3-way fixed effects AKM specification (Abowd, Kramarz and Margolis, 1999) estimated on the entire dataset (described in Appendix B). Finally, UP_{WG} marks those cases, when the controlled net female-male wage gap is higher at the receiving firms compared to the sending ones.[64] The introduced upward mobility measures are defined by Eqs (16-19), where $D(x)$ is a function which returns the deciles of the selected continuous measure, x. t_0 denotes the last month of individuals at their sending firm, while t_1 the month of their new firm entry.

[62] The potential issues related to endogeneity are discussed in later on in this section.
[63] A concern could be raised that the logistic regressions might be more suited for the analysis of binary outcomes. In Appendix Table B6 we show the results by relying on such functional forms - the results did not change substantially.
[64] Calculated as the difference among genders within the given firms in residuals from the AKM model of Appendix Eq. (B1).

$$UP_{w-f\ wage\ decile} = \mathbf{1}\big(D(w_i|r,t_1) > D(w_i|s,t_0)\big) \qquad (16)$$

$$UP_{wage\ decile} = \mathbf{1}\left(D\big(w_{i,r}|t_1\big) > D\big(w_{i,s}|t_0\big)\right) \qquad (17)$$

$$UP_{WG} = \mathbf{1}\left(D\big(WG_{r,t_1}\big) > D\big(WG_{s,t_0}\big)\right) \qquad (18)$$

$$UP_{Firm\ quality} = \mathbf{1}\big(D(\delta_r) > D(\delta_s)\big) \qquad (19)$$

To investigate the heterogenous benefits attributed to social ties, we interacted the sending firm deciles of individuals with the indicator of links and gender. The model specification to estimate is:

$$Y_{isrt} = \beta_0 + \beta_x X_{it} + \beta_z Z_{st} + \beta_v V_{ist} + \beta_M Male_i + \beta_L Link_{irt} + \beta_{ML} Male_i * Link_{irt} + \sum_{d=2}^{9}\left(D_{ist}^d + \beta_{Md} Male_i * D_{ist}^d + \beta_{Ld} Link_{irt} * D_{ist}^d + \beta_{MLd} Male_i * Link_{irt} * D_{ist}^d\right) + \pi_t + \varepsilon_{isrt}, \qquad (20)$$

where D_{ist}^d can take up multiple, already introduced measures related to the individuals' sending firms and jobs: the within-firm wage decile of individuals, their wage decile based on the whole population, their firm quality decile and decile based on the level of the female-male wage gap.

4.4.3 Issues of endogeneity and measurement problems

The job finding estimates are using a quasi-experimental setup, where individuals are forced to re-enter the job market after being displaced due to firm closures. Such setting ensures that the lack of information about mobility decisions (that inherently comes with the use of administrative data) won't bias our results, since displacements exclude the option of voluntary mobility.

In the upward mobility estimates, however, the inclusion of job-to-job movements might let in some unintended selection bias, resulting in the underestimation of the co-worker effects and the contact-related gender differences as well. If we presume, in accordance with the literature, that social ties contribute to the acquiring of better (monetary or career-related) outcomes, then we will observe a higher share of those with links moving. In contrast, among nonlinked workers only those will switch between jobs who can acquire fairly good outcomes even without the help of their contacts. Therefore, the difference between the average outcomes of those with co-workers and those without will be lower, resulting in the underestimation of the parameter of interest. As the sign of the selection bias is negative, significant estimated co-worker effects will still provide support on the existence of positive and non-zero benefits of contacts. With respect to gender, if we assume that men value wage-related gains more compared to other types of amenities as opposed to women (in accordance with Sorkin (2018)), then the absolute value of the selection term will be higher in their case. Therefore, by applying the same logic as before, the differences in co-worker effects by gender might be somewhat underestimated as well.[65]

The coefficients could be also affected by issues related to the proxy of links. Misclassifications, either induced by the (maybe too strict) definition of the co-workers or data-related constraints (such as using only a sample instead of the whole population), might also cause that the measured effect will be biased toward zero if the presence of co-workers couples with better outcomes.

4.5 Results

4.5.1 Role of contacts in finding a job

To advance our understanding on the role of informal ties in the job finding process, the following analysis will address whether the presence of former co-workers at given firms increases the job seekers chances of being hired by these workplaces. To do so, the model defined by Eq. (14) will be estimated on a specific dataset with the observations being worker-sending firm-target firm triads. The outcome variable is a dummy indicating if a given closing firm-target firm job switch has been realized. The first panel of Table 11 presents the overall effect of former co-workers, Panel B shows the heterogeneity of effects by the gender of job seekers, while the bottom panel presents the results by both the gender of the job seekers and their contacts. Three model specifications are introduced for each panel: one without using fixed effects, one with sending firm fixed effects included and one with the use of sending firm-target firm fixed effects.

[65] This may or may not be true for the controlled wage gap: if women prefer firms with lower wage gap more, then the sign of the selection term will reverse and general conclusions might not be made.

Table 11. The effect of former co-workers on job finding

	(1) Without FE	(2) Sending firm FE	(3) Sending-Target FE
Panel A) Baseline			
Co-worker	0.0052***	0.0046***	0.0023***
	(0.0003)	(0.0003)	(0.0002)
R^2	0.0021	0.0073	0.2580
Panel B) Gender of job seeker			
Female with co-worker	0.0049***	0.0041***	0.0015***
	(0.0005)	(0.0005)	(0.0004)
Male with co-worker	0.0053***	0.0049***	0.0025***
	(0.0003)	(0.0003)	(0.0003)
R^2	0.0021	0.0073	0.2580
Panel C) Gender of job seekers and contacts			
Female job seeker			
Female co-worker	0.0043***	0.0034***	0.0012*
	(0.0006)	(0.0006)	(0.0005)
Male co-worker	0.0032***	0.0025***	0.0011*
	(0.0006)	(0.0006)	(0.0005)
Both female and male	0.0127***	0.0131***	0.0040
	(0.0032)	(0.0032)	(0.0029)
Male job seeker			
Female co-worker	0.0028***	0.0024***	0.0018***
	(0.0006)	(0.0006)	(0.0005)
Male co-worker	0.0049***	0.0045***	0.0022***
	(0.0004)	(0.0004)	(0.0003)
Both female and male	0.0104***	0.0106***	0.0050*
	(0.0024)	(0.0024)	(0.0020)
R^2	0.0029	0.0081	0.2582
Observations	1 364 911	1364911	1 355 556
No of job seekers	10 315	10 315	10 044
No of firms	1554	1554	1416
No of sending-target firm pairs	111 307	110 307	101 952

Note: Based on Eq. (14) three specifications are presented: estimates without fixed effects, with only sending firm fixed effects or with sending-target fixed effects (columns (1-3), respectively). The outcome variable measures whether a given closing firm-target firm job switch has been realized. Panel A presents the overall effect of co-workers, Panel B presents the heterogeneity of effects by the job seekers' gender. Panel C shows the results by the gender of job seekers and contacts. In the latter estimates modified proxies were used, which indicate the presence of female, male or both types of contacts at a potential target location. Additional controls cover gender, quadratic age, education and residence dummies, pre-estimated individual fixed effects, the categorized no. of spells before the displacement, tenure length and the 1-digit occupation code at the closing firm, closure year dummies. We also include dummies indicating the sector and ownership of sending firms, the presence of social transfers, and a variable indicating if the job seekers typically work at female-dominated workplaces.[66] Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

[66] Employment spells were categorized as 1, 2, 3, and more than 4. The ratio of female workforce was calculated on a yearly level for each industry sector-occupation cells, and also for each firm-month. Then, the difference of the two was averaged out for the individuals' former employment spells.

According to all model specifications, former co-workers strongly affect the hiring chances of individuals (Table 11, Panel A). When estimating the model without any fixed effects, the parameter of interest is 0.0052, which can be considered a substantial increase compared to the baseline job finding probability in the sample.[67] The presence of former co-workers essentially increases the probability of the realization of given sending firm-target firm matches by 6.2. However, in this specification, the co-worker effect might capture some unintended variation resulting in biased estimates. There could be a chance, for instance, that given closing firms have better quality workers pools, therefore all hiring firms will be more willing to employ their former employees. Thus, the individuals' chance of getting into the same hiring firms could be higher, even without the actual contribution of contacts. A very similar reasoning can be applied to sending firm-target firm pairs as well: some firms might be more willing to hire the former employees of given companies as their skills are better suited to the profile of the firms. By applying sending firm fixed effects and sending firm-target firm pair fixed effects we can exclude the chance that the results are driven by such explanations. In the former case, co-worker effects are identified by comparing the hiring chances of individuals from the same closing firms, with and without co-workers. Regarding the latter, the identification of the parameter of interest is restricted to comparisons within specific firm routes. If we use such specifications, we can see that the coefficients decrease by a meaningful extent, but remain still considerably large (respectively 0.0046 and 0.0023). This signals that the formerly introduced selections are indeed present and are reflected in our covariate of interest in the OLS specification. The drop in the magnitude of the point estimates is less substantial when using sending firm fixed effects, probably because the control variables in the first specification already capture most of the selection issues related to sending firm-level variation.

The positive effects are present for both male and female job seekers (Table 11, Panel B). However, there is a difference with respect to the magnitude of benefits. The point estimates are higher for men and the difference between the estimates for the two groups is significant, irrespective of the model specification used (see Table 12). This suggests that professional ties might produce increased benefits for men in finding a job. The results are in line with the notions of the literature, which propose that the expected

[67] The predicted probability that a given sending firm-target firm match will realize without the effect of co-workers is 0.0010.

returns of the help of informal networks, particularly those ties that were created within workplaces, are worse for women (Ibarra, 1993; Blommaert *et al.*, 2020).

Table 12. Test of the equality of coefficients for Table 11

	Without fixed effect	Sending firm FE	Sending firm-Target firm FE
Panel B)			
Co-workers for females vs. males	-0.73	-1.47	-2.26*
Panel C)			
Female contact for female vs. male	1.82	1.20	-0.80
Male contact for female vs. male	-2.48*	-2.90*	-1.87
Both types for female vs. male	0.60	0.65	-0.29
Female vs. Male contact for female	1.18	0.95	0.14
Female vs. Male contact for male	-3.20*	-3.20*	-0.73

Note: T-values from two-sided t-tests are presented in the table. *Statistically significant at 0.05 level.

Panel C presents estimates where the gender of the job seekers was interacted with the modified versions of the co-worker proxy used so far. Such variables indicate if the workers had at least one female or male former co-worker at the target firms, or if they even have both types.[68] The model without any fixed effects suggests that both types of contacts increase the probability of job seekers for hiring, although the presence of same-gender ties is coupled with a somewhat larger effect compared to opposite-sex ones. The within-gender difference between the effect of same-sex and opposite-sex contacts is only significant for men – for women it seems that both types of ties matter equally. Regarding between-gender differences, the effect of female ties seems higher for women compared to men, although the difference is only significant at a 0.1 level. At the same time, the effect of male co-workers is more substantial for men.

Such results could imply that homophily is indeed an important factor that characterizes the use of work-related ties (and also network building) especially for men. However, when we apply sending firm-target firm fixed effects (3rd specification) the formerly discussed patterns disappear. This could suggest that the observed differences between the effects of male and female contacts are mostly attributable to the prevailing occupation and gender structure of the companies, and gender segregation patterns, rather than other differences related to the use or utilization of contacts. The OLS estimates, although controlling for a comprehensive set of co-variates, cannot capture the effects of career dependency: that people in given occupations are building similar career paths,

[68] By controlling for the presence of both types of ties the other two variables will capture only the effect of male and female ties.

work at companies with specific gender composition and acquire given types of contacts. When it comes to finding a new job, all these career-related features will be reflected in the upcoming firm characteristics of individuals if they remain in a given career track.

4.5.2 Role of the occupation of contacts on job finding

Beside gender, the occupation of contacts might also influence the job seekers chances of hiring. Contacts with a higher position in the occupational ladder may have more knowledge about prospective vacancies and more power to influence hiring decisions and labor market outcomes (Lin, 1999; Boza and Ilyés, 2020).[69] However, according to the literature, the network of women might have lower intensity of such contacts, which could reduce the amount of utilities received from such ties (Moore, 1990; Ibarra, 1993). To reflect on these issues, the forthcoming analyses investigate both the effect of the contacts' occupational position in the job hierarchy (Table 13) and the effect of the contacts' relative position compared to the one of the job seekers on hiring probabilities (Table 15).

As in the previous estimates, the gender of job seekers is interacted with modified proxy variables. The first set of the refined proxies covers variables, which indicate whether individuals have at least one contact who works in higher, equal or lower occupation position compared to the job seeker, based on 1-digit occupation codes. The second set of proxy variables comprises dummies which mark if the job seekers have at least one co-worker in managerial, blue-collar or white-collar positions at the potential target firm. In both cases, the specifications also include dummies which control for the presence of all possible combinations of the introduced relationship types.

Regardless of using the occupational position of contacts in absolute (Table 13) or relative terms (Table 15), the specifications without fixed effects suggest that all types of contacts matter for both genders.[70] However, the observed patterns change if we apply sending-firm–target firm fixed effects: the coefficients decrease with a substantial extent with some of them remaining no longer significant. This especially holds for women, for whom those contacts matter the most, who work in managerial or higher occupational positions.

[69] While occupational similarity may provide a good basis for helping contacts, based on the related theories it seems plausible to assume that the obtainable benefits will be more significant if contacts are higher up the occupational ladder compared to the job seekers. However, overly large differences in the occupational position of acquaintances may work against the probability of such help.

[70] T-values resulting from the two-sided t-tests of the equality of coefficients are presented in Table 14 and Table 16.

This might suggests that contacts in better occupations (both in absolute and relative terms) can be quite valuable assets for women, and the utilization of such ties is at least as effective/productive for women as for men during job search. In the upcoming part of the study, the focus will be shifted toward the quality of those jobs acquired through the help of contacts and the firms that offered them.

Table 13. Role of the occupation of contacts on job finding

	(1) Without FE	(2) Sending firm FE	(3) Sending-Target firm FE
Female job seeker			
$Occup_{co\text{-}worker}$ =Manager	0.0062***	0.0058***	0.0037**
	(0.0015)	(0.0015)	(0.0012)
$Occup_{co\text{-}worker}$ =White-collar	0.0033***	0.0028**	0.0009
	(0.0009)	(0.0009)	(0.0009)
$Occup_{co\text{-}worker}$ =Blue-collar	0.0043***	0.0033***	0.0011*
	(0.0006)	(0.0006)	(0.0005)
Male job seeker			
$Occup_{co\text{-}worker}$ =Manager	0.0054***	0.0051***	0.0030***
	(0.0010)	(0.0010)	(0.0008)
$Occup_{co\text{-}worker}$ =White-collar	0.0017**	0.0013*	0.0005
	(0.0006)	(0.0006)	(0.0005)
$Occup_{co\text{-}worker}$ =Blue-collar	0.0048***	0.0044***	0.0022***
	(0.0004)	(0.0004)	(0.0003)
Observations	1 364 911	1 364 911	1 355 556
No of job seekers	10 315	10 315	10 044
No of firms	1554	1554	1416
No of sending-target firm pairs	111 307	111 307	101 952
R-squared	0.0028	0.0079	0.2582

Note: Based on Eq. (14) three specifications are presented: estimates without fixed effects, with only sending firm fixed effects or with sending-target fixed effects (columns (1-3), respectively). The outcome variable measures whether a given closing firm-target firm job switch has been realized. The co-variates of interest indicate the presence of co-workers in managerial, blue-collar or white-collar positions at a potential target location. We also include dummies for all possible combinations of the various contacts types. Additional controls are listed in Table 11. Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table 14. Test of the equality of coefficients for Table 13

	Without fixed effect	Sending firm FE	Sending firm-Target firm FE
Managers for females vs. males	0.45	0.40	0.47
White-collar ties for females vs. males	1.55	1.44	0.39
Blue-collar ties for females vs. males	-0.87	-1.69	-1.90*
Manager vs. White-collar for females	1.70	1.73	1.82
Manager vs. Blue-collar for females	1.24	1.57	1.96*
White-collar vs. Blue-collar for females	-1.01	-0.52	-0.14
Manager vs. White-collar for males	3.09*	3.13*	2.38*
Manager vs. Blue-collar for males	0.51	0.55	0.93
White-collar vs. Blue-collar for males	-4.34*	-4.34*	-2.67*

Note: T-values from two-sided t-tests are presented in the table. *Statistically significant at 0.05 level.

Table 15. Role of the relative occupation of contacts on job finding

	(1) Without FE	(2) Sending firm FE	(3) Sending-Target firm FE
Female job seeker			
$Occup_{co\text{-}worker} > Occup_{job\ seeker}$	0.0046***	0.0038***	0.0023***
	(0.0008)	(0.0008)	(0.0007)
$Occup_{co\text{-}worker} = Occup_{job\ seeker}$	0.0053***	0.0046***	0.0015
	(0.0009)	(0.0009)	(0.0009)
$Occup_{co\text{-}worker} < Occup_{job\ seeker}$	0.0030***	0.0021***	0.0004
	(0.0006)	(0.0006)	(0.0006)
Male job seeker			
$Occup_{co\text{-}worker} > Occup_{job\ seeker}$	0.0035***	0.0030***	0.0018***
	(0.0005)	(0.0005)	(0.0004)
$Occup_{co\text{-}worker} = Occup_{job\ seeker}$	0.0067***	0.0063***	0.0026***
	(0.0007)	(0.0007)	(0.0005)
$Occup_{co\text{-}worker} < Occup_{job\ seeker}$	0.0028***	0.0024***	0.0018***
	(0.0005)	(0.0005)	(0.0004)
Observations	1 364 911	1 364 911	1 355 556
No of job seekers	10 315	10 315	10 044
No of firms	1554	1554	1416
No of sending-target firm pairs	111 307	111 307	101 952
R-squared	0.0031	0.0083	0.2582

Note: Based on Eq. (14) three specifications are presented: estimates without fixed effects, with only sending firm fixed effects or with sending-target fixed effects (columns (1-3), respectively). The outcome variable measures whether a given closing firm-target firm job switch has been realized. The co-variates of interest indicate the presence of ties employed in higher, equal or lower occupational position compared to the job seekers based on 1-digit occupation codes at a potential target location. We also include dummies for all possible combinations of the various contacts types. Additional controls are listed in Table 11. Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table 16. Test of the equality of coefficients for Table 15

	Without fixed effect	Sending firm FE	Sending firm-Target firm FE
Higher occupation ties for females vs. males	1.24	0.82	0.71
Equal occupation ties for females vs. males	-1.24	-1.53	-1.08
Lower occupation ties for females vs. males	0.24	-0.39	-1.90*
Higher vs. Equal for females	-0.63	-0.75	0.72
Higher vs. Lower for females	1.64	1.68	2.13*
Equal vs. Lower for females	2.14*	2.28*	1.07
Higher vs. Equal for males	-3.86*	-3.99*	-1.26
Higher vs. Lower for males	1.06	0.92	-0.03
Equal vs. Lower for males	4.94*	4.90*	1.21

Note: T-values from two-sided t-tests are presented in the table. *Statistically significant at 0.05 level.

4.5.3 Co-worker effects on upward mobility

By using entries to firms, the upcoming analysis will address whether those individuals who started their new jobs with links got into better firms and jobs compared to those workers without links. Based on the model described in Eq. (15), we estimate linear regression models, where the dependent variables are dummies capturing various aspects of upward mobility. $UP_{Firm\ quality}$ indicates if the new firms of job seekers are of better quality (e. g. provide higher wages) compared to the previous ones, UP_{WG} marks those instances when the controlled within-firm wage advantage of men is lower in the individuals' receiving firms than in their sending firms. $UP_{w-f\ wage\ decile}$ and $UP_{wage\ decile}$ indicates if the individuals fall into either higher within-firm wage deciles at their new workplaces or higher wage deciles[71] across the whole population (in a given month). The covariate of interest is a dummy marking those job entries where the former co-workers of the individuals are present at the time of hiring. Beside estimating the overall effect of co-workers (presented in Table 17, Panel A), the heterogeneity of effects is also assessed by interacting the co-worker proxy with the gender of job seekers (Panel B), the type of job mobility (Panel C) and with both variables (Panel D). The type of job mobility distinguishes entries preceded by closures and job-to-job movements.[72]

According to the results, linked individuals are more likely to get into better firms and jobs, or workplaces where the controlled gender wage gap is lower. The presence of former co-workers is associated with the improved labor market position of individuals: workers with links have higher probability for getting into higher income deciles (calculated based on the entire dataset) compared to their former one (column (1), Table 17). The parameter reflects both the direct and indirect effect of former co-workers. Besides information transmission and referral, former co-workers may contribute to the hiring of their acquaintances, by, for example, indirectly signaling their skills and latent productivity with their own performance. The effects are stronger for male workers, for whom the benefits are more substantial after job-to-job movements. In contrast, we observe more substantial benefits for women when the job entries were preceded by closures (column (1), Panel D).

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[71] Based on the logarithm of daily earnings over the national average of daily earnings.

[72] Nevertheless, we present separate estimates on the sub-sample of entries preceded by closures in Appendix Table B7. The resulting co-worker effects are fairly similar to the ones from the interaction models presented in Table 17.

Table 17. Co-worker effects on upward mobility

	(1)	(2)	(3)	(4)
	$UP_{wage\ decile}$	$UP_{w-f\ wage\ decile}$	$UP_{Firm\ quality}$	UP_{WG}
Panel A) Baseline				
Co-worker	0.0314***	0.0605***	0.0400***	0.0353***
	(0.0022)	(0.0022)	(0.0022)	(0.0025)
Panel B) Gender of job seeker				
Female with co-worker	0.0237***	0.0619***	0.0484***	0.0516***
	(0.0037)	(0.0037)	(0.0038)	(0.0040)
Male with co-worker	0.0355***	0.0598***	0.0357***	0.0252***
	(0.0027)	(0.0026)	(0.0028)	(0.0032)
Panel C) Job mobility type				
Job entries after closures	0.0489*	0.0526**	0.0794***	0.0167
	(0.0199)	(0.0188)	(0.0204)	(0.0200)
Job-to-job mobility	0.0312***	0.0606***	0.0395***	0.0356***
	(0.0022)	(0.0022)	(0.0023)	(0.0025)
Panel D) Job mobility type × Gender of job seeker				
Closure × Female with co-worker	0.0904**	0.0466	0.1380***	0.0203
	(0.0345)	(0.0351)	(0.0361)	(0.0329)
Closure × Male with co-worker	0.0319	0.0551*	0.0561*	0.0149
	(0.0235)	(0.0216)	(0.0239)	(0.0244)
Job-to-job × Female with co-worker	0.0230***	0.0620***	0.0474***	0.0520***
	(0.0038)	(0.0037)	(0.0038)	(0.0040)
Job-to-job × Male with co-worker	0.0355***	0.0598***	0.0354***	0.0253***
	(0.0028)	(0.0027)	(0.0028)	(0.0032)
Observations	595 334	595 334	551 988	392 396

Note: The estimates for column (1-4) are based on Eq. (15). The dependent variables are indicators, which mark if the overall or within-firm wage decile of the individuals is higher at the receiving firms compared to the sending ones (column (1) and (2)), if the quality of the new firms is better (column (3)), or if the controlled gender wage gap is lower at the new firms (column (4)). The covariate of interest, which marks job entries with co-workers present, is used in itself (Panel A) and interacted with other variables, such as the gender of job seekers (Panel B), the type of job mobility (Panel C) and both of these variables (Panel D). The observation number might differ from the one in Table 9 and Table 10 due to missing values. Additional controls are listed in Table 11. Robust standard errors are in parentheses. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

This overall improvement in the individuals' position (measured by the general level of wages), however, is composed of two essential elements. On the one hand, there is a firm-specific component: individuals might get into better quality firms (column (3), Table 17). On the other hand, there is an individual-specific component: they might obtain better within-firm position at their new firms (column (2), Table 17). According to the results, former co-workers affect both of these elements: they increase the sorting of workers into higher-paying firms and also, they enhance the selection of individuals into better jobs which are higher up in the within-firm wage structure (compared to the former jobs of the individuals). While in the latter case we cannot find meaningful gender differences in the magnitude of effects, the impact of contacts on the chance of getting into better firms is significantly stronger for women (column (3), Panel B).[73] The effects on firm quality are more substantial after closures, compared to job-to-job transitions, both in general and within genders (column (3), Panel C and D). Regarding within-firm position improvement, the magnitude of effects is somewhat higher after job-to-job mobility.

Finally, the results suggest that individuals are more likely to get into firms, where the controlled within-firm male-female wage difference is lower. Such result, however, could either reflect that workers enter firms with smaller wage gaps or get into companies where women earn relatively more in contrast to men. Either one is the case, the effects are more substantial for women (according to Panel B). This may imply that former co-workers can actually pull-out female job seekers from firms with relatively large male-female wage gaps and push-them toward more equal or female-favored workplaces in terms of wages. However, this “equalizing” effect of contacts is not necessarily beneficial for women, if firms with lower gender wage gaps tend to be lower wage firms themselves. Since this is the case in Hungary, economic benefits are not necessarily attached to lower gender gaps, however, they may still be considered as additional amenities that women value more.

In order to get a more detailed picture, the heterogeneity of co-worker effects was investigated jointly by gender and the level of those sending firm-specific job-related and firm-related characteristics which were used for the construction of the upward mobility measures. Namely, the within-firm and overall wage decile of individuals, the firm fixed effects and the gender wage gap decile of the sending firms. As it was described in Eq. (20), the proxy of co-workers is interacted with both gender and the sending firm level of these variables. The predicted marginal probabilities for upward mobility are presented in Figure 3.[74]

[73] Regarding firm closures, we might measure smaller increase in the within-firm and overall wage deciles of the individuals due to severance pays, which typically appear in the last month of the individuals’ spell at the sending firm. In order to account for this, the estimates were recalculated by using the wage deciles of individuals in next to last month of the workers at their sending firms. The results remained fairly similar.
[74] In Appendix Figure B1 we also show the predicted marginal probabilities of upward mobility by gender, without additional control variables. The observed patterns are similar to the ones in Figure 3.

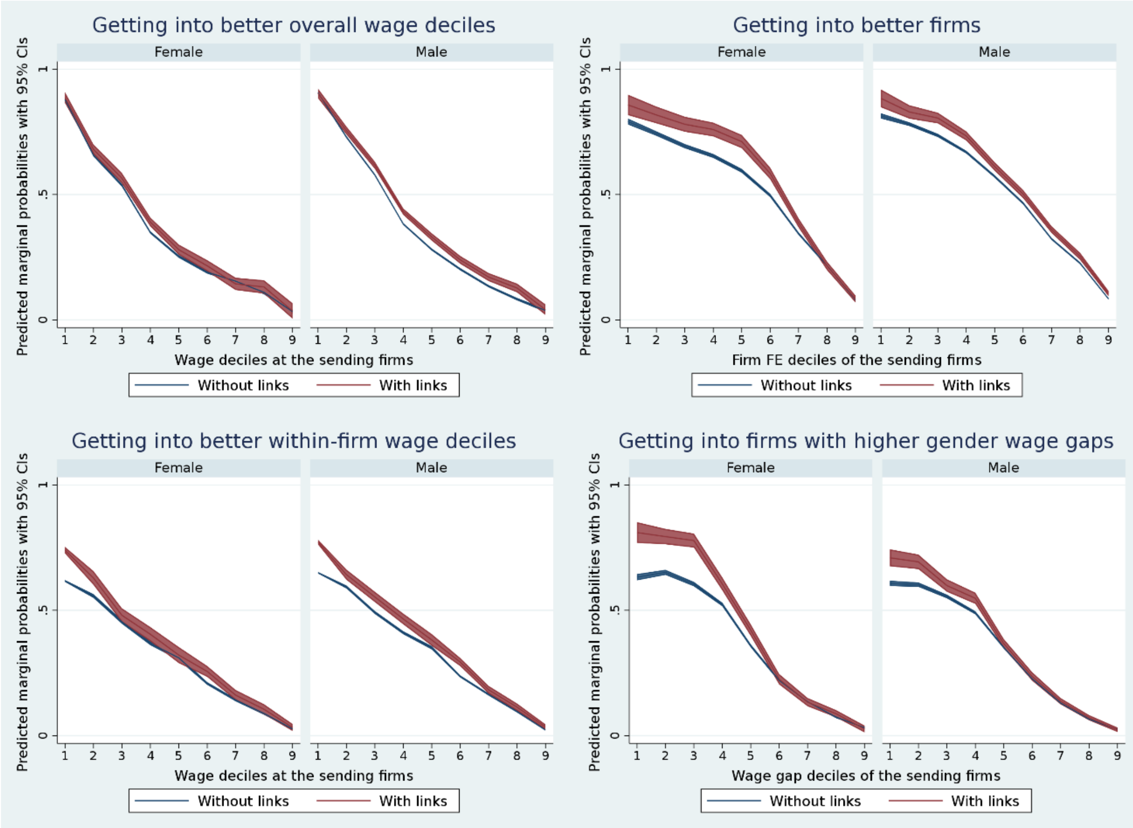

Figure 3. Predicted marginal probabilities of upward mobility by gender

Note: The estimates are based on Eq. (20). Upward mobility is measured by four dummy variables which mark if the individuals fall into higher overall or within-firm wage deciles at their receiving firms (upper left and bottom left panels), hired by better quality firms (upper right panel) or firms with lower average male wage advantages (bottom right panel) after job mobility. The predicted marginal probabilities for upward mobility with 95% confidence intervals are presented by gender and the sending firm level of those variables, which were used for the construction of the given upward mobility measures. As there is no chance for further upward mobility, the highest (10^{th}) decile is always omitted.

The upper left panel displays co-worker effects on the probability of getting into better overall wage decile by gender and the sending firm level of the individuals' overall wage decile. Naturally, the probability of improvement (and also the impact of co-workers) is decreasing as the sending firms' wage deciles increase, but the results reflect an interesting gender difference in the effect of contacts. While for men former co-workers increase the chances of upward mobility in all wage deciles of sending firm, for women the effect of contacts is only present at the lower and middle categories. This implies that although former co-workers promote women to obtain better positions in the wage distribution, their effect is only meaningful for those in worse positions. This way, informal ties appear to contribute to the glass door phenomenon (Hassink and Russo, 2010), as they fail to break those bindings that keep women in average-wage jobs and are unable support women to get into the highest overall wage deciles.[75] In contrast, they can open the way for men to acquire better positions, irrespective of their former income level.

We can observe a quite similar pattern of upward mobility in terms of within-firm wage deciles (bottom left panel), although the effect of social ties seems to be even more moderate for women. The largest gains are observed for women at the bottom of the within-firm wage distribution, while for those in higher deciles we find smaller, almost insignificant effects. As opposed to this, co-workers affect the chances of men for upward mobility up until the highest within-firm quartiles. Regarding firm quality, former co-workers provide essential benefits for both genders. Although the help of contacts is negligible for those women who worked in higher premium firms (captured by higher fixed effects deciles), women who worked in firms with average or below average wage-levels may even realize larger benefits compared to men.

Finally, the bottom right panel of Figure 3, which shows the variation in the gender wage gap by the presence of former co-workers, complements the discussion of the patterns already seen. According to the results of Table 17, former co-worker can actually contribute to the selection of women to those firms, where the wage advantage of men is lower. The current figure indicates that these effects are mostly driven by the tendency, that co-workers pull-out women from firms with higher relative average male wages and push-them toward more gender-equal workplaces.

According to the presented results, former co-workers contribute to the acquisition of better labor market outcomes for both genders. The presence of social ties is associated with the better position of individuals in the overall and in the within-firm wage structure, and it enhances the chance of individuals to be hired by higher wage firms and firms with more gender equal wage-settings after job mobility. However, while for men co-workers could ensure improvement irrespective of their former job-related and firm-specific characteristics, social ties provided help only those women, who were in average or worse labor market situation before changing jobs. These patterns imply, on the one hand, that former co-workers might be essential assets in equalizing of the chances of women for acquiring career advancement. On the other hand, that they also contribute to the

[75] If linked women in worse positions would move up on the average more in general wage hierarchy (e.g. straight to the highest deciles), this conclusion would not hold. However, this is not the case: on average women move up only 1-2 deciles in the sample.

reproduction and perpetuation of the existing gender differences, as they neither can open the glass door for women, nor can break the invisible glass ceiling.

4.6 Discussion

Labor market gender inequality might take various forms: unequal chances of hiring, different level of wages, differential access to higher-paying jobs and firms, or uneven opportunities for upward mobility (Woodcock, 2008; Crespo, Simoes and Moreira, 2014; Blau and Kahn, 2017; González, Cortina and Rodríguez, 2019). The chapter focused on social networks as one essential driver of these disparities and investigated how informal ties contribute to gender differences in hiring and career improvement.

The findings suggest that displaced workers, regardless of gender, are more likely to find employment at those firms where their former co-workers work. The measured effects are higher for male job seekers, and as opposed to our initial expectations, we found no evidence for gender homophily in network use. While in the less controlled specifications we could see the superior role of same-gender contacts, after controlling for dominant worker flows between sending and target firms these patterns disappeared. By contrasting job-related and firm-specific characteristics of individuals before and after a job change, we also show that contacts may contribute to greater chances of upward mobility within the work histories of individuals. This finding applies equally for both genders, which is promising for gender equality. As former co-workers contribute to the higher chances of women for securing jobs at higher-paying firms or getting into better positions in the wage hierarchy, they play an essential role in equalizing the economic opportunities of women. However, such benefits occur only for women, whose labor market position prior to mobility was ordinary or worse. Women with better initial labor market outcomes typically did not acquire meaningful gains through contacts, as opposed to men, indicating that the role of social ties is not as clear-cut positive as it seems.

There is a duality in network effect by gender: contacts might be essential assets to achieve better labor market outcomes, while also being contributors to the reproduction and perpetuation of the existing gender differences. Moreover, social ties can even increase the existing differences by gender if the measured benefits of men in the highest quartiles do not only reflect a within-gender competition for better workplaces, but is associated with the exclusion of women from the highest-paying firms and jobs. The current dataset, however, provides only limited opportunities for the investigation of this question, which nevertheless would be essential to be addressed through further research.

5. University Peers and Career Prospects: The impact of university connections on early labor market outcomes[76]

5.1 Introduction

The labor market integration of graduates became more complex during the last few decades. As school-to-work transitions became extended and de-standardized (Róbert, 2008), the sharp borderlines between higher education and the labor market have become obscured (Dianne and Dwyer, 1998; Teichler, 1998). At the same time, early-career patterns of university graduates became more fragmented and might no longer follow one dominant, linear path (Beck, 1992). Early labor market success – typically characterized by shorter unemployment periods, more stable employment spells, higher wages, and the higher quality of jobs –, depends on the complex combination of individual characteristics and decisions (socio-demographic characteristics, experiences and skills, the chosen study path and career aspirations) and structural factors as well (country-specific economic and political situation, labor market conditions, and firm recruitment practices). The course of career paths, however, might greatly depend on social resources as well, which could essentially shape the individuals' labor market opportunities (Lin, 2001).

Career tracking surveys from a wide range of countries revealed that social ties might have an essential role in acquiring economic opportunities for university graduates (Bartus, 2001; Franzen, 2006; Kogan, 2011; Kogan, Matković and Gebel, 2013). However, the existing evidence is confined chiefly to investigating employment outcomes, and detailed information on utilizing different network segments is moderate. This study aims to partly fill this gap by focusing on one particular, professionally relevant subgroup of the graduates' social network, most notably former university fellows. Using a detailed administrative dataset from Hungary and proxying university acquaintances with overlapping training periods, we investigate the contribution of such ties to the early labor market success of those graduates who finished their master's degrees and entered the labor market.

Due to their similar training profile and professional interest, former university fellows might greatly help each other by providing helpful information on well-matching

[76] This chapter is based on the article "University ties and early labor market outcomes" that soon will be submitted to the Economics of Education Review. The project is a joint work with Anna Sebők, and the work was supported by the European Union, Hungary and the European Social Fund (BCE EFOP-3.6.3-VEKOP-16-2017-00007).

vacancies. Moreover, their shared training period may also serve as a sound basis for recommendations, as it gives the students time and opportunity to become familiar with each other's skills, attitudes, and performance. Zhu (2019) by linking data on community college student transcripts to matched employer-employee records investigated classroom network effects on job finding and the heterogeneity by gender. Using Swedish register data, Eliason et al. (2019) also provided some evidence on the essential role of those ties obtained from primary, second and tertiary education on job finding. However, as their paper focuses on the role of social connections in labor market sorting, they neither reflected on the issues related to the causality of network effects nor investigated the heterogeneity of effects for career entrants.

The present study contributes to the literature on social networks, on graduate labor market entry and on early career success as well. Using large-scale administrative data, we provide empirical evidence that former university ties contribute to the formation of the labor market outcomes of individuals, during the entry phase of their careers. Not only do university acquaintances increase the hiring chances of individuals, but they also contribute to acquiring more prestigious, high-paying jobs as well. However, our results suggests that these benefits are primarily attributable to those ties from bachelor's studies. The effect of master's peers is mostly driven by the selection of individuals alongside prevalent study track-firm pathways. These findings suggest that too much similarity in the educational and career paths of former university peers, especially early in their careers, may limit the chances of individuals providing help to each other and may even be accompanied by crowding out effects. Also, that dissimilarity, to a given extent, might be associated with increased information on available jobs and better economic opportunities. Our results call attention to the importance of university peers as professionally relevant ties and contribute to the discussions about school-to-work transitions as well, by emphasizing the role of university contacts in affecting early career success.

When estimating the effect of former university peers on hiring outcomes, we follow a similar path to those papers that utilized administrative datasets to investigate hiring chances (Kramarz and Skans, 2014; Eliason *et al.*, 2019; Saygin, Weber and Weynandt, 2021). We focus on periods of unemployment for master's students completing their studies, and for each month of such periods, we create individual-firm pairs using companies that may provide relevant employment opportunities for the individuals based on their chosen study program. The pool of potential hiring firms was defined as the collection of companies that ever employ individuals during the observation window, who finished similar master's programs to the job seekers. *Programs,* in our understanding, cover similar study tracks at different universities offering the same type of degree (e.g. MA in Sociology at any universities). We then investigate whether the formation of given employer-employee pairings within the elaborated set of firms is more probable where the individuals' former university acquaintances are present. In addition to controlling for observables affecting hiring chances, we also account for frequent pathways between study tracks and firms that are related to the individual's most recent master's degree. To do so, we introduce joint *program-firm* or joint *institution-program-firm* fixed effects into our model. Thus, the identification of peer effects is restricted to comparing the hiring probabilities of those individuals with and without links at given firms who finished either the same type of master's programs or the exact same programs at the same universities.

Our results suggest that even after accounting for the sorting alongside study program-firm routes, individuals are still more likely to be hired by given firms if their former university peers work there. Although we cannot provide direct evidence on the helping role of peers in getting a job (recommendation or information passing about vacancies), our robustness checks provide suggestive evidence in favor of the existence of such assistance. However, the measured gains are driven mainly by the positive contribution of ties from bachelor's studies. After controlling for frequent labor flows between university programs and firms we could not identify significant benefits by master's contacts. As bachelor's programs typically offer more opportunities for specialization, there may be a greater chance that former university peers follow different career paths and work in different positions. Therefore, the quantity and quality of information about job opportunities may be higher and involve a broader range of firms. Conversely, contacts from master's studies, sharing more similar educational and labor market trajectories and interested in similar occupations may offer fewer job opportunities, perhaps due to firms having a limited capacity (especially in the short term) to employ individuals in the same types of jobs.

In the second part of the analysis, we focus on job entries to firms and compare the employment characteristics of those graduates with and without former university peers present. By introducing a set of linear and logistic regressions, we demonstrate that those who started their new jobs at their peers' workplaces tend to receive higher entry wages and typically work at positions with more prestige and/or higher status. Besides, their chance of having longer tenure at their new workplaces is higher, implying that informal ties can contribute to the creation of better person-job matches. However, part of these gains can be attributed primarily to the selection of linked graduates to firms that provide increased benefits to career entrants in general.

Even though they might seem moderate in magnitude, the measured benefits could represent crucial help for labor market entrants. As early career tracks can influence subsequent labor market outcomes, the initially smaller gains can lead to more significant gaps in economic opportunities in the long run.

5.2 Background

5.2.1 Labor market entry of university graduates

The transition from tertiary education to the labor market and the components of early labor market success are addressed by extensive research from the area of sociology, economics and social policy as well. During recent decades, school-to-work transitions and early career patterns have become more diverse and individualized. The sharp borderline between the school and the labor market has disappeared: working and studying do not necessarily exclude one another, and returning to higher education from the employment market is also a viable option (Teichler, 1998). Transitions typically last longer, de-standardized (Róbert, 2008), while early careers are frequently interrupted and often reflect temporal educational mismatch, underachievement or even frequent job switches (Johnson, 1978; Topel and Ward, 1992).

Demand-side factors, such as the diversification of the labor market, the growing skill intensity of occupations, and the increased expectation from the employers for creativity and the adaptability of knowledge and skills (Mann and Huddleston, 2017), might raise severe challenges against the career entrants to succeed on the labor market. Meanwhile, the expansion of higher education and the growing influx of skilled graduates increase the competition for entry-level employment and contribute to the credential inflation of university qualifications (Tholen *et al.*, 2013).

The lack of significant labor market experience, on the one hand, might entail that finding the right track for career entrants takes longer and requires a higher amount of job search and experimentation with different jobs due to the absence of established job preferences

(Johnson, 1978; Osterman, 1980; Topel and Ward, 1992). On the other hand, it also influences the attitude of firms towards the recruitment of such individuals. Traditional economic models of human capital, screening and signaling (Arrow, 1973; Thurow, 1975) suggest that educational attainment provides information about individual quality. For instance, it can anticipate success at work. Yet the scarcity of direct information on individuals' knowledge and skills makes the recruitment of such candidates risky. The role of social ties might be essential for both graduates and firms, as they can mitigate information asymmetries between companies and individuals (Ullman, 1966; Wanous, 1980).

5.2.2 Social networks and labor market opportunities

Several studies have shown that social networks play an essential role in the labor market (Granovetter, 2019). Our acquaintances can provide us with useful information about job opportunities (Calvó-Armengol and Jackson, 2004, 2007) and they can enhance the creation of employer-employee matches through recommendations (Simon and Warner, 1992; Dustmann *et al.*, 2016; Hensvik and Skans, 2016). As a result, the realized pairings can be of better quality than the ones resulting from formal job search methods, and the characteristics of the acquired jobs are often more favorable (Loury, 2006; Brown, Setren and Topa, 2016). Such gains and, in general, the utilization of social ties might be essential for university graduates. By reducing the firms' uncertainties about the applicants' skills and dispelling the job seekers' doubts about given job opportunities, informal contacts can facilitate employment and contribute to better labor market prospects for jobseekers.

Studies based on surveys from various countries reinforced that informal job search is widespread among graduates (among others, Try (2005), Kogan (2011), Kogan, Matković and Gebel (2013)). For example, in Hungary, approximately 40% of the individuals found new jobs with the help of personal ties around the millennium (Bartus, 2001) and 46% in 2010 (KSH, 2011). Such studies also revealed that informal connections could shorten the length of the individuals' unemployment spells (Holzer, 1988; Bentolila, Michelacci and Suarez, 2010) and lower the number of interviews (Franzen, 2006). However, the evidence on the effect of social relations on the quality of employer-employee pairings, education-job matches, and job quality is mixed. One factor contributing substantially to the heterogeneity of results is the differential effect of given contact segments on labor market outcomes (Granovetter, 1973, 1983).

A number of papers suggest that friends and relatives (i.e. strong ties) are particularly relevant for young labor market entrants. As these relationships are typically characterized by a sense of trust, obligation and commitment, they are more likely to invest time, effort and resources in helping (Bian, 1997; Smith, 2005). However, due to the heterogeneity of career paths, such relationships can easily lead to less suitable jobs in terms of skills and qualifications. Holzer (1988) demonstrated that the use of strong ties could increase the number of job offers and shorten job search. Kramarz and Skans (2014), using Swedish administrative data, showed that graduates often get their first stable position at their parents' workplace (corresponding rates of 14%, 11.5% and 3.2% for those with primary, secondary and tertiary education, respectively). According to their results, employment spells initiated with parental presence ensure better chances for career-building and are more durable in the mid-term and long-term. At the same time, they are typically associated with lower entry wages and form less typical education-job matches (similar results are presented by Bentolila, Michelacci and Suarez (2010) and Obukhova (2012)).

In contrast, professional ties might provide more profile-fitting and qualification-adequate employment opportunities for university graduates (Boaretto *et al.*, 2007). Acquaintances from work or university tend to follow similar career paths, typically share similar professional interests, and are likely to be familiar with each other's skills, morals, and qualities because of their shared work experience or training periods.[77] They can, therefore, be good sources of relevant job opportunities and recommendations (Antoninis, 2006). A stream of studies demonstrated that former co-workers could increase job-finding chances (Granovetter, 2019; Saygin, Weber and Weynandt, 2021) and might contribute to the achievement of a higher level of wages (Hensvik and Skans, 2016; Boza and Ilyés, 2020) and longer tenure (Glitz and Vejlin, 2021). Regarding career entrants, Hensvik and Skans (2014) showed that vocational school graduates are more likely to find their first stable job after graduation at their former internship place or at those companies where their former internship colleagues worked.

Contrary to the fast-growing literature on the role of former co-workers, existing evidence on the labor market role of university ties is scarce. Zhu (2019) by using matched employer-employee data from the US and quasi-random variation in section enrolment

[77] This is especially true for university acquaintances from master's studies, as the number of students is typically lower at this level of study and the joint coursework is more intensive.

within courses showed that having a peer at a given firm increases the propensity of students to get into the same firms. However, the results only refer to two-year college students and mostly address gender differences. Fischer et al. (2021) by focusing on tutorial groups at Copenhagen Business School indicated that students of the same groups are more likely to be hired by the same employer. Eliason et al. (2019), while investigating the role of social ties using similar approach in labor market sorting based on Swedish administrative data, also provided some related evidence. According to their results, contacts from primary, secondary and tertiary education can affect the job-finding chances of those workers who lost their jobs due to closures. However, they neither investigated the heterogeneity of results for career entrants nor tested how these ties affect the quality of the newly acquired jobs.

The current study contributes to the literature on the role of social ties in the labor market and the labor market prospects of career entrants by showing that university ties might be essential assets during the job search and career development. First, we demonstrate that graduate students are more likely to get hired by given firms if their former university peers already work there, even after accounting for frequent education-labor market pathways. We interpret these findings as suggestive evidence of peer effects and present a set of robustness checks to support our claim. Second, we call attention to the similarity of ties as a key determinant of the ability and willingness to provide informal help. We show that the measured hiring benefits are mainly attributable to the positive contribution of bachelor’s contacts, who, unlike master’s ties, can provide more information about vacancies (which are less similar to their own). Third, we demonstrate that university ties can also contribute to acquiring more prestigious and higher status jobs: they can increase the level of entry wages and promote the creation of more stable employment spells. Such findings suggest that university ties can be essential mediators in forming better individual-organization matches.

5.2.3 Institutional background

Since 2005, the Hungarian higher education system has gradually adapted to the international standards set by the Bologna Process (Pusztai and Szabó, 2008). In general, the education system consists of three cycles; universities in Hungary are authorized to launch bachelor’s (3-4 years), master’s (1-2 years), and PhD programs (2+2 years) if the necessary requirements are satisfied. Besides, some study fields still follow a more extended, undivided one-tier study structure (5-6 years).[78] Nowadays, there are more than 60 higher education institutions in Hungary (Oktatas.hu, 2021). Budapest is the most concentrated centre of higher education (HE), but we can find university centres in every region (Horváth, 2010). In the last decade, higher education enrolments amounted to approximately 90000 students per year (Felvi.hu, 2021) and in general, around 300000 students study yearly in HE (KSH, 2022f). In 2011 approximately 30% of the HE degree holders were under 35 years old (KSH, 2022b), and 14% of those under 35 had tertiary education as their highest level of education (KSH, 2022a).

The labor market prospects for young graduates are fairly good: between 2004 and 2018, around 80% of the bachelor's and master's degree holders were employed, and the average wage return for them was 130-160% and 200-250% respectively compared to those in primary education (Varga, 2020).[79] However, the jobs obtained are not always perfectly matched to the educational background of the career entrants: vertical and horizontal educational-job mismatch is considerable in Hungary. In 2016, 54% of those under 30 years old, master's degree holders were overqualified for their job – the same proportion for bachelor's graduates was 42% (Varga, 2020).

5.3 Data and definitions

The study uses a large Hungarian employer-employee administrative panel dataset from the Databank of the Centre for Economic and Regional Studies. The dataset integrates the administrative records of the Pension Directorate, the Tax Office, the Health Insurance Fund, the Office of Education, and the Public Employment Service on an individual level in anonymized form. It comprises monthly information on (among others) employment, health expenditures, and social transfers for a randomly selected 50% sample of the Hungarian population (Sebők, 2019). In addition, detailed education information is available from 2009. For each person, we have information on all the study programs started, the active and passive semesters, and the date of completion of the programs. In terms of program characteristics, the name of the university and the program, the type of

[78] Namely, medical (general medicine, pharmacy, dentistry) and veterinary studies, architecture, law, teacher training, and some specific programs related to arts, crafts and design.
[79] Although the unemployment rates of graduates are quite low, essential differences can be found by the educational level of the degree and by study fields (Bartus and Róbert, 2019). Master's graduates with pre-degree certificates have better chances of finding their first jobs and their job search duration is typically lower compared to those who finished their bachelor's studies. Also, labor market prospects appear to be most favorable for students in IT and education (Bartus and Róbert, 2019).

training (full-time or part-time), the language of instruction and the location of the training are also available.

By using the above information, unique master's and bachelor's programs at given universities are identified based on the combinations of the university faculty, the program name, the type of training (full-time or part-time), the language of instruction and the location of training. Besides, we also identified those programs, which give similar degrees irrespective of the educational institution, based on the name and level of the study programs. In the study, we will refer to the former as *institution-program* categories, while we will call the latter as *programs*.

Our sample covers those graduate students who finished their master's degrees between 2010 and 2017 and entered the labor market. We do not include those who have just completed their bachelor's programs as they are likely to have continued their studies instead of entering the labor market.[80] In addition, we have excluded from the analysis individuals with any missing master's program-related information. As there are often a long time gap between the fulfilment of course requirements and the graduation date (e.g. due to the lack of language exams), we considered the month of the completion of course units as the end of higher education studies (i.e. the date of obtaining the *absolutorium*/ pre-degree certificate).

5.3.1 Identifying university peers and the proxy of informal help

Although our dataset comprises detailed information on the employment spells of a large number of individuals, it does not contain direct information on either job finding methods or personal networks. However, it offers alternative ways to overcome these shortcomings.

The available educational data makes it possible to find those individuals who attended the same university programs at the same time. Thus, we can identify those university acquaintances who are likely to know each other due to their overlapping study periods. In our analysis, we identify former university peers as those individuals who *both* started and completed the same university programs (either bachelor's or master's) in the same semester.[81] This fairly strict definition, which demands an exact match in the students'

[80] Besides, hiring estimations on the sample of bachelor's graduates would be computationally demanding.
[81] When defining contacts, only those individuals are considered for whom all the necessary information was available for a given completed program (i.e. start date of the program, date of completion of units,

training period, will minimize the chance of falsely classifying individuals as acquaintances. However, in return, we might underestimate the actual number of university connections, as we do not consider those contacts who shifted semesters or the ones from neighboring cohorts. If contacts positively affect individual labor market outcomes (as theory would imply), such measurement error will lead to the underestimation of peer effects.[82]

Using the proposed definition and utilizing the dataset's unique individual and employer identifiers, we can track the workplaces of former university acquaintances, and those cases, when former university peers reunite at the same firms after their graduation. For the hiring estimates, the covariate of interest will be a dummy indicating for each month of unemployment of job seekers if they have any former university peers (either from their bachelor's or master's studies) at firms that could potentially offer relevant job opportunities. We will provide a detailed description of such firms in the next section. When focusing on post-hiring individual outcomes, the indicator of peers denotes those instances when the job seekers started their new job at a firm where any of their former university fellows were already working. In both cases, we require former university peers to work for at least six months at given firms beforehand to be considered valid ties and potential sources of help.[83]

To ensure that the proxy variables reliably measure the effect of university peers, we needed to exclude graduate students of specific programs within the fields of teaching, health, art, and religious activities. Such programs typically facilitate a clear transition to jobs in the public sector, where individuals (at the administrative level) are usually employed by umbrella institutions encompassing, for instance, all the educational or religious institutions. In such cases, the reunion of graduates at the same employers would

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faculty, name of the program, type of training, the language of instruction, location of training and the field of training).

[82] If we underestimate the actual number of social contacts, we will consider a proportion of those with contacts as nonlinked individuals. In such a case, if we assume that connected individuals have better outcomes, then the average outcomes of individuals without links will eventually be better, and thus the difference between the connected and unconnected groups will be smaller. However, if we would have used a more inclusive peer definition, we would overestimate the number of peers. Then some nonlinked individuals with worse average outcomes would be considered as linked, and therefore the difference between the two groups will, again, be smaller.

[83] In Table C1 in the Appendix, however, we present the results of our main estimations with the tenure requirements of min. 1 and min. 12 month as well.

not necessarily reflect actual peer effects but would occur due to technical (administrative) reasons. Therefore, we excluded such graduates.

5.3.2 Estimation datasets

Separate estimation datasets are used to analyze hiring chances and post-hiring labor market outcomes. In both cases, we focus on the labor market histories of individuals (either their job search periods or employment spells) starting six months after the completion of their studies. Since many graduates enter the labor market simultaneously, the chance that two former peers get hired by the same company (just a few months apart) would be inherently higher. By omitting this initial period, we can avoid introducing further bias, which may result in overestimating the impact of peers.

In our hiring estimates, we will focus on unemployment periods of individuals between 2011 and 2017, which were either followed by a job entry or not. Such periods cover months when individuals were already in the labor market (i.e. had finished their studies min. six months ago) but did not have a job. For each month of unemployment, we presented the individuals with a uniquely specified set of firms that could provide them with employment opportunities. Thus, the observations of the hiring dataset will be individual-job search month-potential firm triads.[84] The pool of potential hiring firms is defined as the set of those workplaces that ever employed any students in the standard form of employment, who have completed the same *program* as the job seekers prior to entering the labor market. Only those firms form part of the firm pool that existed in a given month and where the given individuals have not previously worked before. The average number of firm alternatives is 582 in the observation period.[85] Job search periods followed by re-entries to the individual's previous firms are excluded from the analysis to rule out the effects of firm-specific knowledge.

The analysis of the individuals' labor market outcomes after hiring (e.g. wages, prestige, tenure) is based on the first month of those employment spells that started between 2011 and 2017. Spells of military service, re-entries to former firms, cases of atypical forms of employment (such as self-employment, temporary and seasonal work) were excluded

[84] Appendix Table C2 provides an example for the format of the estimation dataset.
[85] The detailed description of these companies can be found in Appendix Table C3. Compared to studies with similar empirical strategies (such as Eliason et al., 2019; Saygin, Weber and Weynandt, 2021) the firm pool in the sub-sample is slightly wider. However, since we introduce fixed effects into our model, the actual number of firm alternatives will be much lower and eventually of a similar magnitude to the indicated studies (see Table C4 in the Appendix).

from the analysis. Also, we have removed spells where monthly wages were missing or (most likely) reflected administrative errors. Regarding the tenure estimates, we have applied a further restriction: to ensure a 2-years long follow-up period, we have chosen to focus only on those employment spells that began between 2011 and 2015.

The two estimation datasets are similar in their composition (see Table 18). Regarding the post-hiring estimation sample, we may observe differences between those who started their jobs with or without former university acquaintances. Linked individuals are, on average, younger, and the share of women is lower among them. At the same time, graduates from the study field of engineering and natural sciences are somewhat overrepresented in this group. The raw advantage of linked individuals can also be observed in log hourly wages and tenure.

Table 18. Characteristics of the estimation sub-samples

	Hiring	Post-hiring outcomes		
		All	With peer	Without peer
Individual characteristics				
Gender				
Female	59.5%	59.6%	55.4%*	59.9%*
Male	40.5%	40.4%	44.6%*	40.1%*
Av. age	28.2	28.1	27.6*	28.2*
Field of study				
Agriculture	6.5%	6.3%	2.7%*	6.5%*
Humanities	21.9%	21.3%	20.8%	21.3%
Social Sciences	13.0%	13.0%	9.0%*	13.3%*
Informatics	2.3%	2.4%	3.4%	2.3%
Law	0.8%	0.8%	1.7%*	0.7%*
Public administration	3.5%	3.4%	8.6%*	3.1%*
Economics	26.8%	27.4%	19.8%*	27.9%*
Engineering	13.8%	14.1%	20.7%*	13.7%*
Sports science	1.3%	1.3%	0.7%	1.3%
Natural sciences	10.2%	10.0%	12.5%*	9.8%*
Have work experience	77.4%	77.9%	2.7%	6.5%
Number of individuals	8 284	7 988	584	7 551
Number of job search periods	10 513	-	-	-
Av. no. of firm alternatives	582.0	-	-	-
Number of job entries	10 130	9 983	590	9 393
Av. no. of job search months	8.1	8.0	9.6*	7.9*
Found job at a peer's firm (%)	5.8%	5.9%	-	-
Characteristics of the new job				
Log hourly (entry) wage	7.0	7.05	7.3*	7.0*
Av. tenure	14.9	15.1	19.7*	14.8*
Occupation				
Manager	3.4%	3.9%	3.1%	3.9%
White-collar worker	93.3%	93.3%	96.3%*	93.1%*
Blue-collar worker	3.4%	2.8%	0.7%*	2.9%*

***Note*:** The hiring sample covers job entries between 2011 and 2017, preceded by at least a 1-month long unemployment period. The post-hiring sample comprises job entries between 2011 and 2017 when the employment form was not atypical. The statistics related to the new jobs are calculated based on the first month of the employment spells. Regarding the post-hiring sample, we measured differences between the job entries of individuals, with and without peers, by two-sided t-tests. *Statistically significant at 0.05 level.

5.4 Estimation strategy

5.4.1 Job-finding chances

When estimating hiring probabilities, we focus solely on unemployment periods. In every job search month (e.g. when the individuals are not employed), we link the individuals to a set of firms that could provide them with employment options. In this way, we will test whether those employer-employee pairings are more likely to realize where the individuals' former university peers are present. Specifically, we will estimate the following linear probability model[86]:

$$Hiring_{i(du)jt} = \alpha + \beta_1 X_{it} + \beta_2 Z_{jt} + \gamma Peer_{ijt} + \delta_{dj} + \pi_t + \varepsilon_{ijt} \quad (21),$$

where $Hiring_{ijt}$ is an indicator variable showing if individual i who graduated with a Master's degree d from university u was hired by firm j one month after the job search month t. The chance of hiring is explained by observable individual characteristics (X_{it}) – such as gender, age, the region of residence, the number of bachelor's and master's programs finished, work experience –, and firm-related features (Z_{jt}) – for example, the sector of the target firms. To control for trend effects, year dummies are also included (π_t). Finally, since hiring probabilities might be improved by the help of former university contacts, we have included an indicator variable proxying the presence of such potential help. $Peer_{ijt}$, our covariate of interest, will mark if at least one university peer of i (either from master's or bachelor's) has been working at firm j at time t for a minimum of six months. The independent error term with zero expected value is: ε_{ijt}.

However, as individuals who are similar in terms of their unobserved characteristics tend to follow similar educational and career paths and typically work at the same types of firms, we may overestimate the effect of peers. To account for such sorting patterns, we apply a fixed effect approach and introduce joint *program-firm* fixed effects (δ_{dj}). In such a specification, we essentially restrict the identification of peer effects to comparisons of those individuals whose most recently completed master's *program* is similar[87], and either have or do not have links at given firms. Those firms, where no one or everyone has acquaintances, do not contribute to the identification of peer effects. γ captures whether the probability of getting into a given firm is different for those with links than for others with similar qualifications. Although this specification can capture a significant part of the selection issues, it cannot account for the presence of specific university program-firm paths. Collective agreements between companies and university program directorates and individual or company-specific preferences may also contribute to the

[86] One may raise concerns regarding the chosen statistical methods. To ensure comparability with the literature (Kramarz and Skans, 2014; Eliason *et al.*, 2019) and to avoid the potential concerns regarding fixed effect logit regressions (Stammann, Heiss and McFadden, 2016), we stick with the use of fixed effects linear probability models.

[87] E.g. individuals with a master's degree of the same name obtained from any universities, such as MA in Sociology.

presence of such pathways. Regional differences in the size and diversity of local labor markets could also result in a higher concentration of individuals with similar qualifications in the same firms. To partially account for such mechanisms, we will introduce two additional specifications. One, in which we include both *program-firm* and *institution-program* fixed effects (δ_{dj} and μ_{du}) into our model. As well as another in which we control for both frequent pathways between firms and groups of master's programs within the same counties and *institution-program* fixed effects (δ_{dcj} and μ_{du}, where c refers to the county of the study program).

The use of joint *institution-program-firm* fixed effects (δ_{duj}) could represent another, perhaps the best, available option to account for all the mentioned confounders at once. This approach, however, inherently imposes some additional restrictions when it comes to the identification of peer effects. In such a setting, we compare the hiring probabilities of individuals with or without peers at given firms who obtained the same master's degrees at the same universities before entering the labor market. However, due to applied fixed effects, the contribution of contacts from the most recently completed programs will only measure differences between cohorts since the set of connections (and thus the set of employed acquaintances) does not vary within year groups. As relationships between subsequent cohorts might exist, we can underestimate the effect of such peers.[88] Other types of contacts, namely acquaintances from bachelor's or previous master's programs (if one has completed more than one), are less affected by this issue. As the individuals' bachelor's programs preceding their master's might vary, the number and distribution of ties gathered from bachelor's programs could be quite different.

After examining the overall effect of former university peers on hiring outcomes, we introduce an additional specification (Eq. (22)) to shed light on the heterogeneity of effects by the type ("origin") of relationships. We use separate indicators for marking the presence of bachelor's and master's ties (MA_{ijt} and BA_{ijt}, respectively). In the results section, we will present coefficients from models with all the previously introduced fixed effects, including δ_{dj} and δ_{duj}.

[88] The specification essentially measures whether individuals are more likely to get hired by firms where their same cohort peers work, compared to those individuals who had the exact same qualifications, but attended to different cohorts. As we introduce more strict requirements for the identification of peer effects (e.g. comparisons between cohorts), the downward bias arising from the misclassification of social ties (e.g. falsely classifying individuals as linked or nonlinked) can affect the results more harshly.

$$Hiring_{ijt} = \alpha + \beta_1 X_{it} + \beta_2 Z_{jt} + \beta_3 MA_{ijt} + \beta_4 BA_{ijt} + \delta_{dj} + \pi_t + \varepsilon_{ijt} \quad (22)$$

5.4.2 Post-hire outcomes

To assess the impact of contacts on labor market outcomes after hiring, we compare the job entries of those individuals who started in firms with at least one former university contact and those who started without any. In doing so, the following general model is estimated:

$$Y_{ijt} = \alpha + \beta_1 X_{it} + \beta_2 Z_{jt} + \beta_3 V_{ijt} + \gamma Peer_{ijt} + \mu_j + \pi_t + \varepsilon_{ijt} \quad (23),$$

where the dependent variable (Y_{ijt}) can take multiple measures. When estimating the effect of former university peers on entry wages $Y_{ijt} = \ln(w_{ijt})$, which denotes the log hourly wage of job seeker i at firm j at the first month of the employment spell (t). To test the effect of contacts on job quality, we use three different measures: $FEOR_{ijt}$ indicates the 1-digit occupation category of the individual's new job, $SIOPS_{ijt}$ captures the prestige score of the acquired job based on the Treiman prestige scale (Treiman, 1977) and $ISEI_{ijt}$ measures the status of the new job according to the ISEI index (Ganzeboom, De Graaf and Treiman, 1992).[89] The latter measures can take up values between 0 and 100, where the higher values represent increased prestige and status, respectively. When estimating the university peers' effect on tenure $Y_{ijt} = \log\left(\frac{p_{ijk}}{1-p_{ijk}}\right)$, where p_{ijk} reflects the probability that individual i will spend at least k month(s) at firm j.

We model the introduced outcomes as linear functions of observable individual characteristics (X_{it}), firm-specific features (Z_{jt}), observable characteristics of the employer-employee pairings (V_{ijt}) such as occupation, unobservable firm-related characteristics (μ_j) and trend effects (π_t). The indicator of university peers $(Peer_{ijt})$, marks if individual i had any university contacts at firm j at time t, who started to work there at min. t-6.

[89] The SIOPS index is a common prestige metric, constructed by averaging and rescaling national prestige scores from local prestige surveys of 60 countries that rank occupation titles. The ISEI index ranks occupations by the average level of education and average earnings of job holders based on comparably coded data from 16 countries. The Hungarian 4-digit occupation categories (FEOR-08) were converted into ISCO-08, then the ISEI and SIPOS measures were merged by using the conversion toolkit of (Ganzeboom and Treiman, 2001).

5.5 Results

5.5.1 Peer effects on hiring

We start by estimating the model described in Eq. (21) on a specific dataset comprising individual-job search month-target firm triads as observations. In addition to controlling for observable characteristics, our model accounts for *program-firm* fixed effects. According to the results, individuals' chance of getting into given firms is significantly higher if they have any former university acquaintance there – even if we limit the comparisons to only those with similar type of master's degrees. The parameter of interest (presented in column (1), Table 19) is 0.0007, which can be considered a meaningful increase compared to the baseline job-finding probability in the sample (0.0002).[90]

Table 19. Effect of former university peers on hiring

	(1)	(2)	(3)	(4)
	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}
	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program×County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE
Former university peers	0.000666***	0.000670***	0.000332***	0.000178*
	(0.000076)	(0.000076)	(0.000075)	(0.000081)
Constant	0.000205***	0.000155**	0.000135*	0.000126*
	(0.000051)	(0.000055)	(0.000057)	(0.000060)
R^2	0.00428	0.00437	0.0100	0.0163
Observations	46 263 778	46 263 778	46 262 304	46 256 215
Program	195	195	195	195
Program–Firm	38 884	38 884	38 876	38 875
(Program×County)–Firm	172 174	172 174	170 700	170 660
Institution–Program	767	767	760	748
Institution–Program–Firm	362 182	362 182	360 708	354 619
Individuals	8 282	8 282	8 275	8 263

Note: The models are estimated according to Eq. (21). The indicator of peers denotes whether a job seeker had at least one former university peer (from any study levels) with a minimum six-month-long employment spell at a given target firm at a given job search month. Additional controls include gender, categorized no. of spells before (0, 1, 2, 3 or more), year dummies, the industry of the potential firms, the no. of bachelor's and master's programs finished and age. Except for the latter two, all covariates are dummies. Specifications (1-3) also include region dummies. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

However, since this specification cannot account for frequent labor flows between specific university programs and firms, the obtained effects are likely to be only upper estimates of the impact of former university peers. The measured effects may still reflect the presence of collective agreements between given firms and programs, firm

[90] The baseline hiring probability is estimated as the mean of predicted hiring probabilities to given firms assuming the lack of contacts at the given firms.

preferences for the graduates of specific university master's (due to their matching skills and knowledge) or even individual preferences regarding the choice of workplaces. Besides, geographical determinants might be at play as well – regional inequalities in the size and diversity of local labor markets might also contribute to the repeated hiring of given graduates and, thus, the reunion of former university peers.

We introduced two slightly modified versions of the previous model (columns (2) and (3)), which can partially account for the above presented mechanisms. In the specification presented in column (2), besides controlling for the *program-firm* paths in the form of fixed effects (δ_{dj}), we also account separately for *institution-program* fixed effects (μ_{du}). In this way, we capture all those characteristics related to specific universities' master's programs (e.g. prestige, the extent of knowledge transferred) that might affect the increased employment and reunion of given graduates. In addition to this, the specification presented in column (3) also provide controls for geographical disparities, as we let the *program-firm* paths to vary by counties (δ_{dcj}). Thus, when identifying peer effects, we compare the chances of those graduates getting into given firms who finished the similar *programs* in the same counties.[91] Regardless of the chosen specification, the magnitude of the coefficients decreased in both cases, confirming that the selection mechanisms indeed contribute to the measured effects. However, the role of geographical factors seems to be more prominent than the characteristics of specific universities' master's programs. The inclusion of the regional dimension reduces the impact of the peers by almost half, indicating that geographical disparities do, in fact largely determine the employment choices and firm targets of individuals and may partly explain the observed effects.

Finally, in our last specification by using the *institution-program-firm* fixed effects (δ_{duj}), we aim to for account for all the mentioned confounders at once. After controlling for such pathways, the effect of peers, although being substantially reduced, remains significant.[92] On the one hand, this indicates that there are indeed pre-established,

[91] Although it seems a tough restriction, we found examples of two identical types of *programs* operating in the same counties in all regions of Hungary. In general, 93 programs and 497 *institution-program* combinations are utilized for identifying the effects.
[92] When estimating the impact of peers with different kind of fixed effects, concerns may arise regarding the similarity of the identification sub-samples contributing to the parameter of interest. In Appendix Table C4 we show that these samples are quite similar in composition. In Appendix Table C5 we also present the magnitude of effects when estimating the parameter of interest on the identification samples of the different

prevalent pathways between specific university master's programs and firms, which can be considered significant drivers of the observed effects. On the other hand, the finding that a considerable part of the measured effect cannot be attributed to such selection patterns provides suggestive evidence for the presence of peer effects. However, we still cannot exclude the possibility of chance encounters at given firms. In the upcoming part of the analysis, in addition to examining the heterogeneity of results by the type of relationships, we present some additional robustness tests that may provide further evidence in favor of our interpretation of the results.

5.5.2 Heterogeneity and robustness

To investigate the heterogeneity of effects by the origin of ties, we introduced an additional specification (described in Eq. (22)), where the presence of bachelor's and master's ties are included separately. Table 20 (similarly to Table 19) shows the magnitude of peer effects using all the fixed effect specification presented earlier. According to the specification with *program-firm* fixed effects (column (1)), ties of both levels significantly affect hiring chances. However, the magnitude of the effect of bachelor's relationships is almost twice as large as that of master's degree ties. As we introduce increasingly strict requirements for identifying effects (columns (2) through (4)), the impact of bachelor's ties becomes smaller in magnitude. Still, it remains significant, while the coefficient of master's ties starts converging to zero and becomes negative in the final specification.

fixed effects. As narrower samples are used, the size of the parameter is reduced to some extent, but remains significant and of similar magnitude.

Table 20. Heterogeneity of peer effects by the level of programs from which relationships originate

	(1)	(2)	(3)	(4)
	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}
	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program×County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE
Bachelor's tie	0.000856***	0.000857***	0.000518***	0.000446***
	(0.000103)	(0.000103)	(0.000102)	(0.000106)
Master's tie	0.000484***	0.000489***	0.000185*	-0.000037
	(0.000090)	(0.000091)	(0.000089)	(0.000098)
Constant	0.000208***	0.000158**	0.000137*	0.000128*
	(0.000051)	(0.000055)	(0.000057)	(0.000060)
R^2	0.00429	0.00438	0.0100	0.0163
Observations	46 263 778	46 263 778	46 262 304	46 256 215
Program	195	195	195	195
Program–Firm	38 884	38 884	38 876	38 875
(Program×County)–Firm	172 174	172 174	170 700	170 660
Institution–Program	767	767	760	748
Institution–Program–Firm	362 182	362 182	360 708	354 619
Individuals	8 284	8 284	8 275	8 263

Note: The models are estimated according to Eq. (22). Separate indicators are introduced for the presence of bachelor's and master's ties with at least six months of employment, in each job search month for each target company. For other controls, see Table 19. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

The finding that bachelor's ties are the main drivers of the previously observed effects seems reasonable. As bachelor's programs typically encompass more students and provide a wide range of opportunities for specialization, those with the same bachelor's degrees might greatly differ in terms of their chosen profession, occupation held and career path.[93] Such diversity might be associated with a higher volume of (better quality) information on vacancies across a broader range of firms. These information benefits may be even more significant as part of these contacts may have already entered the labor market and are better integrated by the time their former peers complete their master's degrees. Conversely, ties from master's studies may be less valuable/capable of providing help, as they share similar professional interests and educational paths to the job seekers and entered the labor market at roughly the same time.

We present two additional robustness checks to provide further evidence in favor of interpreting the results as peer effects. In both cases, we examine the heterogeneity of

[93] In those cases when the job search ended up in job finding, we assessed the similarity between the positions of the job seekers and their ties by using the average of the occupational-relatedness measure introduced in Hidalgo *et al.* (2007). Such calculations revealed that there is a greater dissimilarity between the positions of the individuals and their bachelor's ties, which reinforces this reasoning.

results by program characteristics, which may affect the intensity of contact and the likelihood of forming relationships. Hence, the magnitude of the observed peer effects. We slightly modify the formerly used contact variables and introduce separate indicators for ties gathered from programs with different training types (full-time and part-time studies) or different sizes (e.g. small and large). If former university peers directly or indirectly contribute to the hiring chances of individuals, we would expect to see a more pronounced effect for contacts from higher contact-intensity programs.

Table 21 presents the heterogeneity of results by program size. We report the effect of those contacts from small and large bachelor's and master's programs separately (under and over 50 or 25 students, respectively). In line with our expectations, the results show more substantial effects for those peers from smaller programs. The other robustness check yielded similarly encouraging results: the measured effects are more than three times larger for those who conducted their studies full-time and thus interacted with their former peers more often (see Table 22).

Table 21. Peer effects by program size

	(1)	(2)	(3)	(4)
	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}
	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program×County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE
Bachelor's tie (size<=50)	0.001872***	0.001860***	0.001261***	0.001203***
	(0.000320)	(0.000319)	(0.000313)	(0.000332)
Bachelor's tie (size>50)	0.000642***	0.000646***	0.000366**	0.000296*
	(0.000104)	(0.000104)	(0.000111)	(0.000115)
Master's tie (size<=25)	0.000765***	0.000769***	0.000288	-0.000055
	(0.000141)	(0.000142)	(0.000148)	(0.000162)
Master's tie (size>25)	0.000225*	0.000230*	0.000088	-0.000020
	(0.000089)	(0.000090)	(0.000093)	(0.000103)
Constant	0.000206***	0.000155**	0.000136*	0.000129*
	(0.000051)	(0.000054)	(0.000056)	(0.000060)
R^2	0.00429	0.00438	0.010	0.0163
Observations	46 263 778	46 263 778	46 262 304	46 256 215
Program	195	195	195	195
Program–Firm	38 884	38 884	38 876	38 875
(Program×County)–Firm	172 174	172 174	170 700	170 660
Institution–Program	767	767	760	748
Institution–Program–Firm	362 182	362 182	360 708	354 619
Individuals	8 282	8 282	8 275	8 263

Note: The models are estimated according to the slightly modified version of Eq. (22). We introduce separate indicators for those bachelor's and master's ties that originated from either small or large programs (the threshold is 50 students for bachelor's degrees and 25 for master's). For additional controls, see Table 19. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table 22. Peer effect by the form of study

	(1)	(2)	(3)	(4)
	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}
	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program×County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE
Full-time study	0.000858***	0.000857***	0.000447***	0.000275**
	(0.000096)	(0.000096)	(0.000096)	(0.000100)
Part-time study	0.000243**	0.000251**	0.000061	-0.000076
	(0.000090)	(0.000090)	(0.000088)	(0.000100)
Constant	0.000210***	0.000157**	0.000136*	0.000127*
	(0.000049)	(0.000055)	(0.000056)	(0.000060)
R^2	0.00429	0.00438	0.0100	0.0163
Observations	46 263 778	46 263 778	46 262 304	46 256 215
Program	195	195	195	195
Program–Firm	38 884	38 884	38 876	38 875
(Program×County)–Firm	172 174	172 174	170 700	170 660
Institution–Program	767	767	760	748
Institution–Program–Firm	362 182	362 182	360 708	354 619
Individuals	8 282	8 282	8 275	8 263

Note: The models are estimated according to the slightly modified version of Eq. (21). The indicator of peers is interacted with the form of study (full-time or part-time); for additional controls, see Table 19. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

5.5.3 Wage outcomes and job quality

In the second part of the analysis, we take a step forward and test whether those individuals who were hired by the firms of their former university fellows acquired higher wages and better-quality jobs compared to others.[94] We estimate the general model described by Eq. (23) on a subsample comprising the first month of the graduates' employment spells. The indicator of peers marks those instances when the job seekers were hired by firms where their former university fellows had already been working for six months.

When investigating the effect of university peers on monetary outcomes, we start by estimating an OLS regression with the dependent variable being the logarithm of the individuals' hourly wages in the first month of their employment spells (see Table 23). The result of the first specification, in which we did not include additional fixed effects, reflects a significant wage advantage for those who started a job at their peers' firms, notably 16%. However, this gain might capture some unintended bias if students with a

[94] The raw differences with respect to job quality and wages suggest the benefits of individuals with links: their wages are slightly higher, while their tenure is longer (see Table 18). Compared to those without links, a higher share of these individuals starts in white-collar positions, while a lower number of them as managers.

given type of training systematically earn more in the market, while also having a higher chance of getting into the same firms. Such discrepancies might occur both on the aggregate level of *programs* and at the level of specific universities' master's programs.

Table 23. Peer effects on entry wages

	(1)	(2)	(3)	(4)
	No FE	Program FE	Institution-Program FE	Firm FE
Former university peer	0.1639*** (0.0203)	0.1151*** (0.0219)	0.1199*** (0.0210)	0.0140 (0.0194)
Constant	6.3790*** (0.1319)	6.5393*** (0.1270)	6.5169*** (0.1245)	6.9941*** (0.1670)
Job entries	9 968	9 862	9 949	6 190
Job entries with peers	590	590	589	562
No. of firms	4 905	4 895	4 855	1 127
R^2	0.283	0.342	0.288	0.656

Note: The models are estimated according to Eq. (23). The dependent variable is the hourly log wage of the individual in the month of their job entry. The indicator of peers denotes whether the job seekers had any former university peers at their new firms with a minimum six-month tenure. Additional controls include gender, region of residence, the field of study of the latest master's program, the industry of the new firm and the 2-digit occupation category of the new job, year dummies, quadratic age and work experience. Except for the latter two, all covariates are dummies. In the specifications (2-3), we have applied program and institution-program fixed effects, while in the specification (4), we used firm fixed effects. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

We introduce two model specifications to control these issues, accounting for either *program* or *institution-program* fixed effects. Peer effects will capture the average difference between graduates of the same program type who started their jobs with or without peers. Column (2) and (3) shows that the parameter of interest is gradually decreasing as we restrict the comparisons to more similar individuals in terms of their finished master's programs. When applying *institution-program* fixed effects, the wage gain decreases to 11%, which is still quite considerable.

Such wage gain, however, might result from various mechanisms. Besides the direct effect of social ties (e.g. referral), former university ties (through information transmission) might induce the sorting of their high-productivity peers to firms or support the selection of their acquaintances to firms providing higher premia. To shed some light on the underlying mechanisms, we re-estimated our model with firm fixed effects, which can capture the unobserved, time-invariant characteristics of the receiving firms, such as their average level of wage premium. Such a setting restricts the analysis to those job entries where the receiving firms hired at least two individuals in the observation period. We will compare job entries to the same firms with and without peers when identifying peer effects. After including fixed effects, the formerly positive, non-negligible effect essentially disappears. This implies that the measured wage gains are only by-products of a selection mechanism: university contacts seem to promote the hiring of individuals to those firms where fresh graduates (and probably all workers) typically earn more.

The estimated wage gain, irrespective of the mechanism in play, could be an essential help for those who are early in their careers, and it may also create additional advantages in the long run. The same holds for the quality of the newly acquired positions: better entry positions might jumpstart the graduates' careers and lead to more successful career tracks. To investigate the role of former university peers on job-related outcomes, we have introduced three measures as dependent variables, which aim to capture different aspects of job quality. We measure the position of individuals on the occupational ladder with 1-digit occupation codes (which can take up values between 1-9), while we capture the prestige and status of the acquired jobs by the SIOPS (Treiman, 1977) and the ISEI (Ganzeboom, De Graaf and Treiman, 1992) measures (both covers values between 0-100). We estimated two specifications for each outcome variable, with and without firm fixed effects, and have presented the results in Table 24.

The presence of peers affects all the introduced job quality measures. Job seekers with links got into positions with (on average) lower occupational codes, which entail jobs that are higher up in the occupational ladder.[95] Former university fellows also affect the prestige and status of the new jobs: positions acquired though peers have 1.22 point higher SIOPS and 2.84 higher ISEI scores on the average. The effect size is meaningful in both cases; the parameters represent a 2.3% and 4.5% improvement, respectively, compared to the average scores in the estimation sample. When we restricted the identification of peer effects to comparisons within given workplaces (through firm fixed effects), all parameters decreased in magnitude but remained significant. This implies that the position-related advantages of linked individuals are also, to some extent, driven by firm selection – similarly to wage gains –, but not entirely.

[95] We re-estimated our model by using an ordinal logit regression, which is more suited to the measurement level of the dependent variable. The results, presented in Appendix Table C6, suggest that the measured positive effect is mostly driven by the higher chance of linked individuals to acquire jobs in the top occupational categories (encompassing managers and professionals).

Table 24. Peer effects on job quality

	(1)	(2)	(3)	(4)	(5)	(6)
	1-digit occupation		SIOPS		ISEI	
	No FE	Firm FE	No FE	Firm FE	No FE	Firm FE
University peer	-0.1826***	-0.0891*	1.2283**	0.7946*	2.8462***	1.2517*
	(0.0380)	(0.0383)	(0.4007)	(0.4048)	(0.5756)	(0.5823)
Constant	2.8572***	2.8264***	54.0559***	51.1999***	62.5018***	64.0293***
	(0.2665)	(0.1592)	(1.6547)	(1.1214)	(2.6481)	(1.6749)
Job entries	9 983	6 201	9 891	6 121	9 891	6 121
Job entries with peers	590	562	582	554	582	554
No. of firms	4 912	1 130	4 894	1 124	4 894	1 124
R^2	0.110	0.497	0.216	0.617	0.199	0.601

Note: Based on Eq. (23), linear regression models were estimated with three different dependent variables: the 1-digit occupation category of the new jobs, the prestige and the status of the positions (based on the measures of SIOPS and ISEI). The indicator of peers denotes whether the job seekers had any former university peers at their new firms with a minimum of six-month tenure. Additional controls include gender, age, region of residence, the field of study of the latest master's program, the industry of the new firm, and year dummies. Except for work experience and age, all covariates are dummies. In specifications (2), (4) and (6), we used firm fixed effects. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

In our last set of estimates, we investigated whether those individuals with peers have longer expected tenure. We estimated multiple logistic regressions, where the dependent variables mark if the employment spell of the individuals reached a minimum length of 3, 6, 12, 18 or 24 months, respectively. The results of Table 25 suggest that university peers substantially increase the chance that an employment spell becomes stable: linked individuals will be around 1.6 times more likely to have a minimum 1-year or 2-years long spell. However, we can observe that the measured effects disappear when we consider short spells as well (by applying lower minimum tenure requirements).

This suggests that hiring through university peers/from the employees' university networks cannot guarantee that the new workers will surely stay at a given company in the long run and might not reduce fluctuation in the first few months after hiring. However, if the individuals make it through the first few months (essentially passing their probationary period), they are more likely to stay at the company permanently. This finding might be explained by (among other things) the ideas of job shopping models (Johnson, 1978; Topel and Ward, 1992) that propose the frequent job switches of individuals at the beginning of their careers. The lack of peer effects on the probability of having shorter periods at given firms may signal that university ties cannot attenuate all those uncertainties related to given individual-organizational matches or career perspectives.

Table 25. Tenure at the new firms

	(1)	(2)	(3)	(4)	(5)
	Min. 3 months	Min. 6 months	Min. 12 months	Min. 18 months	Min. 24 months
University peer	1.0245	1.3601*	1.6590***	1.5613***	1.7605***
	(0.2061)	(0.2134)	(0.2186)	(0.1880)	(0.2110)
Constant	0.0028***	0.0033***	0.0006***	0.0004***	0.0003***
	(0.0044)	(0.0044)	(0.0006)	(0.0004)	(0.0003)
Job entries	5 618	5 645	5 656	5 654	5 654
Job entries with peers	341	343	343	343	343
No. of firms	3 034	3 044	3 049	3 047	3 047
Pseudo-R^2	0.0960	0.0880	0.0983	0.0935	0.0890

Note: The models are estimated according to Eq. (23). The dependent variables denotes whether the individuals spent at least 3, 6, 12, 18 or 24 months at their new company (specifications (1)-(5), respectively). The indicator of peers denotes whether the job seekers had any former university peers at their new firms with minimum six-month tenure. Additional controls include gender, age, region of residence, the field of study of the latest master's program, the industry of the new firm, and year dummies. Except for work experience and age, all covariates are dummies. The coefficients of the logistic regressions are presented in an exponentiated form. Robust standard errors are in parentheses. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Similarly, as before, we re-estimated our specifications with firm fixed effects included (see Appendix Table C7). As in the former wage regressions, the previously observed effects disappear, suggesting that the positive benefits by university peers in terms of job stability are also to some extent driven by firm selection. Individuals with social ties are more likely to get into firms where all career entrants tend to stay longer. However, the reduced sample size due to the within-firm comparisons and the additional time constraints may also contribute to the lack of effects.

5.6 Discussion

In this study, we aimed to broaden our understanding of a specific group of contacts, namely former university ties, in the labor market and investigate whether such acquaintances contribute to the success of master's graduates entering the labor market. Using a large administrative panel dataset from Hungary and proxying former university peers with shared training periods at the same university programs, our estimations aimed to uncover the role of such relationships on the chances of finding a job and the quality of the jobs acquired.

Our findings suggest that university graduates are more likely to start at those firms where their former peers work. However, a considerable part of the observed effects is attributable to the selection of individuals along existing pathways between university master's programs and firms. Such paths may reflect individual or company preferences, collective agreements between programs and firms (providing mentoring or training for given types of graduates), or even the regional characteristics of employment opportunities. Our results demonstrate that even after accounting for such sorting patterns, individuals are still more likely to end up working at their former peers' firms, providing suggestive evidence for the presence of peer effects. Our further robustness tests also supported this interpretation of the results. When investigating the heterogeneity of results by the type of ties, we show that the beneficial effect of peers on job-finding is mostly driven by the positive contribution of bachelor's ties. Such contacts, unlike peers from master's, can provide more information on a broader range of employment opportunities and, due to their varied educational and career paths they can help rather than crowd each other out of given firms. Finally, when focusing on post-hiring outcomes, we found that the newly acquired jobs of those who started at their peers' workplace are better in some respects. For instance, they are characterized by higher wages, higher status and greater prestige. The benefits related to wages and job stability are primarily attributable to the fact that former peers promote the selection of their acquaintances to firms where all career entrants earn more or stay longer. However, in terms of positional advantages, even after restricting the analysis to within-firm comparisons, we observe an advantage for those with links over those without.

The measured gains can be considered essential benefits for career entrants. Finding the right career track and acquiring suitable employment have always been a challenge for graduate students. Especially nowadays, when the demand-side labor market expectations increase toward graduates and change quickly. Informal ties, especially professionally relevant ones, can speed up the labor market integration of graduates and, by helping them acquire better quality employment, can jumpstart their careers. Moreover, the help of contacts can even contribute to significant benefits in the long run, as early labor market situations might affect later outcomes.

Aside from the presented benefits, university ties may have a crucial role in shortening the job-shopping period of graduates and speeding up their labor market integration. As they can provide useful insights about profile-fitting jobs and potential firms, they might increase the chance of individuals to acquire employment options that fit the best for their interests and skills. Therefore, relying on professional ties might even moderate the creation of horizontal and vertical education-job mismatches. The investigation of this topic is, however, requires additional research as our results only intuitively imply the presence of such benefits.

6. Conclusion and Discussion

Social networks are essential components of the fabric of everyday life. Individual lives are intertwined through social relationships and whether we perceive it or not, social interactions can actually shape our actions, decisions, opportunities and constraints. Therefore, if we want to describe economic outcomes, individual decisions, or social processes properly, we can't do so without taking into account the larger context of our environment, the web of social relations.

This research aims to call the attention to the important role of social networks in shaping labor market outcomes and structuring inequalities. It contributes to the disciplines of sociology, economics and social policy by focusing on the intermediate level of the social world and by facilitating discussions about the dual nature of networks as sources of economic benefits and amplifiers of inequalities. The thesis adds to the long-standing research tradition examining relations of networks and individual economic opportunities by both applying a fresh empirical approach and providing new empirical results and original insights using administrative linked employer-employee data.

The dissertation started with an introduction to classical theories and concepts that provide a good basis for understanding how sociologists and economists think about the role of networks in labor markets. Also, the section presented the main empirical results in the field, as well as methodological advances in the study of network effects in the labor market.

Chapter 3-5 presented three studies on the role of professional network on the labor market. These chapters share many similarities. First, all studies are based on the same Hungarian administrative linked employer-employee dataset, although some differences can be found between the time windows used. The first two empirical chapters are based on the observation period of 2003-2011, while the third one focuses on the years of 2010-2017 (as educational data is available from 2009 onward). Second, the definition of contacts rests on the same ideas and strategies. For the identification of professional ties, we utilized the co-occurrences of individuals working at the same, relatively small firms (Chapter 3 and 4) or attending to the same university programs at the same time (Chapter 5). Third, throughout the chapters we have focused on the same types of economic outcomes and used similar empirical approaches to identify the impact of professional ties. These studies are typically structured to analyze both hiring benefits by social ties and labor market outcomes after hiring. For the hiring analysis, we utilized identifiable cases of job losses (due to closures) or job search periods (unemployment months). As the job application history of individuals is not available in administrative datasets, like other studies in the literature (Kramarz and Skans, 2014; Saygin, Weber and Weynandt, 2021), we linked the individuals to a set of firms that could provide them with employment options. When it comes to labor market outcomes after hiring, we focused on job switches. The investigated outcomes covered wages, the prestige and status of the new jobs and occupational rank. However, while in Chapter 3 and 5, we were interested in mostly the temporal gains after job switches, in Chapter 4 we aimed to identify the effect of social ties on upward mobility through inter-firm mobility (within-individual gains) in terms of job and firm quality. Finally, all studies rely heavily on the use of multidimensional fixed effect approaches (Abowd, Kramarz and Margolis, 1999; Cardoso, Guimarães and Portugal, 2016) as a solution for capturing unintended bias and controlling for the unobserved characteristics of firms and individuals.

The findings suggest that professional ties have a strong impact on all the outcomes examined. On the one hand, both former co-workers and university peers meaningfully increase the hiring benefits of individuals: they are more likely to get a job at those firms where their contacts work. However, the magnitude of effects compared to the within-sample baseline hiring probabilities does vary. While the presence of co-workers increases the hiring probabilities by 6.2 based on Chapter 4, the presence of university peers multiplies the hiring chances of career entrants by only 3 (Chapter 5). The difference in the magnitude of the effects could reflect differences in the capacity of the two types of ties in providing help. In contrast to former co-workers, it seems that university peers may not be able to give as reliable signals about the productivity of their acquaintances or may not be able to counterbalance the negative impact of their peers' limited labor market experience due to their limited bargaining power. These arguments are also consistent with the findings of the post-hiring analysis, namely that the gains realized after hiring are mainly driven by the selection of individuals into firms that offer greater benefits for career entrants. The findings also reinforce the essential role of the characteristics of contacts (such as gender and occupation) and provide evidence on the importance of tie similarity.

Regarding post-hiring outcomes, wages received the most attention. Besides showing the presence of wage gains attributable to professional ties, we made serious endeavors to identify the various sources of such benefits. In Chapter 3, by introducing and applying a wage-decomposition technique, we demonstrated that former co-workers may funnel individuals into high-paying firms and enhance the sorting of good quality workers into firms, besides their direct effect and contribution to the creation of better employer–employee matches. While for men, the gains from the latter two accounted for the largest share of wage growth, for women the individual selection term was the most significant (attributable to the better quality of workers). In contrast, the wage gains from having former university connections were mostly attributable to firm selection, reflecting that individuals can access higher premium firms through their connections. We also found that former co-workers can enhance upward mobility as well. By comparing the firm- and job-related outcomes of individuals at their new firm to the ones of their previous workplace, we showed that workers can acquire better outcomes if they start their new job at the firms of their acquaintances. Individuals with links are more likely to acquire better positions in the general and within-firm wage distribution, and their chances of getting into higher premium firms is also better. However, while contacts benefit men irrespective of their pre-movement labor market situation, among women only those with average or worse firm-specific and job-related characteristics receive such gains.

The presented research provided new empirical results and essential insights on the role of professional relationships in the labor market. However, despite our best efforts, the research has some limitations. It cannot provide detailed, indisputable evidence on the exact drivers of the observed network-related effects, and also, there is no way to distinguish between the all the theoretical explanations that could explain them. Although in Chapter 3, we made serious efforts to connect the most important mechanisms (referral and information transmission) to wage-related gains, the results still only provide suggestive evidence and require accepting some general (yet plausible) assumptions. Additional evidence on this topic or the validation of the results would require the use of either other types of methods (e.g. qualitative analysis[96] or experiments), or different kind of datasets (surveys linked with administrative LEEP datasets).

[96] As supporting evidence, we made interviews with HR managers and CEOs, who reinforced the relevance of the screening and signaling explanations and also highlighted the agency and mentoring role of acquaintances during/after hiring. However, based on such interviews we could not quantify the relative weight of each of these explanations during hiring events.

Another concern might be related to the generalizability of the results. Even though the dataset used covers half of the Hungarian population, the analysis is based on a smaller sub-sample, for instance including only those who are employed with a standard employment contract. Also, for the correct identification of network-related effects, we applied some further restrictions as well (such as having experience at smaller firms). Despite the fact that introducing such requirements somewhat limits the generalizability of results, those still apply to a considerable amount of the society, for which we could present high-quality, reliable estimates. It is also worth to note, that the presented empirical results cannot provide information on the newest trends and patterns related to network effects and refer only to Hungary. Nevertheless, the results are in line with the ones of the international literature.

As the research is based on large datasets, concerns could be raised about the correct interpretation of statistical significance. Since standard errors decrease with larger sample size, even very small differences/effect sizes, which may not be substantial from a practical point of view, could yield significant p-values. This issue, which nevertheless is an important one, however only partially applies for the presented results. On the one hand, most of the specifications focus on specific labor market events (such as job switches), or given sub-samples (e.g. freshly graduated individuals), thus the actual sample sizes are typically not as enormous. On the other hand, it is important to note that we control for fixed effects essentially in all equations, which restricts the analysis to within-unit comparisons. For instance, when using firm fixed effects, we will compare only those individuals working at the same firms, which obviously imply a rather modest set of observations compared to the total sample size. Besides, in each empirical chapter we have conducted a wide range of robustness tests showing that statistical differences are not inherent byproducts of using such data.

Finally, the economic significance of the measured effects could be also questioned. From a practical point of view, the obtained network-related effects may seem small. However, they could still represent essential benefits for the individuals receiving them. We saw that the magnitude of wage gains by former co-workers was on average around 4-5%. By calculating with the net average earnings in Hungary in 2010 (107 000 HUF based on KSH (2022c)), such gains would essentially yield 5000-6000 HUF extra in every month. In 2010, this amount would have been enough to buy a monthly public transport pass, to cover half of the average common costs of a 50-60 m2 apartment or 27% of the monthly food costs of a single person.[97] For those, who actually earned such money, this extra could represent meaningful help and might have improved the perception of their quality of life. Regarding the hiring analysis, the benefits are more tangible intuitively as well: the presence of informal ties multiplies the individuals' chances of getting into given firms. What we deemed important to highlight here is it large depends on the context and a variety of factors that how we think about the magnitude of effect sizes.

Although the research addressed many relevant areas in which social relations may play an important role, there is still room for additional research. As highlighted earlier, the greatest uncertainty in the literature relates to the actual drivers of network effects and their relative importance. However, answering such questions would require either controlled research designs (e.g. experiments) or costly (and sometimes legally infeasible) new data collection methods (e.g. surveys linked with administrative records). Research in this area would certainly be beneficial.

Further work should continue to explore the role of individual networks in shaping individual labor market opportunities, as a number of important questions remain open. For extending the research on match quality, it would be interesting to assess how different types of relationships (e.g. family, neighbors, university peers) may differ in their capacity to link individuals to jobs that fit better for their educational attainment and profession. Intuitively, one might expect that professional ties function as essential forces that keep individuals within the same professions, while being less likely to facilitate transitions characterized by horizontal or vertical mismatches.

On-the-job peer effects can be considered another interesting branch of research, which provides a number of opportunities for additional work. It would be worth exploring how the individuals' networks of colleagues within the companies evolve after recruitment, how the presence of acquaintances at one's new firm affect the speed of networking, the number of colleagues known and, for instance, the presence of bridging links between different work groups. Assessing the role of such network-related characteristics on either intra-organizational outcomes or the likelihood of changing jobs would be an interesting venue. Besides, investigating how group-level characteristics, the quality of teamwork or

[97] The calculations are based on the KSH (2022b) and KSH (2022a)

team productivity affects the long-term outcomes of individuals (e.g. job tenure, occupational mobility) would also be beneficial and could provide useful insights for both firms and individuals as well.

The role of individual relationships in explaining inter-firm cooperation and firm performance is another interesting line of research. The mobility of individuals between firms might induce the spreading of knowledge and information, which then may contribute to regional convergence, but could also create disparities between firms (located in the center versus the periphery of the mobility network). Also, the role of managers in spreading innovation is another key area that needs further research. It would be worth investigating whether managers who changed their jobs can increase the productivity of their new firms by utilizing their existing informal ties (such as co-workers) and former business relationships (the former firms' supplier or client networks). Finally, the role of inter-firm team movements in influencing within-industry or regional competitiveness is also an essential, yet less investigated area.

Social relations will always be a fundamental part of our lives as they imbue the fabric of society. Changes in various aspects of our lives (such as work, private life and lifestyle) clearly have an impact on the social world, which adapts and evolves accordingly to the ever-changing social and market environment. Such spillover effects ensure that research into personal networks will never become obsolete and will always contribute to a better understanding of the cogs and wheels of society. Hopefully this thesis will inspire further academic work on the role of social ties, within or even outside the labor market, and will provide a great basis for further research.

7. Appendix

Appendix A

Table A1. Decomposition of co-worker gains by occupations - female results

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Manager$	−0.0415	−0.0809	0.0168	0.0226	0.0372	0.0804**	−0.0204	−0.0579**
	(0.0416)	(0.0463)	(0.0345)	(0.0216)	(0.0320)	(0.0289)	(0.0141)	(0.0198)
$Skilled_W$	0.1648***	0.0784	0.0157	0.0706**	0.0015	0.0208	0.0142	0.0498*
	(0.0425)	(0.0403)	(0.0293)	(0.0253)	(0.0264)	(0.0352)	(0.0152)	(0.0227)
$Unskilled_W$	0.0503**	0.0081	0.0296*	0.0126	0.0357**	0.0150	−0.0061	−0.0024
	(0.0158)	(0.0194)	(0.0128)	(0.0113)	(0.0115)	(0.0157)	(0.0074)	(0.0098)
$Skilled_B$	0.0275*	−0.0107	0.0343***	0.0039	0.0298***	0.0170	0.0044	−0.0132
	(0.0111)	(0.0140)	(0.0090)	(0.0100)	(0.0074)	(0.0124)	(0.0062)	(0.0089)
$Unskilled_B$	−0.0097	−0.0116	0.0158	−0.0138	0.0178*	−0.0115	−0.0020	−0.0023
	(0.0140)	(0.0190)	(0.0102)	(0.0125)	(0.0090)	(0.0158)	(0.0057)	(0.0109)
N	964 807	501 200	964 807	964 807	943 643	571 443	964 807	964 807
N_i	616 386	197 435	616 386	616 386	616 365	223 021	616 386	616 386
N_j	105 818	61 121	105 818	105 818	84 655	105 778	105 818	105 818
R^2	0.327	0.860	0.190	0.200	0.443	0.612	0.052	0.086

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variable of interest, the proxy for links, is interacted with ten categories based on gender and five occupational categories: managers, skilled white-collar, unskilled white-collar, skilled blue-collar, and unskilled blue-collar workers. Only the parameters for female workers are presented. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table A2. Decomposition of co-worker gains by occupations

	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
$Manager$	−0.0849*** (0.0223)	−0.0262 (0.0259)	−0.0508** (0.0183)	−0.0080 (0.0121)	−0.0397* (0.0171)	0.0378 (0.0196)	−0.0111 (0.0079)	−0.0458*** (0.0109)
$Skilled_W$	0.1146*** (0.0240)	0.0620** (0.0202)	0.0046 (0.0158)	0.0480** (0.0156)	−0.0026 (0.0141)	0.0130 (0.0205)	0.0072 (0.0088)	0.0349* (0.0136)
$Unskilled_W$	0.0558*** (0.0126)	0.0244 (0.0137)	0.0223* (0.0096)	0.0091 (0.0089)	0.0249** (0.0086)	0.0022 (0.0116)	−0.0026 (0.0056)	0.0069 (0.0076)
$Skilled_B$	0.0507*** (0.0060)	0.0159* (0.0068)	0.0236*** (0.0044)	0.0112* (0.0049)	0.0156*** (0.0038)	0.0132* (0.0058)	0.0079** (0.0030)	−0.0020 (0.0043)
$Unskilled_B$	0.0349*** (0.0072)	0.0079 (0.0070)	0.0287*** (0.0044)	−0.0017 (0.0063)	0.0278*** (0.0041)	0.0117 (0.0076)	0.0009 (0.0028)	−0.0134** (0.0051)
N	964 807	501 200	964 807	964 807	943 643	571 443	964 807	964 807
N_i	616 386	197 435	616 386	616 386	616 365	223 022	616 386	616 386
N_j	105 819	61 121	105 819	105 819	84 655	105 779	105 819	105 819
R^2	0.327	0.860	0.204	0.200	0.453	0.612	0.052	0.087

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry (Eq. (7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eqs (8-13)), respectively. The selection parameters in the columns reflect overall, within unit and between unit differences in individual and firm effects, respectively. Our variable of interest, the proxy for links, is interacted with five occupational categories: managers, skilled white-collar, unskilled white-collar, skilled blue-collar, and unskilled blue-collar workers. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table A3. Co-worker gains and skill requirements

	Linked	**Skill**	**Interaction**
Baseline	0.0172*** (0.0046)	−	−
Manual Dexterity	0.0172*** (0.0047)	−0.0449*** (0.0020)	−0.0026 (0.0052)
Stamina	0.0172*** (0.0047)	−0.0445*** (0.0019)	−0.0066 (0.0055)
Persistence	0.0167*** (0.0046)	0.0441*** (0.0014)	0.0037 (0.0052)
Stress Tolerance	0.0174*** (0.0046)	0.0308*** (0.0014)	−0.0024 (0.0050)
Analytical Thinking	0.0164*** (0.0046)	0.0469*** (0.0015)	0.0053 (0.0050)
Complex Problem Solving	0.0159*** (0.0046)	0.0546*** (0.0016)	0.0056 (0.0048)
Active Learning	0.0167*** (0.0046)	0.0528*** (0.0015)	0.0016 (0.0052)
Coordination	0.0174*** (0.0046)	0.0398*** (0.0014)	−0.0025 (0.0044
Cooperation	0.0171*** (0.0046)	0.0203*** (0.0015)	−0.0022 (0.0052)
Adaptability/Flexibility	0.0170*** (0.0046)	0.0329*** (0.0015)	0.0026 (0.0050)
Originality	0.0163*** (0.0046)	0.0436*** (0.0015)	0.0066 (0.0052)
Innovation	0.0158*** (0.0046)	0.0329*** (0.0014)	0.0081 (0.0048)
Independence	0.0172*** (0.0046)	0.0152*** (0.0015)	0.0020 (0.0049)

Note: Estimates result from the main regression on the logarithm of daily earnings upon job entry with two-way fixed effects (Eq. (7)). Our variable of interest, the proxy for links, is interacted with the demeaned values of skill requirement measures from the O*Net database. For the list of additional controls, see Table 3. Standard errors are in parentheses and clustered at both firm level and individual level. All regressions are based on 483 418 observations and have an R2 between 0.860 and 0.861. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Appendix B

In order to quantify the unobserved quality of firms and individuals, an AKM-style (Abowd, Kramarz and Margolis, 1999) wage regression was estimated on the connected set of the entire dataset augmented with occupation effects (see Eq. (B1)). Due to computational feasibility only one individual observation was kept from each quarter of year.

$$\log(w_{ist}) = \beta_0 + \beta_1 X_{it} + \beta_2 Z_{st} + \gamma_i + \delta_j + \omega_k + \pi_t + \varepsilon_{ist}\,, \qquad (B1)$$

The model presumes that the logarithm of individual wages can be decomposed into observable worker and firm characteristics (X_{it} and Z_{st} respectively), time effects (π_t), and unobservable heterogeneity related to persons (γ_i), firms (δ_j) and occupations (ω_k). The decomposition exploits multiple instances of job mobility from the same individuals, and multiple entries to the same firms for the identification of these factors. After estimating Eq. (B1), the obtained fixed effects, which reflect the latent quality of individuals and firms in the form of residualized wage terms, were saved and included in the main regressions.[98] The controlled within-firm gender difference was defined as the difference in residuals coming from Eq. (B1) within firms among genders.[99]

[98] The model was estimated based on the method of (Correia, 2017). The list of controls covers quadratic and cubic age and the logarithm of firm size.
[99] These will be relative gaps compared to the overall gender wage gap between men and women, which is 0.2.

Table B1. Characteristics of the target firms

Size of the firm	N	%
<10	38 983	16.27
11-250	117 717	49.12
250+	82 944	34.61
Ownership		
Foreign	6 169	2.57
Domestic	147 102	61.38
State-owned	33 818	14.11
Unknown/No available data	52 555	21.93
Sector		
Agriculture	5 200	2.17
Industry	67 929	28.35
Trade and Services	102 964	42.97
Education, Social, Other	9 031	3.77
Unknown	54 520	22.75
Productivity (Value-added per worker)		
Below the yearly median productivity	63 694	26.58
Above the yearly median productivity	97 742	40.79
No. of firms with no available data	78 208	32.64
Target firm-month observations	239 644	
Unique target firms	50 377	

Note: The observations are target firm-month units. Yearly median productivity was calculated based on all Hungarian firms with available data.

Table B2. The effect of former co-workers on job finding (by omitting 2011)

	(1) Without FE	(2) Sending firm FE	(3) Sending-Target FE
Panel A) Baseline			
Co-worker	0.0053***	0.0048***	0.0023***
	(0.0003)	(0.0003)	(0.0002)
R^2	0.0021	0.0076	0.2527
Panel B) Gender of job seeker			
Female with co-worker	0.0053***	0.0045***	0.0019***
	(0.0005)	(0.0005)	(0.0004)
Male with co-worker	0.0053***	0.0049***	0.0024***
	(0.0004)	(0.0004)	(0.0003)
R^2	0.0021	0.0076	0.2527
Panel C) Gender of job seekers and contacts			
Female job seeker			
Female co-worker	0.0047***	0.0037***	0.0015*
	(0.0007)	(0.0007)	(0.0006)
Male co-worker	0.0034***	0.0027***	0.0012*
	(0.0007)	(0.0007)	(0.0006)
Both female and male	0.0140***	0.0145***	0.0053
	(0.0038)	(0.0038)	(0.0034)
Male job seeker			
Female co-worker	0.0024***	0.0020***	0.0015**
	(0.0006)	(0.0006)	(0.0006)
Male co-worker	0.0050***	0.0046***	0.0022***
	(0.0004)	(0.0004)	(0.0003)
Both female and male	0.0108***	0.0109***	0.0047*
	(0.0026)	(0.0026)	(0.0022)
R^2	0.0030	0.0085	0.2528
Observations	983 617	983 617	973 886
No of job seekers	8 514	8 514	8 269
No of firms	1380	1380	1231
No of sending-target firm pairs	93 309	93 309	83 578

Note: Based on Eq. (14) three specifications are presented: estimates without fixed effects, with only sending firm fixed effects or with sending-target fixed effects (columns (1-3), respectively). The outcome variable measures whether a given closing firm-target firm job switch has been realized. Panel A presents the overall effect of co-workers, Panel B presents the heterogeneity of effects by the job seekers' gender. Panel C shows the results by the gender of job seekers and contacts. In the latter estimates modified proxies were used, which indicate the presence of female, male or both types of contacts at a potential target location. Additional controls are listed in Table 11. Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table B3. Test of the equality of coefficients for Table B2

	Without fixed effect	Sending firm FE	Sending firm-Target firm FE
Panel B)			
Co-workers for females vs. males	0.04	-0.63	-1.13
Panel C)			
Female contact for female vs. male	2.69*	2.05*	0.02
Male contact for female vs. male	-2.07*	-2.40*	-1.44
Both types for female vs. male	0.76	0.82	0.14
Female vs. Male contact for female	1.29	1.01	0.31
Female vs. Male contact for male	-3.90*	-3.91*	-1.10

Note: T-values from two-sided t-tests are presented in the table. *p < 0.05

Table B4. The effect of former co-workers on job finding – fixed effect logistic regression

	(1) Without FE	(2) Sending firm FE	(3) Sending-Target FE
Panel A) Baseline			
Co-worker	5.9053***	4.5409***	2.3551***
	(0.3949)	(0.2975)	(0.1712)
Pseudo R-squared	0.0606	0.0348	0.0301
Log-likelihood	-14419	-12207	-3748
Panel B) Gender of job seeker			
Female with co-worker	5.3095***	3.6485***	1.8649***
	(0.5953)	(0.3851)	(0.2187)
Male with co-worker	6.1437***	4.9233***	2.5706***
	(0.4509)	(0.3722)	(0.2152)
Pseudo R-squared	0.0606	0.0351	0.0309
Log-likelihood	-14418	-12203	-3745
Observations	1 364 650	1 175 996	17 372
No of job seekers	10 311	7090	2082
No of firms	1553	712	675
No of sending-target firm pairs	111 238	75 415	1267

Note: The coefficients of the fixed effect logistic regression models are displayed in an exponentiated form. The outcome variable measures whether a given closing firm-target firm job switch has been realized. Panel A presents the overall effect of the presence of former co-worker at a given (potential) target location, Panel B presents the heterogeneity of effects by the job seekers' gender. Additional controls cover gender, quadratic age, education and residence dummies, pre-estimated individual fixed effects, the categorized no. of spells before the displacement, tenure length and the 1-digit occupation code at the closing firm, closure year dummies. We also include dummies indicating the sector and ownership of sending firms, the presence of social transfers, and a variable indicating if the job seekers typically work at female-dominated workplaces. Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table B5. Homophily in co-worker effects – fixed effect logistic regressions

	(1) Without FE	(2) Sending firm FE	(3) Sending-Target firm FE
Female job seeker			
Female co-worker present	4.7219***	3.1431***	1.7294**
	(0.6481)	(0.4378)	(0.2914)
Male co-worker present	3.9117***	2.8190***	1.8519***
	(0.6322)	(0.4382)	(0.3204)
Both female and male	0.9886	1.3073	0.7117
	(0.2407)	(0.3368)	(0.2192)
Male job seeker			
Female co-worker present	3.8880***	3.0066***	2.4757***
	(0.6025)	(0.4769)	(0.4444)
Male co-worker present	5.7648***	4.6853***	2.4266***
	(0.4474)	(0.3749)	(0.2324)
Both female and male	0.8379	0.9853	0.5978*
	(0.1678)	(0.2021)	(0.1558)
Observations	1 364 650	1 175 996	17 372
No of job seekers	10 311	7090	2082
No of firms	1553	712	675
No sending-target pairs	111 238	75 415	1267
Pseudo R-squared	0.0665	0.0413	0.0317
Log-likelihood	-14 329	-12 124	-3 742

Note: The coefficients of the fixed effect logistic regression models are displayed in an exponentiated form. The outcome variable measures whether a given closing firm-target firm job switch has been realized. The contact variables indicate the presence of a given type of contact (female, male or both types) at the potential target location. Additional controls are listed in Table 11. Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table B6. Co-worker effects on upward mobility – by using logistic regressions

	(1)	(2)	(3)	(4)
	$UP_{wage\ decile}$	$UP_{w-f\ wage\ decile}$	$UP_{Firm\ quality}$	UP_{WG}
Panel A) Baseline				
Co-worker	1.1834***	1.3909***	1.2443***	1.2383***
	(0.0141)	(0.0162)	(0.0151)	(0.0184)
Panel B) Gender of job seeker				
Female with co-worker	1.1376***	1.3861***	1.3048***	1.3582***
	(0.0235)	(0.0271)	(0.0267)	(0.0314)
Male with co-worker	1.2073***	1.3934***	1.2146***	1.1662***
	(0.0176)	(0.0202)	(0.0182)	(0.0226)
Panel C) Job mobility type				
Job entries after closures	1.2671*	1.3465**	1.4830***	1.1092
	(0.1326)	(0.1413)	(0.1544)	(0.1435)
Job-to-job mobility	1.1826***	1.3913***	1.2416***	1.2399***
	(0.0142)	(0.0163)	(0.0151)	(0.0186)
Panel D) Job mobility type × Gender of job seeker				
Closure × Female with	1.5853*	1.2856	2.0286***	1.1269
co-worker	(0.3098)	(0.2518)	(0.4000)	(0.2322)
Closure × Male with	1.1666	1.3726**	1.3243*	1.0997
co-worker	(0.1411)	(0.1661)	(0.1580)	(0.1757)
Job-to-job x Female with	1.1339***	1.3871***	1.2989***	1.3610***
co-worker	(0.0235)	(0.0272)	(0.0267)	(0.0316)
Job-to-job × Male with	1.2081***	1.3936***	1.2133***	1.1671***
co-worker	(0.0177)	(0.0203)	(0.0183)	1.1269
Observations	574 529	572 323	515 076	374 868

Note: The dependent variables are indicators, which mark if the overall or within-firm wage decile of the individuals is higher at the receiving firms compared to the sending ones (column (1-2)), if the quality of the new firms is better (column (3)), or if the controlled gender wage gap is lower at the new firms (column (4)). The covariate of interest, which marks job entries with co-workers present, is used in itself (Panel A) and interacted with other variables, such as the gender of job seekers (Panel B), the type of job mobility (Panel C) and both of these variables (Panel D). The observation number might differ from the one in Table 9 and Table 10 due to missing values. The coefficients of the logistic regressions are presented in an exponentiated form. Additional controls are listed in Table 11. Robust standard errors are in parentheses. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table B7. Co-worker effects on upward mobility using only job entries after closures

	(1)	(2)	(3)	(4)
	$UP_{wage\ decile}$	$UP_{w-f\ wage\ decile}$	$UP_{Firm\ quality}$	UP_{WG}
Panel A) Baseline				
Co-worker	0.0483*	0.0481*	0.0793***	0.0172
	(0.0201)	(0.0190)	(0.0205)	(0.0206)
Panel B) Gender of job seeker				
Female with co-worker	0.0929**	0.0492	0.1365***	0.0232
	(0.0358)	(0.0365)	(0.0375)	(0.0349)
Male with co-worker	0.0301	0.0476*	0.0568*	0.0141
	(0.0241)	(0.0221)	(0.0244)	(0.0255)
Observations	6 280	6 280	6 171	4 525

Note: The estimates are based on Eq. (15). The dependent variables are indicators, which mark if the overall or within-firm wage decile of the individuals is higher at the receiving firms compared to the sending ones (column (1-2)), if the quality of the new firms is better (column (3)), or if the controlled gender wage gap is lower at the new firms (column (4)). The covariate of interest, which denotes job entries with co-workers present, is used in itself (Panel A) and interacted with the gender of job seekers (Panel B). Additional controls are listed in Table 11. Standard errors are in parentheses and clustered at the sending firm level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

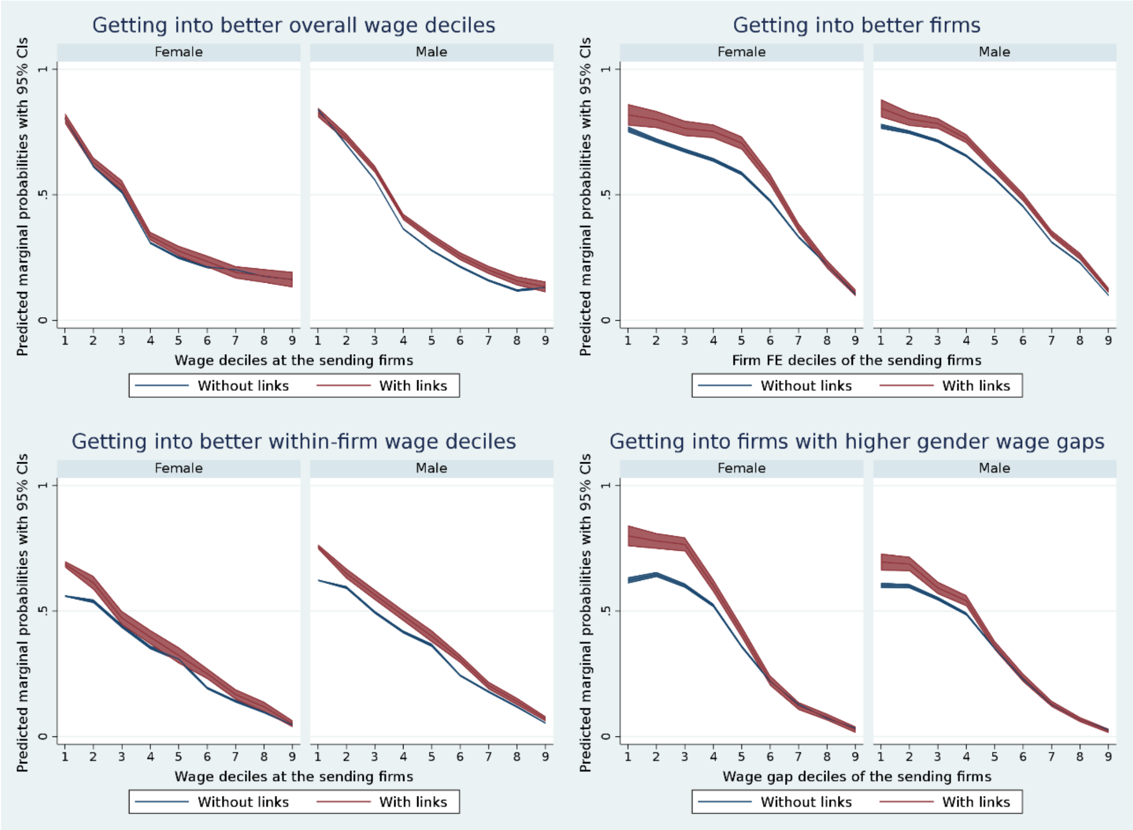

Figure B1. Predicted marginal probabilities of upward mobility by gender, without additional control variables

Note: The estimates are based on Eq. (20) without additional controls. Upward mobility is measured by four dummy variables which mark if the individuals fall into higher overall or within-firm wage deciles at their receiving firms (upper left and bottom left panels), hired by better quality firms (upper right panel) or firms with lower average male wage advantages (bottom right panel) after job mobility. The predicted marginal probabilities for upward mobility with 95% confidence intervals are presented by gender and the sending firm level of those variables, which were used for the construction of the given upward mobility measures. As there is no chance for further upward mobility, the highest (10^{th}) decile is always omitted.

Appendix C

Table C1. Estimations with different tenure restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Min. 1 month		Min. 6 months		Min. 12 months	
	δ_{dj}	δ_{duj}	δ_{dj}	δ_{duj}	δ_{dj}	δ_{duj}
	Program-Firm FE	Institution-Program-Firm FE	Program-Firm FE	Institution-Program-Firm FE	Program-Firm FE	Institution-Program-Firm FE
Former university peers	0.000594***	0.000021	0.000666***	0.000178*	0.000566***	0.000119
	(0.000065)	(0.000068)	(0.000076)	(0.000081)	(0.000082)	(0.000085)
	0.000203***	0.000126*	0.000205***	0.000126*	0.000208***	0.000127*
Constant	(0.000050)	(0.000060)	(0.000051)	(0.000060)	(0.000051)	(0.000060)
R^2	0.00428	0.0163	0.00428	0.0163	0.00427	0.0163
Observations	46 263 778	46 256 215	46 263 778	46 256 215	46 263 778	46 256 215
Program	195	195	195	195	195	195
Program–Firm	38 884	38 875	38 884	38 865	38 884	38 875
Institution–Program	767	748	767	748	767	748
Institution–Program–Firm	362 182	354 619	362 182	354 619	362 182	354 619
Individuals	8 282	8 263	8 282	8 263	8 282	8 263

Note: The models are estimated according to Eq. (21). In columns (3-4) we used our original definition of contacts, in columns (1-2) and (5-6) the indicator for former university contacts only refers to those with at least one month and twelve months of employment, respectively. We introduce either program-firm fixed effects (column (1-3-5)) or institution-program-firm fixed effects (column (2-4-6)) into our model. For additional controls see Table 19 in the main text. Standard errors are in parentheses and clustered at individual and institution-program level. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Description of the estimation dataset

When estimating peer effects on hiring, we focus on those job search months of the former university students that started min. 6 months after the individuals finished all of course requirements (absolutorium). In every job search month (when the individuals had no employment contracts), we present the individuals with firm alternatives that could provide them with employment options. The option pool covers those companies that employed graduates of similar type of master's *programs* as the job seekers. Those firms are excluded from this list where the individuals have formerly worked and that did not existed at the time of job search.

The indicator of hiring shows if the job seekers found employment after a given job search month. If they did, the variable of new firm shows the ID of this company. The indicator of peers marks the presence of former university fellows at given target firms with min. 6 months tenure.

Let us assume that an individual (ID=1) had two employment spells preceded by a 1 and a 2 months long unemployment period respectively (t=7 and t=9, t=10), and her option pool covered the same three firms (A, B, C) at each period. This person got hired by firm A at t=8 and firm B at t=11. She had former university peers at firm A and B in every job search month. The estimation sample used for the hiring estimates will have the following format:

Table C2. Example for the format of the estimation dataset

Individual ID	Job search month (t)	New firm ID	Target firm ID	Indicator of hiring	Indicator of peers
1	7	A	A	1	1
1	7	A	B	0	1
1	7	A	C	0	0
1	9	-	B	0	1
1	9	-	C	0	0
1	10	B	B	1	1
1	10	B	C	0	0

Table C3. Characteristics of the target firms

Size of the firm	**N**	**%**
<10	359 408	30.45
11-250	671 640	56.90
250+	149 328	12.65
Ownership		
Foreign	47 263	4.00
Domestic	675 616	57.24
State-owned	186 002	15.76
Unknown /No available data	271 495	23.00
Sector		
Agriculture	29 089	2.46
Industry	206 883	17.53
Trade and Services	593 661	50.29
Education, Social, Other	127 339	10.79
Unknown	223 404	18.93
Productivity (Value-added per worker)		
Below the yearly median productivity	182 400	15.45
Above the yearly median productivity	731 438	61.97
No. of firms with no available data	266 538	22.58
Observations		
Target firm-month observations	1 180 376	
Unique target firms	18 491	

Note: The observations are target firm-month units. Yearly median productivity was calculated based on all Hungarian firms with available data.

Table C4. Comparisons of the identification sub-samples

	Identification sample		
	δ_{dj}	δ_{dcj}	δ_{duj}
	Program-Firm FE	(Program × County)-Firm FE	Institution-Program-Firm FE
Individual characteristics			
Gender			
Female	59.5%	59.5%	59.4%
Male	40.5%	40.5%	40.6%
Av. age	28.2	28.1	28.2
Field of study			
Agriculture	6.5%	6.5%	6.5%
Humanities	21.6%	21.3%	21.3%
Social Sciences	13.1%	13.1%	13.1%
Informatics	2.4%	2.4%	2.4%
Law	0.8%	0.8%	0.8%
Public administration	3.5%	3.5%	3.5%
Economics	26.9%	27.2%	27.1%
Engineering	13.8%	13.9%	13.9%
Sports science	1.3%	1.3%	1.3%
Natural sciences	10.2%	10.3%	10.2%
Have work experience	77.4%	77.4%	77.4%
Number of individuals	8 234	8 122	8 181
Number of job search periods	10 456	10 315	10 385
Av. no. of firm alternatives	267.9	77.9	134.2
Number of job entries	5 471	4 329	4 784
Av. no. of job search months	8.1	8.1	8.1
Found job at a peer's firm (%)	7.6%	11.9%	10.0%
Characteristics of the new job			
Log entry wage	7.1	7.1	7.1
Av. tenure	19.2	21.0	20.1
Occupation			
Manager	3.0%	2.8%	2.8%
White-collar worker	94.9%	95.3%	95.3%
Blue-collar worker	2.1%	1.9%	1.9%

***Note*:** The hiring sample covers those job entries between 2011 and 2017, preceded by at least a 1-month long unemployment period. The columns show the characteristics of those sub-samples, from which peer effects are identified in the various fixed effect specifications.

Table C5. Magnitude of peer effects on the identification sample of the different fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Program-Firm FE identification sample				(Program × County)-Firm FE identification sample				Institution-Program-Firm FE identification sample			
	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}	δ_{dj}	δ_{dj} and μ_{du}	δ_{dcj} and μ_{du}	δ_{duj}
	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program × County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program × County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE	Program-Firm FE	(Program-Firm) + (Institution-Program) FE	(Program × County)-Firm + (Institution-Program) FE	Institution-Program-Firm FE
Former university peers	0.000664***	0.000650***	0.000325***	0.000176*	0.000445***	0.000425***	0.000320***	0.000174*	0.000320***	0.000301***	0.000203**	0.000174*
	(0.000075)	(0.000076)	(0.000075)	(0.000080)	(0.000075)	(0.000075)	(0.000074)	(0.000080)	(0.000077)	(0.000078)	(0.000078)	(0.000079)
Constant	0.000282*	0.000250*	0.000241*	0.000232*	0.000725**	0.000697**	0.000688**	0.000662**	0.001381**	0.001293**	0.001267**	0.001220**
	(0.000116)	(0.000122)	(0.000119)	(0.000117)	(0.000232)	(0.000227)	(0.000223)	(0.000216)	(0.000425)	(0.000418)	(0.000411)	(0.000407)
Observations	21 114 247	21 114 246	21 113 883	21 111 835	10 283 006	10 283 006	10 283 006	10 282 474	6 066 410	6 066 410	6 066 410	6 066 410
R^2	0.00286	0.00302	0.00654	0.0109	0.00440	0.00465	0.00556	0.00941	0.00554	0.00583	0.00693	0.00815
Program	166	166	166	166	166	166	166	166	166	166	166	166
Program-Firm	14 157	14 157	14 157	14 157	14 144	14 144	14 144	14 144	14 118	14 118	14 118	14 118
(Program×County)-Firm	66 659	66 658	66 295	66 295	17 389	17 389	17 389	17 389	17 346	17 346	17 346	17 346
Institution-Program	732	731	726	715	694	694	694	684	646	646	646	646
Institution-Program-Firm	146 655	146 654	146 291	144 243	53 227	53 227	53 227	52 695	20 901	20 901	20 901	20 901
Individuals	8 234	8 233	8 228	8 217	8 181	8 181	8 181	8 171	8 122	8 122	8 122	8 122

Note: The models are estimated according to Eq. (21). We re-estimated our main specifications (presented in Table 19 in the main text) on three different sub-samples. Columns (1-4) show the results for the sample from which peer effects are identified when program-firm fixed effects are included in the model. Columns (5-8) indicates the findings for the identification sample when (Program × County)-Firm fixed effects are included. Finally, columns (9-12) present the coefficients for the identification sample of institution-program-firm fixed effects specification. For additional controls see Table 19 in the main text. Standard errors are in parentheses and clustered at individual and institution-program levels. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table C6. Peer effects on occupation rank – ordinal regression model

	Coef.
Former university peer	-0.2139**
	(0.0677)
	Marginal probabilities
Category 1	0.0051**
	(0.0016)
Category 2	0.0474**
	(0.0150)
Category 3	-0.0214**
	(0.0068)
Category 4	-0.0224**
	(0.0071)
Category 5	-0.0050**
	(0.0016)
Category 6	-0.0003*
	(0.0001)
Category 7	-0.0004**
	(0.0002)
Category 8	-0.0010**
	(0.0003)
Job entries	9 983
Pseudo- R^2	0.0659

Note: The dependent variable is the 1-digit occupation category of the new jobs. The indicator of peers denotes whether the job seekers had any former university peers at their new firms with a minimum of six-month tenure. Additional controls include gender, age, region of residence, the field of study of the latest master's program, the industry of the new firm, and year dummies. Except for work experience and age, all covariates are dummies *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

Table C7. Tenure at the new firms with fixed effects

	(1)	(2)	(3)	(4)	(5)
	Min. 3 months	Min. 6 months	Min. 12 months	Min. 18 months	Min. 24 months
Former university peer	0.8697	1.1643	1.3473	1.1540	1.2745
	(0.2183)	(0.2225)	(0.2475)	(0.1851)	(0.1915)
Job entries	1 682	2 246	2 522	2 640	2 533
Job entries with peers	217	257	273	296	297
No. of firms	188	301	376	401	373
Pseudo-R^2	0.1102	0.0906	0.0768	0.0453	0.0355

Note: The models are estimated according to Eq. (23). The dependent variables indicate if the individuals spent at least 3, 6, 12, 18 or 24 months at their new company (specifications (1-5) respectively). The indicator of peers denotes whether the job seekers had any former university peers at their new firms with minimum six-month tenure. Additional controls include gender, region of residence, year dummies, age, work experience and the log entry wage of the new job. Except for the latter three, all covariates are dummies. The coefficients of the logistic regressions are presented in an exponentiated form. Robust standard errors are in parentheses. *Statistically significant at 0.05 level; **at 0.01 level; ***at 0.001 level.

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