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Wage Differentials and the Possibilities in Linked Employer-Employee Panel Data

by
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DEPARTMENT OF ECONOMICS AND BUSINESS

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Co-author contribution

Chapter 3: Decomposition of Co-worker Wage Gains

Joint work with Virág Ilyés, published as Boza and Ilyés (2020)

The paper was developed in close cooperation with Virág Ilyés throughout all stages. We contributed equally to the idea of the paper, data management, programming, analysis and writing. All authors contributed equally.

Chapter 4: Wage Gains from Foreign Ownership: Evidence from Linked Employer-Employee Data

Joint work with János Köllő and László Balázsi, published as Köllő et al. (2021)

László Balázsi's contribution was the initial estimation of the contemporaneous wage gap, and the discussion of estimation issues related to multi-way fixed effects models, alongside related technical issues. István Boza handled a large part of data management, worked out part of the methodology, estimated the spillover effects and worked in the revision and publication process. János Köllő managed the project, worked out the paper's structure, the methods and the identification of the various effects, calculated the lagged wage effect, and wrote most of the text. All authors read and approved the final manuscript.

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Abstracts

The thesis consists of four chapters connected by their methodology, data usage and shared focus on wages and inequality. The content of the individual chapters are summarized in the following abstracts.

Chapter 1: Wage Structure and Inequality: The Role of Observed and Unobserved Heterogeneity

This study aims to contribute to the literature of firms and occupations as prominent drivers of wage-inequality in multiple ways. First, we synthesize novel modelling approaches of recent studies in the field and use administrative linked employer-employee panel data from an Eastern European country, Hungary, to assess the contribution of individual, firm and job heterogeneity – and their interactions – to overall wage inequality. Consistent with earlier findings from Western Europe, Scandinavia, the US and Brazil, we show that firm heterogeneity provides around 22%, individual heterogeneity 50%, and occupational heterogeneity 8% of overall wage dispersion, with wage sorting between firms and individuals in itself explaining around 9%. Notably, around half of this contribution is accountable to observable sub-components of individual and firm wage effects. Also, the same magnitude of assortativity can be found between individuals and occupations. Utilizing unique features of our data, we compare mathematics and literature test score records of 10^{th} grade students to their future labor market outcomes, finding a positive correlation between test scores and future firm value added, a direct evidence for assortative matching in productivity. Finally, we assess sorting along observable characteristics such as gender, education, occupation, worker age, and the ownership of employers.

Chapter 2: A Fixed-effect Approach to Estimating Rent-sharing Elasticities

The paper provides rent-sharing elasticity estimates from Hungarian administrative linked employer employee panel data. By combining recent advances in the literature of rent-sharing and firm-specific wage premia, we propose an estimation design which relies on within-firm identification of wage effects of inter-temporal productivity changes of the firm, while also controlling for the heterogeneity in the firm's workforce composition, but still incorporating information on the wages of both stayers and job-switchers. Hence this approach intends to solve the selectivity issues present in the state-of-the-art specification of productivity-wage pass-through estimations. The estimated OLS elasticites range between 0.05-0.16, while estimates relying on internal instruments, such as past productivity, range between 0.12-0.18 across the established specifications. The selectivity problem turns out to be only a second-order issue.

A second set of results focuses on heterogeneity of firms with respect to their rent-sharing behaviour. We find that, while the wage-productivity relation in cross-sectional research designs is weakest in agriculture, firms of this sector show

the strongest response to inter-temporal changes of productivity. Finally, we test whether firms share their rents differently with different sub-groups of their workers as such phenomena can be a prominent source of within-firm wage differences. Even after accounting for the possible non-random sorting of workers into firms with different firm-specific pass-through rates, we find significantly higher wage reactions for males, more educated workers, and for those in better occupations, and minor differences for those with more seniority in the given firm. The gender differences are also found to be stable across different occupations and firm types.

Chapter 3: Decomposition of Co-worker Wage Gains

Published with Virág Ilyés as Boza and Ilyés (2020)

We address the presence, magnitude and composition of wage gains related to former co-workers, and discuss the mechanisms that could explain their existence. Using Hungarian linked employer-employee administrative data and proxying actual co-workership with overlapping work histories, we show that the overall wage gain attributable to former co-workers consists of multiple elements: a contact-specific, an individual-specific, a firm-specific and a match-specific component. Former co-workers, beside the direct effect of their presence may funnel individuals into high-paying firms, enhance the sorting of good quality workers into firms, and may contribute to the creation of better employer-employee matches. By introducing and applying a wage-decomposition technique, we demonstrate that there are non-negligible differences between linked and market hires in all empirically separable wage elements. By focusing on specific scenarios, we provide additional empirical evidence in favor of employee referral and information transmission as the main drivers of co-worker gains.

Chapter 4: Wage Gains from Foreign Ownership: Evidence from Linked Employer-Employee Data

Published with János Köllő and László Balázsi as Köllő et al. (2021)

We compare the wages of skilled workers in multinational enterprises (MNEs) versus domestic firms, the earnings of domestic firm workers with past, future and no MNE experience, and estimate how the presence of ex-MNE peers affects the wages of domestic firm employees. The analysis relies on monthly panel data covering half of the Hungarian population and their employers in 2003–2011. We identify the returns to MNE experience from changes of ownership, wages paid by new firms of different ownership, and the movement of workers between enterprises. We find high contemporaneous and lagged returns to MNE experience and significant spillover effects. Foreign acquisition has a moderate wage impact, but there is a wide gap between new MNEs and domestic firms. The findings, taken together, suggest that MNE employees accumulate partly transferable knowledge, valued in the high-wage segment of the local economy that is connected with the MNEs via worker turnover.

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Acknowledgements

There are two kinds of people a graduating doctoral student can be grateful towards: those who directly helped in writing their particular thesis, and those who supported the general concept of pursuing a PhD – in Economics in my case. Sometimes, however, the two groups intersect or become hard to distinguish. Let's take a look at my (semi-chronological) account of how my PhD came to be, and what interactions formed the flow of events leading up to the point of submitting present dissertation.

My academic career quite probably started near the end of 2012, when János Köllő – for whom I was a teaching assistant for a labor economics course at my undergraduate alma mater, the Eltecon programme – offered me the opportunity do so some 'serious work' as well. I became a research assistant for him, and got introduced to the first – by now, quite archaic – and later the second iteration of the Hungarian administrative panel dataset. While our first project together still remains 'in the drawer', after quite some years – and a story concerning a stolen laptop and a well-hidden USB flash drive – our second project turned into a published paper and eventually a chapter of this thesis. Learning about the research and publication process, including not only the estimation of models, but writing an original manuscript, getting desk rejections, fulfilling revisions, and in the end getting published and then cited is an experience from which I learned a lot and for which I cannot be grateful enough.

Despite this early exposure to a research environment, the aim to pursue a PhD in Economics was not trivial for me. After finishing my MA at ELTE, I spent a year outside the education system – a rare event in the past 25 years. Actually, I became an employee of the Databank of the Centre for Economic and Regional studies, which by that time was still a part of the Hungarian Academy of Sciences. The colleagues from the Databank – including both old and new ones – deserve the utmost gratitude from me, not only for the time I spent there or the direct help they often provided me – including emergency weekend calls regarding unexpected server downtime or the sorting out of surprising data anomalies –, but for their continuous heroic work in curating the administrative panels (and other datasets) for Hungary.

Eventually, applying for the Economics PhD at CEU seemed a logical next step, especially without the menacing commitment of moving abroad for long years – as CEU was still operating mainly in Budapest by the time. Little did I knew back then that the first couple of years will turn out to be a challenging ride. Many times, I felt grateful for my BA and MA teachers, as the strong background in Economics I received during my Eltecon years helped a lot in tackling the mandatory coursework of the first semesters. Also, they – especially Gergely Kőhegyi – often trusted me with teaching opportunities, from which I have learnt a great deal about many aspects of university careers. Another bunch I'm grateful for are my PhD peers, especially Luca Drucker, Lajos Szabó and Ágoston Reguly, with whom we fought in great camaraderie in those quite demanding years – on all the accessible levels of the Nádor 15 building.

I believe, the third and later years of my PhD showed the real values of CEU

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and especially its Department of Economics (and Business). All the great minds that were convinced to and chose to teach in Budapest (instead of probably superb western opportunities) had to teach something new and important about the field I slowly realised to have chosen as my profession. Picking just a few names probably would not do right for the whole faculty, so first I would highlight the generally welcoming atmosphere, the great knowledge and friendliness of the faculty, the professionalism and helpfulness of the staff, and also the faculty's commitment for pushing us into presenting at really important outings such as the PhD research seminars, the Brownbags or the Empirical Research seminars. Even if attending the BESS/CESS or Brownbag seminars sometimes felt a chore, they also taught us a lot about promoting one's research. The faculty members who also contributed directly towards my research include Miklós Koren, Róbert Lieli, Sergey Lychagin, László Mátyás, Arieda Muço, Ádám Szeidl, and most notably Andrea Weber.

During the research years of my PhD, I got under the supervision of Andrea Weber. Although our relation was not as a close one as some students and supervisors develop, she was probably the best supervisor I personally could have asked for. Her knowledge of the latest advances, top journals, conferences and defining researchers in the fields of empirical and labor economics often helped me to advance my research in the best possible way. I also believe that not putting unnecessary pressure upon me actually helped my development as an autonomous researcher in my final PhD years. Also, she was always very supportive of my extra endeavors, such as participation at various conferences, visiting the University of Umeå, teaching at ELTE or even pushing for an extra chapter in my PhD. She also helped a lot in promoting my research, including even the choice of my two fantastic examiners, Ana Cardoso and Tímea Laura Molnár, for whom I once again express my gratitude for the constructive and encouraging feedback they have provided on my dissertation.

Also, CEU provided great facilities and opportunities such as the Center for Academic Writing – where I learned a lot from Ágnes Diós-Tóth – or the generous travel grants that helped greatly in learning about the processes of presenting and promoting one's own research. These grants also helped me with keeping in touch with members of the COIN group from whom I also learned a great deal about research, sociology and the publication process, and the world in general – especially from Andrew, Don and Olivier, but also from Are, Anthony, Lasse, Zoltán and basically all the other COIN members. I also thank David Card for unexpectedly congratulating me on my presentation at a conference in Braga, as that small moment gave me an enormous boost in (self-)confidence.

By the time I'm writing these lines, I have become a (junior) research fellow at the Center for Economic and Regional Studies. Here, I'm once again experiencing a very welcoming atmosphere accompanied by the professionalism of both junior and senior researchers. I'm also grateful for senior researchers Dániel Horn and Balázs Reizer, who chose to work with me on joint projects in the upcoming years – building upon some contents of this thesis.

I left out from the above account one of the most important actors, Virág Ilyés, probably due to her constant presence in my life in this period. During this time she has not only become from girlfriend to fiancée and then wife, but a co-author on multiple papers as well – including Chapter 3 of this thesis, the publication of which was a lifetime experience for both of us. She is also tackling the last months of her PhD at this moment, and her precision and hardworkingness could be an example for all PhD students around the world. She never accepts 95% as good enough, and always pushes to provide perfectly polished work, often pushing me as well into ranges where, by myself, I would have stopped already – just to realize that she was right and to discover some well-hidden data mistake or imprecision in the research design. Her hard-working attitude even had major spillovers on both of our lives, when based upon Virág's merits, we both received visiting research opportunities at Umeå University. On a related note, we're both grateful for László Lőrincz and Rikard Eriksson for having the opportunity to work with Swedish administrative data and to experience being a researcher outside of Hungary as well. Naturally, Virág did not only have a professional contribution, but was my strongest emotional support during the hard times as well. She could always calm me down during my panic periods preceding my presentations, and also would never let me feel let-down after presentations that did not turn out to be as good as I planned. She also tolerated well my all-nighters – or even 'all-dayers' or 'all-weekers' in periods of extensive writing.

Similarly, I cannot acknowledge enough the role that my family and also my friends played in getting this degree. While both groups were (most of the time) reassuring and positive about the meaningfulness of pursuing this degree – even despite my sometimes rather technical topics –, my family also made it possible – through both financial and general support – to concentrate on my studies full-time, which was especially important during my undergraduate years. The ability to do so provided a boost in both my career and my social life (and probably in my ambitions as well), which then had positive spillovers on my PhD degree and quite probably all of my future as well. This is an advantage, that I may never be able to pay back. At the meantime, friends played a crucial role in maintaining my mental health and motivation and also in tackling unexpected challenges of the past six years.

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Introduction: Concerning Wages (and a Data Revolution)

Wages are rather interesting. Although in the most simplest model of economic theory, they act just like the price of any our product or production factor, in the real world the wage someone receives plays a highly important role in their everyday life. Most of the working age people – except for the self-employed – really care about the level of wages they can (or cannot) receive and some individuals even have to decide about the wages of others, while discussing others' income is often considered a taboo. Unlike individuals, however, labor economists have the advantage of having data on the wages of large samples of individuals (or even for the full population of a given country). Observing from this vantage point reveals that –although the top inequality in in incomes largely depends on capital income for instance –, the main mass of the income distribution is still driven by differences in wages. But why could wages differ at all? While basic labor theory suggests that differences in human capital (and worker productivity) could be the sole reason, this view have been significantly changed over the past decades, with focus shifting on the roles of firms, occupations or even social contacts. This dissertation aims to relate to an expanding field of wage differentials, and even if a single answer could not be provided for the above question, we aim to present both theoretical concepts and empirical findings that further our understanding of wages and their differences.

Specifically, in the thesis we focus on four potential phenomena that can explain wage differences. First, we consider that not only individuals, but occupations and employers can be greatly heterogeneous in their observed and (for the econometrist) unobserved characteristics, and that it matters a lot that which type of individuals work at different kind of employers. Firms for example could be not only different in their average productivity levels, but in the magnitude how their productivity differences translate into wage differences of their employees as well – a focus of our second chapter. Beside firms and workers, the current life situations of individuals can be different and may have effects on expected wages. In our third chapter, we investigate one such aspect by focusing on individuals who get into jobs where one of their former colleagues is already employed, and find a non-negligible wage gain for individuals finding jobs through their professional contacts. The final topic assessed in the thesis, in the fourth chapter, relates to the knowledge accumulation of individuals and the transferability of this knowledge across sectors of the economy, which could provide long run gains for not only the individual but for future peers as well.

Before elaborating on our actual research questions, it is important to highlight that the possibility to ask and answer such questions is not trivial. Besides the focus on wages, what links the studies of this thesis is the nature of the data required for such endeavours. To ask the above question, most notably we have to be able to observe individuals (and firms) through multiple periods, that is we need panel data. But that is not all, as observing which workers work together (and when) is essential in all of the above studies. For instance, we have to identify who have worked together in the past, who meet again at a new firm, who has a colleague with multinational experience, or how wages of different individuals relate to each other *within* same firms. Having such data is not trivial, and probably the increasing availability of these *linked employer-employee panel datasets* is one of the most interesting novelty in empirical labor economics in the past few decades, and they will surely define our thinking about some labor market phenomena for the years to come. We have to note that, even more generally, the reliance on data and especially micro-data, has become prominent feature of (labor) economics. A main (but not sole) contributor of this process, David Card has been awarded with the Economics Nobel prize – the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel – in 2021. Also, if one looks at the program of any minor or major labor economics conference, one would find that the majority of new research is empirical, and a growing number of studies rely on employer-employee panels or other administrative datasets. The study of Currie et al. (2020) also presents empirical evidence on these trends by assessing the contents of NBER working papers and articles published in the top five economics journals in the past decades.

So what makes these linked or matched panel datasets special? Being able to observe all colleagues at a given firm have been available to researchers earlier as well with plant level surveys, and observing individuals over their lifetime also had examples in longitudinal survey studies. But being able to follow the same individuals through time, and multiple employers opens up whole new possibilities, as listed above for instance. Besides, these datasets are generally not generated through surveys anymore, but rely on administrative records of individuals' employment spells collected by governmental authorities for the sake of defining taxes, social contributions or pensions. Although, in the case of some countries and datasets the main source of data comes from wage surveys conveyed with firms and public employers, the ability to match individuals across different waves elevates the utilizability of these datasets as well. This administrative nature also has the consequence that the datasets are massive, and quite often relate to the whole population of individuals in the given country – even if only a smaller, but still substantial sample is made available for researchers. Of course, as economists we are aware that trade-offs are present everywhere. The administrative nature of this kind of data often bears the costs of not being able to tell anything (or not much) about employment in the grey or black market, having less detailed educational histories, less info or bonuses, side jobs, working hours or so on.

Nowadays this kind of data gets more and more available. I had the privilege to work with the Comparative Organizational Inequality Network (COIN) project, where we used linked employer-employee data from fifteen countries to compare inequality trends across them (Tomaskovic-Devey et al., 2020). Also at least five other countries that were not represented in this study has such datasets available. As time passes, not only more and more countries create their respective (administrative) panels, but the observation windows in the panels are getting longer, which helps the researchers not only in extending the scope of their studies. For instance, for some econometric methods only these longer panels can lend the necessary statistical power to provide convincing evidence on specific phenomena. At the same time, the computational power required for effectively using such data is getting more cheap and accessible to scholars, and therefore complex (and novel) econometric techniques also get more feasible – and are being improved upon themselves.

Finally, beside this amazing expansion in the extensive margin – the availability and length of panels –, a fascinating intensive expansion is observable as more and more data linkage gets available to researchers with respect to the information and variables incorporated into the core datasets. For instance, in Hungary we already had data on health expenditure previously, but in the latest iteration of the Hungarian dataset we gained access to information on young workers' educational history, including for instance, standardized test scores. In stricter research environments, firm-to-firm level transaction data – coming from administrative VAT records –, or even ownership networks could be also linked to the employer-employee panels, opening doors in front of previously unimaginable research ideas and designs. I firmly believe that the underlying possibilities will turn out to be even more vast, than we can think of now. Therefore being an empirical labor economist has never been so exciting.

While the above outlined datasets convey possibilites not only related to wages – or even to the labor market –, this dissertation aims to demonstrate replicated and novel findings regarding wage related research questions of the past decade, with some focus on methodological innovations. The novel findings and contributions include, but are not limited to, the following.

Chapter 1 demonstrates that assortative matching and wage sorting is important in Hungary – as in many modern economies –, and that part of this phenomena could be already captured in young individuals. Those who have higher test scores at teenage years will end up not only in better occupations (what we expect), but in better, higher wage firms within their respective sectors as well. Wage differences across specific worker groups are also discussed, and relevant differences are found with respect to both sorting into different firms and within-firm (bargaining) differences, along gender, education, occupation and as a novelty, worker age.

Chapter 2 makes a methodological contribution, by solving a sample-selection issue in models estimating the wage pass-through of productivity. The application and comparison of new and pre-existing econometric models reveal the importance of model choice – through the example of sectoral differences in estimates of the wage pass-through of productivity changes. Shifting the focus within the firms reveal substantial differences in rent-sharing behavior of firms between worker groups, especially considering gender: women are found to receive significantly lower part of rents across all occupation categories.

Chapter 3 presents that social or professional ties, particularly former coworkers, can have positive wage effects on new entrants to firms. The decomposition of such wage gains – using a novel decomposition technique – reveals that wage differences are in great part generated by the sorting of high achieving workers into high paying firms. While this will elicit gains for both the individuals hired through contacts, and the firms who employ them, a component increasing social welfare – the enhancement of employer-employee matches– is also present.

Finally, Chapter 4 not only demonstrates the very specific role of multinational employers in the Hungarian labor market, but it may hold more general takeaways

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as well. The findings it provides – that is individuals earn substantially more at foreign owned employers, even after controlling for unobserved worker heterogeneity and that part of their wage advantage persists upon leaving the foreign sector – may serve as evidence, that knowledge accumulated at high performance firms – multinationals in our example – can be transferred to other firms and to some extent to other workers as well, a finding for which little evidence have been available so far.

For more detailed discussions of the problems at hand, their motivation, previous findings in the literature and our contributions, the reader can refer to the individual introductions of the corresponding chapters.

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1 Chapter 1: Wage Structure and Inequality: The Role of Observed and Unobserved Heterogeneity

1.1 Introduction

For more than two decades now, labor economists have been intrigued by whether systematically high wage (or high productivity) workers tend to work at high wage firms. The seminal work of Abowd et al. (1999) – AKM, after the authors' initials – was the first, shortly followed by Goux and Maurin (1999), to propose a model in which wages are log additive in time-invariant individual and firm characteristics and time-varying factors. Using linked employer-employee panel data, these time-invariant (partly unobservable) characteristics can be captured by worker and firm fixed effects respectively, with the latter capturing wage differences among firms, controlled for the composition of their workforce with regard to both observable and unobservable worker skills. The steadily increasing availability of such data – regarding both the number of countries, detailedness, and the length of panels – and advances in econometric concerns regarding the estimation of multi-way, high dimensional fixed effect models gave rise to a series of labor studies, which aim to decompose the overall wage dispersion into differences coming from heterogeneity in the above listed observed (and unobserved) factors. And although AKM effects have been used in studies from a wide range of fields as measures of firm and worker quality, for instance in estimating inter-industry wage differentials, rent-sharing estimations or even job referral effects (Abowd et al., 2019), they may had the most influential effect on the literature of wage and earnings inequalities. Our study contributes to this literature not only by presenting evidence for another country where the sorting of high wage workers to high wage firms is a substantial element of overall wage dispersion – measured by both wage-based and more direct, productivity-based measure –, but also by aiming to uncover potential channels along which this phenomenon emerges. Although most exercises in this study are descriptive in nature, the methods and results presented may further the understanding of determinants of such wage sorting.

Along the natural role of individual diversity in skills, opportunities and ambitions, the heterogeneity of firms' waging schemes – originating in differences in firm productivity or the rent sharing propensity of firms, in compensating differentials or in reliance on efficiency wages – can be an important source of wage variation in the economy in itself. Besides, it may also affect the overall wage dispersion through a sorting channel as well. If positive assortative matching with regard to worker and firm productivity is present in the labor market due to complementarity in production, we would also expect 'high wage' (high productivity) workers to be systematically over-represented in 'high wage' (high productivity) firms. That is, if the individual and firm fixed effects of the AKM model capture underlying productivity differences, then the estimated fixed effect parameters should positively correlate. Although early studies found no or negative such correlation (Abowd et al., 2002; Goux & Maurin, 1999; Gruetter & Lalive, 2009; Iranzo et al., 2008; K. L. Sørensen & Vejlin, 2011; Woodcock, 2008), it had been showed that the variance and covariance terms of the estimated worker and firm effects are affected by an incidental parameter problem, labeled "limited mobility bias" (Andrews et al., 2008, 2012). The lack of observed mobility in the panel data used – on which identification of firm effects rely – and the mechanical negative relation of sampling errors in person and firm effects cause a serious downward bias in the above correlation, especially in short panels or sub-samples, possibly driving the zero or negative results found in early studies.[1]

The view on sorting was changed by the defining study of Card et al. (2013), being the first to show a critical, positive role of wage sorting in overall wage dispersion. Besides, the authors found that the dispersion of firm effects and the correlation between workers and firm effects do not only explain a substantial part of wage variance in a given period, but their increase also critically contributed to the observed increase in wage inequality in West Germany over the period of 1985-2009. The wage decomposition approach proposed by Card et al. (2013) have been reproduced by many studies to follow, including most notably Card et al. (2016), Card et al. (2018) and Torres et al. (2018) for Portugal, and Gerard et al. (2021) for Brazil, Song et al. (2019) and Lamadon et al. (2022) for the US. The findings of these and a handful of other studies are summarized in Appendix Table A.1. An important takeaway from the table is that most studies of this decade find a 10-30% contribution of firm heterogeneity, and around a 10-15% contribution of wage sorting to overall wage variance – results from Italy being the exception with near zero sorting components. Studies from the last couple of years, which develop and apply bias-correction methods for the limited mobility bias of the AKM framework, such as Kline, Saggio, and Sølvsten (2020b) for Italy, and Bonhomme et al. (2020) for the US, Austria, Norway, Sweden and Italy find systematically larger correlations of firm and person effects, larger contribution of the sorting component and lower contribution of the variance in the firm component itself – as predicted by the nature of this bias.[2] The similarity in wage composition, even among these similarly developed, but institutionally different countries is quite fascinating. Yet, there are no published results for Eastern European / post-transition countries that we know of.[3] The results presented for Hungary in this paper, however, will be largely in line with those of the aforementioned authors, further expanding the set of countries with similar wage dispersion structure.

Beside the aforementioned econometric issue, other concerns regarding the AKM-based framework of assessing the wage structure and wage sorting have been raised in the past half decade. To reflect on the most important of such issues, we adapt, and also further develop, some of the novel extensions of the recent literature. These are centered around four major topics.

[1] We will reflect on this issue in more detail throughout the study.

[2] Appendix Table A.2 present bias-corrected and standard results from the same studies. Comparing consecutive rows in the table reveals that bias-corrected estimates include, on average, 6% lower firm shares and 10% higher sorting shares, with substantially higher correlations, even in the range of 0.3-0.4.

[3] The only, unpublished exception being Gyetvai (2017), who uses an earlier iteration of our dataset, consisting of 8 years only, and replicates the ensemble decomposition of Card et al. (2018), finding a 26.5% importance share for firms, 60.3% for workers and 5.1% for occupations as a third source of heterogeneity.

First, to assess not only the wage sorting inferred from AKM wage effects, but assortative matching in productivity as well, we utilize more direct measures of firm and individual productivity, including standardized test scores from high school age – a novelty of our data. Based on a series of studies, Torres et al. (2018) argue for the importance of differentiating wage sorting from assortative matching in productivity (productivity sorting), with the latter term having its origins in the technological complementaries between the productivity of firm and its workforce. The main motivation for the distinction is that, while the wages of workers are expected to be monotonic with regard to their skills, the same may not be true for firm productivity (Eeckhout & Kircher, 2011; Lopes De Melo, 2018). However, using data from financial reports, and estimating production functions for the firms – controlling for skill/ occupational composition – Torres et al. (2018) show that these, directly estimated measures of firm productivity also correlate with worker effects, similarly to the indirect productivity measure of AKM firm effects. The correlations are even stronger, suggesting that non-monotonicities in the productivity-wage relation of firms are not negligible, and therefore distinguishing between wage sorting and productivity sorting is important empirically as well.[4] Using balance sheet data of incorporated firms, we will reinforce these findings. In addition, we use data on standardized test scores measured at the age of 16 – available for a subset of the data – , to propose direct measures of productivity sorting, showing the sorting of high-achievers at teenage years to high wage employers.

Second, we recognize the importance of the heterogeneity of occupations as a potential confounder of sorting mechanisms between individuals and firms, and include high-dimensional occupation fixed effects in our AKM estimations. Besides firm heterogeneity, occupational heterogeneity can be an important aspect of wage formation through multiple channels. Most importantly, one could observe different enumeration levels of different occupations even for workers of the same skills as different jobs can bear different outside options or due to compensating differentials for occupation-specific – and not merely firm-specific – amenities or disamenities. Still, even firms with the same occupational composition can pay on average different premia for all of their workers, so the distinction of firm and occupation heterogeneity can be really important, as the sorting of high wage workers into specific occupations and the clustering of such occupations in high wage firms could both increase the level of inequality, while the joint presence of these phenomena could even confound standard measures of wage sorting (between workers and firms). For similar considerations, Card et al. (2016) and Torres et al. (2018) introduce a third high dimensional fixed effect in the form of "job-title effects", for decomposing the gender wage gap and the overall wage variation into person, firm

[4] Card et al. (2016) and Card et al. (2018) also utilize financial report data, and find such positive correlations. Moreover, both paper, and also Alvarez et al. (2018), use the estimated AKM effects to propose a measure for rent-sharing elasticities, regressing AKM firm effects (instead of average wages) on firm productivity. This measure of elasticity removes the effects of workforce composition of firms, and hence may capture true rent-sharing behaviour better than one derived from average wage levels. A recent branch of the literature – to which we do not relate in this study – investigates the role of compensating differentials as another source of firm wage heterogeneity(Lamadon et al., 2022; Sorkin, 2018).

and occupational components in Portugal respectively.[5] As in Portugal collective bargaining agreements cover most of the work force, these authors define job titles as an occupational category under a given collective agreement, thus allowing occupations in different sectors having different average effects. Using this design, they show that not only the type of the firm and the person matters in wage determination, but indeed the type of work done by the individual as well. Our estimations will also incorporate this approach, although only controlling for 4-digit occupations, and not sector-occupation specific job-titles, as a third, high-dimensional effect.

Third, we differentiate between sorting channels that are empirically observable – such as highly educated individuals sorting into generally high-wage sectors – and those that relate to unobserved (residual) individual and firm quality. While the firm and person effects of the main AKM equations absorb any time-invariant firm or person characteristics, these effects could be further decomposed into elements explained by observable, time-invariant characteristics and an unexplained components as shown by Abowd et al. (1999), and applied by for instance Woodcock (2008) for wage-gap and Gruetter and Lalive (2009) or Torres et al. (2018) for variance decompositions. And although the latter two papers do report the full correlation structure of observed and unobserved wage components, we are the first to directly interpret the shares of the the sub-components of sorting covariance attributable to observable, partly-observable and fully unexplained factors. Also, we will rely on this distinction in our decomposition exercise of some of the most notable observable wage gaps in Hungary.

Finally, we relax and investigate the potentially too restrictive assumption of time-invariant and worker-type-invariant firm effects in the standard AKM framework, as firm effects may not be stable across time or the same for all groups of workers (with respect to their observable characteristics). A detailed assessment of the former problem and a model with time-varying, firm-year effects is presented by Lachowska et al. (2020). Firms, however, may also pay differing premia of workers of different observable characteristics, for instance due to differences in bargaining power and the firms' rent-sharing propensity. By introducing differing firm(-group) effects or firm effects based on race and gender categories, Card et al. (2016) and Gerard et al. (2021) propose a way to decompose the differences in the average firm effects faced by ethnic or gender groups into a bargaining (within-firm) and a sorting component. And while a sorting parameter with respect to observable characteristics could be also captured by decomposing gaps in the standard or three-way AKM model, as in Cardoso et al. (2016), these flexible models may yield more precise estimates through not assuming wage-gaps to be constant across all employers. Our finding that only half of the sorting covariance is attributable to unexplained wage components, motivates us to adapt a slightly modified version of the above models for assessing bargaining and sorting differences across workers

[5] Later, Cardoso et al. (2018) and Addison et al. (2018) also use the same decomposition method as Cardoso et al. (2016), building on Gelbach (2016), to decompose the union-membership wage-gap and the returns of education in Portugal into occupational, individual, firm and match effect components. For Hungary, Gyetvai (2017) presented preliminary results from a wage variance decomposition on a shorter panel.

of different gender, education, occupation, age or tenure – estimating some novel AKM specifications in the process.

By adapting the above listed extensions into the models we use, we aim to contribute to the literature of wage inequalities in more than one ways, with the following main findings. We start by providing evidence on another country where positive wage sorting is strongly prevalent. Although such results are already available from a handful of countries from Western Europe, Scandinavia and also from the US and Brazil, Hungary is the first Eastern European, post-soviet country to present such estimates. Surprisingly similar wage structure patterns are found to those from the countries above, further reinforcing the emerging pattern across studies, that labor markets tend to behave similarly in a wide-range of countries with different historical and institutional backgrounds. While the overall contribution of individual heterogeneity is around 50%, of firm heterogeneity 22% and of occupational heterogeneity only 8%, sorting channels turn out to be rather important. The estimated correlation between person and firm effects is 0.18, with the underlying covariance explaining 9.3% of overall wage variation, while the sorting of high wage workers to high wage occupation also responsible for 10.7%. Exploiting data on firms' financial reports, we also reinforce the findings of Torres et al. (2018) about the relation between wage sorting and actual matching in productivity. Notably, we find that worker heterogeneity captured by person effects is indeed correlated with the observed value added of firms, not just the assumed productivity differences reflected in wage levels.[6] We also utilize the 10^{th} grade results of young workers on The National Assessment of Basic Competences, to assess whether individual literacy and mathematics skills – measured at around the age of 16 – correlate with future worker wage or firm productivity. We find that both absolute and relative, within-school test scores move together with the worker effects, occupation effects, firm effects and firm value added as well. This latter correlation – estimated to be around 0.12-0.14 – is a direct evidence for assortative matching, with value added capturing firm productivity and test scores proxying expected worker productivity.[7]

To better understand the origin of wage sorting, we focus on sorting related to observable characteristics – accounting for half of the overall sorting in the Hungarian labor market. First, following the methodology of Cardoso et al. (2016), we decompose some of the most prevalent wage gaps into individual, firm-specific and occupation-specific components. Doing so we show that within-firm gender-based, educational or residential wage differences can be indeed exaggerated by sorting and segregation mechanisms as well. Also, we reflect on the different selection of workers based on ownership of the hiring firm, finding that multinational employers are substantial contributors to the relatively high wage sorting in Hungary, as besides paying high wages generally, they are also able to hire the most skilled workers as well. Finally, using grouped-AKM specifications that allow for differing firm-

[6] Correlation is 0.37, but the difference is partially due to higher lever of sorting among firms with balance sheet data, as on the sample of incorporated firms the baseline correlation is also higher, 0.31.

[7] Due to the limited time-frame for which this data is available, this correlation relates to the sample of young workers only (7% of the sample), for whom wage sorting measured by AKM effects is substantially weaker than in the full sample.

effects for workers of different observable characteristics, we also present evidence for workers of different occupations and age sorting into firms of different average wage-premia, also amplifying the corresponding within-firm wage differences.

The paper is structured as follows. Section 1.2 presents our take on the wage variance and wage-gap decomposition techniques established in recent literature. Section 1.3 discusses the sources of data. Section 1.4 contains the main wage variance decomposition and discusses indirect (wage-based) and direct (productivity-based) measures of assortative matching, while Section 1.5 pursues observable patterns of sorting, by utilizing wage-gap decompositions and alternative specifications of the AKM model. Section 1.6 concludes.

1.2 Wage models and decompositions

1.2.1 The log-additive model of wages

Building upon Abowd et al. (1999), Card et al. (2013) and Torres et al. (2018), let us consider the following, log-additive model of wages:

$$ln\ w_{ijt} = \boldsymbol{X_{ijt}\beta} + \theta_i + \psi_j + \lambda_{k(ijt)} + \varepsilon_{ijt} \tag{1.1}$$

where w_{ijt} is the wage of person i working for employer j in occupation k at time t. $\boldsymbol{X_{ijt}}$ consists of observable, time-varying characteristics (age, firm size, year), and the other terms are the time-invariant worker, firm and occupation effects respectively, with a zero-mean, independent residual term added. That is, we consider occupations as a third high-dimensional fixed effect instead of $\boldsymbol{X_{ijt}}$ containing hundreds of occupation dummies.[8] If such model is estimated by OLS, the effect of time-invariant characteristics of individuals and firms are absorbed by the fixed effects, and can only be obtained by running second stage regressions on the estimated fixed effect parameters.[9] Specifically one can estimate:

$$\hat{\theta}_i = \boldsymbol{W_i\eta} + \varepsilon_i^I \tag{1.2}$$

$$\hat{\psi}_j = \boldsymbol{Z_j\gamma} + \varepsilon_j^J \tag{1.3}$$

In these second stages[10] , $\boldsymbol{W_i}$ contains time-invariant, although observable char-

[8] Torres et al. (2018) argues that using even highly detailed occupations may not be ideal, as occupational wage standards may be different across different sectors. For instance, a secretary of an IT firm may not face the same occupational wage standard as a secretary in an assembly firm. As in Hungary sectoral collective agreements are not as prevalent, and as we also lack such data, in our empirical exercises we rely only on occupations, but include sector-occupation interactions in one of our tests for model robustness.

[9] For instance the Stata routine of Correia (2017), *reghdfe* implements the estimation of such multi-way high dimensional fixed effects model, based on the algorithm of Guimarães and Portugal (2010). The connected set on which firm, person and occupation effects are not only identified but are also comparable is more restricted than in the two-way fixed effect case, and has to be defined according to the algorithm of Weeks and Williams (1964), as noted by both Cardoso et al. (2016), Gyetvai (2017), and Torres et al. (2018)

[10] The concept of which is already present in Abowd et al. (1999) and later utilized, for instance, by Woodcock (2008), Gruetter and Lalive (2009), T. Sørensen and Vejlin (2013), Torres et al. (2018) and Alvarez et al. (2018).

acteristics of the workers, like gender, birth cohort or highest achieved education, and an estimated $\hat{\varepsilon}_i^I$ will reflect directly unobservable individual heterogeneity. Similarly $\boldsymbol{Z_j}$ contains observable firm characteristics, like industry of operations or majority ownership, while $\hat{\varepsilon}_j^J$ will incorporate the unobservable factors defining the wages of the firm, such as reliance on specific waging schemes or rent-sharing from productivity.[11] This two-stage model will serve as the basis of most of the exercises presented in this study.

1.2.2 Wage variance decompositions

Through the past decade, labor economists decomposed the variance of wages in multiple different ways. In this sub-chapter, we present the established approaches and link them to the above-presented three-way fixed effects model. For the sake of notational simplicity, we omit the subscripts/indices of the wage components, with all corresponding to their respective terms defined in Equations 1.1-1.3. The most simple decomposition of the variance of wages within a two-way fixed effect framework can be found, among others, in Card et al. (2013) as follows:

$$\begin{aligned} Var(w) = Var(\theta) + Var(\psi) + Var(X\beta) + Var(\epsilon) + \\ 2Cov(\theta, \psi) + 2Cov(\theta, X\beta) + 2Cov(\psi, X\beta) \end{aligned} \tag{1.4}$$

Besides the variation of individual, firm and time-varying characteristics and the unexplained, residual variation, this form highlights the role of the double co-variance terms. Among these, the most notable one is the covariance between individual and firm effects, a common measure of wage sorting in the labor market, signalling how commonly do better (higher wage) workers match with better (higher wage) firms. If we add additional components , the formulae expand substantially as more terms appear. For example, with the addition of occupation fixed effects, λ, we get:

$$\begin{aligned} Var(w) = Var(\theta) + Var(\psi) + Var(X\beta) + Var(\lambda) + Var(\epsilon) + \\ 2Cov(\theta, \psi) + 2Cov(\lambda, \psi) + 2Cov(\theta, \lambda) + \\ 2Cov(\theta, X\beta) + 2Cov(\lambda, X\beta) + 2Cov(\psi, X\beta) \end{aligned} \tag{1.5}$$

This formula now assesses not only the role of diversity in the average wages different occupations pay, but the possible sorting pattern between high productivity firms and specific occupations (that is the occupational compositions of different types of firms), and also the non-random selection of individuals into occupations. For instance, if the highest paying occupations are getting more and more dominated by those who would be high-achievers in other jobs as well, inequality will increase. Also, in the standard AKM model with two fixed effects, we may overstate the role of firm effects if 'high paying jobs tend to go hand in hand with high-paying firms' as Torres et al. (2018) finds.

[11] Technically some variables, such as industry can be considered and estimated as fixed effects themselves, but for the sake of simplicity we assume all observable characteristics as part of $\boldsymbol{W_i}$,$\boldsymbol{Z_j}$ or $\boldsymbol{X_{ijt}}$.

To assess the evolution of wage inequality in the US, Song et al. (2019) builds upon the decomposition of Card et al. (2013), but further decomposes the variance terms into between and within firm elements. [12] Suppressing the role of time-varying components, $X\beta$, the core of their decomposition is as follows:

$$Var(w) = \underbrace{Var(\theta - \bar{\theta}) + Var(\varepsilon)}_{Within-firm} + \underbrace{Var(\psi) + 2Cov(\bar{\theta}, \psi) + Var(\bar{\theta})}_{Between-firm} \tag{1.6}$$

Within-firm inequality can only originate in the difference of workers effects within the firm and the residual terms. Between-firm differences, however, incorporate three factors: firms can be different in their average wage levels as captured by the firm effects, different quality workers may be employed by different firms – wage sorting – , and finally firms can differ in the *average* quality of workers they employ. The authors label the latter term *segregation*, capturing the fact that differently qualified workers may tend to work at different employers, even among firms with similar wage premia. If we include occupations and the $X\beta$ terms, the above formula becomes[13]

$$\begin{aligned} Var(w) = {} & \underbrace{Var(\theta - \bar{\theta}) + Var(\lambda - \bar{\lambda}) + Var(\varepsilon) + 2Cov(\theta - \bar{\theta}, \lambda - \bar{\lambda}) + W}_{Within-firm} + \\ & \underbrace{Var(\psi) + 2Cov(\bar{\theta}, \psi) + 2Cov(\bar{\theta}, \bar{\lambda}) + 2Cov(\bar{\lambda}, \psi) + Var(\bar{\theta}) + Var(\bar{\lambda}) + B}_{Between-firm} \end{aligned} \tag{1.7}$$

In this somewhat complicated setup, we can observe whether the different valuation of some occupation is generated between or within firms. Similarly to workers, occupations may be specific to high wage or low wage sectors of the labor market, creating another form of segregation. Thus, we can capture, that the sorting of individuals into differently valued occupations can happen in two ways. First, as firms that tend to use the high wage occupations also employ highly qualified workers – even compared to their occupational average –. Secondly, because even within firms, the better workers achieve higher paying occupations, such as managerial positions.

As an alternative to Equation 1.4, Card et al. (2018), and previously Gruetter and Lalive (2009), introduce a formula decomposing the variance of wages into only covariance terms with the additive wage components, as follows:

$$Var(w) = Cov(\theta, w) + Cov(\psi, w) + Cov(X\beta, w) + Cov(\lambda, w) + Cov(\epsilon, w) \tag{1.8}$$

This way, we can predict how much less wages would differ if, for instance, all firms or all workers would be extremely similar. In this setup – labeled as *ensemble* decomposition by the authors – the pair-wise covariance terms from Equation 1.4 are equally accounted to both of their corresponding wage components. For instance,

[12] A concept also presented in Abowd and Kramarz (2004).

[13] Slightly important terms are suppressed. $W = Var(X\beta - \bar{X}\beta) + 2Cov((\theta - \bar{\theta}) + (\lambda - \bar{\lambda}), X\beta - \bar{X}\beta) + 2Cov((\theta - \bar{\theta}) + (\lambda - \bar{\lambda}) + (X\beta - \bar{X}\beta), \varepsilon)$ and $B = 2Cov(\bar{\theta} + \bar{\psi} + \bar{\lambda}, \bar{X}\beta)$.

the contribution of wage sorting – a double covariance term – will now appear partly in the contribution of firm effects and partly in the contribution of worker effects, thus we could not observe its importance directly from this decomposition. Torres et al. (2018) augments the above *ensemble* decomposition by differentiating between observable and unobservable components within the time-invariant firm and person characteristics, as shown in Equations 1.2 and 1.3. Accordingly the variance decomposition will also include multiple individual and employer related terms, specifically:

$$Cov(\theta, w) + Cov(\psi, w) = \underbrace{Cov(W\eta, w) + Cov(Z\gamma, w)}_{Observable} + \underbrace{Cov(\varepsilon^I, w) + |Cov(\varepsilon^J, w)}_{Unobservable} \tag{1.9}$$

We would add, that by differentiating the observable and unobservable terms, the wage sorting component of previous equations could be also further decomposed into (at least) four meaningful components:

$$Cov(\theta, \psi) = Cov(W\eta, Z\gamma) + Cov(\varepsilon^I, Z\gamma) + Cov(W\eta, \varepsilon^J) + Cov(\varepsilon^I, \varepsilon^J) \tag{1.10}$$

Accordingly, we can differentiate between the part of wage sorting that could be fully or partially attributed to observable characteristics, such as the sector or ownership of the firms, or education of workers. And also a part which only reflects assortativity between unobservable firm wage components (premium) and individual heterogeneity (productivity and skills). This provides us with a more detailed analytical tool to assess the source of overall wage sorting, and the potential prevalence of assortative matching in worker and firm (unobserved) productivity.

1.2.3 Indirect and direct measures of assortative matching

To assess the role of assortative matching and wage sorting, we will first estimate the standard sample correlation coefficient between estimated firm and person effects, which are often interpreted as indirect measures of firm and individual productivity. We have to note, that even if the model is correctly specified, AKM estimations suffer from a now well-explored incidental parameter problem, labeled as limited mobility bias by Andrews et al. (2008). As firm effects are identified only from movers switching between firms, if the mobility in the labor market is low – for instance, because of short observation periods, using subsamples, or simply having few movers in given sectors – then AKM effects will be estimated with high variance, and sample variances and covariances of the estimated effects will be biased measures of the actual moments of their respective distributions. Specifically one would overstate the variance of firm effects, and thus their importance in wage variation, and also understate the covariance between firm and person effects, due to the negative correlation between sampling errors of parameters of the same observation.

While bias-correction methods have been developed by Andrews et al. (2012) and recently by Bonhomme et al. (2019) and Kline, Saggio, and Sølvsten (2020b), these methods are often computationally exhaustive or only work subsets of data, most authors, including Card et al. (2013) and Song et al. (2019), just acknowledge the possible presence of this bias and note that it could be safely assumed that observed trends are not affected. As our panel is only a 50% sample of the population, the limited mobility bias problem probably should not be neglected, but our computational setup do not (yet) allow us to implement the methods of Bonhomme et al. (2019) and Kline, Saggio, and Sølvsten (2020b). However, similar to Torres et al. (2018) and relying on some reassuring examples in the literature[14] , we argue that having fifteen years of data in the same panel may help overcoming this issue. Additionally, we can also observe within-year movements as well, which may also increase the number of job switches per firm used for identification. Nevertheless, we include some additional robustness estimates in which we artificially decrease the observed mobility in the data, and find only a small, although non-negligible change in the main parameters of interest.

Besides the usually reported correlation of the firm and worker effects, we will report $Cov(\varepsilon^I, \varepsilon^J)$ as well. This term captures correlation between the unobservable (residual) firm and person specific components, and therefore may reflect complementarity in productivity better. For instance, the standard measure would incorporate segregation effects as well, if women (lower person effect) would more often match with low wage sectors (low firm effect) for reasons other than productivity, such as different taste for amenities at these firms or discrimination on the employers' side, forcing women out of better workplaces.

Additionaly, we will also rely on firm value added per person as a direct measure of productivity, and following Torres et al. (2018) we will report a correlation between person effects and observed firm productivity as well. This measure is proposed by the authors as a response to critiques of interpreting wage sorting as assortative matching (in productivity) arguing that AKM firm-effects may not be monotonous in firm productivity as not only productivity and rent-sharing may shape average wage-levels of firms. Relying on a direct measure for firm productivity overcomes this issue.

Finally, we would also utilize test scores of individuals from an assessment of mathematics and literacy skills taken at around the age of 16. First, by looking at $corr(\hat{\theta}_i, score_i)$, we can check on the individuals' level, whether a high test score predicts high worker effect. If we uphold that the tested skills measure otherwise unobserved worker skill and productivity, we can test whether worker fixed effects are indeed monotonous in worker productivity.[15] Also if this assumption holds, then $corr(score, \hat{\psi})$ and $corr(score, \hat{VA})$ will serve as direct measures of productivity sorting, complementing the findings relying only on the AKM framework.

[14]For instance, Lachowska et al. (2020) show (their Table 4) that in a panel of 13 years, the correction of Kline, Saggio, and Sølvsten (2020b) alters estimated results in an almost negligible manner.

[15]If one takes as given that worker productivity is well reflected in wages, then this test could be instead used to answer whether the NBAC indeed measures things that are related to future labour market outcomes of students. However, testing for both ways of this relation is fundamentally impossible without outside measures of productivity, which we lack unfortunately. Still, we assume that literacy and mathematics skills – which these tests hopefully measure reliably – are positively correlated with a wide set of essential skills used in the labor market.

1.2.4 Wage-gap decompositions

Due to the linearity of our wage model, the overall wage difference among groups by any observable control, C, can be decomposed the following way in a similar fashion as in Cardoso et al. (2016). [16]

$$\frac{\partial \ln w_{ijt}}{\partial C} = \frac{\partial \theta_i}{\partial C} + \frac{\partial \psi_j}{\partial C} + \frac{\partial \lambda_{k(it)}}{\partial C} + \frac{\partial \boldsymbol{X_{ijt}\beta}}{\partial C} \tag{1.11}$$

In order to provide a more detailed assessment of differences across observable and unobservable time-invariant characteristics, we can use the second stage decompositions of Equations 1.2 and 1.3, by substituting the (linear) detailed decomposition of firm and person effects for their corresponding composite terms.

$$\frac{\partial \ln w_{ijt}}{\partial C} = \frac{\boldsymbol{W_i\eta}}{\partial C} + \frac{\varepsilon_i^I}{\partial C} + \frac{\boldsymbol{Z_j\gamma}}{\partial C} + \frac{\varepsilon_j^J}{\partial C} + \frac{\partial \lambda_{k(it)}}{\partial C} + \frac{\partial \boldsymbol{X_{ijt}\beta}}{\partial C} \tag{1.12}$$

Alternately, we would note that the differences in person traits could be decomposed into differences generated within and across firms in the spirit of the Song et al. (2019) approach. Similarly, distinguishing whether the workers of a given type of firms are generally prone to work in high wage firms or that they only earn higher wages in given types of firms can help in understanding the segregation mechanisms at hand. Accordingly, the following decomposition also holds, with barred variables denoting the firm-level mean individual effects or the person-level mean firm effects.

$$\frac{\partial \ln w_{ijt}}{\partial C} = \frac{\partial(\theta_i - \bar{\theta}_{j(i)})}{\partial C} + \frac{\partial \bar{\theta}_{j(i)}}{\partial C} + \frac{\partial(\psi_j - \bar{\psi}_{i(j)})}{\partial C} + \frac{\partial \bar{\psi}_{i(j)}}{\partial C} + \frac{\partial \lambda_{k(it)}}{\partial C} + \frac{\partial \boldsymbol{X_{ijt}\beta}}{\partial C} \tag{1.13}$$

Now, if we instead of a general Z consider a time-invariant, observable personal characteristic, G, the above two approaches from Equations 1.12 and 1.13 could be combined in a tractable way, as some components are again zero by definition in such case.[17] A detailed decomposition - after controlling for differences in time-varying and occupation effects - by G would then be the following.

$$\frac{\partial \ln w_{ijt} - \boldsymbol{X_{ijt}\beta}}{\partial G} - \frac{\partial \lambda_{k(it)}}{\partial G} = \underbrace{\frac{\partial(\theta_i - \bar{\theta}_{j(i)})}{\partial G}}_{\text{Within-firm gap}} + \underbrace{\frac{\partial \bar{\theta}_{j(i)}}{\partial G} + \frac{\boldsymbol{Z_j\gamma}}{\partial G} + \frac{\varepsilon_j^J}{\partial G}}_{\text{Between-firm gap}} \tag{1.14}$$

For instance, if G stands for a dummy on gender, this decomposition would tell us the following. How different premium firms do male and female workers sort into comes from a part that is explainable by observable firm differences, such as sectors and ownership, and a component coming from unexplained firm premia.[18] Besides,

[16] As ε is by design independent of any characteristic of $C \in X$, $\frac{\partial \varepsilon}{\partial C}$ is zero. The same holds true for elements of the person and firm effects, that is for $C \in Z$ or $C \in W$.

[17] Specifically, ε_i^I is independent of G, and there are no within person deviations in the person effect during one's lifetime.

[18] Following Woodcock (2008) we could label these terms inter-industry sorting and intra-industry sorting respectively, despite being derived slightly differently.

the average person effect difference between males and females can on one hand generated within firms, due productivity differences or discrimination for instance. However, another, between-firm component is present as well if, for instance, males workers tend to work at firms that usually employ highly productive, high wage individuals. This element is naturally related to the segregation component of Song et al. (2019), and accordingly, if males and females would be equally represented in firms (no segregation), it would be zero.

Considering a time-invariant firm characteristic, F, a similar decomposition is as follows.

$$\frac{\partial \mathrm{ln} w_{ijt} - \boldsymbol{X_{ijt}\beta}}{\partial H} - \frac{\partial \lambda_{k(it)}}{\partial H} = \underbrace{\frac{\boldsymbol{W_i \eta}}{\partial H} + \frac{\varepsilon_i^I}{\partial H} + \frac{\partial \bar{\psi}_{i(j)}}{\partial H}}_{\text{Between-individual gap}} + \underbrace{\frac{\partial(\psi_j - \bar{\psi}_{i(j)})}{\partial H}}_{\text{Within-individual gap}} \tag{1.15}$$

The interpretation of this equation is similar to the case of individual characteristics. Let us consider, for example, the case of firms with majority foreign ownership. These firms may employ workforce that would earn higher wages anywhere, either because high observable (education) or unobservable skills. Besides it is not irrelevant, whether employment spells at multinationals are especially important in workers lifetime, or these workers generally tend to enter high premia firms, foreign-owned ones not being more special than other high-quality workplaces.[19]

1.2.5 Alternative specifications of the AKM model

One common alternative to compare the two-way, additive AKM model to is the match model, in which all employer-employee matches can have their own wage component, providing a fully elastic representation of firm premia. Even if most firms and workers don't meet more than once, in such models the age and tenure effects are calculated within employment spells of the same employer-employee matches. The estimated match effects can be then, in a second stage decomposed into firm and person effects, with the residuals of that equation, $\tilde{\omega}_{ij}$, representing the (orthogonal) match components (Woodcock, 2015). [20]

$$ln\ w_{ijt} = \boldsymbol{X_{ijt}\beta} + \omega_{ij} + \lambda_{k(ijt)} + \varepsilon_{ijt} \tag{1.16}$$

$$\omega_{ij} = \tilde{\theta}_i + \tilde{\psi}_j + \tilde{\omega}_{ij} \tag{1.17}$$

Due to the flexible assumptions of the models on firm-worker relation, these models are expected to provide an overall better fit, and more precise assessment of firm and person effects. Most authors, however, argue that the improvements by applying such models, measured by the decrease in model RMSE for instance,

[19] The decompositions in Equations 1.14 and 1.15 are also special cases of the decomposition what Boza and Ilyés (2020) proposes and applies for assessing the effect of the presence of former coworkers on entry wages.

[20] By including occupation effects as well, we actually have four fixed effects, that can be estimated in two consecutive steps, similarly as in the decomposition exercises of Cardoso et al. (2018) and Addison et al. (2018).

are marginal, and hence the linear, additive assumptions of the AKM model are not essentially mistaken. The importance of the orthogonal match terms can be also measured by $\frac{cov(\tilde{\omega}_{ij},w)}{var(w)}$, and we will use this formulation to reflect on model robustness later in the paper.

As a middle ground between standard AKM and match models, one can also allow for the firm effects to only vary over specific observable characteristics. Examples for interacting firm effects with person characteristics appear in Card et al. (2016) and Card et al. (2018), who use these specifications to test for differential rent-sharing, their main assumptions being that firms may not pay the same premia for their male and female (or educated versus non-educated) workers. If firms share their rents differently with such groups, for instance due to differing bargaining power of individuals, we expect to find differences in the average firm-group level fixed effects / wage components across the grouping characteristic. One way to formulate such model in a simple equation is:

$$ln\ w_{ijtg} = \boldsymbol{X_{ijtg}\beta} + \theta_i + \Psi_{jg} + \lambda_{k(ij)} + \varepsilon_{ijtg} \tag{1.18}$$

The above formulation is somewhat different from that of Card et al. (2016), Card et al. (2018) and Gerard et al. (2021), who in practice fit separate AKM models on male and female, educated and non-educated, or white and non-white subsamples, allowing for different returns for all included observable controls for the given subgroups. When testing the robustness of the AKM model, we will pertain the setup of Equation 1.18, assuming the same occupation, age and tenure effects for all sub-groups. Beside groupings based on gender or education, we propose three new specifications, in which the firms pay different premia for their workers of different occupations (job model), completed tenure or age. Let us refer to the family of all such models throughout the article G-AKM – after*grouped*-AKM. [21]

Similarly as in the match model, the estimated firm-group effects can be, in a second stage decomposed into the composite of the predicted effect of the grouping variable, and the (baseline) firm effects:

$$\Psi_{jg} = G\tilde{\beta}_g + \tilde{\psi}_j + \varepsilon^G_{jg} \tag{1.19}$$

The residual of this step conveys how much explanatory power we gain by allowing the firm effects to vary across group members. For instance, if the gender wage-gap would be the same across all-firms then a β_g parameter and the firm effects capturing the mean firm premia would already perfectly explain the firm-gender effects. The large role of ε^G_{jg} would, however, suggests that the gap may widely differ across the range of firms. Checking the differences in the average firm-group effects and the firm-effects in the second step also provides an alternative to the approach of Card et al. (2016) for assessing bargaining differences and sorting with respect to observed characteristics.

[21] The G-AKM firm-group effects could be also used to assess differential rent-sharing behaviour of firms, using the firm-group effects as measures of wage net of skill composition effects (Card et al., 2018; Card et al., 2016). For results on differential rent-sharing on Hungarian data, see Chapter 2 of this thesis.

$$\frac{\partial \Psi_{jg}}{\partial G} = \tilde{\beta}_g + \frac{\partial \tilde{\psi}_j}{\partial G} \tag{1.20}$$

The LHS term in Equation 1.20 is the overall difference in firm effects based on an observable characteristic, say gender, while the right hand side is the composite of a term capturing the within-firm wage differences, and a term capturing the different sorting of groups in G. As Appendix B demonstrates, this method will provide an estimate which is the weighted average of the two decomposition proposed in Card et al. (2016), with the additional advantage of being easily generalisable for G-s of more than two groups.[22]

Finally we note, that in a similar fashion, one may also allow firm effects to vary over time. This allows for firms paying a different premia in different periods or even year-by-year.[23] This model has been previously proposed and used by Macis and Schivardi (2016), Lamadon et al. (2022) and by Lachowska et al. (2020), with the latter labeling the model TV-AKM. In Section 1.5.2 we will estimate this alternative specification as well, alongside the above outlined models with firm-group interactions.

1.3 Source of data

In the empirical part of this paper we estimate the AKM model from Equations 1.1-1.3, and report the expanded decompositions from Equations 1.5, 1.7 and 1.8 through 1.10 to characterize wage dispersion in Hungary. Utilizing correlations between individual effects and firm effects or the value added of employers we check for wage and productivity sorting as well. By regressing firm effects on firm's value added in a simple OLS we measure cross-sectional rent-sharing elasticites as well. We will also rely on data on test scores to deliver a direct measure for assortative matching (as opposed to measures inferring productivity from wage levels). To asses sorting mechanisms attributable to observable characteristics we first decompose some common wage-gaps across observable person or firm characteristics. Then, after testing the fit of grouped AKM models, we use such specifications to decompose differences in firm-group effects into bargaining and sorting components.

The estimations use data from the Databank of the Research Centre for Economic and Regional Studies[24]. The Panel of Administrative Data from CERS is a large, administrative, linked employer-employee panel dataset, covering a random fifty percent of the Hungarian population. The two-way panel spans from 2003

[22]As $\tilde{\psi}_j$ captures the average premium of the firm after controlling for its composition with respect to G, $\tilde{\psi}_j$ should be roughly equal to ψ_j of the original AKM specification, and therefore of the decomposition in Equation 1.11. The difference of the wage gap estimators, $\frac{\partial \psi}{\partial G}$ and $\frac{\partial \tilde{\psi}}{\partial G}$ signals that the assumption of a constant gap is too restrictive in decompositions using the original AKM model, such as Cardoso et al. (2016). In our estimation, while the correlation between these terms and the standard AKM firm-effects is around 0.99, we will find meaningful differences in the partial derivatives.

[23]Which assumption – not accounting for computational constraints – would in the worst case result in loss of efficiency, if the firm effects are in fact, stable over time.

[24]Formerly of the Hungarian Academy of Sciences, now of Eötvös Loránd Research Network.

through 2017 and contains labor market data in monthly resolution, such as an ID for the employer, earnings in given month, occupation information[25] and balance sheet data for incorporated employers.[26] We observe all taxed earnings from the given employer during the given month, but cannot differentiate between bonuses, and general wage.[27] The data does not convey any family-related information, only individual characteristics like gender, age, residence and also some variables on healthcare expenditures and specific transfers received by the individuals, which we do not utilize in this research.

The data also has some unique features regarding education. Although we do not have a common "highest education" variable available for the full panel, in the second half of the observation period we have information on the high school and university attendance of the individuals. Also we observe test scores earned on a standardized country-wide test of mathematics and literacy skills for some young cohorts in the data. The National Assessment of Basic Competences (NBAC) is conducted in each year with the participation of all students in Hungary in 6^{th}, 8^{th} and 10^{th} grades, that is around the ages of 12, 14 and 16 respectively. As we observe these scores and school identifiers only for those who have still attended one of these tests in and after 2011, the utility of this information is somewhat limited by the end of our panel. Specifically part of these cohorts – those who aim for a university degree – may be just entering the labor market after 2014 or even later, leaving only a few years of observations about labor market participation. Accordingly while we have test score data available for 6.85% of the individuals in our sample, this corresponds only to 1.67% of total wage observations. Nevertheless, we try to make use of both the NBAC scores and high school identifiers in trying to assess the extent of assortative matching with regard to labor market entrants (without higher education). Choices about the included variables, approximations and sample restrictions are detailed in Appendix C.

Our results comprise of two larger sections. The first, Section 1.4, contains the results of the main decomposition techniques presented in Section 1.2.2 for the largest sample we had and the discussion of the role of wage components and sources of wage sorting, along the direct assessment of worker and firm productivity and tests for the validity and robustness of the model. The second set of results, in Section 1.5, focuses on the role of observable characteristics in wage-sorting. The section first presents decompositions of the most relevant wage-gaps in the Hungarian labor market using the three-way AKM model, then introduces the grouped-AKM approach to decompose differences in firm effects into bargaining and sorting components, building on Card et al. (2016).

[25]Occupational heterogeneity throughout our paper refers to 332 occupational categories obtained from the harmonization of two sets of 4-digit occupations based on the Hungarian equivalent of the ISCO system – one for the years before 2011 and one thereafter.

[26]Unlike LEED data from many other countries we lack establishment identifiers, so we can treat only whole firms and institutions as the unit of observations.

[27]The social contribution reports which form the basis of the data have to be submitted on a monthly basis since 2012. Before that, yearly earnings from an employer were attributed to calendar months accordingly to the number of days of the employment spell belonging to the given month.

1.4 Results I. – Variance decompositions and sorting

1.4.1 Main decomposition and evidence for sorting

We start by presenting results from estimating the AKM model with additive firm, person and occupation effects, on the full sample of fifteen years of (quarterly) data pooled together, alongside the second stage regressions of estimated fixed effects on observable time-invariant components. Table 1.1 contains three panels corresponding to the detailed variance decomposition of Equations 1.8 and 1.9, following Torres et al. (2018), the main moments characterizing wage sorting, and also the main between-firm elements of the decomposition of Equation 1.6, based on Song et al. (2019).

The main decomposition provides importance shares of the wage components of similar magnitudes as previous studies shown in Appendix Table A.1. Even after controlling for firm and occupational heterogeneity, individual time-invariant differences contribute to half of the overall wage variation. Of that, around one-third could be attributable to gender and skill differences – proxied by educational requirement of highest occupation –, and most part of the individual heterogeneity remains unexplained. This unexplained part, comprising, for instance, unobserved skills in itself give almost 30% of the overall wage dispersion.[28] Occupations capture around 8% of overall variation. This component is also similar to the finding of Torres et al. (2018), who find a 15% share for the total explanatory power of additive occupation and the collective agreement of the firm.[29] The firm component accounts for a bit more than fifth of overall dispersion, with two-thirds of it accounting for factors other than sectoral differences or the type of majority ownership, while the between sector (owner type) differences in firm premia accounts for 2.4% (4.1%) of the overall dispersion. The observable elements are not negligible either. If foreign-owned, domestic private and state-owned firms and institutions would not differ systematically in their wage policies, overall wage variance would be almost 4% lower in Hungary. Finally we would note that the share of residual variation, not explained by observed factors or fixed effects, is slightly higher than in previous studies, being 14.6%.[30]

[28] As education is only crudely proxied – as discussed in Appendix A.3 –, we probably underestimate the role of observable characteristics and overestimate the role of unobservable ones compared to the results we would have if data on detailed educational attainment would be available for all cohorts.

[29] The authors do not report the shares attributable to the two factors separately.

[30] As we present later, the model fit is somewhat stronger in the earlier periods of the data, and weaker for the final years of the data.

Table 1.1: Decomposition of wage variance, Full sample

Variance of log wages	0.338	
Ensemble decomp. (and sub-shares) (%)		
Contribution of XB	**5.40**	
— Year	*1.98*	36.8
— age*, firm size, contract, tenure*	*3.41*	63.2
Contribution of individual heterogeneity	**49.85**	
— Unobserved individual heterogeneity	*29.00*	58.2
— Observed individual (gender, quasi ed.)	*17.62*	35.3
— Birth year	*0.32*	0.6
— Region	*2.91*	5.8
Contribution of firm heterogeneity	**22.21**	
— Unobserved firm heterogeneity	*15.69*	70.6
— Observed firm heterogeneity (ownership)	*4.14*	18.6
— Sector	*2.38*	10.7
Contribution of occupations	**7.93**	
Residual variation	**14.61**	
Correlations (and contr. to overall)		
Corr(θ_i,ψ_j)	0.175	9.3%
Corr(ε_i^I,ε_j^J)	0.138	4.4%
Corr(θ_i,ψ_j) for inc. firms	0.310	15.5%
Corr(θ_i,VA_j) for inc. firms	0.364	
Corr(ψ_j,VA_j) for inc. firms	0.615	
Between-within decomposition (%)		
Between-firm share	47.5	
— Ind. segregation	11.3	
— Var(ψ_j)	18.3	
— Sorting	9.3	
Number of Observations (1000)	66155	
Number of Firms (1000)	144	
Number of Workers (1000)	2462	

Notes: The table conveys moments relating to the components of the estimated model of Equations 1.1-1.3. The first panel comprises the ensemble decomposition based on Equation 1.8. Second panel contains sample correlations of estimated firm and person effects (both overall and unobserved parts) and firm value added. The third panel represents the between elements of the decomposition of Equations 1.6 and 1.7. The exact sample and variables used are defined in Appendix A.3.

Considering, whether the overall wage dispersion is generated between or within firms we apply the (modified) decomposition of Song et al. (2019) from Equations 1.6 and 1.7 and present the main components in Table 1.1, with the full decomposition of Equation 1.7 presented in Appendix table A.3. The figures in Table 1.1 highlight that around half of wage differences originates in differences between firms. This share is higher than the 22% percent share from the first panel, as it encompasses not only the fact that firms differ in their average premium (18.5%), but also the full effect of high wage workers sorting into high-wage firms (9.1%), and the fact that workers of different skills (different person effects) segregate into different firms (11.3%). The detailed decomposition also reveals that workers with higher individual wage components tend to work in higher wage occupations. This also affects the between firm differences as the occupational composition of firms and the quality of their workforce is related, accounting for another 4.1% – a pattern observed by Torres et al. (2018) as well. Even within firms, better workers get into better occupations. Specifically, two thirds of the dispersion in occupation effects happens within firms, contributing to the overall within variation by 3%.

As this decomposition already highlights, there is a positive correlation between firm and worker effects, accounting for almost one-tenth of overall dispersion in wages. The corresponding correlation is 0.17, that is not as high as in some previous studies, but definitely positive. Using the notion that this covariance term could be further decomposed according to 1.10, we can check in more detail the source of this sorting pattern. Unlike Torres et al. (2018), whose Table 4 reports a negative correlation between the unobserved sub-components of the fixed effects, we find a smaller, although positive correlation even for this moment as it is presented in the middle panel of Table 1.1, and in the detailed decomposition of Table 1.2. The latter table also reveals that a relatively small fraction of the covariance could be attributable to correlations between observable person and observable firm characteristics. Instead, better latent skill workers tend to sort into different sectors and ownership categories, and workers of different regions and education end up in firms with different unobserved wage components, with the co-movement of unobserved components accounting for 47% of the covariance term. That is around half of the estimated wage sorting relates, at least partially, to observable characteristics. In order to understand the channels in which wage gaps along observable characteristics shape the wage distribution, we revisit this question in Section 1.5.

Table 1.2: Sources of Covariance Between Firm and Worker Effects

	θ_i	Unobs.	Gender, educ.	Region	Cohort
ψ_j	**0.175**	74.1%	19.8%	14.7%	-8.6%
Unobs	86.0%	47.0%	29.3%	11.5%	-1.8%
Ownership	11.1%	15.9%	-2.2%	2.5%	-5.1%
Sector	3.0%	11.3%	-7.3%	0.7%	-1.7%

Notes: Column variables correspond to the second stage decomposition of worker effects into (proxied) education, gender, birth cohort, and residential components, while row variables further decompose firm effects into ownership and industry components, as proposed in Equations 1.2 and 1.3. The first row and column decompose the covariance between worker and firm effects along one dimension, and the bottom-right (main) panel of the table presents the two-dimensional covariance decomposition proposed in Equation 1.10.

Another way to characterize the sorting patterns is to explore which parts of the joint distribution of worker and firm effects causes the correlation. To check this we assign workers into ten quartiles both alongside their estimated worker and firm effect, then plot the joint distribution of observations in our sample with regard to these two discretized dimensions. Figure 1.1 suggests that while it is clear that high wage workers end up at high wage firms, it does not hold that the lowest quality workers end up in the worst firms. Instead, inferior workers match with middling firms, and the lowest premium firms employ various types of workers, which is still consistent with a positive correlation.

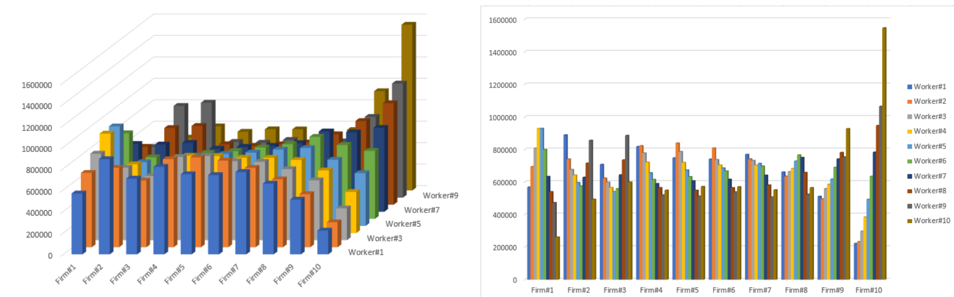


Figure 1.1: Joint distribution of firm and person effect deciles

Notes: The left panel presents the number of observations by cells defined along 10 deciles of estimated firm effects and 10 deciles of estimated person effects. The right panel presents the same numbers for these cells, first grouped by the firm effect deciles.

Although we lack the computational infrastructure for reproducing the bias correction methods of Kline, Saggio, and Sølvsten (2020b) or Bonhomme et al. (2020), we would like the assess the severity of limited mobility bias in our sample. While the long panel and the ability to observe within-year mobility works in our favor, our dataset being only a 50% sample of the population decreases the level of observed mobility per firm. In Appendix Table A.4 we repeat our main estimations after further decreasing our data, simulating scenarios if the dataset would be only a 20% or 10% random sample from the Hungarian population. Accordingly to the predictions of studies on limited mobility, as we artificially decrease sample size, estimated the correlation between firm and person effects decreases in columns 2 and 3, while sampling after estimating the model (columns 4 and 5) in itself does not alter the estimated moments. However, we do not see any substantial increase in the contribution of firm heterogeneity or in the share explained by sorting, even after dropping 80% of our original sample. This somewhat reassures as that our main estimations can be considered mostly reliable.[31]

[31]The last column of this table comprises results from using wage data from February, May, August and November, instead of January, April, July and October, suggesting a marginal role of choice among the two sampling methods.

1.4.2 Firm productivity, rent-sharing and student skills

Following Torres et al. (2018), we also show that the implicit productivity measure of firm effects and the value added parameters calculated from balance sheet data are substantially correlated, as $corr(\hat{\psi}, VA)$ is around 0.6 for those firms who have such data available.[32] The correlation between this direct measure of firm productivity and AKM worker effects is 0.36. However, we have to note that on the sample of incorporated firm, which have balance sheet data available the correlation between AKM firm and person effects is also above 0.3 – see Table 1.1. Nevertheless, this result implies that the wage sorting inferred from AKM effects indeed reflects productivity sorting and positive assortative matching as well, reinforcing the notions of Torres et al. (2018).

These strong correlations also suggest that the wage level or premia of firms highly depends on firm productivity. This, of course, could happen due to various reasons. For instance, high wage firms may operate in dangerous sectors or demand more overtime, paying high compensating differentials. Alternately productive firms may rely more on rewarding wage schemes like efficient wages. Also, they may share the rents of being productive with their workers through higher wages. Quite importantly, the correlation of wages and productivity could be the result of more productive workers employing higher quality workforce. Following Card et al. (2018) and Card et al. (2016) we regress the estimated firm effects on the value added per worker of the firms, while controlling for 2-digit sector codes and ownership, and get an elasticity of 0.15, which is cross-sectional estimate of rent-sharing, albeit pure of workforce composition effects. This elasticity is quite similar to the findings of Card et al. (2016) – 0.16 for males, 0.14 for females – and Card et al. (2018) – 0.12 for males, controlling for broad sectors and cities. Using log sales or the lagged value of firm productivity as an instrument for productivity, we get somewhat higher estimates, similarly as the authors cited above.[33]

In the final exercise of this sub-section we focus on observations of those young workers who were in tenth grade in the academic years of 2011/2012, 2012/2013 or 2013/2014, as for these students we have data on their test scores achieved on the National Assessment of Basic Competencies – a compulsory test in mathematics and literacy skills.[34] Unfortunately, we can follow these cohorts only for 3-5 years

[32] Although, unlike Torres et al. (2018) we use only the value added per worker values calculated directly from balance sheet data and do not estimate production functions controlling for capital and labor composition as the authors did. For the same, raw measure of productivity the aforementioned authors found a correlation of 0.55 and we can find similar correlation in the work of Card et al. (2016) as well – 0.42 for male and 0.38 for female workers.

[33] Appendix Table A.5 presents the result for the cross-sectional rent-sharing estimations, both with using the firm-year level average wage, and the AKM firm effects as outcomes. All models control for sectors defined by the interaction of majority ownership (3 categories) and one digit industry codes (15 categories). Firms belonging to the 'no surplus' range, defined as in Card et al. (2016) are omitted from the estimations. In an accompanying paper on differential rent-sharing, the second chapter of this thesis, we introduce a more nuanced measure for rent-sharing, using temporal variation in firm wage levels and productivity, while still controlling for composition – by using TV-AKM effects –, and also use G-AKM models to investigate whether productivity rents are shared differently across workers of different gender, age, education, occupations or seniority within the same firms.

[34] This test does not serve as a basis for any further academic outcome, for instance university

after taking the tests, that is typically only 1-3 years on the labor market, at the age between 19 and 22, even if they did not take higher education.[35] This substantially limits the scope of conclusions to be drawn from the following results. Nevertheless, we try to explore the relation between these scores and some specific labour market outcomes of those who enter the labor market after (or instead of) high-school graduation. [36]

For Figure 1.2, we collapsed the mathematics test results of student into seven quantiles in two different ways. First, by generating the septiles across all students taking the NBAC in the same year, then only across given schools and testing years. The latter septiles therefore correspond to the within-school relative performance of students. We then plot the mean of future wage observations, estimated AKM individual effects and firm effects, and – if available – the value added measures from firms' balance sheet data in employment spells in employment spells between 2014 and 2017, by these septiles. As we can observe, students with better test results will generally earn more in their early career, have larger individual effects – which can not reflect higher education, due to the limited sample window –, and more importantly, end up with better quality firms. The latter observation holds true for both the indirect productivity measure of AKM firm effects, and the value added of firms as well. This latter pattern serves as another, more direct evidence for the presence of assortative matching if we accept that these test scores are indicative of future unobserved worker productivity.[37] Considering within-school relative test results, it seems that better students of the same cohort and school also tend to have higher wages, but a previously observable advantage of the top septile disappears, suggesting a role of between-school score differences in forming the score-wage relation. The same results for literacy test scores can be found in Appendix Figure A.1. Patterns are weaker in these plots, and while literacy scores seem correlated with wage outcomes, and firm productivity, they correlate with worker-specific wage components in a less monotonous way.

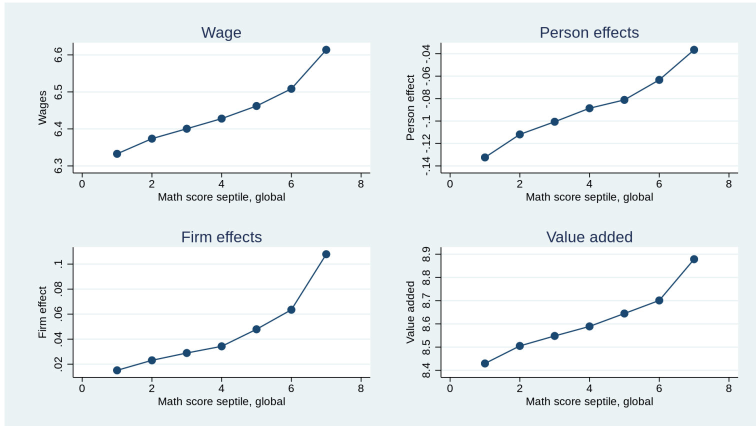
Finally, in Table 1.3 we include the correlations between continuous test scores and the introduced wage components, now including the unexplained part of firm effects as well. These correlations are shown for both the sub-sample of all young workers with test scores and those working at firms with available balance sheet data. Also school-year level observations are generated by taking the mean of above variables in such units. All correlations in the table are positive, although most may be considered modest in size, with the main exception being the systematic sorting of students with better test scores into higher wage occupations. For the samples

admission, therefore the effort put into preparation for the test may depend on student's general attitude besides their skills. For this very reason, those who are absent on the day of test do not have to retake it, so the data may bear a slight selectivity bias as well, besides not being a perfect measure of skills due to the lack of real stakes of the exam.

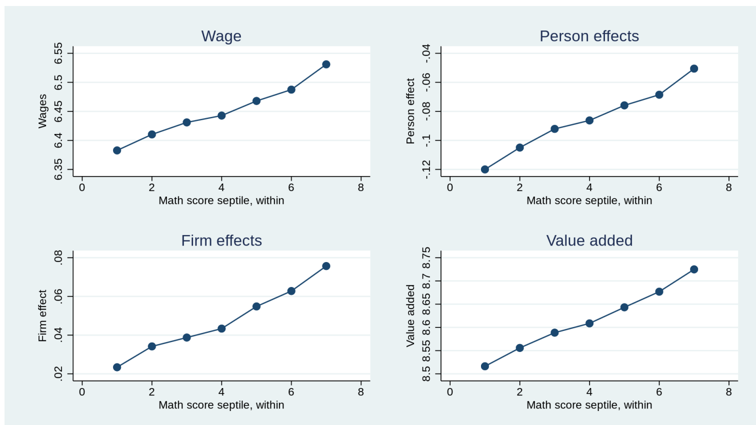
[35] Therefore, even those who choose a 3-year BA program, and don't work beside their university studies will not be part of the sample used for this exercise.

[36] A more direct assessment of the relation of these scores and future employment and (standard) wages can be found in Hermann et al. (2019).

[37] We have to note, however, that we may actually overestimate the importance of this sorting pattern, if the in-flow of productive workers increases firm-level productivity – which is a quite plausible scenario. To assess this possibility, the relation of worker composition and firm value added should be investigated in a dynamic setting, which is out of the scope of this study.



(a) Overall septiles



(b) Within-school septiles

Figure 1.2: Wage components and value added along 10^{th} grade mathematics score septiles from NABC

Notes: The seven quartiles are created along the distribution of literacy scores in year the students took the test (top panel), or within the distribution of the given school-year (bottom panel). The figures relate to those students for whom we have a test score observation no sooner than 2008 and also at least one wage observation anytime in the panel. The value added measure is available only for incorporated firms and not public institutions.

of incorporated firms, we generally find stronger correlations between test scores and firm quality, even regarding the non-sectoral firm component. Those workers who earn higher points on the NBAC test, tend to end up in firms with higher value added. This measure of assortative matching is of similar magnitude as the correlations between AKM effects in the full sample. Furthermore, we see that the correlation with the within-school relative score is substantially weaker, suggesting that the segregation of capable students at teen age indeed plays an important factor in creating sorting between high wage schools and firms (or occupations) reflected in correlations with the school-year level observations as well.[38]

Table 1.3: Correlation of NBAC scores and measures of productivity

	w_{ijt}	θ_i	ψ_j	ψ_j^U	VA_j	λ_k
All						
Math	0.222	0.147	0.125	0.132	.	0.279
Lit	0.165	0.056	0.079	0.115	.	0.316
Incorporated						
Math	0.241	0.164	0.156	0.142	0.173	0.307
Lit	0.191	0.080	0.137	0.126	0.133	0.298
Relative to school						
Math	0.116	0.115	0.069	0.050	.	0.079
Lit	0.063	0.025	0.037	0.039	.	0.093
Relative, Inc.						
Math	0.119	0.119	0.061	0.053	0.081	0.123
Lit	0.068	0.030	0.051	0.042	0.047	0.094
School level						
Math	0.280	0.115	0.133	0.426	.	0.246
Lit	0.261	0.084	0.100	0.233	.	0.478
School level, Inc.						
Math	0.344	0.123	0.183	0.274	0.272	0.487
Lit	0.326	0.094	0.147	0.257	0.247	0.542

Notes: sample correlations relate to around 1.02 million (0.78 million) employment observations of individuals and 9700 (9200) corresponding school-years, in the whole (incorporated) sample. θ_i, ψ_j and λ_k are the wage components from the main AKM estimations. ψ_j^U is the unexplained component of firm effects and VA_j refers to yearly firm value added.

1.4.3 Validity and robustness of main results

As Card et al. (2013) discusses, one form of misspecification of the AKM model would occur if worker mobility, and thus the design matrices of who works where, would depend on employer-employee match effects, and hence these match effects

[38]The topic of segregation with respect to future labor market prospects is a topic to be explored in more detail in future studies. Naturally, using a longer period of labor market outcomes would be desirable for this exercise, but the administrative data covering years after 2017 will not be available sooner than 2023.

could not be independent components of the error terms. To asses the relevance of match components, instead of the additive, separate worker and firm effects Card et al. (2013) estimate a model with worker-firm fixed effects to show that such a model only has a slightly larger explanatory power.[39] After estimating the same model, we also further decompose matches into separate worker, firm and (orthogonal) match effects to see, whether the importance weights of wage components in overall wage variation and sorting patterns are affected by applying such two-step model, as defined in Equations 1.16 and 1.17.

The results, presented in Appendix Table A.6, suggest that, while the residual variation in this model has decreased by around five percentage points, the firm-person match effects can contribute to a similar share of overall variance. The share of other components remain quite stable, with only the contribution of occupations showing a stronger decrease. This already suggests that matches only capture residual variation unrelated to the original AKM person and firm effects. In the first panel of Appendix Figure A.2 we plot the average of estimated match effects alongside the firm and individual effect deciles, and find patterns similar to that of the distribution of residuals from the original model (second panel). This plot also suggests that the included match effects were able to capture most of the residual variation which previously was coming mostly from the lowest deciles of workers and firms regarding their corresponding AKM effects. For these cells in the joint distribution, the mean residuals could be as large as 0.02-0.03 log points, indicating 2-3% average difference between predicted and actual wages for these worker-firm pairs. So, while it seems that in Hungary the match model would yield slightly superior explanatory power to the additive firm and person effect model, the assumption of exogenous mobility with respect to match effects may still hold.

We also reproduce the event study analysis presented in Card et al. (2013), investigating the wage evolution of job switchers before and after changing their employer, looking for signs of mobility depending on transitory wage-components. If the exogenous mobility assumptions of the AKM model holds, we expect to observe similar wage gains for those who move from one wage quartile to another as the losses expected for those who experience the reverse path of mobility – and no wage gains for those who remain at similar quality firms. On the other hand, no trends should be present in wages either before or after the job-switches. Appendix Figure A.3 presents the mobility patterns for four wage quartiles, based on AKM firm effects, in the preceding and subsequent six months of job switches. The presented wage profiles are mostly consistent with these expectations, showing only signs of slight wage gains over time for workers leaving the bottom quartile of firms.

A main contribution of Card et al. (2013) and Song et al. (2019) is presenting the evolution of the wage decompositions over time, characterizing the sources of increase in wage inequality. As we could estimate the AKM model on the whole 15-year period, we first report the decompositions on three overlapping subsamples from the overall estimation in Table 1.4. However, we also allow firm, individual,

[39]This (and the consecutive tests) later also appear in Card et al. (2016), Card et al. (2018), Gerard et al. (2021), and also in Macis and Schivardi (2016), Fanfani (2018), Alvarez et al. (2018) and Casarico and Lattanzio (2019).

and occupation effects to be different in the three periods by re-estimating the AKM models on these three, shorter time periods. This may be reasonable, as assuming time-invariant firm-effects may be a source of misspecification in long panels if firms can alter their wage schemes either due to the sharing of rents from productivity changes, introducing amenities or disamenities or applying specific contracting strategies. The comparison of the two set of results also provides a way to assess how severe threat the limited mobility bias is when one has to rely on data from shorter panels – a possible drawback of using subsamples.

As the first three columns of Table 1.4 suggest, in Hungary overall wage dispersion did not change substantially during the 2003-2017 period, as only a slight decrease of variance is present. Accordingly, alongside the overall variation, the contribution of most wage components also remain stable over these three periods. However, the last period is slightly different, as within that period wages increase more rapidly, increasing the contribution of year effects.There is a slight increase in the total of between-firm inequality components, consistent with the comparative study of Tomaskovic-Devey et al. (2020), who show increasing trends in the between share for twelve of the fourteen countries included, Hungary being one of the two exceptions with a steady trend (of one of the highest between-firm share) during the 2003-2011 period. On the other hand, unexplained variation is quite higher in the years following 2010.[40] Compared to the decompositions from the full period AKM estimation, in the models estimated on subsamples (columns 4-6) we achieve around 3 percentage better fit in all three periods. Surprisingly, despite the concern that a shorter panel comes with more serious limited mobility bias, the estimated share of sorting and the corresponding correlations of AKM effects are not lower. On the contrary, we estimate slightly larger sorting parameters. [41]

[40]This may relate to changes in how social contributions had to be reported in the primary of source of data. Since 2011, monthly reports are required from all employers, while previously most employers had the option to report payments to workers only once per year.

[41]As Bonhomme et al. (2020) find evidence for non-negligible limited mobility bias using six years of data per country, the robustness of our results are indeed surprising. This stability may suggest, that the inclusion of within-year mobility or simply the average mobility level of the Hungarian labor market is substantially high to overcome severe limited mobility bias. Nevertheless, only applying a bias correction method would provide a clear verdict on this issue.

Table 1.4: Decomposition of wage variance, over time

Estimation sample	Full	Full	Full	Sub	Sub	Sub
Reporting sample						
From	2003	2007	2011	2003	2007	2011
Until	2009	2013	2017	2009	2013	2017
Variance of log wages	0.348	0.331	0.325	0.346	0.331	0.322
Ensemble decomp. (%)						
Contribution of XB	4.47	3.56	5.88	4.69	3.29	5.77
— Year	0.30	0.25	3.20	0.50	0.30	3.12
— age*, firm size, contract, tenure*	4.16	3.31	2.68	4.15	2.54	1.95
Contribution of individual heterogeneity	53.47	51.67	46.21	54.87	58.18	52.65
— Unobserved individual heterogeneity	31.71	30.11	26.20	31.60	34.45	32.59
— Observed individual (gender, quasi ed.)	17.77	17.95	17.37	18.53	19.88	17.65
— Birth year	0.86	0.51	-0.01	1.32	0.39	-0.06
— Region	3.12	3.10	2.65	3.42	3.45	2.47
Contribution of firm heterogeneity	20.96	22.88	23.22	23.11	21.39	20.10
— Unobserved firm heterogeneity	15.78	15.60	15.53	16.46	14.90	13.97
— Observed firm heterogeneity (ownership)	3.54	4.40	4.67	4.37	3.55	3.65
— Sector	1.64	2.88	3.02	2.28	2.93	2.48
Contribution of occupations	8.37	8.04	7.41	7.97	6.16	6.72
Residual variation	12.74	13.85	17.28	9.36	10.98	14.76
Correlations						
Corr(θ_i,ψ_j)	0.173	0.186	0.173	0.164	0.152	0.181
Corr(ε_i^I,ε_j^J)	0.16	0.154	0.109	0.098	0.116	0.12
Corr(θ_i,ψ_j) for inc. firms	0.311	0.33	0.305	0.286	0.308	0.308
Corr(θ_i,VA_j) for inc. firms	0.382	0.382	0.351	0.357	0.399	0.386
Corr(ψ_j,VA_j) for inc. firms	0.618	0.626	0.609	0.61	0.597	0.576
Between-within decomposition (%)						
Between-firm share	46.3	48.1	48.8	45.9	47.6	48.7
— Ind. segregation	11.1	11.5	11.5	12.3	13.3	13.8
— $Var(\psi_j)$	17.4	18.9	19.4	17.0	17.2	16.4
— Sorting	9.1	9.9	9.5	9.4	10.3	10.3
Number of Observations (1000)	30885	30973	30869	28373	28531	28484
Number of Firms (1000)	77	86	94	66	74	83
Number of Workers (1000)	1923	1966	1932	1789	1831	1806

Notes: See Table 1.1. The first three columns report decompositions of the AKM model estimated using all years, on different subsamples. The last three columns report decompositions on models re-estimated on the corresponding subsamples.

We also present our results for various subsamples of interest. Table 1.5 comprises results for workers employed through typical labor contracts – that is we exclude public servants from the sample –, male workers, and for workers who were below the age of 27 during the whole sample period. Important differences compared to the full sample include a stronger role of firm heterogeneity in all subsamples. Sorting is also somewhat stronger for males and those who work with typical contracts. For the youth sample, however, we find a lower explanatory power of the AKM model, mainly coming from lower contributions of individual differences, especially unobserved individual heterogeneity. As the overall variation is also lower in this sample, this reflects substantially lower variation in person effects. This may signal that career paths do not diverge enough by this age to allow individual differences alter wages substantially. The contribution of sorting, on the other hand, remains strong, despite a low estimated correlation.[42]

In this table, we also divide the sample based on our proxy of education – that is based on the highest educational requirement met by the individual in any of their observed occupations. Naturally, the role of occupations explain less variation within each of the three categories, with the largest differences being among occupation requiring higher education. In this sample overall dispersion and the contribution of unobserved worker quality is also higher, suggesting a more substantial role of soft skills for educated workers.[43] Accordingly, among workers who never work in occupations with strong educational requirements both observed and unobserved aspects of firm heterogeneity contribute to larger shares of the (within-occupation) variation. The unexplained, residual shares on these subsamples are lower, as one of the main drivers of overall variation is educational attainment in itself. Interestingly, within the educational subsamples correlations between worker and firm effects are somewhat larger than in the full sample, reinforcing that education also plays an important role in allocating workers to different sets of firms, as Table 1.2 suggested beforehand.

[42] The decrease is partially in line with Torres et al. (2018), who find negative sorting for workers below the age of 26.

[43] And therefore partially explaining the lower role of individual heterogeneity in the sample of young workers, of whom only a smaller share works already in occupations with graduate requirements.

Table 1.5: Decomposition of wage variance, subsamples

Sample	Typical.	Male	Young(≤26)	Low ed.	Mid ed.	High ed.
Variance of log wages	0.35	0.369	0.194	0.198	0.218	0.348
Ensemble decomp. (%)						
Contribution of XB	5.18	5.84	13.75	5.75	6.97	3.83
— Year	2.07	1.86	9.83	3.52	3.11	1.75
— age*, firm size, contract, tenure*	3.11	3.98	3.92	2.23	3.86	2.08
Contribution of individual heterogeneity	46.60	47.07	27.58	38.86	37.87	48.82
— Unobserved individual heterogeneity	28.02	28.31	16.45	33.34	33.49	43.21
— Observed individual (gender, quasi ed.)	15.87	15.67	7.82	4.35	1.14	1.77
— Birth year	-0.28	0.27	1.53	-0.69	0.67	1.41
— Region	2.98	2.81	1.77	1.85	2.56	2.43
Contribution of firm heterogeneity	27.97	26.27	34.52	34.61	31.96	23.91
— Unobserved firm heterogeneity	18.47	18.22	22.10	21.40	21.01	15.07
— Observed firm heterogeneity (ownership)	5.98	5.56	8.24	6.74	6.50	4.86
— Sector	3.51	2.49	4.17	6.46	4.45	3.98
Contribution of occupations	6.88	7.84	3.79	3.40	2.63	5.53
Residual variation	13.37	12.97	20.37	17.38	20.57	17.91
Correlations						
Corr(θ_i,ψ_j))	0.291	0.244	0.027	0.238	0.193	0.206
Corr(ε_i^I,ε_j^J)	0.157	0.158	-0.085	0.141	0.133	0.148
Corr(θ_i,ψ_j) for inc. firms	0.31	0.322	-0.026	0.219	0.235	0.337
Corr(θ_i,VA_j) for inc. firms	0.364	0.356	0.116	0.276	0.309	0.356
Corr(ψ_j,VA_j) for inc. firms	0.615	0.582	0.572	0.563	0.593	0.643
Between-within decomposition (%)						
Between-firm share	53.8	49.0	77.5	63.0	66.4	46.1
— Ind. segregation	11.2	10.0	14.9	12.7	14.0	11.3
— $Var(\psi_j)$	18.8	18.8	31.9	28.0	26.6	20.2
— Sorting	13.9	10.6	12.9	9.7	12.7	11.5
Number of Observations (1000)	50956	32157	1971	8243	35797	22115
Number of Firms (1000)	138	128	71	64	133	104
Number of Workers (1000)	2332	1225	306	389	1378	695

Notes: See Table 1.1. Subsamples are formed based on contract type, gender, age, and proxied education. Due to an inconsistency in the recording of contract types in the original data source, in the years 2010 and 2011 most of public workers and public servants are coded as having typical contracts. This will be amended in a new iteration of the dataset we have access to in the near future. Also military and police personnel are now excluded from the main estimations.

To check for patterns in sorting within different occupations, regions or employers, we also estimated the correlation of individual and firm effects and the contribution of sorting to wage variation in the given segment for 1-digit, broad categories of occupations, regions, and industries, as presented in Appendix Table

A.7.[44] Similarly to Torres et al. (2018) and Dauth et al. (2019) we find larger assortative matching for the capital, Budapest and Central Hungary, the NUTS-2 region it is embedded in. In line with Table 1.5, sorting is stronger in superior occupations, but the relation is non-monotonous. Sorting is absent for agricultural jobs and firms, and is the strongest in supporting sectors, logistics, transportation and energetic sectors. When collapsed to the given categories by year, cross-industry and cross-region correlations of mean firm and worker effects increase substantially, suggesting a strong role of the average difference across these units in sorting patterns, again in line with the findings from Table 1.2. Also these patterns are consistent with the notion that a large portion of the universities and jobs demanding professional qualifications in Hungary are centered in Budapest.

Finally, to relate to the "job-title" model of Torres et al. (2018), we tested whether a model with occupation-sector combined fixed effects would fit the model better than our main specification with only occupation fixed effects. These interacted effects would allow for different average wage levels for workers in a given occupation across different sectors, for instance allowing secretaries at IT firms and at industrial firms to receive different premia, a plausible assumption. The decomposition – including a second-stage decomposition of the combined effects – based on such model is presented in Appendix Table A.8. As we can infer from this table, 95% of occupational and sectoral differences can be attributed to separate, additive wage effects of occupations and sectors, with only 5% of the variation coming from the joint effects, indicating a rather low overall role of the above inter-sectoral differences in the valuation of occupations.

1.5 Results II. – Gaps and sorting along observables

1.5.1 Wage-gap decompositions

While the previous section of the result focused on presenting evidence for the overall presence of wage (and productivity) sorting in the Hungarian labor market, in this section we aim to uncover some of the channels in which the positive correlation between AKM person and firm effects could be generated. Specifically we focus on sorting along observable characteristics of both workers and employers, such as gender or education of individuals, the ownership of firms. First, we decompose some specific wage gaps into person, firm and occupation components, according to the main AKM model, building on Cardoso et al. (2016). Later on, we will refine and complement this set of result by models using a grouped AKM approach and a decomposition inspired by Card et al. (2016). For the initial approach, we use the specifications defined in Equations 1.14 and 1.15. That is, besides characterizing differences in occupation, firm and person effects, we further distinguish between observable and unobservable components, and also within-unit and between-unit (segregational) aspects. Table 1.6 contains results for the former decomposition, concerning individual characteristics.

[44] For these estimations we use data only after 2011, as the occupation categorization system in effect changed that year, hence the categorization presented in Appendix C could not be assigned unambiguously to these broad categories.

Table 1.6: Person characteristics

Difference in:	(1) Wage	(2) Individual	(3) Firm	(4) Occupation
Male	0.160***	0.131***	0.031***	-0.002*
	(0.007)	(0.004)	(0.004)	(0.001)
Non-skilled	-0.138***	-0.109***	-0.008	-0.021***
	(0.012)	(0.006)	(0.005)	(0.002)
Higher education	0.541***	0.389***	0.045***	0.107***
	(0.007)	(0.004)	(0.004)	(0.001)
Lives in Budapest	0.176***	0.111***	0.057***	0.008***
	(0.025)	(0.011)	(0.014)	(0.001)
Observations	66,155,127	66,155,127	66,155,127	66,155,127
R-squared	0.316	0.420	0.201	0.497
Difference in:	(3a) Observed firm	(3b) unobserved firm	(2a) Within firm	(2b) Between firm
Male	0.012***	0.019***	0.114***	0.017***
	(0.003)	(0.003)	(0.003)	(0.003)
Non-skilled	0.001	-0.009*	-0.107***	-0.002
	(0.003)	(0.004)	(0.006)	(0.003)
Higher education	0.005*	0.039***	0.353***	0.036***
	(0.002)	(0.003)	(0.005)	(0.003)
Lives in Budapest	0.010	0.047***	0.060***	0.051***
	(0.009)	(0.007)	(0.004)	(0.009)
Observations	66,155,127	66,155,127	66,155,127	66,155,127
R-squared	0.309	0.045	0.532	0.146

Notes: The parameters in the table are results from regression estimates of the effect of gender, education and residence (Budapest) dummies on wage components defined in Equations 1.11 (first panel) and 1.14 (bottom panel) as outcomes. The elements of X and W are included as additional controls. Such variables include quadratic age, quadratic tenure, firm size, year, contract type and birth cohort. Two-way clustered standard errors are in parentheses, with *** p<0.001, ** p<0.01, * p<0.05.

While naturally the individual component dominates the gender wage gap, there is also around 3% wage difference attributable of males sorting into better firms. Of this component, about one third is explained by sector or ownership, while almost 2% is due to intra-industry differences in firm premia. This may reflect exclusionary hiring, but also different preference of male and female workers for firms with different amenities (Sorkin, 2018). Somewhat surprisingly, we don't find an important role of sorting into different occupations. That does not mean, per se, that men and women work hold similar positions, but they are quite equally represented in low and high wage occupations. Differences of the individual wage component can also be generated both within-firms, or across firms. By including firm fixed effects in the regressions on estimated AKM worker effects, we can learn that the within firm wage gap, which is often the focus of studies on the gender wage gap studies, is actually 1.7% lower than what the decomposition of the first panel would suggest, due to segregation effects. For instance, either higher skill males or lower skill women may tend to work in more sex-segregated workplaces, resulting in a lower gap identified only from sex-integrated ones. Hence, alongside sorting into different premia firm, the non-random allocation of males and females of different (unobserved) skills into different firms, with respect to the quality of workforce they employ. will contribute – with a rather important magnitude – to the between-firm wage advantage of men.

Regarding education, we do not find really surprising patterns. The occupations filled by higher (proxied) educated groups are (even by definition) more prestigious occupations, paying higher wages. But as it turns out more educated workers can also get into better firms, even within the same sectors, as it is suggested by the dominance of the unobserved firm component.[45] Finally, the within-firm differences dominate the gap in individual effects, with only high skilled workers showing signs of segregation compared to baseline category. Still, this segregation component is almost as important for between firm differences, as the sorting of workers with high education into higher wage firms.[46]

Finally, we'd like to understand why do people who live in the capital earn almost 20% more than workers living outside Budapest.[47] While certain jobs may be over or underrepresented in the capital, Budapestian workers only work in slightly better occupations on average. However, the firms Budapestians work for are considerably better, and mostly not because different ownership or sector, but due to unobserved, within-sector differences. We note that as we do not have establishment level data these firms may include cross-country chains as well. A surprising find is that Budapest residents earn higher wages for other reasons as well, both within, both between firms. The within channel may happen due to better skills, or national employers simply having to offer higher wages for their establishments in Budapest. Naturally a quite strong segregation component is present due to the spatial nature of our question, and the somewhat superior skills of those who (can) live in Budapest.

[45] The findings are similar to the figures presented by Cardoso et al. (2018) for Portugal, with the highest education category differing the most from the first two. Although we lack precise educational data, except for the youngest cohorts, investigating wage differences in more detail could be possible in the future , relying on information on exact education, on peers at school, and also utilizing the test scores presented earlier in this paper.

[46] The way we proxy education may elicit some bias on these estimations. Namely, as we may never observe some individuals with higher education actually working in occupations requiring this qualification, individuals could be misclassified as belonging in lower educational categories. If individuals with more human capital (intentionally) work in lower wage positions, we may underestimate the returns to higher education, for instance.

[47] The dummy for Budapest relates only to workers who live within the administrative borders of the city of Budapest for most of our 15-year observation window. As commuters living in the agglomeration are not included in this definition, the gaps presented here are probably somewhat underestimate the gap we would get by focusing on those who actually work in Budapest.

Table 1.7: Ownership gaps decomposed

Difference:	(1) Wage	(2) Individual	(3) Firm	(4) Occupation
Foreign-owned firm	0.385***	0.120***	0.264***	0.002
	(0.023)	(0.011)	(0.012)	(0.003)
State-owned firm	0.105*	0.042**	0.061*	0.003
	(0.042)	(0.015)	(0.024)	(0.007)
Public institution	0.014	0.051***	-0.064***	0.027***
	(0.022)	(0.010)	(0.010)	(0.003)
Observations	66,155,127	66,155,127	66,155,127	66,155,127
R-squared	0.121	0.105	0.366	0.177
Difference:	(2a) Observed individual	(2b) Unobserved individual	(3a) Within individual	(3b) Between individual
Foreign-owned firm	0.037***	0.083***	0.166***	0.097***
	(0.006)	(0.008)	(0.006)	(0.010)
State-owned firm	0.011	0.031**	0.038***	0.023
	(0.010)	(0.010)	(0.008)	(0.021)
Public institution	0.039***	0.012*	-0.029***	-0.035***
	(0.006)	(0.006)	(0.005)	(0.008)
Observations	66,155,127	66,155,127	66,155,127	66,155,127
R-squared	0.070	0.072	0.835	0.136

Notes: The parameters in the table are results from regression estimates of the effect of majority ownership dummies on wage components defined in Equations 1.11 (first panel) and 1.15 (bottom panel) as outcomes. The benchmark category consists of domestic, private-owned firms. The elements of X and are included as additional controls. Such variables include quadratic age, quadratic tenure, firm size, year and contract type. Two-way clustered standard errors are in parentheses, with *** $p<0.001$, ** $p<0.01$, * $p<0.05$.

Considering the ownership of firms, in Table 1.7 we can see that firms with either domestic, foreign, private or state majority ownership roughly employ the same mixture of occupations, at least considering wage levels. However, compare to these market firms, which accounts for the main body of the economy, public institutions generally make use of higher paying occupations. This is probably due to many of the skill dependent occupations and the lower share of elementary/manual work present in schools or hospitals for instance.[48] Consistent with this notion,

[48]For estimating this table we did not include industry dummies among the controls, as for public institutions that variable is mostly non-available, and also as many industries are mostly exclusive for the either the private or the public sector, e.g. healthcare, education or agriculture and industry. Appendix Table A.9 contains the replication of Table 1.7 on a sample excluding public institutions, but including industry controls. While within-sectors the advantage of state-

individuals with better observed skills (higher education) sort into these institutions. While foreign owned firms can also employ a more educated workforce, they can also poach the best workers regarding unobserved characteristics as well, substantially increasing the difference in AKM worker effects. This channel in itself can explain around a quarter of the foreign-domestic wage gap.[49] As these firms pay also higher premia even without these selection/sorting channels, they clearly contribute to overall wage sorting substantially. The strong segregation component, which accounts for 40% of the total difference in the firm component can be interpreted in this setting as a result of the differing work histories of workers who never work for multinationals versus those who tend do, with the latter group mostly getting into higher wage firms. Even within the lifetime of individuals who work in both the foreign and domestic sectors, working in the former usually includes firms with 17% higher premia. The distinction between state owned and private owned firms is not that harsh, although somewhat better wages can be also earned at these firms, by somewhat better workers in the former category.

As we have seen, the sorting of males, highly educated workers and even residents of Budapest , into high wage firms, as these workers would earn higher wages anywhere, clearly contribute to the overall wage sorting observed in Section 1.4. Similarly, while foreign-owned firms tend to pay higher wages, they are also able to poach the best workers in the labor market, strengthening the correlation between worker and firm productivity and corresponding wage levels. These findings are consistent with Table 1.2, which suggested that half of the observed wage sorting could be attributable to observable characteristics. In the remaining section of the study, we aim to further assess the level of observable sorting mechanisms, with more flexible model specifications.

1.5.2 Firm-group (G-AKM) specifications

In this section, we present some alternative - partly novel - specifications of the AKM model. We will use these models to assess sorting, but beforehand, we also discuss whether they provide reasonable alternatives to the standard AKM specification. In each experiment, we relax the assumption that firms have one (relative) wage premia that they pay for all they workers in all time periods. Building upon Equations 1.18 and 1.19, we first generate firm-group identifiers, then estimate our three-way AKM model with these identifiers as substitutes for the original firms. We will apply this method for six variables: calendar year, gender, education, occupation, tenure categories and age categories. Due to the different nature of these variables, the connected sets on which the AKM models can be estimated and interpreted can differ substantially.[50] The results of all six models are presented

owned firms turns into a slight disadvantage, and foreign owned firms seem to employ more workers in high-wage occupations, the main conclusions drawn about the sign of sorting are not affected.

[49] The importance of multi-national employers in Hungary is discussed in detail in Earle et al. (2018) and Köllő et al. (2021).

[50] Specifically, we define the connected sets based on the firm-group identifiers, person IDs and occupations according to the algorithm of Weeks and Williams (1964), except for the models with gender and education as interacting variables. We will discuss the identification challenge in these models at a later point.

in Table 1.8. Besides the residual variation of models, an important new row in this table compared to Table 1.1 is the '*Interacting Variable*' one. This row refers to the wage variation explained by the variance of ε_{jg}^{G} from 1.19, signalling how important is the assumption that firm effects are flexible along the given observable characteristic.[51]

Time-invariance assumptions

As we noted when discussing the evolution of wage inequality, the assumption that firms have the same firm-effect over a longer period may be too restrictive, as wage policies of any firm or even whole sectors could alter during a decade, for instance. In the most extreme case, we can assume that firms may change their wage schemes in any year. The AKM model could be altered to allow for such flexibility, by including firm-year interaction effects instead of the time-invariant firm-effects. With such a model we may lose efficiency as we have a magnitude larger set of extra parameters to identify, with the year-to-year changes in firm-effects being identified mostly from the wage variation of workers staying in the given firms, and partially from the inter-firm mobility in the given year.[52] While Macis and Schivardi (2016) already use firm-year effects, Lachowska et al. (2020) proposes the detailed investigation of this alternative model, naming it time-varying or TV-AKM, which we adapt as well. In their study, the authors conclude that this misspecification issue is a 'second order concern' in the AKM framework, as allowing for time-varying firm effects does not significantly alter results on wage decompositions, neither on inference relying on AKM firm effects.[53]

[51] For instance, if the gender gap would be the same in all firms a gender dummy and firm effects could perfectly predict firm-gender effects, resulting in near zero residuals in the second stage, and hence only a negligible portion of variance explained by this term.

[52] The connected set remains roughly the same as in the main model, as only those firm-year units become disconnected, where the whole workforce is replaced from the end of one year to the start of the next.

[53] As Lachowska et al. (2020) note, one possible reason for the emergence of time-varying firm effects would be the rent-sharing of firms going through productivity changes as presented in Lamadon et al. (2022). Previously, Macis and Schivardi (2016) also interprets the firm-year AKM effects as measures of the firms' rent sharing behaviour. Accordingly these effects may be used for a within-firm approach of estimating rent-sharing elasticities – a question to be considered in Chapter 2 of this thesis.

Table 1.8: Decomposition of wage variance, interacted effects

Interacting variable in the G-AKM	year	gender	educ.	occup.	tenure	age
Sub-units within firms (max. number)	15	2	3	7	4	4
Variance of log wages	0.334	0.339	0.339	0.332	0.33	0.337
Ensemble decomp. (%)						
Contribution of XB	7.2	10.9	10.8	10.6	10.3	9.2
Contribution of individual heterogeneity	48.8	48.5	47.9	45.0	52.7	50.8
— Unobserved individual heterogeneity	28.1	28.8	28.0	27.3	31.2	29.2
— Observed individual (educ,gender,birth,res.)	20.7	19.8	19.9	17.7	21.5	21.6
Contribution of firm-group heterogeneity	26.1	23.7	24.9	35.6	21.7	22.3
— Interacting variable (see at bottom)	2.0	0.1	1.1	8.6	0.9	0.2
— Unexplained	2.1	0.5	1.0	2.2	0.4	0.3
— Firm constant	22.1	23.1	22.8	24.8	20.4	21.8
— — Unobserved firm heterogeneity	15.6	16.2	16.0	17.4	14.4	15.5
— — Observed firm heterogeneity (sector/own.)	6.4	6.9	6.8	7.4	6.0	6.3
Contribution of occupations	8.6	7.7	7.4		6.7	8.1
Residual variation	12.9	14.6	14.4	14.1	13.8	14.2
Correlations (and contr. to overall)						
Corr(θ_i,ψ_{jt})	0.143	0.177	0.19	0.2	0.119	0.145
Corr(ε_i^I,ε_{jt}^J)	0.113	0.124	0.152	0.141	0.131	0.151
Explained by 2Cov(θ_i,ψ_{jt}) (%)	8.4	9.5	9.8	10.2	6.7	8.0
Explained by 2Cov(ε_i^I,ε_{jt}^J) (%)	4.2	4.1	4.3	4.4	3.6	4.3
Between-within decomposition						
Between-firm share	47.6	46.9	47.2	47.8	47.2	47.4
— Ind. segregation	10.9	10.3	10.2	8.9	13.9	12.0
— Var(ψ_j)	20.3	18.8	18.2	19.2	18.5	18.6
— Sorting	8.7	9.6	9.8	10.2	6.7	8.0
Sample						
Number of Observations (1000)	62441	69982	69304	71064	41899	63964
Number of Firms (1000)	141	142	144	163	108	142
Number of Workers (1000)	2387	2584	2554	2620	2074	2368

Notes: See Table 1.1. The first stages are estimated according to Equation 1.18, and then decomposed according to Equation 1.19.

Indeed, relaxing the assumptions of the AKM model in our setting provided a model fit better only by less than two percentage points. By regressing firm-year joint effects on separate firm and year effects, we can observe that the separate components, mostly the firm effects, explain 92% of the variation in the joint parameters. The remaining 8% accounts for time variation of wage premia per year within firms. So, although conclusions regarding wage sorting remain mostly unaffected, an argument could be made that we may have a drift in at least part of the firm effects. If that is the case, the rolling-AKM approach, such as the one implemented in Table 1.4 may provide a better characterization of the labor market

– given that the limited mobility bias from using a shorter panel is corrected for.

Gender and education

A specification linking the standard AKM and the previously presented match model, in which all individual-firm match can have a different wage component, could be defined by assuming that firm-effects are heterogeneous across different groups of individuals, based either on exogenous (gender) or endogenous (educational level) person characteristics. However, the estimation of such models is not straightforward. As these variables are time-invariant for individuals in our data, there is not any mobility between the two or three distinct sets of firm-gender or firm-education units. Accordingly, we will have two or three, non-overlapping components in the mobility network, and as one normalizing condition is required in all connected components, female and male firm effects (or those of different educational groups) will not necessarily be measured on the same scale, rendering them incomparable. Hence, we follow Card et al. (2016) and re-scale the estimated effects along the assumption that the least productive firms achieve no rents, and hence compensate their workers independent of their characteristics. [54] Appendix Figure A.4 illustrates the relationship between the re-scaled effects and productivity, suggesting different rent-sharing elasticities for individuals of different gender and education.[55] While the importance of this approach regarding differential rent-sharing is evident from this graph, Table 1.8 suggests that the model fares only slightly better than the standard AKM in explaining overall wage variation. While the explained share of observed individual heterogeneity decreases, firm-group heterogeneity increases with roughly the same extent. The component which could not be explained previously by orthogonal gender, education and firm effects accounts for 0.5% or 1% of the overall wage distribution.

Jobs as unit of interest

The main specification in our paper, and previously the findings of Torres et al. (2018), already highlighted that besides who works for whom, what people work is also an important factor in explaining wage variation. A remaining question to answer is whether it matters where someone does what she does. In a previous model (Appendix Table A.8), we already assumed that working in the same occupation may be rewarded differently in different industries. As a generalization of this concept, we could also assume that the work done in a given occupation differs not only across sectors, but in every firm. Some studies in wage inequality, for instance Petersen and Morgan (1995) and Petersen et al. (1997) or Avent-Holt et al. (2020) already treat 'jobs' – defined as firm-occupation interacted categories – as a relevant level of investigating wage inequalities, albeit without controlling for

[54] Specifically, by fitting a kinked function on our data, we also identify a set of firms for whom the increase of productivity, measured by value added per worker is not reflected in an increase of AKM firm effects. Then, assuming that firm effects of gender or education groups should be equal in these 'no surplus' firms, we normalize firm effects across groups, so that their average will be the same for this set of firms.

[55] These differences are discussed in detail in the second chapter of this thesis on differential rent-sharing.

unobserved individual heterogeneity. In the AKM setting, this approach relaxes the assumption of having the same wage premia for all different jobs within the firm, in a similar fashion as one can allow for different premia based on gender or race. We provide the first results from estimating this specification with both person and job effects, the latter being defined by firm times occupation (seven, 1-digit categories) interaction fixed effects, revealing that firm premia indeed varies between different groups of workers within the same firm.[56] Of the variance of the estimated joint (job) effects, more than 6% originates from sources other than the baseline difference of wage levels across occupations or firms.[57] Of the overall wage variation, this can explain 2.2%, while the magnitude of person, firm and occupation effects remain roughly the same as in the main estimations. The possible applications of this design includes, for instance, the investigation of differential rent-sharing and bargaining across occupations and the detailed assessment of the gender wage gap and differential sorting of women across occupations.

Seniority of workers

Before discussing the final set of specifications, we note that with the TV-AKM, the match model, and the job model, we have derived three main alternative parametrization of the four fixed effects in Equation 1.1, combining firms with time, persons and occupations respectively. Along these four dimensions, higher order combinations of fixed effects are also possible. For instance, one could define gender-firm-occupation interactions for assessing specific problems.Finally, one could also combine firm effects with the elements of time-varying characteristics, X. In this exercise, we divide firms into sub-units based on the seniority of its current workers.[58]

First, we define four groups based on completed tenure at the firm, with the categories consisting of those with less than one, three or five years of tenure, and those who had spent more than 60 months already at the given firm – although not necessarily in the same job. Accordingly, in this setup we can only use the last ten years of data, as the completed tenure category can not be defined properly for preceding observations. In a second model we use the age of the workers to form four categories, the cutoffs being at 26, 41 and 56 years. The results, presented in the last two columns of Table 1.8 suggest a very limited role of between-firm differences in how firms treat their workers of different seniority, as firm-tenure interactions and firm-age interaction can both explain less than 0.5% of overall wage variation.

[56] Identification of the job cell effects relies both on between-firm mobility and within-firm promotions or re-specializations. Also, as people can move between different occupations, we don't face a scaling problem as before, and estimated firm effects are directly comparable within the largest connected set.

[57] An alternative specification, with 37 distinct (2-digit) occupation categories yielded generally similar results, with 9% of the job effects attributable to the second stage residuals.

[58] The gender-firm, education-firm and occupation-sector models could be considered as special cases, in which one variable of the observable part of the person or firm characteristic is removed from its corresponding fixed effect, and is interacted with one of the high-dimensional variables of the first-stage AKM equation.

1.5.3 Bargaining and sorting in G-AKM

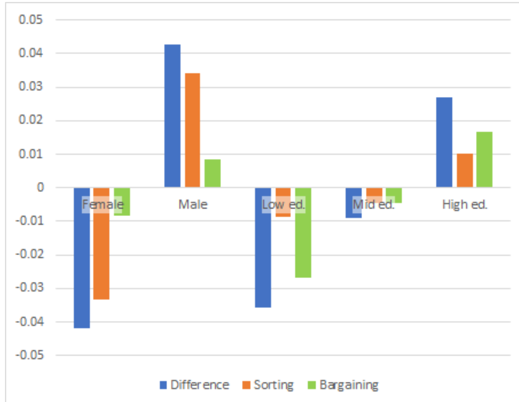
Finally, we present a simple application of the previously introduced grouped AKM specifications, building on Card et al. (2016). Namely, we will use the alternative estimation of the bargaining-sorting decomposition of firm effects, we proposed in Equations 1.18 through 1.20. We will present the average differences in Ψ_{jg} and ψ_j, the sorting effect, alongside the bargaining effect β_g, using the fact that this parameter from Equation 1.20 can be rewritten as

$$\tilde{\beta}_g = \frac{\partial(\Psi_{jg} - \psi_j)}{\partial G} \tag{1.21}$$

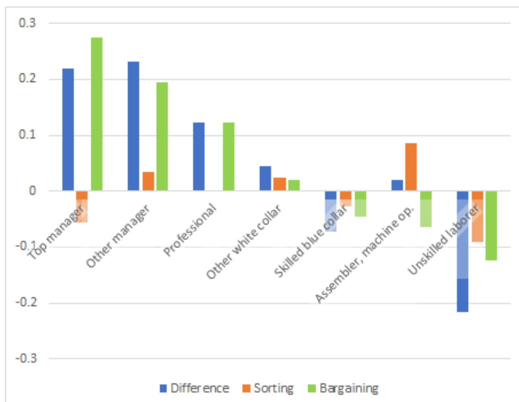
We present the results graphically in Figure 1.3, with bars representing deviations from the sample-level mean firm-group effects (scaled to zero), instead of arbitrarily chosen reference categories. Hence, to directly compare any two groups, the difference between their respective firm effect components should be considered.[59] The graphs clearly suggest that sorting can quite substantially form wage-level patterns across different observed characteristics. Similarly as Card et al. (2016) – and later by Casarico and Lattanzio (2019) and Lamadon et al. (2022) (in their Appendix) –, the difference between average male and female firm effects are largely attributable to the sorting channel, with bargaining comprising only around 20% of the overall difference. Regarding the educational attainment of individuals, we observe a much weaker role, as most of the wage differences are generated within and not across firms, like Table 1.6 also suggested earlier. The same holds also for broad job categories within firms, regarding which, sorting can even counteract the within firm differences. For instance, while assemblers and machine operators are slightly underpaid in their firms, they usually work at employers with the highest wage premia (such as multinational car manufacturers), leading to an average lower job effect than – more skilled – blue-collar workers. Sorting patterns with regards to tenure probably reflect high wage firms having lower turnover rates. Therefore those with long employment history in a given firm earn more not only because the within-firm wage-path (reflected in the increasing bargaining component), but because such spells occur more probably in higher premia firms. Finally, while the standard age-earnings profile are reflected in the overall and bargaining components, it seems that younger workers sort more frequently into high wage firms. [60].

[59]We also note, that these parameters are not controlled for other X variables, so are not directly comparable to Table 1.6. Appendix B reflects on this issue in the context of gender differences. The minor differences between Appendix Table A.10 and Figure 1.3 are due to the slightly different sub-sample used, as the Appendix exercise is constrained to the dual-connected sample (for comparative reasons), whereas here we include all firms for which firm-gender effects are identified, even if they are gender-segregated.

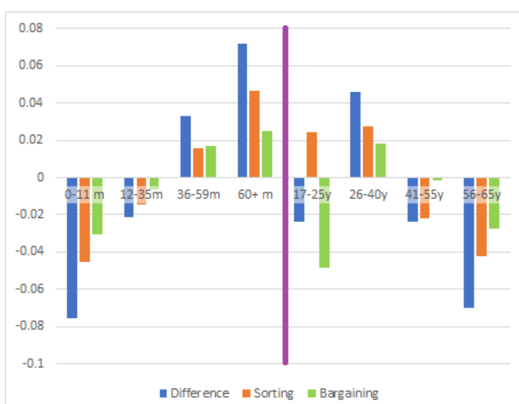
[60]Whether this initial advantage is driven by supply or demand-side factor, and whether this pattern have emerged only during recent years could be in the focus of future research.



(a) Gender and (proxied) education



(b) Occupations



(c) Completed tenure and age

Figure 1.3: Bargaining and sorting effects based on G-AKM models

Notes: The bars represent the mean values of Ψ_{jg} (*difference*), $\tilde{\psi}_j$ (*sorting*) and ε^G_{jg} (*bargaining*) from Equation 1.19 across the given categories. The firm effects with gender or education interactions are normalized to have zero as their mean in the full sample. On the third sub-figure the first four column sets refer to completed tenure at the firm (measured in months), while the second set refers to the age of workers (measured in years).

1.6 Discussion

Beside providing evidence on a substantial level of wage sorting in Hungary, throughout the different exercises presented in this paper, we were able to identify some of the observable channels in which wage sorting could emerge. For instance, it turns out that firms of foreign majority ownership are quite important contributors to inequality as they both pay high wage premia, even within the sectors they operate in, and are able to hire high wage workers. Second, the sorting problem clearly has some spatial aspect as well, with highest earners and high-paying firms both being over-represented in Budapest, with sorting itself also being stronger within (and around) the capital. Whether these descriptive patterns are driven by the residential mobility of skilled individuals, inter-generational inheritance of residence and positions or the endogenous location choice of (new) firms remains an open question. Not surprisingly, education is also a main driver of wage differences, but our results suggest that these differences are not only due to the accumulation of human capital, as higher education levels clearly open the door into high wage firms as well. Gender differences persist both within and across firms, with male workers being over-represented in firms which both employ higher wage workers on average and offer a higher wage premia as well. However, how much of the between firm differences are due to discriminatory hiring and what fraction could be explained by different preferences of male and female workers for certain amenities or disamenities of workplaces also remains an issue to be investigated in detail. As besides the sorting channels, within-firm differences in the remuneration of workers of different types turned out to be important in all observable aspects we investigate, the question of the extent to which differential rent sharing could account for such differences arises naturally.

Even after accounting for these observable channels, around half of the overall wage sorting remains as an unexplained, intra-industry sorting component of workers with better unobserved qualities, responsible for almost 5% of overall wage variation. Even if this share largely depends on the availability of worker or firm specific features in the dataset used by the econometrician, the further understanding of the unexplained component may be also a research aim to pursue, by collecting or linking additional data sources for instance. One such feature we were able to utilize was data on the (high-school) test scores of young workers. We have observed that within the same high-schools better students will end up not only in better occupations in the future, but also at better firms as well. However, our results also suggested that being the student of given schools is already indicative of future prospects, and hence high schools may play a strong allocative role as well. An aim to pursue by future studies can be to investigate whether some high schools are indeed able to funnel their students into better firms through providing more human capital, good labor market signals or access to superior social networks, or the sorting between high wage schools and high wage firms is just an empirical artifact caused by selection already present at the time of high-school admission.

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2 Chapter 2: A Fixed-effect Approach to Estimating Rent-sharing Elasticities

2.1 Introduction

Whether driven by bargaining about productivity rents generated at the firm, or originating in the monopsonic power of firms facing upward sloping firm-specific labor supply, the strong, positive correlation between wages and productivity have been long observed by labor economists. The data revolution in labor economics, that is the increased availability of linked administrative data with respect to both sheer size and detailedness, formed the findings in this field as well. Card et al. (2018) provides a detailed summary on the evolution of estimation designs aiming to capture this relation over years. While early studies mostly relied only on cross-sectional comparison of industry productivity and wages, the increased availability of firm-level productivity and at first aggregate, then worker-level wage data allowed researchers to investigate the wage-productivity relation through much more rigorous lenses.

The broad variety of econometric approaches on which labor economists have relied signals the non-triviality of the estimation problem of capturing the effect of productivity differences across or even within firms. While cross-sectional estimations have their merits in capturing wage dispersion caused by (long-term) productivity differences between firms – as opposed to the more transitional focus of within-firm models –, the problem of confounding correlations arises for these methods, with the most important confounder being (unobservable) skill composition of firms. The literature offers two main solutions, with one relying on within-person identification of wage reactions to productivity changes, while the second approach – proposed by Card et al. (2016) and Card et al. (2018) – utilizes AKM (Abowd et al., 1999) firm-effects as firm-level wage measures that control for skill composition. While the former model inevitably relies only on the wages of workers who stay at their employers, the latter relies on wage data of only those workers who switch between firms in the data observation period. More importantly, as AKM firm-effects are fixed within employers, they are limited to be used along with the cross-sectional variation of firm productivity and wage premia and hence fail to account for all potential confounders. Accordingly all previous approaches had to face major or minor limitations, which we aim to overcome in this paper.

Besides presenting and summarizing the estimation designs established in recent literature –alongside the corresponding econometric difficulties and available solutions –, we nest these into a single regression formula.[61] Then, we propose a novel solution for the selectivity issues of the stayer – and also the AKM-based – designs by using firm-year fixed effects –introduced by (Lachowska et al., 2020), among others – as outcome variables in regressions on firm productivity. As this time-varying measure of firm premia captures wage information of both stayers and

[61] In this process, we provide a within-match, fixed effect alternative to the stayer designs that conventionally use (first) differences. Despite some minor differences in identification, these model present similar behavior.

job-switchers – while still controlling for unobserved worker heterogeneity – and also could be included in longitudinal models, we bridge multiple gaps between the existing modelling approaches. Therefore, given a sufficiently long panel of data, and properly accounting for measurement error issues – through instrumental variables –, we arrive at a theoretically superior estimator of the rent-sharing elasticity or productivity-wage pass-through parameter of firms in our labor market. The empirical comparison of this novel specification to previous approaches reveals, that although the selectivity issue of stayer-focused models is present, its small magnitude probably makes it a second-order issue – at least in the dataset we use.

In the second half of our paper we also contribute to the literature of differential rent-sharing by highlighting the role of sorting with respect to different rent-sharing elasticites of firms and also by presenting an estimation design that allows for decomposing rent-sharing differentials into sorting and within-firm, bargaining differences, building on the works of Card et al. (2016) and Card et al. (2018). Conventional, cross-sectional measures of differential rent-sharing do not take into the account that the different groups of individuals, for instance highly educated workers do not only have better bargaining positions – or better outside options putting an upward pressure on their wages – within their respective firms – therefore benefiting more from a potential productivity increase of the employer –, but may systematically select into firms that share rents to a higher extent with all of their workers. If such sorting channel is present, then cross-sectional measure of rent-sharing differences will overstate the underlying (within-firm) differences. We also assess, to a smaller extent, whether there are substantial differences in the productivity-wage relation across sectors, or the cross-sectoral differences in wages and productivity are simply the results of between-firm differences being aggregated.

In our empirical results we find rent-sharing elasticites ranging between 0.05-0.16 across the established specifications from OLS regressions, and between 0.12-0.18 in estimations relying on (internal) instruments. By comparing the different specifications and considering the relevant econometric concerns in each, we find suggestive evidence for the strong role of skill composition – affecting mainly cross-sectional models – and the attenuation bias induced by measurement errors in longitudinal models. Opposing our expectation, selectivity plays a minor or even negligible role, despite some of our exercises suggesting that such concerns are not unfounded. Allowing for heterogeneous effects across sets of firms also reveal the important differences between cross-sectional and longitudinal estimations. For instance, while agricultural firms react harshly to inter-temporal variation in productivity, these differences do not translate into cross-sectional differences among the set of these firms. By focusing on within-firm differences in the wage reaction of different worker groups, we find that productivity dispersion is reflected more in the wages of males, of more educated workers, and of those in better occupations. Workers with less than one-year of completed tenure at their current firms also seem to benefit somewhat less from an increase in productivity compared to their co-workers, motivating the importance of the selectivity in stayer designs. Finally, we also show, that the gender differences in rent-sharing parameters are not driven by the different composition of male and female workers with respect to occupations, as differences are quite stable across broad occupation categories and also across different types of firms.

The remainder of the paper is structured as follows. Section 2.2 reflects on the underlying mechanisms of the productivity-wage relation, assessing recent findings of the literature. A simple framework to estimate this relation is proposed, discussing also the main threats to identification of interpretable effects, with Section 2.2.5 presenting the main extension we apply to the simple model in order to assess these econometric challenges. Section 2.3 assesses some further issues of identification and the necessary data restrictions, and also introduces our models for assessing differences in rent-sharing behavior, both across groups of firms or workers. Section 2.4 contains our results from comparing different specification, and also the differential rent-sharing estimations, alongside a more detailed assessment of gender differences. Section 2.5 concludes.

2.2 Relation of wages and productivity

2.2.1 The underlying mechanisms

Before assessing the econometric difficulties in capturing the mechanisms that can shape the wage-productivity relation within and across firms, we start by presenting the main theoretical considerations about such phenomena. Even conceptually, the emergence of a (positive) relation between firm level productivity and wages – as opposed to the competitive labor market model, which predicts no such correlation – may be attributed to (at least) two different underlying mechanisms. For instance, Criscuolo et al. (2021) differentiates between explanations relying on the dispersion of marginal labour productivity between firms caused by market imperfections, and the sharing of productivity-related rents between firms and workers as a result of bargaining processes. Without presenting a formal model, we summarize the factors that could give rise to the former channel first.

Either due to labor market frictions – such as (high) costs of job search, job mobility or residential mobility – or formed by individuals preferences across non-wage characteristics of different workplaces, individual firms may face an upward sloping labor supply curve as opposed to the perfectly elastic supply in a basic competitive model. In such settings firms can only hire additional workers by paying an increased wage that covers the disutility or costs of not working at another employer, sector or region. Similarly, they also have the (monopsony) power to employ less workers, those with the most limited outside options, at lower wage levels – provided no wage floors are present in the given sector.[62] Even if firms would act as wage takers, in response to an increase of (marginal) productivity, the firm would not only increase its employment level – as in a competitive setting – but would (have to) increase wages as well to be able to hire the additional workers needed to equalize marginal productivity with marginal cost of labor. However, a productivity change will not pass-through into wages one-on-one as

[62] Manning (2021) gives a review of the recent resurgence of monopsony models in labor economic studies. The prime examples of using such models in the context of productivity pass-through/ rent-sharing are provided by Card et al. (2018) and Lamadon et al. (2022).

a non-discriminating monopsony has to pay the same wage for all of its workers, and hence a profit-maximizing firm will choose a somewhat lower level of both wages and employment.

According to the predictions of the basic monopsony model, Criscuolo et al. (2021) presents three main factors that should define firms' reaction to productivity changes of the firm. First, the degree of productivity pass-through is expected to decline with the elasticity of the firm-level labour supply. That is it will depend on the extent to which job mobility is constrained among potential employees of the given firm – due to factors such as available vacancies, fixed costs of job search or institutional barriers of job-switching.[63] Second, according to the model, the pass-through rate increases with the elasticity of labour demand, either due to changes in the price-elasticity of final demand – depending on product market competition – or the elasticity of substitution between labor and capital or services – defined by the prevalence of automation and outsourcing. Finally, institutional wage floors – set by either collective bargaining or centralised decisions – can naturally dampen the relation between productivity changes and the wages set by firms.[64]

An other branch of explanations relies on the bargaining power of workers , in face of their employers, about the productivity rents generated at the firm. Due to the presence of outside options of workers, and the potential costs for the firm arising form searching for new workforce, in search models employers may be willing to decrease profits in favor of increasing worker salaries. The extent to which increases in quasi-rents translate to worker wages also becomes an empirically interesting question (Card et al., 2018). So while the two theoretical concepts that may elicit a relation between productivity shocks and wages are somewhat related – especially with regards the role of outside options –, a clear interpretation of such an empirical pattern as either supply-driven or as one caused by bargaining is not trivial, especially in models focusing on between-firm variation, as these differences are at least in part defined by the market possibilities of workers, and the market power of firms (Criscuolo et al., 2021). Nevertheless, in the larger body of this paper we will focus on firm-specific and even intra-firm differences, and hence will prefer to use the term *rent-sharing elasticity* in favor of *pass-through rate* throughout the paper.

2.2.2 Variation in firm-level productivity

The other factor that complicates the empirical assessment is the presence of multiple possible sources of variation in productivity. On one hand, we have to differentiate between sectoral and firm-specific productivity shocks, but implications may also depend on whether these shocks are only transitory in nature or are

[63]As the authors note, due to this channel, a strong correlation between wages and productivity may actually signal a stronger presence of labor market imperfections.

[64]Motivated by the policy relevance of these factors, Criscuolo et al. (2021) test the prediction of these hypotheses on inter-industry differences across multiple countries and find significantly higher pass-through rates in industries with low job-mobility, high presence of foreign value added (competition), lower presence of minimum wage regulations and collective bargaining. Also, using detailed data from Portugal, they find that higher employment concentration on the level of local labor markets also lead to higher elasticites.

they persistent innovations to the given firms' productivity. Regarding the former, Carlsson et al. (2016), Friedrich et al. (2019) and Lamadon et al. (2022) provides evidence that wage reactions are expected to be stronger for industry level shocks common to all firms, as these will not only shift the labor demand of the given firm, but alter the outside option of workers as well.[65] Considering the timing of firms' reactions, we would expect that in most industries wages are fixed in the short run and do not depend on temporary productivity fluctuations[66]. As Guiso et al. (2015) (and later Juhn et al. (2018) and Lamadon et al. (2022)) show, firms indeed 'insure' workers against short-term variation in productivity – hence average transitory rent-sharing elascticities are minimal or virtually zero.

On the other hand, long-term changes in the firm-specific productivity could elicit a move across the firm's own (upward sloping) labor supply curve. Given enough time to observe a firm and enough (non-transitory) variation in its productivity, the firm-level pass-through (or even the firm-level labor supply), could be identified (Lamadon et al., 2022). The reactions provided to such changes, will be then reflected in wage differences across firms of differing productivity. The relation within each sector will be formed based on the 'average' of sector specific features discussed above (job mobility, etc.) As Criscuolo et al. (2021) notes, relying on this cross-sectional variation "directly addresses the question of the long-term relation between the dispersion in firm wage premia and dispersion in productivity rather than the short-term response of wage premia to productivity shocks". Besides the responses to industry-wide shocks, the long-term reactions to firm-specific innovations will also translate into inter-industry differences.

For each industries, the final wage outcome will therefore also depend on industry-specific market characteristics that define how much productivity variation translates into wage variation in the given segment of the economy. While a strong positive correlation between industry-level average wages and productivity would emerge if pass-through rates would be constant across sectors, if more productive industries have larger pass-through rates, the inter-industry wage differences would be further magnified. Therefore, both focusing only on within-sector differences (and controlling away the fundamental underlying differences), and the investigation of effect heterogeneity could be an important aim to pursue. Unfortunately, many studies in the literature had to focus on smaller segments of a given economy – or did not aim to assess difference across sectors –, with a notable exception being Bagger and Lentz (2014). While in this study we don't give as a detailed assessment as Criscuolo et al. (2021), we will estimate heterogeneous effects, and check the relation of sector-specific elasticities to some industry-level factors

Besides focusing on both the within-sector differences across firms, and the firms' wage responses over time, we would also like to go deeper within firms as well. Although differences in wages, and their responsiveness to productivity

[65] Even more surprisingly Carlsson et al. (2016) finds that industry-level shocks can affect wages of firms independent of their own productivity shocks.

[66] At least, while these are small. However, even in the case of extreme events, such as the lockdowns in response to COVID-19, probably it is employment that adjusts more, by the lay-off of workers or the cut of working hours. Employers may be especially averse of cutting wages(Juhn et al., 2018).

changes, across different groups of workers within the same firm could be driven by differing preferences, outside options, or regulation of workers, they may reflect variation in bargaining power as well. Such differences may emerge based on fixed characteristics such as gender or education (Card et al., 2018; Card et al., 2016; Criscuolo et al., 2021), but also based on the given occupation or seniority of workers as well. Although we will not be able to clearly differentiate between scenarios driven by bargaining power and the role of outside options, we report evidence on the presence of differential rent-sharing across the above listed worker groups, assessing whether the same productivity shocks could have different effects on different workers of the same firm.

2.2.3 A simple model to nest previous estimation designs

Let us consider the following simple regression for modeling the relation between rents generated at a firm and the wages of its workers, supposing we have data on individuals and firms from multiple years.

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{RENT}_{jt} + \beta \mathbf{X}_{ijt} + \theta_k + \omega_t + \varepsilon_{ijt} \tag{2.1}$$

Subscript i relates to individuals working at a firm j in period t. W_{ijt} is an individual-level measure of wages, while RENT is a measure of firm-level rents. Firm value added is often considered the prime candidate for a rent measure as it captures the additional value created at the firm, which then would be spent either on the remuneration of workers or serve as the profit of the firm, with taxes imposed on both components.[67] X_{ijt} may include both firm or worker characteristics – measured either on the level of individuals or using firm-level aggregates. ω_t captures general trends, and country-wide shocks, in wages (and productivity) over time. Due to the log-log specification, γ will measure the expected percentage increase in the wages of workers in response to the one percentage increase in rents – the sum of wages and profits –, hence indeed capturing on elasticity of the wage share with respect to the increase in available rents.

In our formulation, θ_k is a placeholder term for additional fixed effects in the model, the choice of which substantially defines the models interpretation. Without any fixed effects, the parameter on log-rents would capture all covariation between productivity and wages, even the substantial inter-industry differences, including general differences in technologies or corporate culture. To focus only on intra-industry variation, sector fixed effects should be included in the model. Identification of γ in these models would then rely both on within-industry, cross-sectional and within-firm, temporal variation of wages and productivity, generally asking the question, whether more productive firms pay larger wages. In the remainder of the paper, similarly as modern studies on rent sharing, we mostly ignore the effects of industry wide productivity shifts on wages and focus on within sector, more firm-specific productivity components. A simple cross-sectional estimation,

[67] If there is no information on the costs of production, then sales per worker can serve as a second-best option. Card et al. (2018) discusses the conditions under which sales per worker and value added per worker could capture the same mechanism. Also, quasi-rents, that is measures that control for the workers' outside wage options, could be used as an outcome variable.

hence has the following form, with $s(j)$ reflecting the sector in which employer j operates.

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \lambda_{s(j)} + \omega_t + \varepsilon_{ijt} \tag{2.2}$$

Provided we observe the same firms over multiple years, if we add firm fixed-effects to the model we will use only longitudinal variation by focusing on the effect of changes in productivity over time at the same workplace. Therefore, the underlying research question becomes whether firms pay higher *when* they are more productive.[68] This variation incorporates both short-term, transitory shocks to productivity and – given we observe the firms over many years – the long-term evolution of productivity for firms. Reactions to the former are often observed to be minimal, and researchers are more often interested in the underlying, long-term relation, which would be also translated into the cross-sectional variation as well. Accordingly, as Card et al. (2018) shows, studies relying on within-firm variation, including their own results, often find lower elasticites.[69] Relying on within-firm variation of productivity unfortunately not only magnifies the relevance of transitory shocks, but the measurement errors in the firm-level performance measures as well. Still, the cross-sectional comparison of wages could be problematic as well due to a set of possible confounders. Issues with both approaches, and the available solutions proposed in the literature, are discussed in Section 2.2.4. Making use of firm fixed effects, a very simple longitudinal model would take the following form.

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \psi_j + \omega_t + \varepsilon_{ijt} \tag{2.3}$$

We note that both cross-sectional and longitudinal models could be estimated even if firm-level wage data is available. If no or only limited amount of data is available on worker characteristics, all variables could be aggregated to the firm-year level (using shares, for instance), and with the number of workers used as weights the firm-year-level, (weighted least squares) regressions will yield the same parameters as individual-level regressions would.[70] However, if one can observe worker characteristics – as is the often the case with the increasing availability of high-quality micro-data – the regression could be estimated on the level of individuals, controlling for observed heterogeneity in the composition of workforce. Furthermore, if individuals are linked across periods in a panel structure, one can even control for unobservable worker heterogeneity by including worker fixed effects, or for more precise assessment, worker-firm match fixed effects, giving way

[68] We note, however, that a within-firm approach will not necessarily relate to only firm-specific shocks, as the variation in productivity within a firm could still be a result of an industry-wide shock. Some recent studies as Carlsson et al. (2016), Friedrich et al. (2019), Lamadon et al. (2022) assess this issue, for instance by removing the sector-wide innovations in productivity in an extra, initial step. The inclusion of sector fixed effects and time dummies in the models, however, should at least partially overcome the confounding effect of shocks to competing firms.

[69] Lamadon et al. (2022) uses short term productivity changes as an instrument for identifying long-term elasticites. Juhn et al. (2018) shows that in models written up for wage and productivity changes (of stayers at the firm), either instrumenting long-term changes with short-term ones – over symmetric windows – or vice versa could eliminate the effect of transitory wage innovations.

[70] This approach would also eliminate the group structure in error terms caused by the firm-year level frequency of productivity measures. Still, due to the potential cross-period correlation of productivity measures, in both settings firm-level clustering of standard errors is necessary.

for a within-spell identification. Hence the simple model of Equation 2.1 also nests a formulation related to the approaches that rely on the wage-changes of individuals staying at their employers, answering whether the given workers benefit from changes in the firm's productivity:

$$\ln W_{ijt} = \alpha + \gamma \ln \text{VA}_{jt} + \beta X_{ijt} + \mu_{ij} + \omega_t + \varepsilon_{ijt} \tag{2.4}$$

While these *stayer* models are often formulated in terms of (first) differences [71], they capture the same match-specific heterogeneity, as the above equation would. However, some differences are present. Instead of asking how large long-term wage change is expected due to long-run changes in productivity over a given period, this formulation asks whether the wage of a given individual is relatively higher, within the given employment spell, when the firm is (relatively) more productive. Therefore, this model is slightly different in relying on the variation in short-term fluctuations as well, while stayer models include less and less of the transitory terms as the observation window is being increased (Juhn et al., 2018).[72] This latter feature, however, comes at the cost of sample selectivity, as individuals who are not present in the firms for the given number of consecutive years are excluded from all estimations. In our formulation, each individual – except for those whose employment is restricted to only a given year – is included for the full (observed) length of the given employment spell, and is implicitly weighted by this length. Hence while we have many observations relating to short-run fluctuations, long-run changes are also incorporated across many observations, balancing out the weight of transitory shocks and errors. In Appendix B.4 we relate the within-match fixed effect design to classical stayer formulations, and show that the former provides estimates close to models relying on differences taken over around 5 years – in our sample, at least.

2.2.4 Threats to identification

As given sufficient cross-sectional or temporal variation in firm rents γ is identified, the estimation on any form of Equation 2.1 proves no econometric difficulty. Still, the economic interpretation of γ as a parameter capturing the rent-sharing behavior of firms, either driven by bargaining or monopsonic considerations, may be unwarranted as Equation 2.1 is affected by almost all of the most common biases that may arise in such a simple regression setting. Specifically, endogeneity originating both in simultaneity or reverse causality and omitted variable or selection biases are present alongside the biases caused by measurement errors in the regressors and the selectivity of the sample. In this section, we reflect on each of these concerns and the solution methods proposed by prominent authors of the literature.

[71] For instance, Card et al. (2018) presents models estimating the effects of productivity change of firms over 4 years on the wage increase of individuals in the same period.

[72] The issue of measurement errors may be also less severe in the within-match specification, as the deviation of productivity from the spell-mean may contain less noise than the difference between two arbitrary observation in time. The precise assessment of this, however, is left to be a topic for future studies.

The simultaneity problem

The most fundamental issue barring a causal interpretation of the effect of wages on productivity relates to simultaneity or even reverse causality, partially originating in the granularity of observations, namely only having firm productivity measured most often on a yearly basis. For instance, if we assume that firm productivity is a function of (the sum of) the productivity of workers, an increase in the latter will increase the yearly output and value added of the firm. However, if firms employ workers with salary schemes including production bonuses (performance pay), their wages will adjust automatically. While this phenomena could be considered a form of sharing rents *ex ante*, the effects exerted on worker motivation – which could be also imposed by any unexpected wage raise – and productivity will confound the sharing of rents from productivity shocks.[73] Hence, in order to provide a reassuring estimation of the effects of such shocks, the use of external sources of variation in productivity is necessary, either by focusing on the wage effects of such factors or using them as instruments in an instrumental variable approach. Examples for such external instruments may include winning patents (Kline et al., 2019; Van Reenen, 1996), measures of innovation (Hildreth, 1998), demand or export (price) shocks specific to the given markets (Abowd & Lemieux, 1993; Arai & Heyman, 2009; P. S. Martins, 2009) or even the productivity measures of similar firms in other local labor markets (Barth et al., 2016; Card et al., 2014). The availability of such external instruments is often limited and even some of the previous studies could focus only on specific industries or subsets of workers. For instance, although we have data on export sales, only around 5% of firms export at all, hence an IV method using the external variation in exchange-rate shocks would only estimate effects local to this subset of firms.

The workforce composition problem

Without a perfectly reliable external instrument – which is often not available, or the variation it uses, and hence the local effect it can capture is limited only to a given industry or time period –, the econometrist faces a set of important measurement issues. Even if we'd like to capture the correlation of productivity and wages precisely – and not pursuing a causal interpretation – , we still has to account for confounding factors that could cause spuriosity in this correlation. The most prominent of such confounders is caused by the phenomena that more productive firms may employ complementary, high skilled workers, for whom they naturally pay higher wages on average.[74] Naturally, while this issue is the most prominent in cross-sectional designs, over a longer period a given firm could also alter its workforce composition, either in response to or in anticipation of a productivity

[73]For instance, Reizer (2019) finds a stronger reaction to changes in the sales of Hungarian firms on the wages of workers with flexible wage components than those without such remuneration elements. Juhn et al. (2018) also shows stronger reactions among the top earners of the firms and in sectors where performance pay may play a stronger role.

[74]Boza (2021) presents evidence for strong sorting patterns in Hungary, in-part driven by observable phenomena, such as high productivity foreign-owned employers hiring workers with both better observed and unobserved skills compared to domestic employers.

increase. A simple solution would be controlling for observable worker characteristics or observed worker composition of firms – depending on data aggregation –, but this may not capture the quite important, unobserved heterogeneity in worker skills. The principal proposed solution in the literature therefore is the reliance on the wage change of incumbent workers over a few years to the productivity change of firms over the same period. A prominent example of such models is provided by Juhn et al. (2018), while being featured in Card et al. (2018) as well.

Card et al. (2016) and Card et al. (2018), on the other hand, propose substituting raw wages with AKM firm effects as a firm-level outcome variable, getting rid of the composition effects in a preliminary step. AKM – after Abowd et al. (1999) – firm effects could be obtained from estimating the following two-way fixed effect wage equation.

$$\ln w_{ijt} = \mathbf{X}_{ijt}\beta + \theta_i + \psi_j + \epsilon_{ijt}. \tag{2.5}$$

The ψ_j parameters of this model are firm-related wage residuals, being controlled for time-varying observable characteristics and time-invariant person characteristics (both observed and unobserved), and thus providing an indirect measure of firm-level wage premia. As the authors argue, regressing firm productivity on this wage measure – which is devoid of wage components of worker composition –, removes the effect of worker sorting or up-scaling and therefore provides a clear estimation of the rent-sharing elasticity.[75] Equation 2.1 of our framework can also nest this approach, by substituting the outcome, W, with ψ_j firm effects.

$$\psi_j = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \lambda_{s(j)} + \omega_t + \varepsilon_{ijt} \tag{2.6}$$

By the design of the standard AKM model, as firm effects do not vary within firms (or employment spells), the use of this measure is naturally limited to cross-sectional, within-sector identification only. However, in Section 2.2.5 we propose the use of time-varying firm(-year) effects as outcomes to overcome this limitation.

Confounders, measurement errors, transitory effects

Skill composition, however, may not be the only confounding channel to be aware of when comparing wages of different firms, especially as firm-level wage premia may be guided by factors other than the pass-through of productivity into wages. For instance, if larger – often more productive – firms rely on effective wage schemes more extensively – due to the costs of monitoring increasing with size –, we would observe a positive correlation between wage levels and firm size, and hence productivity – even if measured per capita. Although this channel could be captured by the inclusion of size controls, similarly as with large cross-industry differences in wage regulations, there are intra-industry differences between firms, for which we often cannot account for. Notably, firms may differ in the level of amenities they offer, such as the amount of overtime hours or weekend workdays or even the presence of family-friendly facilities (Sorkin, 2018). As a trade-off is expected to

[75] This method is adapted by Allan, David Maré and Corey (2021) as well in their paper investigating wage evolution in New Zealand.

be between paying higher wages for workers or providing better amenities, if more (or less) productive firms rely on the former with a greater extent, the correlation between productivity and wages will be decreased (increased). Again, this issue is more probably present in cross-sectional models than in longitudinal ones, as firms rarely alter their waging policies, therefore any type of within-firm or within-match specifications would capture the effects of this confounder. Hence, any difference between the results of a stayer model and the design of Card et al. (2016) and Card et al. (2018) may be partially driven by this notion – as the authors themselves emphasize as well.

Another trade-off in the choice between models using cross-sectional and longitudinal variation in firm productivity emerges due to an increased importance of measurement errors in the regressors. As yearly financial reports – the most common source of productivity data – are not perfect measures of the underlying firm-year level productivity, the relevance of measurements errors in these variables is more important in models relying on within-firm identification due to the substantially higher noise-to-signal ratio compared to the cross-sectional comparisons – in which the relevant errors compared to the variaton experienced by any individual firm are dominated by the larger, between-firm differences. As in all similar cases, this noise will attenuate the estimated regression coefficients, putting a potentially serious downward bias on parameters in the longitudinal models. Due to the very same reasons, longitudinal models are more effected by firms' reactions to transitory productivity shocks, against which we believe workers are generally insured (Guiso et al., 2015; Juhn et al., 2018), and hence are again expected to result in lower estimated elasticities. The severity of this issue should decrease with the length of periods over which we can observe the same firms (Juhn et al., 2018). If, however, one would like to particularly focus only on long run productivity changes, instrumental variable approaches should be adapted.

Even in the lack of good external instruments, internal ones – that is those that can be constructed using variables already included in the model, such as lags of firm productivity (Gürtzgen, 2009; Hildreth & Oswald, 1997) – may offer a second best solution in overcoming the above issues. However, while they definitely help in decreasing the bias caused by measurement errors , their reliability often depends on the validity of some model assumptions. A state of the art example is the approach of Lamadon et al. (2022), who instruments long run changes of a firm's productivity by short term fluctuations. The authors argue that if the error structure is contemporaneous, or at least the effect of transitory shocks disappear in a finite horizon, the firm-specific pass-through parameters are identified. A similar concept appears in Juhn et al. (2018) in the context of stayer models. Namely, the authors show that either short run productivity changes can be used to instrument long run ones, or vice versa. This approach will take care of measurement error problem and even partially the issue of smaller responses to transitory innovation if measurement errors and transitory shock components are indeed uncorrelated across years. In our empirical exercises, we will rely on two simple instrumental variables, the lag of productivity and a bracketed sales instrument used by Card et al. (2018), which also rely on similar assumptions. The exact way these instruments help in identification of rent-sharing elasticities are discussed in section 2.3.2.

Selectivity

Assuming that the attenuation bias caused by measurement errors is taken care of, within spell (stayer) models may seem the superior way to estimate rent-sharing elasticites. However, the trivial issue of sample selection emerges, as we can only rely on the wage variation of individuals staying at their employers over longer periods. Hence, the wages of those who often switch employers will not contribute to the estimation of the parameters we seek or, as in our formulation of Equation 2.4, only with smaller weights than long run stayers.[76] We also note that the approach of Card et al. (2016) and Card et al. (2018) is not devoid of the selectivity problem either, as AKM firm effects are identified only from wage observations of job-switchers. This problem would be especially constraining if the observation window is short or individuals tend to stay for prolonged times in the same jobs.[77] If firms tend to share rents with short term and long-term workers differently – which we suspect to be the case –, neither approach could capture the true average rent-sharing behaviour of the firms.[78] Also, firm choice itself may be endogenous as well, with a possibly higher level of fluctuation at firms with low rent-sharing propensity. Finally, as Friedrich et al. (2019) notes, the reaction to negative productivity shocks may also suffer from a censoring problem, as workers may quit the firm instead of accepting the lower wage levels, leading to stayers of the firms presenting higher expected wage growth. Hence, within-match models are expected to overstate true rent-sharing, even if the magnitude of this issue is small – as for instance Card et al. (2016) argues. In the following section we propose a model that solves the composition issue appearing both in stayer designs and the AKM approach as well, while not constraining the identification sample to either of the above subsets.

2.2.5 Solving the selectivity problem with TV-AKM

As we have seen, solving the problem of confounding worker composition leads either to the necessary focus on cross-sectional comparisons (still confounded by amenities) or within individual designs, that do not use any information on individuals who often switch between employers. To provide a feasible alternative to these methods, which can both incorporate information on job switchers and stayers and at the same time still controls for the undesired heterogeneity in firms'

[76] In our sample of 15 years, 42.9% of individuals stay at the same firm for all of their observed periods. This corresponds to 30.3% of all observation, as only 3.7% of such individuals work at the same employer during all 60 quarter years of our observation period, while many individuals have only one employer due to entering or leaving the labor market during the data window. Still, the average spell length of individuals with only one employer in the sample is 9 years – a formidable length.

[77] Therefore, given a fixed (short) observation window, one approach will only rely on wage variation of job-switchers and the other only on wages of stayers, while being fundamentally the same model. Namely, $W = \alpha + \bar{\gamma}VA + \beta X + \psi_j + [\theta_i]$ represents the same model as $\hat{\psi}_j = (W - \theta_i - \beta X) = a + \tilde{\gamma}VA)$, with the two models relying on different sources of identifying variation.

[78] Besides the result we present later in Section 2.4.3, Juhn et al. (2018) also discusses a specification in which (at least in some industries) rent-sharing elasticites are somewhat larger for individuals who have at least one year of tenure at the given firm.

wage schemes and worker composition, we propose a novel specification, estimating rent-sharing elasticities from the following formulation.

$$\psi_{jt} = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \tilde{\psi}_j + \omega_t + \varepsilon_{ijt} \tag{2.7}$$

Where ψ_{jt} is the time-varying firm-year effect from

$$ln\ w_{ijt} = \boldsymbol{X_{ijt}\beta} + \theta_i + \psi_{jt} + \lambda_{k(ij)} + \epsilon_{ijt} \tag{2.8}$$

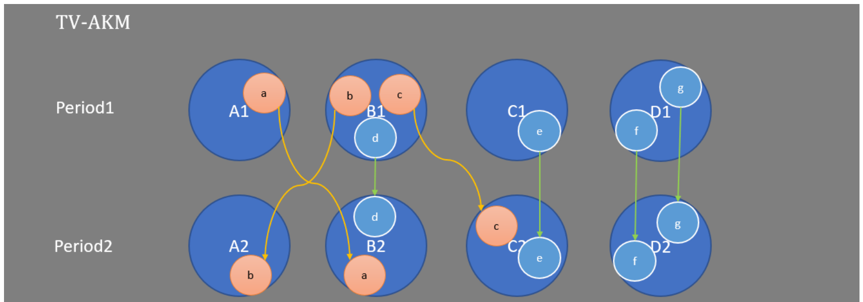
, a model proposed also by Macis and Schivardi (2016), Lachowska et al. (2020) and Lamadon et al. (2022). This wage model is an extension to the standard AKM model, allowing firm fixed effects to vary over time, even within the given firm. Lachowska et al. (2020) labels this specification TV-AKM and discusses its stability and contribution to overall wage dispersion, while also proposing rent-sharing as a possible reason for the emergence of such, time-varying components. As discussed by the authors, the identification of firm-year effects relies on the same assumptions as the firm effects of the standard AKM model, with firms being substituted by firm-year units. Accordingly, firm-year effects are identified by sufficient mobility between the firm-year observations. This mobility comes on one hand from individuals changing employers as in the AKM model, and on the other hand from individuals staying in the same firms. Hence the wage changes of stayers also contribute to the identification of firm-year effects.[79] As in the case of the AKM model, a normalizing condition is required in all connected set of firm-year units in order to achieve the full rank of the design matrices. This connected set, however, is not expected to be substantially smaller than in the baseline AKM, as only those firm-year cells get disconnected over time where all of the (observed) workforce of the firm changes between two consecutive years – a rather rare phenomenon. The natural computational trade-off compared to the standard model is the magnitude larger set of estimable parameters. While the average mobility per unit may increase with the inclusion of stayers, the average variation in wages per unit may somewhat decrease due to the wage stability of stayers, hence the severity of the limited mobility bias, and thus the need for a correction method may be increased.

As opposed to the conventional AKM firm effects, the estimated firm-year fixed effects of this model ψ_{jt} can be used in within-firm, cross-temporal comparisons. Accordingly, as Equation 2.7 includes a set of firm fixed effects on the right-hand side, we will identify rent-sharing elasticities from within-firm changes of this firm-year level wage measure. The advantage of this slight modification is that while we use within-firm variation of productivity, we do not focus only on wages of stayers of the firm, as this outcome incorporates information on the wages of leaving and arriving job-switchers as well. At the same time, unlike the approach of Card et al. (2016) or Card et al. (2018) this measure will not only reflect information in the wages of job-switchers neither – from what observations the conventional AKM effects are identified.

[79]"Key source of identification of ψ_{jt} is ... average wage change of incumbent workers ... the same source of variation is typically used to identify rent sharing elasticities." (Lachowska et al., 2020)

The difference between the three different specifications – stayer, AKM and TV-AKM designs – is illustrated in Figure 2.1, showing which workers' wage variation will be represented in the rent-sharing parameter estimations in a two-period economy depending on model choice. As the figure suggests, our proposed measure lack both kind of selectivity bias discussed in Section 2.2.4.To observe this, consider first a scenario in which we alter the wage of person d in either of the two time periods. As this individual is incumbent to firm B, none of his or her wage observations will contribute to the identification of the AKM firm effects – as it relies only on the variation of wages of the same individual across different employers. Therefore, the rent-sharing estimation proposed in Equation 2.6 will be insensitive to changes in these wage observations, alongside the wages of any stayers. At the same time, considering an alteration to the wages of individual a, b or c will not affect the within-match or stayer designs, as individuals switching firms during the observation window – even if it would be only two years – are naturally excluded from the estimation sample. However, any observation in the largest connected set of firm-year clusters contributes to the identification of the TV-AKM firm-year effects, and hence the rent-sharing specification proposed in Equation 2.7.[80]

[80]Firm D and workers f and g are not part of the largest connected set in the labor market, on which AKM models are generally estimated. The reason for omitting observation in such smaller components is that the identification of AKM firm effects requires one normalizing condition per connected set, and therefore the estimated parameters of different components are not directly comparable / measured on the same scale. However, if one applies the within-firm approach when estimating the rent-sharing relation, these observations could be used as well as their within-unit differences are still measured in log-wage units. In practice, however, we will not consider using such observations.



Firms	A	B	C	D
Stayer model		✓	✓	✓
AKM model	✓	✓	✓	
TV-AKM model	✓	✓	✓	

Individuals	a	b	c	d	e	f	g
Stayer model				✓	✓	✓	✓
AKM model	✓	✓	✓				
TV-AKM	✓	✓	✓	✓	✓	*	*

Figure 2.1: Wage observations contributing to identification in different models

Notes: Large circles and capital letters represent firms, while small circles (lowercase letters) are individuals. Green lines correspond to workers staying in their firms, while orange lines represent worker mobility between the two periods.

2.2.6 Comparison of conventional and novel methods

Table 2.1: Conventional and novel approaches for estimating the rent-sharing elasticity

Setting	Classic CS	Classic L.	Stayer	CC(H)K	Boza
Equation	(2.2)	(2.3)	(2.4)	(2.6)	(2.7)
Setup					
Wage measure	$\bar{w}_{jt}/w_{ijt}$	$\bar{w}_{jt}/w_{ijt}$	w_{ijt}	ψ_j	ψ_{jt}
Outcome level	firm/ind.	firm/ind.	ind.	firm	firm-year
Fixed effect	sector	firm	match	sector	firm/match
Identifying variaton					
Productivity	CS	long.	long.	CS	long.
Wage of stayers	yes	yes	yes	no	yes
Wage of switchers	yes	yes	no	yes	yes
Bias due (expected sign)					
Simultaneity (+)	needs IV	needs IV	needs IV	needs IV	needs IV
Skill composition (+)	issue	issue	solved	solved	solved
Amenities, comp. diff. (-)	issue	solved	solved	issue	solved
Measurement error (-)	issue	issue+	issue+	issue	issue+
Selection (+/-)	n.a.	n.a.	issue	issue	negligible[a]
Notes					
Data requirements	*	*	**	**	***

Notes: [a] refers to the omission of observations not in the largest connected set in the date – which is avoidable when using longitudinal models. CS stands for cross-sectional, L and long. for longitudinal variation of productivity. CC(H)K stands for the studies of Card et al. (2016) and Card et al. (2018).

Table 2.1 summarizes the sources of biases we may face in the simple cross-sectional and longitudinal (within-firm) models, in the designs relying on stayer subsamples and in the proposed solutions of Card et al. (2016) and Card et al. (2018), alongside the claimed properties of the approach we propose. The first two rows identify the model specification alongside the corresponding equation in our text. The used outcome variables and fixed effects are also presented as a reminder. The third panel of the table presents whether the specification relies (mainly) on cross-sectional or longitudinal variation in firm productivity, and – following our argumentation in Section 2.2.5 – it is indicated which set of wage observations contribute to the identification of rent-sharing elasticities in the given model formulation. Finally, the bottom panel lists the main confounders and measurement issues presented in Section 2.2.4. As illustrated, our proposition solves the skill composition bias just like stayer (within-match) models and the approach presented in Card et al. (2016) and Card et al. (2018). However, due to the within-firm or within-match design the confounding role of amenities and compensating differentials are mitigated – as long as firms don't alter their waging schemes drastically between periods. However, the longitudinal nature of the design magnifies the role of measurement errors and the downward biased caused by the insurance firms provide against short term, transitory fluctuations – therefore as in all longitudinal design, the use of internal instruments is warranted. Notably, our design solves the selectivity of the identifying sample used in the previous designs controlling for compositional changes. However, as the final row indicates, this novelty comes with larger estimation burdens, as a large set of firm-year effects has to be estimated using the TV-AKM approach – which may not be a strong limitation due advancements in estimation methods and data availability.

2.3 Empirical strategy

The main aim of our empirical exercise is to estimate both the models established in previous literature (Equations 2.2, 2.3, 2.4 and 2.6) and the novel approach proposed in Equation 2.7, using the same general framework –Equation 2.1 – nesting these specifications. Then, by comparing the results of these different specifications, we can assess the severity of the estimation issues discussed in Section 2.2.4 and summarized in Table 2.1. Of distinguished importance is the role of sample selection and how the proposed model relying on firm-year effects as wage outcome performs compared to the methods of Card et al. (2016) and Card et al. (2018), and our formulation of the stayer models.[81] As the differences between the models could be in large part driven by the magnified measurement error issues in longitudinal models – alongside the inclusion of short-term fluctuations in the identification – we will rely on simple instrumental variable strategies as well, to make our parameter estimations more comparable across specifications.

The second set of our empirical estimations rely on assessing the heterogeneity of pass-through rates both across different types of firms (of different industries, ownership categories or size), and across sets of different workers. For the latter exercise we adapt – and slightly alter – the approach of Card et al. (2016) and Card et al. (2018) of estimating parameters of differential rent-sharing for workers of different gender or educational attainment. Using models for differential rent-sharing, we will also directly illustrate, that even within the same firms recent entrants and senior workers benefit (slightly) differently from productivity changes, somewhat validating our concerns regarding selectivity as an issue. Gender differences across industries or occupations will be discussed in more detail.

2.3.1 Sources of data, definition of variables

Our estimations use data from the Databank of the Research Centre for Economic and Regional Studies[82]. The Panel of Administrative Data from CERS is a large, administrative, linked employer-employee panel dataset, covering a random fifty percent of the Hungarian population. The two-way panel spans from 2003 through 2017 and contains labor market data in monthly resolution, such as an ID for the employer, earnings in given month, occupation information and balance sheet data for incorporated employers. We observe all taxed earnings from the given

[81] The comparison of our particular within-spell specification 2.4 and the commonly used identification methods that relate (long) differences in wages and productivity of stayers is included in Appendix B.4.

[82] Formerly of the Hungarian Academy of Sciences, now of the Eötvös Loránd Research Network.

employer during the given month, but cannot differentiate between bonuses and the contractual wage. The data does not convey any family-related information, only individual characteristics like gender, age, residence and also some variables on healthcare expenditures and specific transfers received by the individuals – of which the latter sets of information we do not utilize in this research.

The most important feature of the data, besides being a linked employer-employee dataset, is that we have access to balance sheet data and financial reports for the set of incorporated firms. Using such data, we define firm value added by deducting from sales the material costs of production and the 'activated values of own production' – a proxy for interim goods. After dividing by the reported average number of workers of the given year, we winsorize the per worker value added – replacing the top and bottom 1% of observations with the corresponding percentile values – and then take logarithms.

For wage measures, we will use hourly wages[83] or the firm-year effects defined in Equation 2.8 or the corresponding time-invariant parameter from a model not allowing firm effects to vary across years. When estimating the AKM and TV-AKM models we follow Boza (2021) – the first chapter of this thesis – and use the estimated firm and firm-year effects of the corresponding specifications in that paper.[84] A minor, although important extension in our approach compared to Equation 2.8 is the inclusion of around three hundred occupation fixed effects, capturing occupational heterogeneity. Therefore firm and firm-year level wage measures will not be only devoid of unobserved worker skill composition, but occupational composition effects as well. For the importance of this distinction, see Boza (2021) or the survey of Portugal (2020). Our only additional control variables are the size of the firm (number of observed employees in the given month), and its square. Accordingly, although we have individual data available, in our baseline estimations we do not control for observed worker heterogeneity – for instance, through the inclusion of quasi-education or age dummies. This way, we will illustrate the importance of controlling for both observed and unobserved worker heterogeneity in one step – somewhat magnifying differences between the most simple and more advanced models.

For our estimations, we make three restrictions regarding our sample. First, we can rely only on the subset of incorporated firms, for whom firm value added could be estimated using the available financial reports –this leaves 66.8% of all wage observations from the sample defined in Appendix B.2 and 85.0% of private sector employees. Second, in models using AKM or TV-AKM firm(-year) effects, we have to rely on the largest connected components in which the corresponding effects are identified, leaving 89.5% of observations with value added data for the AKM firm effect models and 82.9% for firm-year effects. Finally, following Card et al. (2016) and Card et al. (2018), we will also limit the sample to the subset of firm-year observations where the wage-productivity relation is not 'flat'. Specifically we first capture this relation by collapsing firms into percentiles based on

[83] Monthly earnings at the given employer divided by four times the reported weekly working hours, or by 40 hours if such data is not available.

[84] Nevertheless, the samples restrictions and variables choices for the AKM models are included in Appendix B.2.

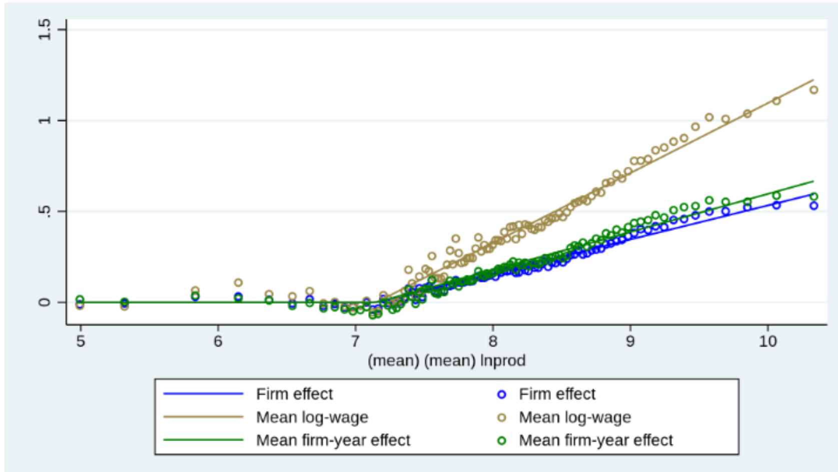


Figure 2.2: The relation between wage and productivity measures

Notes: Data points correspond to a hundred percentiles of firm-year observations along the distribution of the logarithm of value added per worker for firms with balance sheet data available. For the sake of illustration, mean wages and AKM and TV-AKM firm(-year) effects are normalized by setting their mean value for the flat region – below a log value added of 7.15 – of the fitted kinked regressions to zero.

productivity. Then, by fitting a kinked function on our data, we also identify a set of firms – those within the lowest productivity percentiles – for whom an increase in productivity, measured by value added per worker, is not reflected in an increase of wages or AKM / TV-AKM firm(-year) effects. This restriction is motivated by the assumption that the most underperforming firms may have no rents (in given years) to share with their employees.[85] The kink-points using the three different wage-measures coincide almost perfectly, and as Figure 2.2 illustrates we will exclude observations corresponding to around 15% of firm-year observations.[86]

2.3.2 Estimation and inference issues

For our main estimation, we will report estimations based on the Equation 2.1 using three wage measures and using three different levels of interpretation, based on the sets of fixed effects used. Specifically, our wage measure will be either log-wage, AKM firm effects, or TV-AKM firm-year effect of the corresponding observation,

[85] Also, there is an evident structural break in the relation between the outcome and our explanatory variable.

[86] We will use the flat section defined the same way to normalize firm-gender and firm-education fixed effects of the grouped-AKM models – to be presented in 2.3.3 –, as Appendix Figure B.1 illustrates.

while in the models we include sector, firm or firm-worker match fixed effects. As standard AKM firm effects do not vary within any firm (or match), we will have seven main specification – instead of the nine possible combinations. These will correspond to the five main specifications discussed in detail earlier in the paper – Equations 2.2, 2.3, 2.4 and 2.6 and 2.7 – and one alternative versions for both Equations 2.4 and 2.6 in which we substitute wages or firm-effects (respectively) with firm-year TV-AKM effects as the outcome.

In practice, as we do not include observable worker characteristics in our models, for computational reasons we will aggregate data to firm-year level in models with sector or firm effects and use the number of wage observations as weights.[87] In all specifications we cluster standard errors by firms and years. As productivity is measured at the firm level, and hence does not vary between observations from the same firm and year, firm-year level clustering would be a minimal necessary step. However, we assume that a correlation structure may be present across productivity observations of the same firm from multiple years. Therefore broader, firm level clusters are assumed as the main source of group-structure in residuals.[88]

As discussed in Section 2.2.4, both the magnified importance of measurement errors and temporary productivity fluctuations in longitudinal designs call for the use of (internal) instruments, even if we can not account for the simultaneity of wage and output decisions of the firm with truly exogenous shocks. To provide one of the most simple solutions of the above problems, let us first consider that productivity of any given period can be modeled as the sum of an underlying (long-run) productivity component, a transitory component (capturing short-term fluctuations), a classical measurement error and the residual error term, with the latter three components having zero mean and being mutually independent of each other. That is, we assume that the productivity of firm j in period t takes the following form.

$$\mathrm{VA}_{jt} = \mathrm{VA}_{jt}^{LR} + \mathrm{VA}_{jt}^{SR} + \mathrm{me}_{jt} + \epsilon_{jt} \tag{2.9}$$

If measurement errors in consecutive years, that is me_{jt} and me_{jt-1} are uncorrelated for any given t (and j), then instrumentation with even the simple lag of productivity could solve the measurement error problem in the OLS regression of wages on value added. Considering a simplified formulation of Equation 2.2, with value added as the only explanatory variable – instrumented by its lagged value –, we can show the validity of the instrument under the above condition. With leaving redundant subscripts and assuming the short run fluctuation term to be constantly zero, we can simply illustrate that the 2SLS parameter will capture the true rent-sharing elasticity, γ.[89]

[87] This weighted least squares approach provides identical results as would the individual level regressions. We also note, that as we rely on quarterly level data, workers with less than four wage observation in the given firm-year are taken into account with a correspondingly lower weight.

[88] The additional layer of clustering across years does not alter errors substantially.

[89] The illustration also requires an exogeneity assumption from the original panel, $cov(\mathrm{VA}_{t-1}, \varepsilon_t) = 0$, where ε_t is the error term in the regression of wages on productivity.

$$
\begin{aligned}
\gamma_{IV} &= \frac{cov(\mathrm{VA}_{t-1}, w_t)}{cov(\mathrm{VA}_{t-1}, \mathrm{VA}_t)} \\
&= \frac{cov(\mathrm{VA}_{t-1}, \alpha + \gamma \mathrm{VA}_t + \varepsilon_t)}{cov(\mathrm{VA}_{t-1}, \mathrm{VA}_t)} \\
&= \frac{cov(\mathrm{VA}_{t-1}^{LR} + \mathrm{me}_{t-1} + \epsilon_{t-1}, \gamma \mathrm{VA}_t^{LR} + \gamma \mathrm{me}_t + \gamma \epsilon_t t)}{cov(\mathrm{VA}_{t-1}^{LR} + \mathrm{me}_{t-1} + \epsilon_{t-1}, \mathrm{VA}_t^{LR} + \mathrm{me}_t + \epsilon_t)} \\
&= \\
&\frac{cov(\mathrm{VA}_{t-1}^{LR}, \gamma \mathrm{VA}_t^{LR})}{cov(\mathrm{VA}_{t-1}^{LR}, \mathrm{VA}_t^{LR})} \\
&= \gamma
\end{aligned}
\tag{2.10}
$$

Card et al. (2018) propose that under the same conditions, bracketed sales (that is the mean sales over a larger period) can also offer, at least a partial, solution for measurement error problems – a method we also adapt for the sake of comparison.[90] If the same argument holds for the temporary productivity components – that is $Corr(\mathrm{VA}_{jt}^{SR}, \mathrm{VA}_{jt-1}^{SR}) = 0$ – , then the effect of such fluctuations will be also eliminated by the 2SLS approach. As in many specifications we rely on fixed effect designs, for us the independence conditions also have to hold for the deviation of errors from their average over the within-unit observations, hence instead of errors being independent across consecutive years, we have to assume independence across all periods – a somewhat more strict, although not implausible exogeneity assumption.[91]

Finally, we note that– as all research relying on AKM models – we also have to consider the issue of limited mobility bias. Although the two-way fixed effects model estimating AKM provide unbiased firm(-year) effect parameters, the variances of them are affected by limited mobility bias if there is not enough identifying mobility – job switchers per firm over the observation period – in the sample. For a recent assessment of the severity of LMB, see Bonhomme et al. (2020). The most important implication for this study is that in any projection on the estimated AKM effects – such as regressing the estimated firm effect parameters on firm productivity – standard errors have to be corrected, for which Kline, Saggio, and Sølvsten (2020b) proposes an appealing method. While our computational infrastructure does not allow (yet) for adabting this correction, having 15 years of quarterly data may help at least partially overcome the limited mobility bias problem.[92] Also, for the same dataset Boza (2021) presents some reassuring exercises, showing that LMB is probably not extremely severe on this dataset. Nevertheless,

[90]Similarly, in stayer designs Juhn et al. (2018) proposes instrumenting productivity changes over a given period, with changes over either a longer or shorter period with the same mid-point. Gürtzgen (2009), on the other hand, proposes higher order lags as valid instruments.

[91]As the within-unit average error term converges to zero, our instrumentation may be even more robust in within-match designs than in the conventional stayer formulations, as the measurement error of any given period will enter the 2SLS formula only with a lower weight. This argument however should be formally discussed – and tested with simulations – which are out of the scope of the current study.

[92]The data servers we use unfortunately lack the access to softwares in which this method is already implemented.

when interpreting the rent-sharing elasticites, especially when making across group comparisons as in the exercises of Section 2.3.3, we have to bear in mind that the reported standard errors may be underestimated.

2.3.3 Models of differential rent-sharing

To assess the heterogeneity of the wage-productivity relation across sectors, we modify Equation 2.1 the following way.

$$\ln W_{h(j)ijt} = \alpha + \Sigma_{h \in H} \gamma_h \mathrm{I}_{h(j)} \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \theta_k + \varepsilon_{ijt} \tag{2.11}$$

In this form I_h represents subsets of firms based on majority ownership category, industry, size or combinations of these.[93] θ_k, depending on the actual specification may refer to sector, firm or worker-firm match fixed effects. When estimated, these models yield separate parameters for the productivity-wage relation in all specified sub-groups, allowing for a more detailed assessment of inter-sectoral differences.

Similarly, to assess differential rent-sharing across groups of workers, we first introduce the following modification for the general specification of Equation 2.1.

$$\ln W_{g(it)ijt} = \alpha + \Sigma_{g \in G} \gamma_g \mathrm{I}_{g(it)} \ln \mathrm{VA}_{jt} + +\beta_I \mathrm{I}_{g(it)} \beta X_{ijt} + \theta_{gk} + \varepsilon_{ijt} \tag{2.12}$$

In this setup I_g represents some group membership – based on, for instance, gender, education, occupation or seniority (tenure or age) –, while W represents either individual or some firm-group-level wage measures, with θ_{gk} corresponding to sector-group or firm-group fixed effects (or simple employer-employee match effects) – depending on model choice. As firm or firm-year fixed effects are the same for all workers within a firm, using those as an outcome would only capture sorting of different workers into firms with different rent-sharing propensity. However, we can use the approach proposed in Card et al. (2016) and Card et al. (2018) and rely on firm-group effects instead of log-wage terms. While the authors fit different AKM models based on gender or education, we estimate the following model as formulated in Boza (2021).

$$\ln w_{ijtg} = \boldsymbol{X_{ijtg}\beta} + \theta_i + \Psi_{jg} + \lambda_{k(ij)} + \varepsilon_{ijtg} \tag{2.13}$$

That is, while we allow firm effects to vary by group membership – gender for instance –, the returns to observables and occupations are assumed to be constant across groups, hence controlling for composition effects in wages the same way for all sub-units within firms. If these group indicators are time-invariant within persons, then the the mobility network will contain more than one distinct giant components, across which firm-group effects will not be directly comparable. While for estimating rent-sharing elasticities we make mostly within group comparisons, which does not necessarily require it, the firm-group effects could be normalized according to Card et al. (2016) and Card et al. (2018).

[93] As we give firms a new identifier whenever they undergo acquisition, disinvestment or a change in the main industry they operate in, the former two segmentation will be always firm specific, while a firm may have variation over time in the size category it belongs to.

While estimating differential rent-sharing using individuals' wage in a within-spell design – with different parameters for members of different groups – is trivial, when using firm-group fixed effects, ψ_{jg}, we could consider differentiating between overall, within-firm and sorting differences. To show the importance of the distinction, we consider the following formula from Boza (2021):

$$\Psi_{jg} = \boldsymbol{G}\tilde{\boldsymbol{\beta}}_g + \tilde{\psi}_j + \varepsilon_{jg}^G \tag{2.14}$$

The middle term on the right-hand side represents the average premia of the firm – approximately what would be estimated by the standard AKM model. The first term captures the average wage differences between groups in G – across all employers –, while any difference of the last term within a firm captures deviation from this cross-firm, average gap. For instance, a firm, with an above-than-average gender wage gap, would have a positive ε_{jg}^G for male and a negative ε_{jg}^G for its female workers. Let us now differentiate the equation with respect to productivity and take the difference of two groups G_2 and G_1:

$$\underbrace{\frac{\partial \Psi_{j2}}{\partial VA}|G_2 - \frac{\partial \Psi_{j1}}{\partial VA}|G_1}_{\text{Differential rent-sharing}} = \underbrace{\frac{\partial \tilde{\psi}_j}{\partial VA}|G_2 - \frac{\partial \tilde{\psi}_j}{\partial VA}|G_1}_{\text{Sorting w.r.t. rent-sharing}} + \underbrace{\frac{\partial \varepsilon_{j2}^G}{\partial VA}|G_2 - \frac{\partial \varepsilon_{j1}^G}{\partial VA}|G_1}_{\text{Bargaining diff. within firm}} \tag{2.15}$$

A simple regression of the form of Equation 2.12 would capture the left-hand side term, which is while being a good estimator of differential rent-sharing is in fact a composite of two additive terms. Differences in the first component capture sorting differences with respect to the rent-sharing elasticity of firms, representing that how different is – on average – the average rent-sharing propensity of firms where the members of the two groups tend to work. For instance, a difference in the estimated rent-sharing parameters of males and females could emerge even if males and females only select differently into firms of different (average) elasticites. The second difference on the right-hand side, however, captures the difference in parameters what we would get by adding firm fixed effects to the regression on firm-group effects in Equation 2.12. The parameter from such a regression in any group would indicate that – on average – how much more or less members of the given group benefit (relatively) from additional productivity *within* their respective firms. For instance, low and mid-educated workers may receive a below average share of rents, while the most educated workers may benefit more from such changes even at the same employer. Following Card et al. (2016) we may interpret this term as a measure of differences in bargaining power of the different groups – as an analogue to their decomposition of wage differentials. We believe, that differentiating between these two components is important to further our understanding of differential rent-sharing (a potential driver of wage inequalities), therefore in Section 2.4.3 we will report both between-firm and within-firm differences in a graphical form – with the corresponding regression tables being presented in Appendix B.3.

2.4 Results

2.4.1 Comparison of conventional and novel methods

Table 2.2 comprises our results estimated by OLS for the seven different specifications defined by the combination of the available wage measures (individual wage, firm effect or firm-year effect) and the included fixed effects defining the level of variation in wages and productivity we are interested in, corresponding to within-sector cross-sectional, within-firm longitudinal and within-match (stayer) designs. Across the conventional models using log wages, we observe that while the simple cross-sectional measure of the pass-through rate is is 0.34, by relying only on within-firm variation of productivity, we find an effect of almost one fifth of the size, 0.07.[94] This substantial drop in the magnitude of the parameter may be either a result of capturing less of the long-term effects of productivity changes than in the cross-sectional model or may be due to the absorbed effect of confounders such as firm-specific wage schemes or compensating differentials. Although firms can change or upgrade their skill composition over time, the role of this confounder seems way less substantial in the within-firm design. Still, assessing observed and unobserved individual heterogeneity – by the inclusion of match fixed effects – further decreases the parameter to 0.05. We have to note, however, that this difference may be also driven by the sample selectivity imposed by relying (more) on long-term stayers at the firms.[95]

[94] By omitting even the sector fixed effects, and hence not controlling for inter-sectoral differences, the first parameter would be 0.49. This is consistent with the findings of Carlsson et al. (2016), Card et al. (2018) and Friedrich et al. (2019) who also find larger effects of inter-sectional productivity differences.

[95] Appendix B.4 compares our within-match specification to more traditional formulation of stayer models. The parameters of our designs are the closest to stayer models with observation windows of 5-6 years, and are considerably larger than those of shorter windows.

Table 2.2: Rent sharing elasticities from conventional and novel specifications (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Within:	sector	sector	sector	firm	firm	match	match
Outcome:	lnW	ψ_{jt}	ψ_j	lnW	ψ_{jt}	lnW	ψ_{jt}
Design:	CS	CC(H)K^a	CC(H)K	Long.	Ours	stayer	stayer^a
Equation:	Eq. 2.2	Eq. 2.6^a	Eq. 2.6	Eq. 2.3	Eq. 2.7	Eq. 2.4	Eq. 2.4^a
Ln VA	0.346	0.159	0.153	0.072	0.053	0.048	0.046
	(0.010)	(0.006)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)
Obs. (1000)	395	349	363	368	326	41,688	34,676
R^2	0.618	0.538	0.525	0.950	0.901	0.897	0.936
#units	45	44	44	61751	54362	3415K	2862K

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. The specifications with firm-level outcomes are estimated using weighted least squares. All parameters significant at p<0.001. Additional controls include logarithmic firm size and its square. The first set of models include 45 sector fixed effects defined as the interaction of fifteen industry categories and (domestic private, public or foreign private) majority ownership. Specification (3) follows Card et al. (2016) and Card et al. (2018), specification (6) is what we refer to as a *stayer* design, and specification (5) is the estimator we propose. Models denoted with superscript a correspond to modified versions of the given model, with firm-year effects as the outcome variable. Models were estimated in Stata17, using *reghdfe* by Correia (2017).

Relying on the approach of Card et al. (2016) and Card et al. (2018) – in specification (3) –, we also see a substantial drop in the cross-sectional parameter estimates after controlling for skill composition through using firm-level wage premia as the outcome variable.[96] This pattern also suggests a substantial role of the worker composition of firms in defining the cross-sectional relation of productivity and wages.[97] Still, this parameter is three times larger than the one provided by the within-match design. As Card et al. (2018) discuss as well, the difference between their estimation and the stayer models may originate in the role of amenities as important wage-defining firm characteristics, the larger attenuation bias and larger role of transitory shocks – against which employers insure workers (Guiso et al., 2015) – in longitudinal models, and the selectivity bias in the within-match model.[98]

[96] The recent estimations of Criscuolo et al. (2021) for Hungary (for a shorter time period) fall between these two specifications, as they control for the observed observed skills of workers, but not for unobserved heterogeneity.

[97] Also the way larger difference between specifications (1) and (3) and between specifications (4) and (6) suggests that within-firm designs already control for a substantial part of the composition problem, as between-firm composition differences seem more important, than the variation in workforce over time at the same employer.

[98] The authors argue, however, that the latter plays only a small role, based on findings in Card

By estimating the novel specification proposed in Section 2.2.5, we may be able to tell slightly more about what causes the differences across models, as – based on the arguments made previously – this approach removes the issue of selectivity appearing in the stayer design – and to a lower extent, in the AKM approach as well. We start by noting that in the model relying on cross-sectional variation, specification (2), the estimated elasticity of firm-year fixed effects is not significantly different from the one using conventional firm effects.[99] Utilizing the notion that firm-year effects can vary over time, in Specification (5) we estimate the response of such effects to within-firm, longitudinal changes in productivity. We find that the parameter estimated this way is almost as different from the cross-sectional firm effect specification as from the stayer design, but still smaller than the within-firm model with conventional wage as the outcome measure. This is not surprising, as this model also takes care of most confounder issues, and relies on longitudinal variation in productivity.

However, the proposed estimator is devoid of the selection issue, which we suppose could bias upward the estimations relying on variation of wages of incumbent workers. As composition effects are already taken care of, the difference between specification (5) and the last two columns of the table, should come from only the selectivity of workers with shorter employment spells. Contrary to our expectation, we find larger parameters in the novel design, which would suggest that instead of the long-run stayers of the firms, individuals with short spells have higher rent-sharing elasticites. This is both against intuition and the findings of Juhn et al. (2018), who finds smaller pass-through rates for newcomers to the firms compared to workers with at least one year of tenure. However, as the employment spells used in the latter specification contain shorter observation windows than the full firm histories used in the within-firm designs, a somewhat larger role of measurements errors may also cause the differences. Therefore, for the sake of proper comparison we need to use an instrumental variable approach for reducing the role of measurement errors.

et al. (2016).

[99]This finding itself is in line with Lachowska et al. (2020), who show that the AKM firm effects are quite robust for allowing them to vary across time.

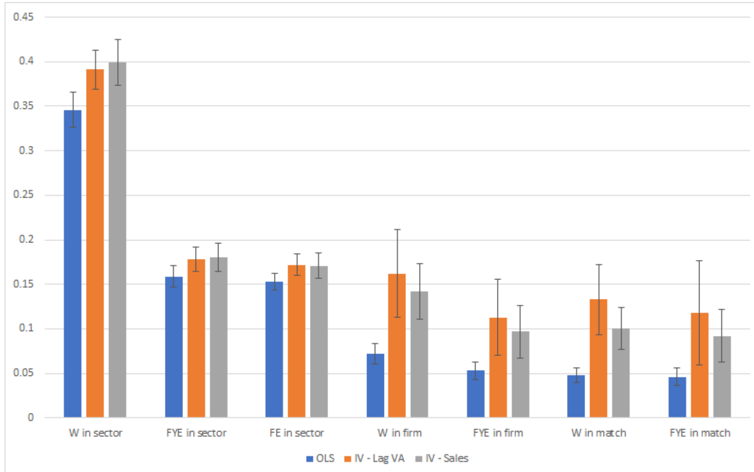


Figure 2.3: Rent sharing elasticities from conventional and novel specifications (OLS and 2 IVs)

Notes: Regression estimations are presented in Table 2.2 and Appendix Tables B.1 and B.2. Models were estimated in Stata17, using *ivreghdfe* by Correia (2018). The first columns in each set contains the OLS estimate, while the second column relies on using the one-year lag of firm value added (per worker) as an instrument for contemporaneous productivity. The third columns use a bracketed sales instrument from a 3-year window.

In order to limit the role that attenuation bias originating in the measurement errors of productivity – and its differing relevance across models – plays in shaping our estimated parameters and comparisons, we rely on an instrumental variables approach. We apply two different internal instruments established in the literature, namely the lagged values of productivity and the bracketed sales – mean (log) sales of three consecutive years – of the firm in the given year. From Figure 2.3, presenting these estimates alongside the OLS results of Table 2.2, the relevance of the attenuation bias is evident. In specifications using either of the instruments, while all parameters turn out larger than the corresponding OLS estimates, the increase is almost a magnitude larger for within-firm and within-spell designs. The difference between the AKM-based cross-sectional models – which are barely affected by instrumentation – and the longitudinal specifications become most less substantial, suggesting a much smaller role of differences in the not skill-related components of the firm-level wage premia, like compensating differentials for disamenities. Notably, the differences between our model – fifth specification – and stayer designs – sixth set of columns –, that we could previously attribute to a selectivity issue, now has the expected sign. However the difference is neither significant statistically, neither seems substantial in magnitude. Hence, it seems that although the specification with TV-AKM firm-year effects is the theoretically superior way to estimate rent-sharing elasticites – if data and computational constraints are not limiting –, the innovation it makes does not translate into major practical implications, at least when tested against our dataset. This does not mean, however, that in other countries or different datasets the selectivity bias of stayer designs should be as negligible as it turned to be for Hungary.

2.4.2 Heterogeneous effects across firms

In this sub-section, we estimate models of the form proposed in 2.11 for the three main specifications of Section 2.4.1: the approach of Card et al. (2016) and Card et al. (2018), the within-match (stayer) wage design and the within-firm specifications with time-varying firm-year effects. All models will be estimated by using the lagged productivity of the firm as an instrumental variable.[100] We will assess differences in rent-sharing propensity along the majority ownership of the firm, the main industry of the economy the firms operate in and size categories.

All results are presented in Figure 2.4. Although we know that foreign-owned firms pay the highest wages and have the highest average productivity, results from the within-firm and within-match designs suggest that the relation of these two measures within the set of such firms is smaller than for domestic-owned counterparts. Quite interestingly, the cross-sectional specification does not reveal such pattern. The discrepancy between the implication of the models may suggest either that within the set of foreign-owned firms the role of amenities or other wage components is weaker – putting a smaller downward bias on the parameter –, or that wages paid by foreign employers react less harshly to transitory or short-term productivity changes – as they may insure their workers more against such fluctuations.

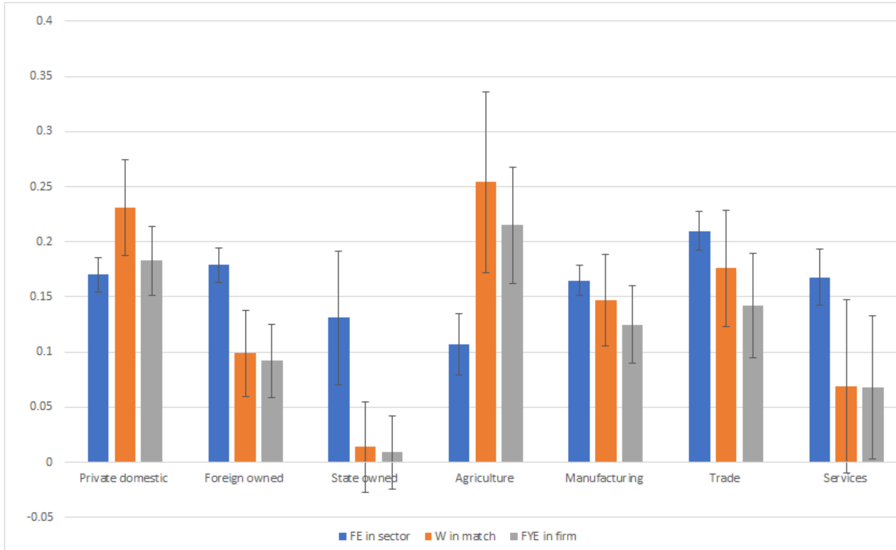
The relevance of the latter channel seems an important factor in explaining the emerging patterns in our estimations across different industries as well. While based on the cross-sectional estimations, wages in agriculture show the weakest reaction to (between-firm) productivity differences, the relative order of parameters between industries is almost reversed in the longitudinal approaches. As productivity in agriculture can change quite substantially even on a yearly basis – due to either the direct effects of local weather or from spillovers across the production chain –, and as most agricultural work contracts are short-term and seasonal this finding is not surprising. Nevertheless, the fact that differences in transitory and long-term reactions are so strong that the ordering of industries based on the pass-through rates can reverse – based on the choice of specification – is a formidable find, and highlights the importance of model selection. We note, however, that as the ownership and sectoral models were estimated separately, the findings in the former may also be partially explained by composition effects with respect to the latter categories, considering for instance the low rate of foreign capital in agriculture.

The bottom panel of Figure 2.4 also presents results based on average firm size.

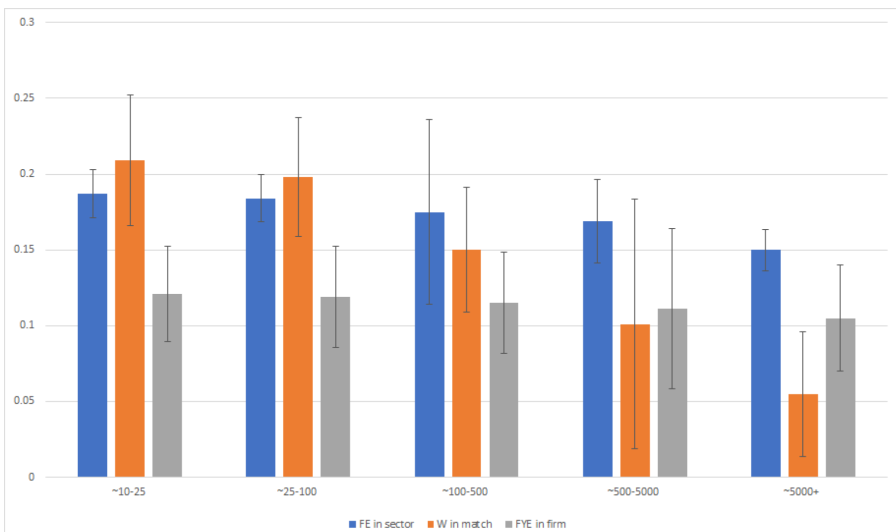
[100] Technically as many instruments are included as the number of interacted parameters of interests, which we generate by interacting the lagged productivity measure with the corresponding firm types, similarly as Juhn et al. (2018).

Interestingly, in stayer-focused models – the middle columns – a strong downward trend is apparent as we focus on larger and larger firms, while we find no such pattern or only a very weak one in models using AKM or TV-AKM firm(-year) effects as the outcome measure. One possible way to interpret this discrepancy is that fluctuation of workers is larger in small firms, hence the identifying samples relating to small firms may be more effected by sample selectivity. This pattern, however, should be investigated in more depth in the future.[101]

[101] We also estimated a model with 45 different elasticities for 45 sectors defined by joint categories of fifteen industries and three ownership categories, but due to computational constraints we could obtain only the OLS estimates – and not the IV ones – for that number of parameters. We checked whether the estimated parameters move in tandem with some sector-level aggregates – such as mean productivity, wages, AKM firm-effects or the average firm size in the given sector –, but did not find any noteworthy patterns.



(a) Ownership and industries



(b) Firm size

Figure 2.4: Rent-sharing elasticites across sets of firms – IV estimates

Notes: In all set of columns, the first estimation relates to the heterogeneous parameter estimates based on Equation 2.6, the second to those based on Equation 2.4, and the third column relates to the model we propose in Equation 2.7. Regression estimations are presented in Appendix Tables B.3, B.4 and B.5.

2.4.3 Differential rent-sharing

In this section we turn our focus to differences across groups of workers instead of sets of different firms. Although this question could be assessed using any of the specifications presented in this paper, including within-match designs, we rely on the approach of Card et al. (2016) and Card et al. (2018), estimating AKM models allowing for different firm effects for worker groups. However, motivated by the former study and using the methodology presented in Section 2.3.3, we also differentiate between the within-firm (bargaining) differences in rent-sharing elasticities and the potential sorting mechanisms through which different groups of workers may end up in firms with systematically larger (or smaller) rent-sharing propensities. This modification is important, as the sorting channel could either aggravate or dampen the within-firm differences – originating in either different bargaining power or outside options of subsets of workers –, or may even signal the presence of (taste-based) discriminatory practices of employers. Beside presenting results on gender and education – which have been assessed by other authors as well – , we will use a job (firm-occupation) effect model to assess occupational differences, and also test for differences along the seniority of workers – measured by age and the completed tenure at the firm.[102]

Our first set of results focuses on rent-sharing differences by (proxied) education and gender. As Figure 2.5 suggests, the baseline estimation relying on cross-sectional variaton in both productivity and firm-group effects shows significant differences between the least and the most educated groups, with the estimated elasticities being around 0.16 and 0.21 for the two groups respectively. However, part of this difference is attributable to the fact, that firms who employ more skilled workers also tend to share rents to a higher extent with all of their workers. Still, by focusing on firms employing more than one type of workers, we find a within-firm difference of 0.026, which is around half of the size of the cross-sectional difference.[103] This finding is consistent with the literature. While Card et al. (2018) finds no substantial difference with respect to the education of workers, Gürtzgen (2009) finds larger pass-through rates for more skilled workers but also parameters of similar size for white-collar and blue-collar workers.

Regarding gender, the sorting channel reflects virtually no difference in sorting with respect to the average rent-sharing of firms, while both the cross-sectional and within-firm approaches yield a significant difference in rent-sharing elasticites, with the latter difference being of the same magnitude as the one between low and high-educated workers.[104] Whether this difference originates in gender-based

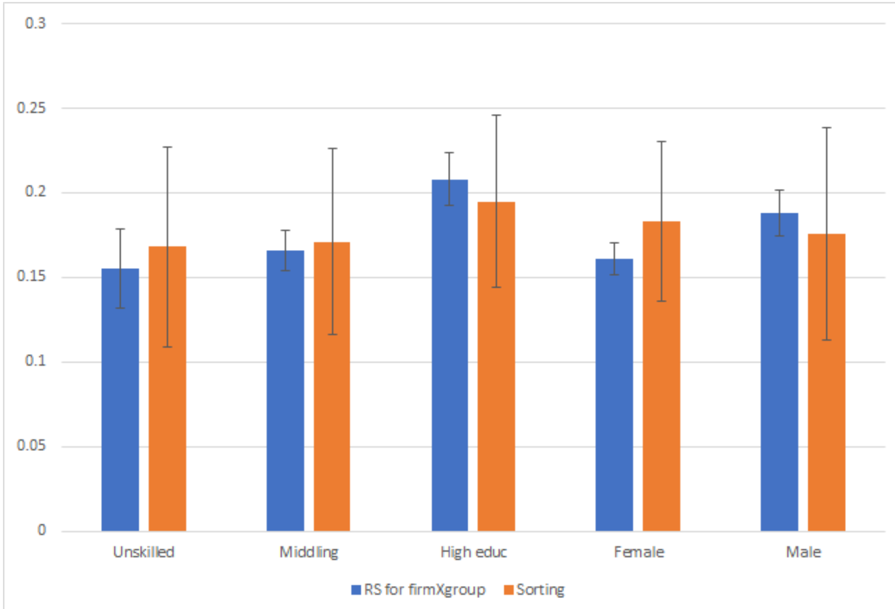
[102]Results estimated by the within-match design of Equation 2.4 are also presented in Appendix C – without further discussion –, as an additional column of the tables presenting the regression results for the specifications of this section.

[103]Although the plotted confidence interval suggest otherwise, the difference is significant at the 0.05, but not at the 0.01 level – as the t-tests of Appendix Table B.6 suggest. We note however, that these standard errors might be somewhat underestimated due to limited mobility bias, increasing the uncertainty of the differences we interpret.

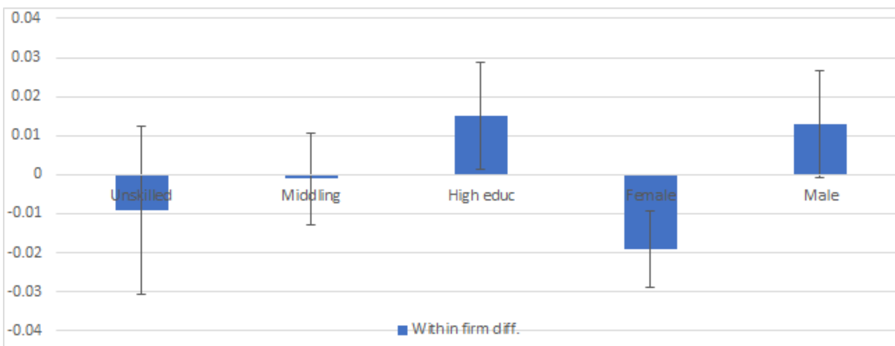
[104]Using a simpler, cross-sectional approach Criscuolo et al. (2021) investigates differences in pass-through rates by both gender and two skill categories across many OECD countries. For most countries, including Hungary as well, they found somewhat higher elasticities for males and high skilled workers, with the differences being of similar magnitude as presented here. These

differences in bargaining power or outside options, or being present due to simply occupational composition differences between the male and female halves of workplaces remains an open question. In Section 2.4.4, however, we further investigate the heterogeneity of this difference by sectors and across occupations and will present evidence that suggests that the differences cannot be solely accounted to composition effects, and that the same gap persists across a range of occupations and firm types.

patterns are also significantly present in the studies of Gürtzgen (2009), Card et al. (2016) and recently in Sin et al. (2020).



(a) Overall and between differences



(b) Within-firm differences

Figure 2.5: Differential rent sharing elasticities across gender and education

Notes: The left columns in the top panel present differential rent-sharing estimates based on Card et al. (2016) and Card et al. (2018). the right columns in the top panel correspond to the effects of productivity on the average firm-premia of firms where the given individuals are employed, illustrating sorting differences. The bottom panel presents parameters from regressions of firm-group effects on productivity, with firm fixed effects included on the right hand side, capturing within-firm differences in rent-sharing behavior compared to the average level of rent-sharing of the corresponding firm. Parameters of the regression estimations are presented in Appendix Tables B.7 and B.6.

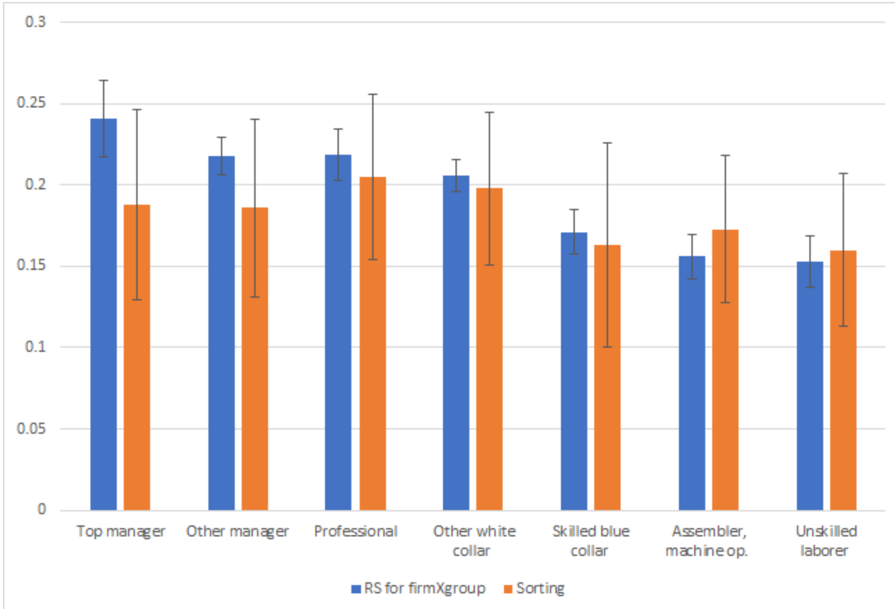
To assess the role of occupational differences, we will rely on the job effect model introduced by Boza (2021), in which we allow different firm effects to vary across broad occupation categories.[105] As Figure 2.6 suggests, similarly as for education, the large overall differences are accompanied by a sorting pattern as well.[106] Workers in superior positions tend to work in firms where the average wage premia – that would be captured by the standard AKM model – reacts more strongly to productivity changes. We note, that these average differences could be in part driven by the higher representation of workers bearing strong bargaining power in these firm, which may spill over into the wages of all workers. Despite the high uncertainty of our IV estimations, less prestigious categories seem to significantly differ from the managerial occupations in the within-firm design. The within firm difference between the least skilled workers and top managers is only 0.059 in within-firm comparisons, and 0.088 in the cross-sectional ones. While these differences seem substantial, we would not consider the around 0.25 elasticity of managers as extreme high or the around 0.15 elasticity of unskilled laborers as extreme low.

Finally, we check whether workers of different age or of different completed tenure at their current firms benefit differently from productivity changes. Based on Figure 2.7, while the estimated differences are not statistically significant, it seems that young workers, below the age of 26, receive a somewhat lower share of the rents at their employers, although this pattern can be potentially driven by the lower share of high-education workers in this cohort. On the other hand, the within-firm differences according to tenure at the firm suggests a slight disadvantage of new-entrants of the firm or a better bargaining position for more senior workers at the firm – with the difference being significant in both the cross-sectional and the within-firm designs. This pattern is in line with Kline et al. (2019), who find that patent induced revenues pass through into wages of senior workers, but not into entry wages.[107] Although the differences are not substantial, this pattern may explain the differences between models that rely on workers who stay at firms for multiple years to a different extent – for instance, the difference we found between the stayer and TV-AKM based specifications in Figure 2.3.

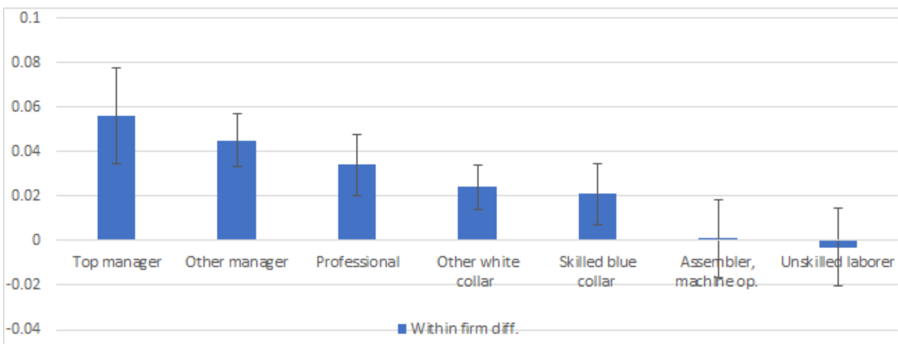
[105] The grouped-AKM model includes 37 categories based on 2-digit occupation codes, nested in 7 broader categories, which latter we will use in the rent-sharing regressions of the form of Equation 2.12.

[106] As our education variable is only a proxy – based on the occupation with the highest educational requirement achieved by the individuals during our data window –, we expect that results based on the two variables to be strongly correlated due to those working in professional occupations always having high quasi-education as well.

[107] Juhn et al. (2018) also finds slightly higher pass-through rates for workers with at least one-year completed tenure at the firm – at least in retail and professional services and conditional on that the workers remain incumbent to the firm for at least four consecutive years.



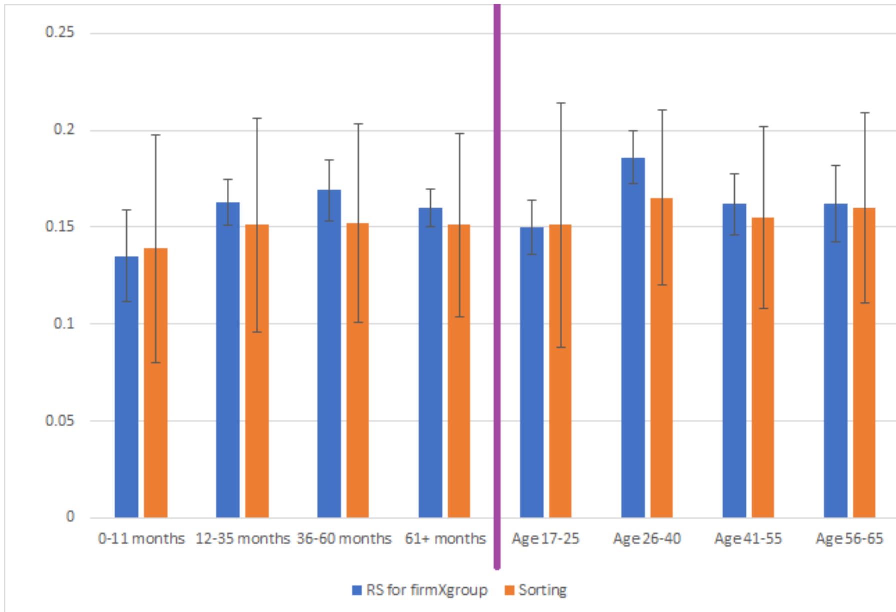
(a) Overall and between differences



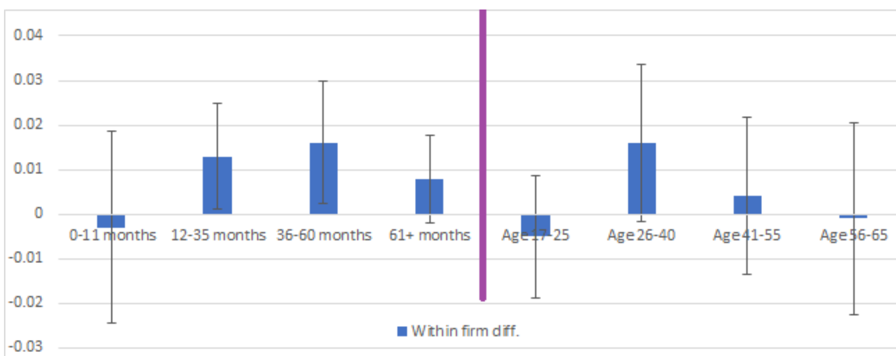
(b) Within-firm differences

Figure 2.6: Differential rent sharing elasticities across occupations

Notes: the right columns in the top panel correspond to the effects of productivity on the average firm-premia of firms where the given individuals are employed, illustrating sorting differences. The bottom panel presents parameters from regressions of firm-group effects on productivity, with firm fixed effects included on the right hand side, capturing within-firm differences in rent-sharing behavior compared to the average level of rent-sharing of the corresponding firm. Regression estimations are presented in Appendix Table B.8.



(a) Overall and between differences



(b) Within-firm differences

Figure 2.7: Differential rent sharing elasticities across age and completed tenure

Notes: the right columns in the top panel correspond to the effects of productivity on the average firm-premia of firms where the given individuals are employed, illustrating sorting differences. The bottom panel presents parameters from regressions of firm-group effects on productivity, with firm fixed effects included on the right hand side, capturing within-firm differences in rent-sharing behavior compared to the average level of rent-sharing of the corresponding firm. Regression estimations are presented in Appendix Tables B.9 and B.10.

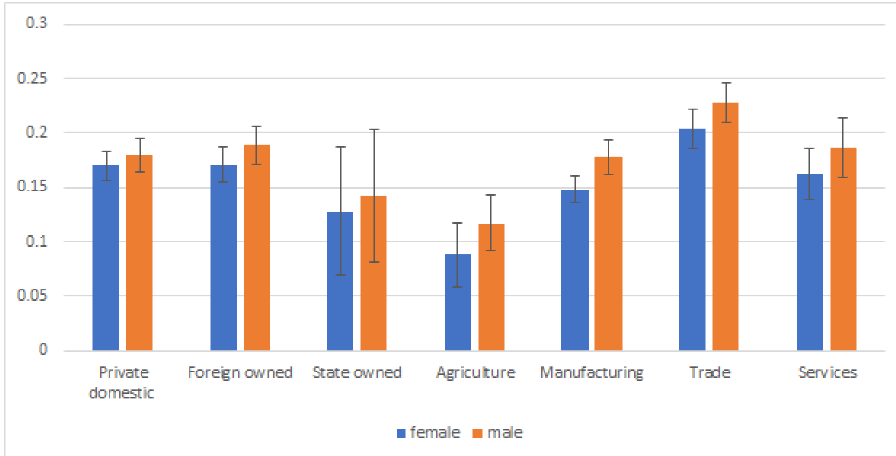
2.4.4 Gender differences across firms and occupations

Noting that given sufficient data, the fixed effect approaches presented in this study can be combined to some extent, we aim to investigate differential rent-sharing with respect to gender in more detail. For our first such exercise, we combine the approaches of Equations 2.11 and 2.12 to provide further evidence on the nature of gender differences, and re-estimate the model allowing for gender differences to also differ across sectors – based on either ownership or broad industry categories. In the bottom panel of Figure 2.8, we also include the within-firm estimations we introduced besides the sorting differences and the cross-sectional estimator proposed by Card et al. (2016) and Card et al. (2018). Based on both panels of the figure, it seems that while rent-sharing elasticities vary across sectors – as already presented in Figure 2.4 –, the gender differences in *absolute* terms are quite stable, and are significantly present within all segments.[108] Accordingly, the largest *relative* differences can be found in agriculture, where women benefit substantially less than males from productivity differences.

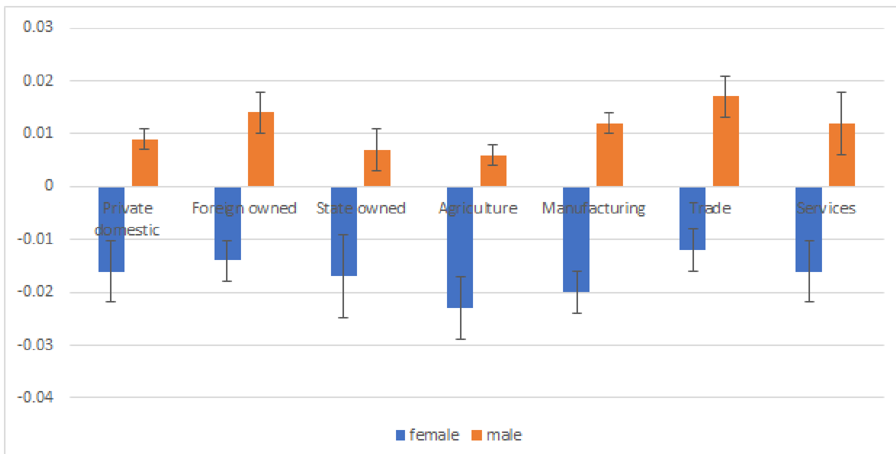
Finally, we estimate a modified AKM model with gender-firm-occupation cells as the units of wage aggregation. Using these, we estimate the overall and within-firm differences in the estimated elasticities of the corresponding firm-group effects with respect to productivity changes. Accordingly, Figure 2.9 assesses gender and occupational differences at the same time. Somewhat shockingly, we can observe that within the same firms even the less skilled male workers receive the average level of productivity rents, while the elasticities of female managers are even smaller than of any of the skilled male worker categories. This comparison is just slightly less severe regarding the cross-sectional patterns. While for skilled occupations the around 0.03 difference in elasticites between males and females persist, the differences for unskilled workers are somewhat more moderate or even insignificant.[109] These patterns suggest that the lower pass-through of productivity into female wages is not generally driven by occupational – and therefore probably not by educational – differences. Instead, other underlying factors – such as differences in willingness to bargain or the propensity to leave firms, or in worst case the presence of pure (taste-based) discrimination on the employers side – should be investigated in more detail. Also whether the gender difference in rent-sharing is more prominent for low-tenure or young worker groups – for instance for those around child-bearing age – could be a prominent direction for further research, for which the framework used in this chapter could serve as a methodological basis.

[108] The substantially smaller standard errors in the bottom panel suggest, that there is a large variation for both genders in what kind of firms – regarding their firm-level pass-through – they work at, even if on average they select into similar firms. Within every given firm, however, differences across the two groups become more evident. Interestingly, the only category in which the cross-sectional parameters of males and females do not differ significantly is the group of private domestic firms.

[109] While within-firm gender differences are significant for all occupations, except for assemblers, the overall differences for unskilled laborer are insignificant as well.



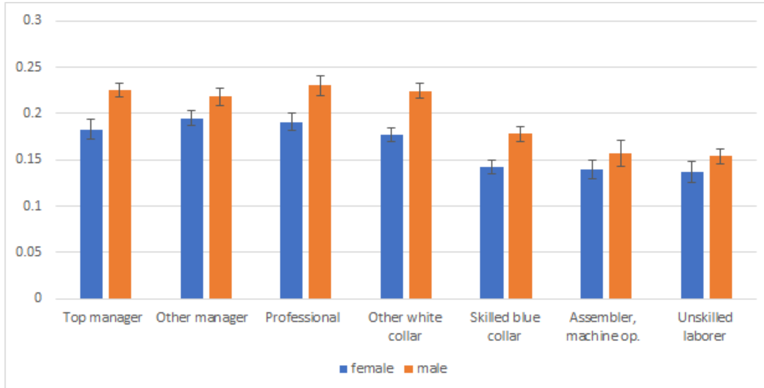
(a) Overall differences



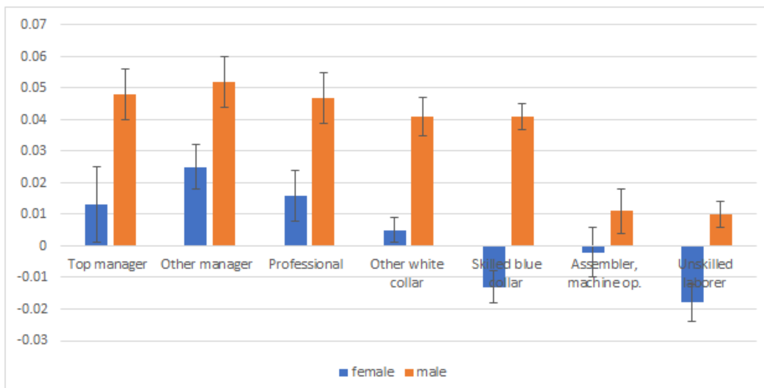
(b) Within-firm differences

Figure 2.8: Gender X heterogeneity results

Notes: the right columns in the top panel correspond to the effects of productivity on the average firm-premia of firms where the given individuals are employed, illustrating sorting differences. The bottom panel presents parameters from regressions of firm-group effects on productivity, with firm fixed effects included on the right hand side, capturing within-firm differences in rent-sharing behavior compared to the average level of rent-sharing of the corresponding firm. Regression estimations are presented in Appendix Tables B.11 and B.12.



(a) Overall differences



(b) Within-firm differences by firm-type

Figure 2.9: Gender X Diff results

Notes: the right columns in the top panel correspond to the effects of productivity on the average firm-premia of firms where the given individuals are employed, illustrating sorting differences. The bottom panel presents parameters from regressions of firm-group effects on productivity, with firm fixed effects included on the right hand side, capturing within-firm differences in rent-sharing behavior compared to the average level of rent-sharing of the corresponding firm. Regression estimations are presented in Appendix Table B.13.

2.5 Discussion

As our study makes both methodological and empirical contributions, we consider multiple main strands of future research that could potentially build on the presented findings. First, as the differences between the within-spell, match fixed effect designs and the conventional approach of taking (first) differences – in the wages of incumbent workers and the firms' productivity – could be explored in a more rigorous way. The implications for standard errors and the role of measurement errors or transitory fluctuations, as well the properties of the instrumental variable estimation techniques in this setting could be assessed both formally and by using simulation techniques. Most importantly, testing whether the issue of sample selectivity is more severe in other countries, datasets or time periods would be important to ultimately assess the importance of our methodological innovation. Although in our empirical exercises selectivity turned out to be of minor importance, this finding may not necessarily generalize to other scenarios.

The assessment of heterogeneity in rent-sharing elasticities across sectors could be investigated in more detail, for instance following the very promising research designs presented in Criscuolo et al. (2021), relating cross-sectoral differences in pass-through rates across local labor markets with different employment dynamics. By providing evidence that pass-through rates vary – between markets with different levels of vacancies, worker fluctuation or worker concentration – according to what monopsony theory would imply, these models could be (partially) tested against explanations relying on the search models, which derive rent-sharing elasticites from assuming a bargaining power of workers and a rent-sharing behavior of employers.

Considering differential rent-sharing, a focus for future research follows naturally from the findings of this study and the one of Boza (2021) – the first chapter of this thesis –, presenting substantial wage differentials among groups of workers within the same firms, as part of these differentials could be accounted to the differing rent-sharing propensity of firms. The actual contribution of this channel could be quantified using proper decomposition methods. For instance, we would expect that only part of the within-firm gender differences in wage could be explained by the differences in rent-sharing elasticites, and hence only a part of within-firm gender gaps could be confidently interpreted as differences in bargaining – with the unexplained part corresponding to discrimination, for instance. For a proper assessment, the differences in pass-through rates should be related to the level of different amenities or waging-schemes across firms, investigating for instance, whether male workers tend to have a higher taste for performance pay or overtime work – building on the work of Sorkin (2018). Finally, gender differences in rent-sharing elasticites – and in the level of wages – could be investigated in more detail across the changing life situations of individuals, such as before and after maternity (or paternity).

10.14754/CEU.2022.01

3 Chapter 3: Decomposition of Co-worker Wage Gains

Joint work with Virág Ilyés, published as Boza and Ilyés (2020)

3.1 Introduction

The group of former co-workers forms an essential part of our social networks. As early survey-based evidence demonstrated, one's co-worker acquaintances can be essential sources of job-related information and they might have an important role in the job acquiring process as well (Corcoran et al., 1980; Granovetter, 1995; Holzer, 1988). Beside this type of studies, which typically exploited self-reported information about the individuals' job search process, in recent years many 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 & Rosolia, 2012; Glitz, 2017; Saygin et al., 2019), tenure length and turnover (Glitz & Vejlin, 2019), and quite notably, wages (Glitz & Vejlin, 2019; Hensvik & Skans, 2016). The explanations for the existence of these beneficial effects mostly highlighted the role of two mechanisms: information transmission and employee referral.

In this paper 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 estimations, 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 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 & Vejlin, 2019). However, a few other papers revealed additional channels through which gains are generated. Hensvik and Skans (2016) showed that homophily in the 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 has 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 referral activity of former co-workers via 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. Secondly, to assess in depth the presence and relative importance of selection channels in overall wage gains, we augment and apply the decomposition method proposed by Woodcock (2008). To interpret our findings, we link differences in wage components to 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, is considered less probable.

In order 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 co-worker links. The superior skills of new hires will be only responsible 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 from 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.

The rest of the paper is structured as follows. Section 3.2 summarizes previous empirical and theoretical literature and based on those systematically presents the channels through which wage differences could be generated. Section 3.3 establishes our model and proposes a decompositon strategy. Section 3.4 presents the utilized dataset, the definition of co-worker links and discusses identification issues. Section 3.5.1 contains the main results of the decompositions, Section 3.5.2 provides additional evidence by utilizing exogenous job losses, while Section 3.5.3 presents alternative estimation strategies for capturing referral and information transmission effects. Finally, Section 3.6 concludes.

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 on 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 a form of recommendation.[110] To this distinction we would add an additional layer of cases, when, upon hiring a new applicant, the referrer continues to act as a provider of information, either about the applicant's behaviour 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 the various potential components of wage gains generated by former co-workers and also 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 the 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 (Ekinci, 2016; Saloner, 1985; Smith, 2005) – and indirectly as well, if the applicants will increase their productivity to compensate the referrers' favor (Smith, 2005). Also, referrers might have an important role in the integration of workforce, as their presence might support smooth knowledge sharing and better cooperation at work (Castilla, 2005; Fernandez et al., 2000). The enhanced productivity of workers and lower monitoring costs could both 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

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

compensated by the firm as *presence* effects.[111]

Beside 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 are equally important as focusing on causal channels only.

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 the firm's wage level and the individual's outside options.[112] Dustmann et al. (2016) show that the wage prospects of non-referred 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 have generally better skills and would earn more at any firm compared to someone with similar observable characteristics.[113] As referrers can decrease screening costs either by providing information about their former co-workers or by signalling worker quality with their own productivity based on the assumption of network homophily in productivity (Hensvik & Skans, 2016; Montgomery, 1991; Munshi, 2003), they can contribute to the reduction of information asymmetry about the applicants' general characteristics.[114] This way firms may avoid the low-quality workers and on average hire better quality applicants, even if they are not better matched ones (Saloner, 1985; Ullman, 1966).[115]

Selection to 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 & Jackson, 2004, 2007; Granovetter, 1995), and their information might mitigate

[111] 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 et al., 2015).

[112] 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 et al., 2015).

[113] 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 their relevant acquaintances or only to those who would be a good fit for the specific opening.

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

[115] 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 our study.

the job-seekers' uncertainties about the possible employers (Tate, 1994; Wanous, 1980). By choosing from a larger set of vacancies, the expected quality of one's new firm could be 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 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 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 think within the above framework to interpret the results.

3.2.2 Empirical evidence

In this section, we survey recent empirical evidence coming from papers that are based on matched employer-employee administrative data and focus on wage effects of various social contacts. 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. Our paper is related to both lines of research, both in theoretical approach and the utilized methods as well.

In order to study the role of employee referrals, Glitz and Vejlin (2019) 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.[116] Besides, they also demonstrated that the initial wage gains of 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 also 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 hires 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

[116] This difference also includes gains related to the superior unobserved quality of workers hired this way.

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 on referral coming from survey data. Their model of wages incorporated both individual and firm fixed effects, which account for the non-random 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 et al. (2019) 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 though the co-worker networks. Furthermore, Saygin et al. (2019) also found 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 more likely become co-workers of their neighbors from the same block as the individual, than those from their broader neighborhood. After estimating an AKM (Abowd et al., 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 argues that employee referral in itself can not explain this set of results, and that information transmission over the job information network also has to play a critical role.

Beside additional evidence on selection patterns and homophily, inequality consequences are also documented in Eliason et al. (2019). The authors constructed a proxy of local labor market for displace 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 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 scope of our paper, 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 paper 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 (2019), and using multi-way fixed effects approach similar to Dustmann et al. (2016). However, we utilize a framework which can capture selections induced by co-workers as well. 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 tries to assess 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 at, and firms can increase the quality of their worker pool with referral hires.

3.3 Model and Empirical Strategy

To investigate the mechanisms discussed in the Section 3.2.1 we estimate differences in specific wage components. We start by introducing an AKM model of wage-setting (Abowd et al., 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.

$$w_{ijt} = \alpha + \theta T_{ijt} + \beta_X X_{it} + \beta_Y Y_{jt} + \beta_Z Z_{ijt} + \delta_i + \gamma_j + \mu_{ij} + \pi_t + \varepsilon_{ijt} \qquad (3.1)$$

In Equation (3.1) w_{ijt} denotes the starting wage earned by person i at firm j at time t. X_{it} contains the observable characteristics of the individual, such as age and education. Y_{jt} comprises the properties of the firm, like sector and ownership. Finally, Z_{ijt} includes variables corresponding to the actual employment spell of individual i at firm j, among others 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}. Unfortunately, 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.

Beside 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), the wage levels of firms (γ_j), and the quality of the employer-employee match (μ_{ij}) 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}).

3.3.1 Identification of match and presence effects

Unfortunately, the proper estimation of the full model is unfeasible. To obtain the match effects, we would have to compare multiple entries to the same firm by the same person. Although such 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 Equation (3.1).

$$w = \theta T + \beta X + D\delta + F\gamma + G\mu + \varepsilon \tag{3.2}$$

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.

$$E[\theta_{TWFE}] = \theta + (T'M_{XDF}T)^{-1}T'M_{XDF}G\mu \tag{3.3}$$

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 ($G\mu$).[117] 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.[118]

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 very much 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 Equation (3.3), $(T'M_{XDF}T)^{-1}T'M_{XDF}G\mu$, 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. In Section 3.5.3, however, we attempt to bypass this limitation.

[117] 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.

[118] 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 paper.

3.3.2 Individual and firm selections

Beside 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}.

$$E[\theta_{OLS}] = E[\theta_{TWFE}] + \underbrace{(T'M_XT)^{-1}T'M_XD\delta}_{\psi_{ind}} + \underbrace{(T'M_XT)^{-1}T'M_XF\gamma}_{\psi_{firm}} \quad (3.4)$$

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 non-linked 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 into jobs or more prone to applying to firms with their acquaintances present.

The bias created by omitting 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 aggregate level as well, we further decompose the above introduced selection terms.[119]

$$\underbrace{(T'M_XT)^{-1}T'M_XD\delta}_{\psi_{ind}} = \underbrace{(T'M_{XF}T)^{-1}T'M_{XF}D\delta}_{\xi_{ind}} + \underbrace{(T'M_XT)^{-1}T'M_XF\gamma^S}_{\omega_{ind}} \quad (3.5)$$

$$\underbrace{(T'M_XT)^{-1}T'M_XF\gamma}_{\psi_{firm}} = \underbrace{(T'M_{XF}T)^{-1}T'M_{XD}F\gamma}_{\xi_{firm}} + \underbrace{(T'M_XT)^{-1}T'M_XD\delta^S}_{\omega_{firm}} \quad (3.6)$$

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 the value of its firm effects. Similarly, δ_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

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

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 (3.5), therefore, shows that the *average* difference in the worker effects between linked and non-linked hires is the sum of the average difference *within* firms (ξ_{ind}) where 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 characterise the firms that are generally accessed by these workers even when they are hired without links. Table 3.1 briefly summarizes all the introduced parameters.

Table 3.1: Summary of parameters in our model

		Parameter	Interpretation
0.		$\hat{\theta}_{OLS}$	wage differential between linked and non-linked hires, controlling for only *observed* worker and firm characteristics
1.		$\hat{\theta}_{TWFE}$	the wage differential between linked and non-linked hires, controlling for *unobserved* firm and worker heterogeneity (but not 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 non-linked hires
	2a.	$\hat{\xi}_{ind}$	the average of worker effect differentials between linked and non-linked 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 non-linked hires
3.		$\hat{\psi}_{firm}$	the average firm effect differential between linked and non-linked hires
	3a.	$\hat{\xi}_{firm}$	the average of firm effect differentials between linked and non-linked 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 Equation 3.1, 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 get the parameters of the proposed decompositions, we will estimate the following set of equations. First, we estimate the wage equation introduced in Equation (3.1), but without match effects.[120]

$$w_{ijt} = \alpha + \theta_{TWFE} T_{ijt} + \beta_X X_{it} + \beta_Y Y_{jt} + \beta_Z Z_{ijt} + \delta_i + \gamma_j + \pi_t + \varepsilon_{ijt} \tag{3.7}$$

[120] 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.

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 Equations (3.4), (3.5) and (3.6).

$$\hat{\delta}_i = \alpha_2 + \psi_{ind} T_{ijt} + \beta_{2X} X_{it} + \beta_{2Y} Y_{jt} + \beta_{2Z} Z_{ijt} + \pi_{2t} + \varepsilon_{2ijt} \tag{3.8}$$

$$\hat{\gamma}_j = \alpha_3 + \psi_{firm} T_{ijt} + \beta_{3X} X_{it} + \beta_{3Y} Y_{jt} + \beta_{3Z} Z_{ijt} + \pi_{3t} + \varepsilon_{3ijt} \tag{3.9}$$

$$\hat{\delta}_i = \alpha_4 + \xi_{ind} T_{ijt} + \beta_{4X} X_{it} + \beta_{4Y} Y_{jt} + \beta_{4Z} Z_{ijt} + \gamma_j^S + \pi_{4t} + \varepsilon_{4ijt} \tag{3.10}$$

$$\hat{\gamma}_j = \alpha_5 + \xi_{firm} T_{ijt} + \beta_{5X} X_{it} + \beta_{5Y} Y_{jt} + \beta_{5Z} Z_{ijt} + \delta_i^S + \pi_{5t} + \varepsilon_{5ijt} \tag{3.11}$$

$$\hat{\gamma}_j^S = \alpha_6 + \omega_{ind} T_{ijt} + \beta_{6X} X_{it} + \beta_{6Y} Y_{jt} + \beta_{6Z} Z_{ijt} + \pi_{6t} + \varepsilon_{6ijt} \tag{3.12}$$

$$\hat{\delta}_i^S = \alpha_7 + \omega_{firm} T_{ijt} + \beta_{7X} X_{it} + \beta_{7Y} Y_{jt} + \beta_{7Z} Z_{ijt} + \pi_{7t} + \varepsilon_{7ijt} \tag{3.13}$$

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

As Kline, Saggio, and Sølvsten (2020a) demonstrates, 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

[121] 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 $E[\gamma_{TWFE}] = \gamma + (F' M_{TXD} F)^{-1} F' M_{TXD} G\mu$ (Woodcock, 2008).

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 estimations. In our example, we could overstate the role of the firm component and understate the role of the individual component in the overall wage difference. The authors propose a correction method for standard errors, which accounts for this possibility, and corrects inference. Unfortunately, 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.[122]

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.[123]

Based on the above, it is important to note that we cannot tell what would happen in those sectors where hiring through links is not prevalent, or in population sub-samples where no such events are observed.[124] 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 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

[122] 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 estimations 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 coming from our main estimations 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 in accordance with the predicted consequences of limited mobility bias.

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

[124] Or in sectors where everyone is linked, or with persons who are always linked.

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.[125]

After discussing our main estimation 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 percent 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 was 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.[126] 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.

[125]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.

[126]While the source data contains all legal employment spells which generate social contribution obligations, in the final structure we do not observe employment spells that are shorter than one month and are not present on the 15th day of any 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 maximum 50 observed employees, before their reunion at another firm. Setting a limit on 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 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 twelve months in the past, they had reunited at a firm with a maximum of 250 observed employees[127], and the incumbent employees had arrived at least one month before their former co-workers did.[128] Also, as weaker social connections usually erode over time, we restricted the time that could pass between the two encounters to five 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 that met the above criteria. Among those who had no such relations, we differentiated three groups. Regarding two of these, we cannot observe any links 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 estimations, we included only those 15-65 years-old private-sector employees who had no more than 15 distinct employment spells over nine 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 twelve 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

[127] We restricted the possible size of the receiving firm, as our data does 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.

[128] 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 one month gap will reflect a 2 to 60 days difference between the starting dates of the two workers.

comobility in itself can provide a substantial wage premium (Marx & 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 in order 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 Appendix C.1). Hence we used the first three years of data as the connection-forming period, and only the later years (2006-2011) for estimations. As we focus on linked entries only in small and medium firms, we dropped entries from the non-linked groups at firms with more than 250 observed employees as well. In order 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 five 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.[129] Furthermore, as opposed to our definition, some connections may form in large companies or may not erode even after four years. Either allocating high-wage, linked workers to the low-wage, non-linked group, or vica versa results in a lower observable wage difference between the two groups. Therefore, both types of misclassification have the same effect on our estimations: the measured difference between the linked and non-linked groups will be lower than their true values and the estimated effects will be biased towards zero.

3.4.2 Definition of variables

To estimate the parameters of the model defined in Equations (3.7) through (3.13) 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 consists of the interaction of (quadratic) age and imputed

[129]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 non-linked hires would actually be linked given this sampling issue.

education[130], residence[131] 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 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 & Vejlin, 2019).

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 non-linked 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 distuinguished. Table 3.2 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 more fine 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. While the distribution of these observable characteristics is similar in the two groups, in our estimations we control for them. 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

130 Unfortunately, we can observe the actual level of education only in special cases. Therefore, in our estimations 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.

131 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.

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 differences in age, 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 less employers in the observation period. This latter may suggest another beneficial effect of links, longer expected tenure. Finally, this descriptive comparison also suggests that contacts reduce the average length of job search by around a half month.[132]

Regarding the other non-linked 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 less employment spells in the period and being on average younger than linked workers. Their wage advantage compared to the other non-linked groups might originate from working at large firms who, especially multinational employers, pay significantly higher wages in Hungary than their domestic counterparts (Köllő et al., 2020).

[132] Workers in the identifying sample for person effects, e.g. 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 difference mostly come from the requirement of observing more hiring events for these workers.

Table 3.2: Summary statistics: job entrants, freshly acquired jobs and receiving firms

	Sub-sample		Non-linked subgroups			Control group	
	Linked	Non-linked	First spell	W/o small firm experience	Control group	Always non-linked	Non-linked and linked
No. of observations	20227	*944579*	*135818*	*147600*	*661161*	*645253*	*15908*
Log of relative daily earnings	-0.470	*-0.580*	*-0.745*	*-0.468*	*-0.571*	*-0.572*	*-0.552*
Monthly earnings (HUF)	128511	*108053*	*80897*	127616	*109264*	*109245*	*110046*
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*
Occupation specific experience	70%	*47%*	-	*37%*	*59%*	*59%*	*69%*
Length of prev. unemp.	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 months 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 upper part of the table comprises the average wage outcomes of individuals upon entry to a new firm, the second section demonstrates the personal traits of workers, while the third and fourth sections contains the characteristics of the workers' new jobs and firms. Indented figures reflect statistically significant differences (p<.05) from the linked group, according to t-tests.

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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 Equation (3.7), then we decompose the gains according to Equation (3.8) through (3.13). Additionally, we calculate a pooled OLS panel regression (Equation (3.7) without any fixed effects) as well. The main results are presented in Tables 3.3 and 3.4, of which the former shows the results for the estimations in which the variable of interest is interacted with gender.

Table 3.3: 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	964806	501200	964806	964806	943643	571441	964806	964806
N_i	616386	197435	616386	616386	616365	223021	616386	616386
N_j	105818	61121	105818	105818	84655	105778	105818	105818
R^2	0.327	0.860	0.204	0.200	0.453	0.612	0.052	0.087

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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) consists 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 year. All controls are interacted with the indicators for first employment spells and for non-linked 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 the .05 level; ** at the .01 level; *** at the .001 level.

While the descriptive statistics (Table 3.2) demonstrate that there is a significant difference in raw earnings between linked and non-linked entrants, the OLS results indicate that even after controlling for observable characteristics, the difference between the two groups is still present.[133] We can observe a 4.65% wage gain for linked male and 3.13% for linked female workers compared to their non-linked counterparts. This gross premium is, however, composed of various elements.

[133] 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.

By estimating the two-way fixed effects model from Equation (3.7) 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 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 results in a significantly positive wage advantage. The magnitude of this estimation 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 non-linked workers due to their higher individual effects. Accordingly, more than a 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 signalling, 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 a sixth of the male wage difference (0.86%) is explained by the higher premium of firms linked individuals work at when they are linked.[134] 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 is not statistically significant. The most dominant element of their overall wage difference is the individual selection term.

Although $\hat{\psi}_{ind}$ and $\hat{\psi}_{firm}$ provide some insight into the average difference between linked and non-linked 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 ($\hat{\omega}$) unit components.

The $\hat{\omega}_{firm}$ 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 of $\hat{\xi}_{firm}$ 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 non-significant overall difference.

[134] 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.

Parameter $\hat{\omega}_{ind}$ 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 Hensvik and Skans (2016) and Glitz and Vejlin (2019), 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.[135]

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.

[135]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%.

Table 3.4: 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	964806	501200	964806	964806	943643	571442	964806	964806
N_i	616386	197435	616386	616386	616365	223021	616386	616386
N_j	105818	61121	105818	105818	84655	105778	105818	105818
R^2	0.327	0.860	0.190	0.200	0.443	0.612	0.052	0.086

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 and unskilled white-collar and blue-collar workers. Only the parameters for male workers are presented. For the list of additional controls, see Table 3.3. Standard errors are in parentheses and clustered at both firm-level and individual-level. *Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

Next, we investigated the effect of links in interaction with gender and occupation. Table 3.4 comprises the parameters for male workers.[136] 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.[137] Regarding this latter group, individual selection is the 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 gen-

[136] Parameters for the female occupation categories coming from the same regression are in Appendix Table C.1, while Appendix Table C.2 presents the model with only occupation categories not differentiated by gender.

[137] 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.

eral skills and productivity of workers ($\hat{\psi}_{ind}$), which is presumably more important in these occupations.

Selection into higher premium 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 drive 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 non-linked managers. Added together, these elements result in a lower expected wage for linked managers.[138]

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 5.3 we aim to 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 scholars often face in this literature is that employee movements are most often endogenous, especially job-to-job transitions. Papers focusing on reemployment outcomes via contacts naturally focus on exogenous job loss (e.g. plant closures, mass layoffs), 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 three months long period as exogenous job losses.[139] Then, we interacted our original proxy variable with mobility type (and gender). The results are presented in Table 3.5.

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

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

[139] 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.

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 of $\hat{\psi}_{firm}$ 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 & Storrie, 2006).

Table 3.5: 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***	0.0162**	0.0188***	0.0086*	0.0161***	0.0071	0.0027	0.0015
	(0.0058)	(0.0055)	(0.0041)	(0.0044)	(0.0037)	(0.0054)	(0.0025)	(0.0037)
Exog.	0.0620***	0.0423***	0.0103	0.0095	-0.0003	0.0366***	0.0106**	-0.0272***
	(0.0119)	(0.0117)	(0.0080)	(0.0087)	(0.0072)	(0.0107)	(0.0041)	(0.0073)
N	964806	501200	964806	964806	943643	571442	964806	964806
N_i	616386	197435	616386	616386	616365	223021	616386	616386
N_j	105818	61121	105818	105818	84655	105778	105818	105818
R^2	0.327	0.860	0.203	0.200	0.453	0.612	0.052	0.087

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 2. Standard errors are in parentheses and clustered at both firm-level and individual-level. *Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

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, some empirical artifacts. 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 wage 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 estimations 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.[140] Information flows, on the other hand, may be actually less common between different occupational levels.

Table 3.6: 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	964806	501200	964806	964806	943643	571441	964806	964806
N_i	616386	197435	616386	616386	616365	223021	616386	616386
N_j	105818	61121	105818	105818	84655	105778	105818	105818
R^2	0.327	0.860	0.204	0.200	0.453	0.612	0.052	0.087

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 3.3. Standard errors are in parentheses and clustered at both firm-level and individual-level. *Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

Table 3.6 comprises the results of the regression, in which we used the alternative proxies.[141] If the occupation of the links is similar compared to the job

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

[141] In the upcoming estimations, as before, we interact our key variables with gender, but report

entrants, we find gains of a similar magnitude as in our main estimations. 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 that higher position peers may provide better quality, more reliable signals about the applicants' match-specific and general productivity, enhancing the corresponding selection aspects. The effect of inferior peers is insignificant regarding all wage components, being mostly near zero or slightly negative.

Next, we check whether the contacts' tenure can also affect the newcomers' wage gains similarly as 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, hence they can generate more reliable signals about newcomers' productivity. 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 passed 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. 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.[142]

the parameters for only male workers.

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

Table 3.7: Heterogeneity of co-worker gains by link and tie characteristics

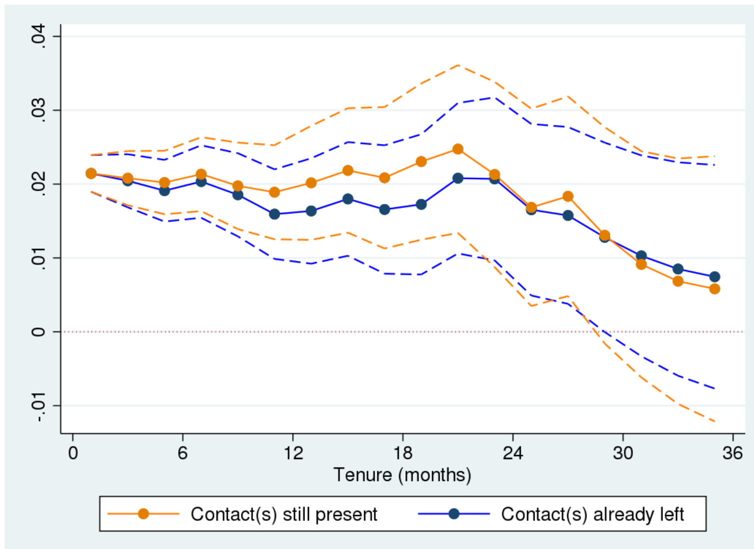
	$\hat{\theta}_{OLS}$	$\hat{\theta}_{TWFE}$	$\hat{\psi}_{ind}$	$\hat{\psi}_{firm}$	$\hat{\xi}_{ind}$	$\hat{\xi}_{firm}$	$\hat{\omega}_{ind}$	$\hat{\omega}_{firm}$
Linked	0.0382***	0.0202***	0.0173***	0.0008	0.0142***	-0.0002	0.0031	0.0009
	(0.0054)	(0.0054)	(0.0038)	(0.0041)	(0.0034)	(0.0052)	(0.0023)	(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	964806	501200	964806	964806	943643	571441	964806	964806
N_i	616386	197435	616386	616386	616365	223021	616386	616386
N_j	105818	61121	105818	105818	84655	105778	105818	105818
R^2	0.345	0.860	0.203	0.232	0.453	0.628	0.069	0.059

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 3.3. Standard errors are in parentheses and clustered at both firm-level and individual-level. *Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

In line with our expectations, both the links' tenure and the length of the common working experience enhance the individual and the firm selections, while we see no effect on the $\hat{\theta}_{TWFE}$ parameter (see Table 3.7). This suggests more intense information transmission both to applicants and firms, but only about general qualities. Also it seems that the age of the tie is not a relevant factor regarding the presence of such selections. Still, the fact that part of the gains are higher when social links tend to be stronger suggests that the selection terms are indeed driven by the participation of peers.

In an attempt to capture the relevance of referral gains that depend on the continuous presence of the referrer, we exploit that peers may leave the firm earlier than their new, referred colleagues. While we assume that expected voluntary monitoring of the peer and knowledge sharing are already evaluated in starting wages, due to the loss of productivity enhancing features the separation of the referrer will probably weaken the bargaining position of the worker in the firm, reducing the wage advantage in the long run. Even if this does not lead to the drop of wages, further wage increase may be hindered, and the advantage of referred workers over market hires could dissolve. 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.[143]

Figure 3.1: Wage-tenure profiles and referrer presence



Note: The figure displays point estimations and 95% confidence intervals for two sets of estimations. 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 estimations, 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 3.3. Standard errors used for the confidence intervals are clustered at both firm-level and individual-level.

We observe that referral gains, similarly as documented in Dustmann et al. (2016), disappear over time as actual productivity of all workers get revealed, and inferior quality workers leave the firm (Figure 3.1). 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 peer 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 sharing and various off-cv elements will be more valuable.

[143] 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 of the estimation.

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.[144] 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 Appendix Table C.3). 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, has a pattern similar to what we observed in the specification with occupational categories (Table 3.4). 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 extra indicators in the general model from Equation (3.7). The first one indicates the presence of those non-linked 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.[145] 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 gains from the presence of both second-links and of those whose firm histories overlap – but not their employment spells.[146]

[144] Although the database is based on US occupation surveys, the scores could provide some insights for Hungary as well. Among others, Handel (2012) confirmed 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.

[145] Workers whom one shared a common workplace with, but at a different time tend to mechanically become second links.

[146] 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.

Table 3.8: 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	938791	479919	938791	938791	917835	550362	938791	938791
N_i	603975	189756	603975	603975	603955	215546	603975	603975
N_j	105061	60367	105061	105061	84105	105022	105061	105061
R^2	0.326	0.860	0.199	0.198	0.452	0.611	0.050	0.088

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 3.3. Standard errors are in parentheses and clustered at both firm-level and individual-level. *Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

The results presented in Table 3.8 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 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 non-random sorting of workers, and capture the effects of information transmission and referral mechanisms. When we utilized scenarios which 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 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 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 via referral as referred hires are generally better workers compared to the firm's own average worker pool. Additional 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 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 being high-paying firm themselves. 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.

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4 Chapter 4: Wage Gains from Foreign Ownership: Evidence from Linked Employer-Employee Data

Joint work with János Köllő and László Balázsi, published as Köllő et al. (2021)

4.1 Introduction

While policymakers in developing countries are often criticized for 'selling out' the country to foreigners, FDI can actually bring valuable knowledge to a less developed economy, spreading through labor mobility channels. Undeniably, corporate revenues can find their way back home via profit repatriation and transfer pricing, and many MNEs enjoy a generous initial tax holiday. However, MNE workers' wage premium over similar domestic-sector employees in comparable firms directly benefits society, especially if the underlying excess productivity is portable and exerts positive spillover effects. Unlike the returns to capital investment and part of the profit, the wage surplus predominantly remains and is spent in the host country.

The literature provides ample evidence to call into question the general validity of such an optimistic scenario. The foreign-domestic wage gap is negligible in countries close to the productivity frontier (Andrews et al., 2007; Balsvik, 2011; Heyman et al., 2011; Malchow-Møller et al., 2007). An adverse competition effect often offsets the positive direct impact of FDI on productivity and wages even in relatively undeveloped economies (Aitken & Harrison, 1999; Barry et al., 2005; Djankov & Hoekman, 2000; Konings, 2001). The positive spillovers are often restricted to specific sectors (Fons-Rosen et al., 2017; Keller & Yeaple, 2009; Suyanto et al., 2014). Still, the existence of a vast MNE premium in the emerging and transition economies (Chen et al., 2017; Lipsey & Sjöholm, 2004; OECD, 2008a), and the findings of positive spillovers (Görg & Strobl, 2005; Gorodnichenko et al., 2014; Kosová, 2010; Poole, 2013; Smarzynska Javorcik, 2004) encourage us to seek evidence for a 'knowledge flows' scenario.[147] To assess the magnitude of the potentially beneficial impact of FDI, we study the direct and indirect wage effects of work experience in multinational enterprises (MNEs) using linked employer–employee data on skilled workers in Hungary, 2003–2011.

We contribute to the literature by empirically showing in a single study that (i) MNEs pay markedly higher wages than similar domestic firms. (ii) MNE employees lose a part of their wage advantage upon leaving the foreign-owned sector. (iii) Even so, they earn more than their colleagues in domestic enterprises. (iv) Domestic firm employees benefit from having ex-MNE peers. We interpret the coincidence of the MNE premium, partial wage loss from separation, lagged returns to MNE experience, and wage spillovers as a signal of knowledge transfer from MNEs to domestic firms. While alternative explanations exist for each of the presented

[147] Section 4.2 provides a detailed introduction to the previous literature including the sources cited here.

symptoms,[148] In the last section of the paper we argue that a 'knowledge flows' scenario has the best chance to produce all of the four outcomes.

Regarding methodology, we draw attention to the difficulties of identification coming from the non-random selection of firms into foreign ownership and of differently skilled workers into foreign enterprises. We find trade-offs between model quality and unbiasedness of the samples on which the first-best models can be estimated.

The analysis is based on a big administrative panel data set covering half of the Hungarian population and their employers in 2003–2011. We restrict the analysis to skilled workers for three reasons.[149] First, the traces of knowledge transfer are easier to find in the skilled labor market. Second, data discussed later suggest that a part of the MNE premium compensates unskilled workers for non-wage disamenities like shift work, weekend work, and a higher probability of becoming unemployed. The data does not indicate ownership-specific differences of this kind among highly skilled workers. Third, repeating the estimations for middling and unskilled workers would triple the statistics to be presented, with minimal added content. Estimation on a pooled sample would only attenuate the relevant parameters.

We start by estimating the foreign-domestic wage gap using panel regressions. By gradually removing the effects of observed and unobserved worker and firm characteristics, we get from a substantial raw gap of 0.75 log points to 0.24 points after controlling for worker fixed effects and a mere 0.03 points' pure ownership-specific wage differential estimated with both worker and firm fixed effects (2FE henceforth).

While a 2FE model can answer how an existing firm's wage level changes in response to a change in ownership, the effect it identifies is unsuitable for out-of-sample prediction. Only 5.3 percent of the observed firms changed the majority owner during the observation period in our sample. These companies paid significantly higher wages than 'always domestic' firms (when they were domestic) and significantly lower wages than 'always foreign' companies (when they were foreign-owned): this is how the 2FE model arrives at a close-to-zero estimate of the ownership-specific wage gap. These firms' experience can hardly predict how big MNEs like Mercedes-Benz or IBM would pay their employees in the unlikely event of takeover by a local business person. It also tells nothing about the potential wage gains from greenfield investments, which played a significant role in the 1990s (Calderón et al., 2004).[150] We utilize a difference-in-difference estimation of wage gains from joining a new MNE over joining a new domestic firm to learn about the ownership-specific wage gap between 'always foreign' and 'always

[148] MNEs may pay high wages to skim the cream of the labor force, buy loyalty, contain turnover, stimulate work effort, or prevent information leakage. Workers' wages may fall upon leaving the MNE sector for losing these wage components and because employers perceive their dismissal as a negative signal. Ex-MNE workers may have high wages in domestic firms because they have high reservation wages and belong to the lucky few to find a well-paying job in the domestic sector. Spillover effects may arise from the employer's wish to keep within-job wage differentials within tolerable limits.

[149] We justify this choice and present some results on less skilled workers in Sect. 7.

[150] Antalóczy,K and Sass (2001) estimate that the share of greenfield FDI in total inward FDI amounted to 25–30 percent in Hungary and other CEE countries during the transition.

domestic' companies. This approach suggests that the employees of new MNEs earn 15 percent more than their domestic counterparts.

Turning to the MNE premium's portability, we have to deal with endogeneity and ability biases, as worker mobility is not random. If a worker is fired from an MNE, it may be because her marginal product is lower than average. If a domestic employer attracts a worker, it may be because she has a higher-than-average marginal product irrespective of the sector of employment. To address the first problem, we compare domestic firm employees with recent MNE experience to their peers who had outside experience in the domestic sector. We focus on workers losing or leaving their jobs in times of mass dismissals when separations are more likely to be exogenous to the individual worker's productivity. The model controls for heterogeneity of the sending firms via observable controls and use fixed effects for the receiving ones. We find that former MNE employees earn more by 13 percent than similar workers coming from collapsing domestic enterprises. Workers separating from their employers for reasons other than mass dismissals acquire a significantly lower (5 percent) lagged MNE premium.

Satisfactory model quality comes at the cost of distortions in the sample and a significant loss of observations in this case, too. Only about 7 percent of the person-months in our data makes it to the estimation sample of a model in which work histories and characteristics of the sending and receiving firms are adequately controlled. The problem would be further aggravated by the inclusion of worker fixed effects to reduce ability bias.[151] To avoid this issue while utilizing more data and still controlling for the potential bias, we rely on a less demanding 'overlapping cohorts' model that compares domestic firm employees with future and past experience in foreign versus domestic firms. This model can utilize a much broader sample, as workers with only two observed spells can contribute to the estimation if any of those is at a foreign-owned employer. The estimated return to prior MNE experience amounts to 0.07 log points.[152]

Finally, we estimate spillover effects for incumbent domestic firm employees, controlling for observed and unobserved worker and firm characteristics. We deviate from a similar attempt by Poole (2013) in two ways. First, we also study how skilled incumbents' wages respond to the presence of less qualified ex-MNE peers. Second, and more importantly, we address the selection problem that arises when the analysis is restricted to incumbents (domestic workers with no experience outside their firms). Incumbents in our data account for only 22 percent of the workers ever employed in the domestic sector. Their exposure to peers with MNE experience differs substantially from that of the average worker. In an alternative specification, we ensure the identification of within-firm spillovers using a

[151] With the requirement of controlling for lagged size changes, we would need workers with at least four employment spells in a nine-year-long period, with a specific pattern DDFD, where F and D stand for foreign-owned and domestic firms. Identification in this setting would come from comparing the second and fourth domestic job entries. The third, F spell is the treatment, and a first spell is required for the inclusion of firm sizes. Besides, in this setting the ex-MNE spells would sistematically happen later on in worker's career, so life-cycle wage changes may be potentially captured by the parameter as well.

[152] Which is a lower bound as in this model, we do not control for employment change in the sending firm.

2FE model. We find that a one-standard-deviation difference in the share of high skilled ex-MNE peers shifts peers' wages with no MNE past up by slightly more than one percent. Having qualified peers with outside experience in the domestic sector and having low-skilled peers with MNE experience do not affect wages.

Section 4.2 discusses previous findings on the paper's topic and prewarns the reader of our estimates. Section 4.3 introduces the data and the local context. Section 4.4 is devoted to the study of the foreign-domestic wage gap. Sections 4.5 and 4.6 present the results on lagged returns and spillover effects, respectively. Section 4.7 briefly comments on differences by skill levels and industries. Section 4.8 sums up the results and argues that the empirical findings, taken together, yield support to a 'skills diffusion' scenario.

4.2 Previous findings on the foreign-domestic wage gap, lagged returns and spillovers

Estimates of the foreign-domestic wage gap vary widely, with the MNE premium found to be nearly negligible in the most developed market economies. In Norway, the OLS estimate by Balsvik (2011), controlled for worker and plant characteristics, amounts to 3 percent, which falls to 0.3 percent once she includes worker fixed effects. An OLS estimate for Sweden by Heyman et al. (2011) is even lower at 2 percent. Andrews et al. (2007) and Malchow-Møller et al. (2007) detect positive gaps in the range of 1 and 3 percent in Germany and Denmark. The OLS estimate of P. Martins (2004) for Portugal is higher (11 percent), but he finds that the MNE wage premium virtually disappears after controlling for worker selection. These figures compare to 32 percent (pooled OLS for all skill levels) and 13 percent (after adding worker fixed effects) in our sample. Workers moving from domestic to foreign-owned firms are estimated to gain 6 percent in Germany and 8 percent in Norway (Andrews et al., 2007; Balsvik, 2011), which compares to 53 percent in the Hungarian sample for all skill levels.[153]

The foreign-domestic gap is much broader in less developed countries: according to raw data presented in Lipsey and Sjöholm (2004), in Indonesian manufacturing, the MNE premium amounts to 47 percent for blue collars and 55 percent for white collars (41 and 73 percent in Hungary). Chen et al. (2017) report a gap of 40 percent in Chinese manufacturing. An overview of data in OECD (2008a), based on the World Bank Enterprise Survey, indicates raw gaps of between 40 and 50 percent in Africa, Asia, the Middle East, and combining all these regions and adding Central and Eastern Europe.

A more detailed analysis of the sources of the gaps in Germany, Portugal, the UK, and Brazil (OECD, 2008b) finds that takeovers' marginal effect on wages falls short of 3 percent in all of these countries.[154] Results from Hungary point to similar patterns. Csengödi et al. (2008) use a different data set from ours (the

[153]Note that in the Norwegian case, workers moving from MNEs to domestic firms also acquire a gain of 7 percent, while in our sample they lose 11 percent. The median loss amounts to 26 percent in the case of skilled workers. See Table 2.

[154]In the Czech Republic, Jurajda, S and Stancik (2012) detect sigificantly faster wage bill growth in (and only in) manufacturing firms with a low export share. They cannot decompose the wage bill effect into wage and employment effects.

Wage Survey, a repeated cross-section LEED which allows the linking of firms but not workers) and find that after adding firm fixed effects, the MNE wage premium falls to a mere 3 percent as it does in our case.[155] Earle et al. (2018) use the same data source and detect a slightly higher premium of 7 percent that is still very far from the estimates they get without controlling for unobserved firm characteristics and firm-specific trends. The effects identified using data on worker mobility by OECD (2008b) are more substantial: the estimates vary between 6 and 8 percent in Germany and the UK, more than 10 percent in Portugal, and 20 percent in Brazil. The authors argue that the discrepancy between the estimates based on takeovers versus worker flows are explained by foreign firms' propensity to share their productivity advantage more extensively with new workers than with workers who do not change firms. We believe that the difference instead roots in the non-random selection of firms to acquisition, as will be discussed in more detail later.

To our knowledge, Balsvik (2011) is the only one estimating the wage advantage of ex-MNE employees in domestic firms. She identifies a premium of 6.9 percent for workers with three or more years of tenure in an MNE. However, she also detects an advantage of 3.3 percent on the part of workers arriving from local firms, suggesting a net benefit from MNE experience of 3.6 percent (and smaller advantages in case of shorter completed tenure in the previous job). We find that domestic firm employees, who left an MNE because of mass dismissals, closure, or relocation earn more than their ex-domestic counterparts by 13 percent.

The empirical evidence on wage and productivity spillovers are mixed. Starting with papers that depict a not too rosy picture of how MNEs affect the rest of the economy, Aitken and Harrison (1999) and Djankov and Hoekman (2000) identify a positive direct effect of foreign ownership on productivity in Venezuela and the Czech Republic, but negative spillovers. Konings (2001) suggests that the adverse competition effect is stronger than the positive direct productivity effect of FDI in Bulgaria, Romania, and Poland. Barry et al. (2005) found that foreign presence in a sector hurts wages and productivity in domestic exporting firms in the same industry (but does not affect wages in domestic non-exporters) in Ireland. Fons-Rosen et al. (2017) conclude that in six advanced European countries, positive spillovers are restricted to sectors where domestic enterprises are technologically close to MNEs.Suyanto et al. (2014)find the opposite in Indonesia.Keller and Yeaple (2009) detect significant worker-level wage spillovers only in high-skill-intensive industries in US manufacturing. By looking at existing firms in an Audi plant's supplier industries in Hungary, Bisztray (2016) finds no positive effect on productivity. She also finds that firms with foreign owners account for all the positive impact on sales and employment, suggesting a foreign-to-foreign complementarity rather than a galvanizing effect on the domestic sector.

At the same time, several studies have identified positive spillovers. Using Lithuanian data, Smarzynska Javorcik (2004) detects positive productivity spillovers from MNEs to local suppliers. Similarly, Gorodnichenko et al. (2014) find that backward linkages positively affect the productivity of domestic firms (while horizontal and forward linkages show no consistent effect) in 17 transition countries.

[155] They also show that domestic firms subject to foreign acquisition pay higher-than-average wages already before the takeover, hinting at a non-random selection to foreign buy-out.

Using Czech data, Kosová (2010) demonstrates that crowding out is short-term: after an initial shock, domestic firm growth accelerates, and survival rates improve. Görg and Strobl (2005) show that entrepreneurs with MNE experience start more productive small businesses in Ghana. Bisztray (2016) found that new entrants' growth in productivity was significantly higher when located close to Audi and operated in a supplier industry.

Importantly, from this paper's point of view, Poole (2013) estimates that the wages of incumbent domestic firm employees in Brazil rise by about 0.6 percent if the share of ex-MNE employees increases by 10 percent, while the effect of outside experience in local firms is about ten times weaker than that. While the effect she estimates is not particularly strong, it is statistically significant at conventional levels.

One can also find indirect evidence on spillovers, considering that MNEs are more productive and more likely to export and engage in R&D. Stoyanov and Zubanov (2012) show that (in Denmark) workers coming from more productive firms experience productivity gains. Similar results are presented for Hungary by Csáfordi et al. (2018). Mion et al. (2013) show that export experience implies higher export performance and a sizable wage premium for Portuguese managers, who leave for non-exporters. In Finland, Maliranta et al. (2008) identify positive impact of hiring workers with previous R&D experience to non-R&D jobs.

4.3 Data and the local context

4.3.1 Data sources

Our estimation samples have been drawn from a big longitudinal data set covering a randomly chosen 50 percent of Hungary's population aged 5–74 in January 2003. Each person in the sample is followed monthly, from January 2003 until December 2011, or exit from the registers for death or permanent out-migration. The data collect information from the Pension Directorate, the Tax Office, the Health Insurance Fund, the Office of Education, and the Public Employment Service. We use information on the highest paying job of a given person in a given month, days in work, and amounts earned in that job. Throughout the paper, we use daily wages (the monthly value divided by days in work) normalized for the given month's national average. We have data on occupation, type of employment relationship, registration at a labor office, receipt of transfers, and several proxies of the person's state of health. We do not observe educational attainment—this is approximated with the person's highest occupational status in 2003–2011.[156] The data on firms come from the annual tax reports of businesses obliged to conduct double book-keeping. The firm-level variables are merged into the respective person-month observations. We regard a firm as MNE if foreigners' share in subscribed capital exceeds 50 percent.[157]

We restrict the analysis to skilled workers employed at least once in a foreign or domestic private enterprise the employment level of which exceeded the ten

[156] See Appendix D.2 for variable definitions.

[157] Setting the limit elsewhere does not affect the results, since 93 percent of the firms with nonzero foreign presence are majority foreign-owned.

workers limit at least once in 2003–2011. We have several reasons to set a size limit. First, foreign firms are nearly absent in the small firm sector.[158] Second, financial data are not available for sole proprietorships and unincorporated small businesses. Third, the financial reports of incorporated small firms are often incomplete and erroneous. Finally, the earnings data of small firms are flawed by paying "disguised" minimum wages.[159] Small firms' inclusion would also raise the risk of measurement error in the analysis of spillover effects since the probability of not observing an ex-MNE employee in a 50-percent sample is much higher in small establishments. We iteratively removed workers and firms with less than two data points, zero wages, and missing covariates.

After these steps of data cleaning, we are left with a sample of 19,961,622 person-months belonging to 344,203 skilled workers and 119,580 firms. 52.6 percent of the workers had at least one spell of employment in the foreign-owned sector, of which 21.5 percent worked only in MNEs. We draw special sub-samples from this starting population for the study of new firms, lagged returns and spillover effects. Descriptive statistics are presented in Table 11 of Appendix D.1.

Even though our firm-level variables are of annual frequency, we prefer to analyze the data at a monthly level for several reasons. First, the affiliation of a worker cannot be precisely measured on a yearly basis. About 25 percent of the workers employed by an MNE for at least one month in a given year also had one or more spells in the domestic sector in the same year. Second, turning to a yearly basis would impair the precise measurement of tenure and the time between two jobs—essential controls in the analysis of lagged returns. Third, higher observed mobility helps in identifying firm and person effects. The problem raised by inflating observations at the same firm is taken care of by the worker and firm-level clustering of errors.

4.3.2 MNEs in Hungary

In the first decade after the start of the transition, Hungary was the most successful country within the former Soviet bloc in attracting foreign capital. By 2003, the beginning of our observation period, cumulative FDI inflows exceeded 40 percent of the GDP,[160] multinationals employed 15 percent of the labor force (including self-employment and the public sector into the denominator) and more than 30 percent of private-sector employees. They produced 20 percent of the GDP and delivered over two-thirds of the exports (Balatoni, A & Pitz, 2012). Large multinationals, including Audi, General Motors, and Suzuki, dominated the motor industry. Foreign presence was already significant in the tobacco, leather, chemical, rubber, and electronics industries, with employment shares of between 50 and 80 percent.

Almost three-fourths of the cumulative FDI inflows have arrived in sectors out-

[158] In 2014, MNEs had a 4.5 percent employment share in the 1–10 workers category (Authors' calculation based on the 2014 Q4 wave of the Labor Force Survey).

[159] This term hints at the practice of paying workers the minimum wage (subject to taxation) and the rest of their remuneration in cash. Elek et al. (2012) estimate that in 2006 the share of workers paid in this way amounted to 20 percent in firms employing 5–10 workers, 10 percent in slightly higher firms (11–20 workers) and less than 3 percent in larger enterprises.

[160] UN ECE (2001), p. 190.

side of manufacturing. As shown in column 4 of Table 4.1, nearly 60 percent of the skilled employees within the MNE sector worked in the tertiary sector. Therefore, we do not restrict the analysis to manufacturing, as most papers do in the strand of the literature we follow (see Barry et al. (2005);Görg and Strobl (2005); Lipsey and Sjöholm (2004); Smarzynska Javorcik (2004); Balsvik (2011) as opposed to Poole (2013), whose study covers all sectors in Brazil). While FDI typically boosts exports and generates demand for domestic manufacturers producing intermediate goods, its contribution to the quality of retail trade, banking and services can be equally important, especially in the former state-socialist countries, which started the transition with critically undeveloped non-tradable sectors. The foreign-owned and domestic parts of the economy are closely connected via labor turnover. In the skilled labor market, 37.2 percent of the domestic firms, employing 69 percent of the domestic labor force, hired at least one ex-MNE worker in 2003–2011.

Table 4.1: Foreign ownership in Hungary, 2003

	Fraction employed in MNEs (percent of all person-months in the given industry)		**Industrial composition of MNEs (percent of all person-months in the MNE sector)**	
	All workers	**Skilled workers**	All workers	Skilled workers
Agriculture	5.0	6.1	0.8	0.5
Manufacturing	46.5	48.4	59.9	40.5
Construction	7.7	10.6	1.5	1.9
Energy, water, gas	57.5	55.6	3.3	3.1
Wholesale and retail trade	25.9	34.5	16.3	31.5
Finance and insurance	52.7	80.0	11.4	11.5
Services	20.7	24.3	6.8	11.0
Average/total	34.8	37.6	100.0	100.0

Notes: The data are annual averages observed in the estimation sample in 2003. The number of person-months amount to 8,704,486 (all workers) and 2,068,556 (skilled workers)

4.3.3 Descriptive statistics on wages and wage change

Table 4.2 presents raw statistics on wage levels across ownership categories and wage changes associated with skilled workers' shifts between them. The data shows vast differences between workers in MNEs versus domestic firms, on the one hand, and domestic firm employees hired from MNEs versus workers coming from other domestic enterprises, on the other.

Table 4.2: Descriptive statistics: wage levels and wage changes of skilled workers

	Mean	**St. dev.**	**Observations**
Wage levels[a]			
Employer=MNE	309	288	7,937,675[b]
Employer=domestic firm	143	161	12,023,947[b]
Wage change upon leaving an MNE for a domestic firm[c]			
Mean	-57	146	42,479
Median	-26		42,479
Wage change upon leaving a domestic firm for an MNE[c]			
Mean	64	126	46,590
Median	39		46,590
Wages of domestic firm employees with recent outside experience[d]			
Previous employer=MNE	171	193	963,075[b]
Previous employer=domestic firm	118	122	3,557,788[b]

Notes: [a] Wage in month t relative to the national average wage in month t, per cent
[b] Person-months observed in 2003–2011
[c] The figures relate to persons moving from MNEs to domestic firms and vice versa. Mean earnings in the receiving firm is compared to the same worker's mean earnings in the sending firm. Wages are deflated with the national average wage in the same month
[d] The figures relate the mean earnings of domestic firm employees with previous outside experience to the mean earnings of incumbent domestic firm employees, percent

According to the raw data, MNE employees earn more than twice as much as domestic sector workers. Persons moving from domestic firms to MNEs gain 64 percentage points on average, while individuals who move to the other direction lose 57 points. Measured with the median rather than the mean, the gain and the loss amount to 39 and -26 percentage points, respectively.[161] The bottom block suggests a substantial raw premium for outside experience in foreign-owned enterprises. In the forthcoming sections, we try to disentangle a 'pure' ownership-specific effect from differences in composition.

4.4 Foreign-domestic wage gap

4.4.1 Benchmark model

Our first model estimates the foreign-domestic wage gap in the following way:

$$\ln w_{ijt} = \delta F_{ijt} + [\varphi P_i] + \alpha X_{it} + \beta Y_{ijt} + \gamma V_{jt} + [v_i + f_j] + s_{jt} + \varepsilon_{ijt} \tag{4.1}$$

where wijt is the daily average (relative) earnings of person i at firm j and month t, F is a dummy for being employed in a majority foreign-owned firm, P_i and X_{it} are fixed and time-varying individual attributes, Y_{ijt} stands for job-specific variables

[161]See Appendix D.1: Fig. D.2 for a box-and-whiskers plot of wage changes.

(like occupation and tenure), V_{jt} denotes time-varying firm-specific covariates, v_i and f_j are worker and firm fixed effects, respectively, and ε_{ijt} is an error term. We allow for unobserved shocks to productivity by including sector–year interactions s_{jt}. The firm-level variables are size, the capital-labor ratio, and a dummy for exporters. Alternatively, we use indicators of investment and productivity. We gradually move from an OLS equation only controlled for s_{jt} to fixed-effects models with all the covariates except for the Pi variables.

When the equation is estimated with OLS, the δ parameter captures the ownership effect, plus the employment-duration weighted average residual worker and firm effects given personal characteristics P and X (Abowd et al., 2006). The person fixed effects absorb the unobserved time-invariant mean "qualities" of workers. However, the estimated gap is still affected by the employment-duration weighted average of the firm effects for the firms in which the worker was employed. When both person and firm fixed effects are included, δ captures a pure ownership effect identified from worker flows between ownership categories, on the one hand, and changes in ownership, on the other.[162] It shows the wage advantage of a foreign firm employee over a domestic worker with similar observable attributes, controlled for their average wages in the entire period of observation, and also controlled for average wages of the firms where they worked during the period of observation. For our multiple fixed effect estimations, we use a model proposed by Correia (2017), and implemented in Stata as reghdfe.[163]

In Model A of Table 4.3, which measures MNE employees' wage advantage relative to domestic firm employees, the estimate rises from 0.745 log points to 0.763 after controlling for observed worker characteristics. The inclusion of firm size, the capital-labor ratio, and exports bring the estimated MNE premium down to 0.437, while adding worker fixed effects reduces it to 0.236. Adding firm fixed effects results in a major drop to only 0.026.

[162] The only exception would be observations on firms that, at the same time as changing ownership, would change all of their employees. We do not have such cases in the data.

[163] Several methods have been developed in the last ten years (following the pioneering work of Abowd et al. (1999)) to deal with two or more high dimensional fixed effects. The iterative methods (Carneiro et al., 2012; Cornelissen, 2008; Guimarães & Portugal, 2010; P. S. Martins, 2009; Mittag, 2019) solve the problem by shuffling between the estimation of the slope and the intercept parameters. Balazsi et al. (2018) yield an alternative, which presses more on memory but runs faster. Early drafts of this paper experimented with this method. With the size of the final data, iterative approaches turned out to be more productive.

Table 4.3: Estimates of the foreign-domestic wage gap for skilled workers

	(1)	(2)	(3)	(4)	(5)	(6)
Model A						
Foreign-owned	0.745 (25.5)	0.763 (20.3)	0.718 (31.6)	0.437 (23.4)	0.236 (28.6)	0.026 (3.5)
aR^2/within R^2	0.260	0.329	0.414	0.480	0.238	0.103
Model B						
Always foreign-owned	0.794 (25.7)	0.817 (29.5)	0.772 (31.3)	0.507 (23.3)	0.307 (26.8)	
Temporarily foreign-owned	0.569 (8.1)	0.574 (8.7)	0.564 (12.6)	0.334 (5.2)	0.209 (14.1)	0.026 (3.5)
Temporarily domestic	0.408 (10.5)	0.408 (10.5)	0.462 (11.3)	0.269 (8.7)	0.157 (11.6)	
aR^2/within R^2	0.267	0.327	0.422	0.482	0.242	0.103
Controls						
Sector×year	Yes	Yes	Yes	Yes	Yes	Yes
Person	No	Yes	Yes	Yes	Yes	Yes
Job	No	No	Yes	Yes	Yes	Yes
Firm	No	No	No	Yes	Yes	Yes
Person FE	No	No	No	No	Yes	Yes
Firm FE	No	No	No	No	No	Yes

Notes: All coefficients are significant at 0.01 level, t-values in parentheses. The standard errors are adjusted for clustering by persons and firms. Sample: 19,961,622 person-months belonging to 344,203 skilled workers in 119,580 firms. Singleton observations are excluded from the panel regressions Dependent variable: log daily wage in the given month relative to the national mean. Reference categories: employed in a domestic firm (Model A), employed in an' always domestic' firm (Model B). Controls: person, job and firm characteristics plus sector–year interactions. See Appendix D.1: Table D.2 for variable definitions. Specifications 5 and 6 include only time-varying covariates and worker and firm fixed effects. Estimation: all models were estimated with Stata's *reghdfe* models

Controlling the worker fixed-effect model for TFP or value-added per worker instead of the firm fixed effects yield estimates of 0.218 and 0.206, respectively. Including TFP into the set of firm controls in specification (4) results in a coefficient of 0.209. Including investment as well, which controls for the potential coincidence of positive productivity shocks and the hiring of high-quality labor, produces an estimate of 0.216. By contrast, adding firm fixed effects to specification (4) without including worker fixed effects decreases the estimate from 0.437 to 0.036, clearly indicating that selection to acquisition drives the result of the 2FE model.

In Model B of Table 4.3, the observed person-months are classified by the ownership histories of employers. 'Always domestic' (the reference category) and 'always foreign' denote enterprises that did not change owner in 2003–2011. 'Temporarily foreign' and 'temporarily domestic' indicate the current majority owner of the workers' employer, for firms which underwent acquisition at least once in 2003–2011. The 'temporary foreign' dummy, for instance, is set to one for a person-month spent in a foreign-owned enterprise, which operated under domestic ownership in a part of the observed period.[164]

[164] Model B with added firm effects (Specification 6) is identical to Model B, as the always foreign

The estimates suggest that firms involved in takeovers and currently operating under domestic ownership pay more than incumbent domestic firms (by 0.157 log points in specification 5 where worker quality is controlled for). Switching firms currently under foreign ownership pay lower wages than always foreign-owned companies by 0.099 log points. The gap between the coefficients for employment spells under 'temporarily foreign' and 'temporarily domestic' ownership (0.052 log points) is an alternative measure of how ownership changes affect the wage. The magnitudes make it clear that switching firms substantially differ from any of the incumbent categories.

4.4.2 Exploiting information on new firms

As much as 94.8 percent of the firms in our estimation sample did not change majority owner in the period covered by the data: 7.3 percent was foreign-owned, and 87.5 percent was domestic throughout 2003–2011. Rather than merely neglecting the huge wage difference between them (as does the 2FE model), we exploit information on newly established and subsequently incumbent foreign and domestic enterprises. The critical event under examination here is not the takeover of an existing firm, but the birth of an incumbent firm. The analysis relates to firms established after 2003 and staying under majority foreign or domestic control until 2011. We compare the earnings of incumbent workers in these firms to the wages they earned before their entry. Formally, we estimate the following difference-in-difference model:

$$\ln w_{ijt} = \beta_1 F^0_{ijt} + \beta_2 D^0_{ijt} + \beta_3 F^1_{ijt} + \beta_4 D^1_{ijt} + \mathbf{Z}\boldsymbol{\gamma} + \varepsilon_{ijt} \tag{4.2}$$

F and D are the acronyms for foreign-owned and domestic firms. F^0 and D^0 are set to one for person-months preceding the worker's entry date to a newly established F or D firm. F^1 and D^1 are set to one for the months of service in a newly established firm. For instance, for a worker hired by a new foreign-owned company in month $t = 37$, $F^0 = 1$ if $t < 37$ and $F^1 = 1$ if $t \geq 37$. $\mathbf{Z}$ denotes controls listed in the notes to Table 4.6.

$\beta_1 - \beta_2$ is the estimated wage difference between future F and D employees, whereas $\beta_3 - \beta_4$ measures the wage difference between the employees of new F and D firms. The double difference $(\beta_3 - \beta_4) - (\beta_1 - \beta_2)$ removes the gap in the quality of F and D workers as measured with their pre-entry wages. Since assignment to the groups compared is person-specific, and the firms do not change owner, we estimate the equation with pooled OLS. A large battery of controls guarantees that we compare workers and firms with similar characteristics.

Note that we base the definition of a 'new firm' on its employment dynamics rather than its date of registration since the latter is often associated with break-ups, mergers and acquisitions, rather than the birth of a new economic actor. We rely on the fact that a medium-sized or large firm's creation typically begins with hiring a small group of managers who arrange the start-up. This preparatory

indicator is absorbed by the added firm effects. Hence only a parameter on being temporarily foreign-owned can be estimated, which is identified only from firms going through acquisitions or divestments and accordingly, coincides with the parameter on the F dummy of Model A.

stage is followed by a 'big bang' when the firm hires rank-and-file employees. We speak of a big bang when a firm's staff jumps from an initial level of $L_{t-1} \leq 5$ to $L_t \geq 50$, or, from $L_{t-1} \leq 50$ to $L_t \geq 300$ within a month. We found 519 such firms with no subsequent change of ownership. Combined employment in these enterprises jumped from 13 to 253 thousand (see Appendix D.1: Fig. D.3). Finally, the sample consists of 471,489 person-months belonging to 8225 skilled workers hired by and staying until December 2011 in 366 new domestic and 147 new foreign-owned firms.

The results in Table 4.4 indicate a wage gap of 0.391 log points between skilled workers in new MNEs versus new domestic firms—this is reasonably close to the 0.437 log points gap estimated with a fully controlled OLS for all firms in Table 4.3, specification 4.4. New foreign firms' workers also earned more than their domestic counterparts before they entered the new firms by 0.245 log points on average. After deducting this difference from the post-entry gap, an ownership-specific wage differential of 0.146 log points remains between incumbent workers in incumbent firms. This point estimate falls between the individual only and the two fixed-effects parameters, suggesting a significantly stronger pure ownership-specific effect than the 2FE model.

Table 4.4: Wages before and after entry to new MNEs and new domestic firms

	Coeff.	**t-test**	**Person-months**
Workers of domestic start-ups, before their entry	0		115,443
Workers of foreign start-ups, before their entry	0.245***	8.3	146,585
Workers of domestic start-ups, after their entry	-0.014	0.3	84,018
Workers of foreign start-ups, after their entry	0.391***	8.9	125,247
Double difference (point estimate, F-test)	0.146**	9.5	

Notes: Significant at the **0.05, ***0.01 level. The t-values are based on standard errors adjusted for clustering by persons and firms OLS regression with dummies standing for the four distinct groups. Dependent variable: log daily wage in the given month relative to the national mean. Sample: 471,489 person-months belonging to 8225 skilled workers hired by and staying until December 2011 in 519 newly established firms (366 domestic and 147 foreign-owned). We considered a firm newly established if its staff number jumped from less than 5 to more than 50, or, from less than 50 to more than 300 within a month. Workers employed by new firms before their 'big bang', workers leaving the new firms and firms changing owner after the big bang are excluded. Controls: person, job and firm characteristics and sector-year interactions. See Appendix D.1: Table D.2 for variable definitions

4.5 Lagged returns

4.5.1 Are ex-MNE workers paid more in the domestic sector?

In Eq. 4.2, we compare workers in domestic firms, who arrived at their employer from MNEs versus other domestic firms. The estimates are controlled for personal characteristics, current and past job attributes, tenure in the last job, months between the two jobs, selected indicators of the sending and receiving firms, and sector-year interactions. We retain firms with at least one ex-MNE and one ex-domestic employee and exclude firms undergoing acquisition.

$$\ln w_{ijt} = \alpha X_{it} + \beta_1 \text{F\_After}_{ijt} + \beta_2 dL_{jt} + \beta_3(\text{F\_After}_{ijt} dL_{jt}) + f_j + s_{jt} + \varepsilon_{ijt} \quad (4.3)$$

F_After_{ijt} is a dummy set to 1 for workers who arrived from foreign firms and 0 for workers coming from domestic companies. $dL_{jt} = \text{L}_{j,t+1}/\text{L}_{j,t+1}$ measures the change of employment in the sending firm between year $t-1$ and $t+1$, with t denoting the year when the worker left the firm. The coefficient β_2 measures how wages vary with employment dynamics of the sending domestic firms while the parameter β_3 of the interaction term $\text{F\_After}_{ijt} * dL_{jt}$ captures the impact of $dL_j t$ on workers arriving from foreign employers. The wage advantage of workers coming from MNEs over workers arriving from domestic firms, conditional on employment dynamics of the sending firm, is given by $\beta_1 + \beta_3 dL_{jt}$. Alternatively, we estimate the equation for two groups distinguished based on dL_{jt} (lower or higher than 0.5), without the size-change and interaction terms.

Since we are interested in the within-firm wage differences between ex-MNE and ex-domestic entrants (rather than how a worker's wage changes upon entering a domestic firm), we include firm fixed effects, but not worker fixed effects.

The upper block of Table 4.5 shows the results of the first variant of the model. The wage advantage of an ex-MNE employee arriving from a firm where staff numbers did not change around the year of the worker's separation (dL_{jt}=1) amounts to 0.057 log points, while it is estimated to be 0.074 points in case the sending firm was closed or relocated (dL_{jt}=0). We added a dummy indicating if the worker had arrived from another domestic firm but previously had some experience in one or more MNEs. These workers have an advantage of 0.064 log points. Only a part of these gaps results from within-firm premia, as suggested by the differences between the specifications with and without firm fixed effects.

Table 4.5: The wage advantage of ex-MNE workers in domestic firms over coworkers having arrived from other domestic firms—regression estimates

	Firm fixed effects: **No**	**Yes**
Model A: entire sample		
Sending firm is MNE (F_After)	0.091*** (4.4)	0.077*** (4.9)
Change of employment in the sending firm (dL)	0.010* (1.8)	0.011*** (3.0)
Interaction term (F_After×dL)	-0.028** (2.4)	-0.024** (2.3)
MNE experience before entry to the sending firm (dummy)	0.059*** (6.0)	0.038*** (5.0)
Number of observations	723,421	722.913
aR^2/within R^2	0.461	0.288
Model B: workers arriving from mass layoffs and all workers		
Employment change in the sending firm: $L_{t+1}/L_{t-1} \leq 0.5$		
Sending firm is MNE	0.134*** (3.3)	0.109** (2.4)
MNE experience before entry to the sending firm (dummy)	0.068*** (2.9)	0.056*** (2.7)
Number of observations	153,482	153,213
aR^2/within R^2	0.479	0.277
Entire sample		
Sending firm is MNE	0.060*** (4.1)	0.049*** (5.1)
MNE experience before entry to the sending firm (dummy)	0.058*** (6.0)	0.037*** (5.0)
Number of observations	723,421	722,913
aR^2/within R^2	0.461	0.288

Notes: Significant at the *0.1, **0.05, ***0.01 level. The standard errors are adjusted for clustering by persons and firms. Sample: 723,421 person-months belonging to 96,277 skilled workers in 19,449 domestic firms, who had arrived from MNEs versus other domestic firms. 508 singleton observations are excluded from the equation with firm fixed effects. Estimation: Stata *reghdfe*. Change of employment in the sending firm: L_{t+1}/L_{t-1}, where t is the year of the worker's separation. Controls: person and job controls, contemporaneous and lagged firm-level controls, as listed in Table D.2. Additional controls are completed tenure in the sending firm, dummy for unobserved tenure, months between exit from the sending firm and entry to the receiving firm, one-digit sectoral affiliation of the sending and receiving firms and year dummies

The lower blocks of the table display estimates on two sub-samples distinguished along dL_{jt}. Former MNE workers who lost or left their jobs during mass dismissals ($dL_{jt} < 0.5$) had substantially higher wage advantages over their ex-domestic counterparts (0.134 log points) than did those ex-MNE workers, who arrived from slightly contracting, stable or expanding firms (0.06).[165]

[165] Workers who leave well-paying jobs in the MNE sector individually can be either negatively or positively selected. On the one hand, MNE employees fired individually are likely to be less productive than the average. On the other hand, those who manage to find a well-paid domestic job are predictably over-represented among voluntary quitters. The comparison of group-level estimates suggests that the first effect dominates: workers separating from their firms for reasons other than mass dismissals earn a lower lagged MNE premium on average.

4.5.2 An overlapping cohorts model of lagged returns to MNE experience

The estimates presented in the preceding sub-section are potentially subject to ability bias: workers returning to the domestic sector can be more productive wherever they work. As it was put forward in the Introduction, addressing this problem by adding worker fixed effects to model (2) is not a feasible option. Therefore, we estimate an alternative model that compares the wages of domestic firm employees with past and future experience in MNEs versus domestic companies other than their current employer. This approach is close in spirit to models that study the wage effect of incarceration by comparing past and future convicts (Czafit & Köllő, 2015; Grogger, 1995; Lalonde & Cho, 2008; Pettit & Lyons, 2009) under the assumption that the date of incarceration (mutatis mutandis the dates of entry to and exit from MNEs) can be treated as random. We can reasonably assume that future MNE workers are closer to former MNE employees in terms of unobserved characteristics than any control person selected from the general population based on observables. A further advantage of this choice is a gain in sample size: 3,841,561 person-months instead of 797,261 in Model (2).

We define a collectively exhaustive classification making a distinction between domestic firm employees with past MNE experience in month t (PF), workers with future but no past MNE experience (FF), workers with prior experience in other domestic firms and no MNE experience (PD) and workers with future domestic sector experience and none of the types mentioned earlier (FD). Incumbent workers who had no contact with other employers in 2003–2011 constitute the reference category. The sample we work with consists of domestic firm employees in companies employing at least one worker belonging to the categories mentioned above and one incumbent worker. We restrict the analysis to 2005–2009 to have sufficient observations on past and future experiences outside the workers' current firms.

We regress log wages on the respective dummies and person, job, and firm-specific controls plus sector-year interactions. Choosing incumbents as the reference category and denoting the controls with **Z**, the estimated equation with or without firm fixed effects (v_j) is:

$$\ln w_{ijt} = \beta_1 PF_{ijt} + \beta_2 PD_{ijt} + \beta_3 FF_{ijt} + \beta_4 FD_{ijt} + \mathbf{Z}\boldsymbol{\gamma} + \varepsilon_{ijt} \tag{4.4}$$

We measure the effect of foreign sector experience with the double difference $(\beta_1 - \beta_2) - (\beta_3 - \beta_4)$ or equivalently $(\beta_1 - \beta_3) - (\beta_2 - \beta_4)$. The model controls for unobserved differentials in worker quality as long as workers' wages with future outside experience can be treated as a counterfactual for the wages of workers with prior experience. However, it cannot address the possibly endogenous selection of workers to separation from their previous employers.

The results in Table 4.6 show that workers with past MNE experience earn more by 0.112 log points than their counterparts with outside domestic experience. This difference overestimates the returns to foreign sector experience since those domestic workers who are on their way to an MNE also earn more by 0.043 log points than those about to leave for another domestic employer. Ex-MNE workers earn more than future MNE employees by 0.048 log points while those with outside domestic experience earn less by 0.021 log points than their counterparts, leaving for another domestic firm later.

Table 4.6: Wage difference between domestic workers with/without outside work experience

	Dependent variable: log daily wage	
	OLS	**Firm fixed effects**
Coefficients (t-test)		
Past MNE experience (PF)	0.060*** (4.0)	0.005 (1.0)
Future MNE experience (FF)	0.012 (0.9)	0.001 (0.6)
Past domestic experience (PD)	-0.052*** (5.6)	-0.030*** (7.3)
Future domestic experience (FD)	-0.031*** (3.5)	-0.016*** (3.6)
Differences by type of outside experience (F-test)		
Past MNE-past domestic	0.112*** (61.6)	0.035*** (53.4)
Future MNE-future domestic	0.043*** (15.0)	0.017** (5.5)
Past MNE-future MNE	0.048** (6.3)	0.004 (0.4)
Past domestic-future domestic	-0.021** (4.2)	-0.014*** (10.0)
Double difference	0.069*** (15.0)	0.018*** (11.5)
aR^2/within R^2	0.453	0.342

Notes: Regression estimates. The reported coefficients are significant at the *0.1, **0.05, ***0.01 level. Unmarked coefficients are not significant at the 0.1 level. Sample: 3,841,561 person-months belonging to 153,323 persons and 18,510 firms. The sample covers domestic firm employees in firms employing at least one worker with past or future experience in foreign-owned or domestic firms, and one incumbent worker. The coefficients measure wage advantages relative to incumbent workers. Observations for 2005–2009 are used. Estimation: reghdfe without and with firm fixed effects. The standard errors are adjusted for clustering by persons and firms. Controls: person, job and firm controls, and sector–year interactions

Using these estimates, we can approximate the return to MNE work experience as the double-difference equal to 0.069 log points. The two models' main results aimed at measuring lagged wage effects (Tables 4.5 and 4.6) are similar. The first model identified a 0.060 log points advantage on the part of the median worker coming from an MNE over a worker arriving from domestic company (Table 4.5 bottom block).

While the main results are close to each other, some details differ in the two models. The wage difference between workers arriving from foreign-owned versus other domestic firms appear to be more prominent here: 0.112 points as opposed to 0.060 points in Table 4.5, model B, the estimate for all workers.[166] Second, when we reestimate the model by adding firm fixed effects (column 2 of Table 4.6), the contrasts fade away: the within-firm wage differentials between the PF-FD groups

[166]The difference may stem from differences in the samples and the periods covered by the data as well as from the influence of experience in MNEs other than the sending firm. This effect is directly estimated in Table 4.5 but not in Table 4.6.

are smaller, and the double-difference drops to only 0.018 log points. Unlike our first model, the second one suggests that the lagged MNE premium predominantly stems from past and future MNE employees' crowding in high-wage domestic firms.

4.6 Spillover effects

4.6.1 Effect of ex-MNE peers on incumbent domestic firm employees

We estimate the effect of ex-MNE peers on incumbent workers' wage, that is, for domestic firm employees who did not leave their firm in the observed period. Their wages are regressed on a set of controls and variables measuring the share of workers with previous outside experience within the worker's company and skill category. We deviate from Poole (2013) in that we also study how skilled incumbents' wages respond to the presence of less skilled ex-MNE peers. $\text{Share}_{jt}^{\text{MNE,unskilled}}$, for instance, measures the ratio of unskilled employees with recent MNE experience.

$$\begin{aligned}\ln w_{ijt} = \theta_{F3}\text{Share}_{jt}^{\text{MNE,skilled}} + \theta_{F2}\text{Share}_{jt}^{\text{MNE,middling}} + \theta_{F1}\text{Share}_{jt}^{\text{MNE,unskilled}} + \\ \theta_{D3}\text{Share}_{jt}^{\text{domestic,skilled}} + \theta_{D2}\text{Share}_{jt}^{\text{domestic,middling}} + \theta_{D1}\text{Share}_{jt}^{\text{domestic,unskilled}} + \\ + \beta Y_{ijt} + \gamma V_{jt} + v_i + s_{jt} + \varepsilon_{ijt} \quad (4.5)\end{aligned}$$

We estimate the model including only worker fixed effects, which also absorb the firm effects since the estimates relate to incumbent workers. The controls are identical to those used in Eq. 4.1. We restrict the time window to 2005–2011 to leave time for the accumulation of an ex-MNE stock. The equations are estimated separately for smaller (11–50) and larger (50+) firms, taking into consideration the higher risk of measurement error in small establishments.[167]

The fixed-effects panel equations summarized in Table 4.7 regress the log wages of incumbent skilled domestic workers on the share of workers with outside experience within the worker's firm and skill group. The estimated own effect for skilled workers in a medium-sized or large firm (θ_{F3}=0.074) implies that a one-standard-deviation difference in the share of high skilled ex-MNE employees (0.18) shifts the wages of skilled incumbents up by 1.3 percent. Having more skilled peers with outside experience in the domestic sector has no effect.

[167] The fact that the Hungarian administrative panel is only a 50% sample on the individual level, has some unfortunate implications for the spillover estimates. We observe only around half of any given firm's labour force—estimates instead of the actual shares. As not observing ex-MNE workers has the same, 50% probability as those with no such experience, in large firms we will only experience extra noise in the share variables. This noise in our explanatory variable will attenuate the estimated θ_{es} parameters, biasing them towards zero. However, in firms with a small number of workers, if the average share of given type is also low, we may mistakenly not observe any variation in our variables of interest, while we should. If a firm which previously never had a foreign worker acquires a skilled manager with foreign experience, and we do not observe the given person, observations at this firm will not have variation in the share of skilled ex-MNE workers, thus this firm will not contribute to the identification of our parameter of interest in our model with firm fixed effects. Considering these two processes we not only keep solely the firms with at least 10 employees, as in most of the paper, but also focus on larger (50+ firms), where the (predicted) share variables are less volatile.

Table 4.7: The effect of coworkers with recent outside work experience on the wages of skilled incumbents in domestic firms 2005–2011

	Share of coworkers with recent MNE experience within skill groups			**Share of coworkers with recent experience in other domestic firms within skill groups**		
	Unskilled	Middling	Skilled	Unskilled	Middling	Skilled
Notations in Eq. 4.3:	θ_{F1}	θ_{F2}	θ_{F3}	θ_{D1}	θ_{D2}	θ_{D3}
All firms	0.012	0.003	0.042***	0.015**	0.010	-0.031***
	(1.5)	(0.4)	(3.9)	(3.1)	(1.5)	(-4.5)
Firms employing>50 workers	0.000	0.028	0.074***	0.005	0.042***	-0.027**
	(0.0)	(1.2)	(4.3)	(0.7)	(2.8)	(-2.1)

Notes: Significant at *0.1, **0.05, ***0.01 level. The t-values are based on standard errors adjusted for clustering by persons and firms. θ_{F3} is significantly larger than θ_{F1}, θ_{F2} and θ_{D3}. Sample: 3,730,789 person-months in 116,204 firms in the full sample, 2,474,830 person-months in 77,411 firms in the 50+ sample. Dependent variable: log daily wage in the given month relative to the national mean. Controls: person, job and firm characteristics, sector–year interactions, and worker fixed-effects

In evaluating the cross effects, one should consider the relevant range in the share of ex-MNE workers. While a jump from zero to 50 or 100 percent in the share of ex-foreign workers within the unskilled or medium-skilled workforce is beyond the realm of reality, which renders the spillover effect to be weak, this can happen in the high skilled category. Domestic firms employing 50 workers have 7 high skilled workers on average. Hiring two managers or professionals with foreign sector experience can increase the ex-MNE share from zero to almost 30 percent overnight, which implies a 0.022 log points wage increase for skilled incumbents.

4.6.2 Reestimating spillover effects for all domestic firm employees

Incumbents in our data account for only 22 percent of the workers ever employed in the domestic sector and 34 percent of the workers never employed outside the domestic sector. The estimates of spillover effects using their sample may be biased because their exposure to peers with MNE experience differs substantially from that of the average worker. As shown in Table 4.8, the mean within-firm share of skilled MNE-experienced peers amounts to 9 percent in the case incumbents instead of 14.6 percent in the case of their non-incumbent counterparts—a predictable pattern since incumbents are more likely to be found in firms with low labor turnover.

Table 4.8: Mean within-firm share of coworkers with past MNE experience (percent)

	Skilled incumbents in domestic firms		**Skilled domestic firm employees without MNE experience**	
	Share of coworkers with MNE experience	*Number of workers*	*Share of coworkers with MNE experience*	*Number of workers*
Unskilled	7.0	38,355	13.3	73,320
Medium skilled	9.3	53,896	15.4	103,871
Skilled	9.0	55,900	14.6	107,250

Notes: Incumbents are workers, who had only a single domestic-owned employer in 2003–2011. The mean within-firm shares are weighted with firm size and relate to 2003–2011

A higher share of ex-MNE peers increases the likelihood of personal contacts, thereby assisting the diffusion of MNE-based skills within the firm. At the same time, the typical incumbent worker spends more time with the firm, so she has a better chance to absorb the imported knowledge. Because of the potential bias in either direction, we reestimate the spillover model for all domestic workers, including firm fixed effects on top of the worker fixed effects in the model to ensure that it identifies within-firm impacts.

The results for firms with more than 50 workers and all firms are presented in Table 4.9. Starting with the former: the own effect (0.060) is slightly lower than the point estimate for incumbents (0.074 in Table 4.4). Less skilled ex-MNE workers exert a weak effect—the respective coefficients are only significant at the 5 percent level. Having more skilled peers with recent outside experience in domestic firms do not affect wages positively at all. The estimates for all firms are much lower and insignificant at 5 percent level. The inward bias is probably explained by the noisy measurement of the F and D ratios in smaller enterprises.

Table 4.9: The effect of coworkers with recent outside work experience on the wages of skilled workers in domestic enterprises 2005–2011

	Share of coworkers with recent MNE experience within skill groups			**Share of coworkers with recent experience in other domestic firms within skill groups**		
	Unskilled	Middling	Skilled	Unskilled	Middling	Skilled
Notations in Eq. 4.3:	θ_{F1}	θ_{F2}	θ_{F3}	θ_{D1}	θ_{D2}	θ_{D3}
All domestic firms	0.007	0.013	0.020*	0.016***	0.024***	-0.037***
	(0.9)	(1.5)	(1.9)	(3.2)	(3.4)	(-4.5)
Domestic firms employing>50 workers	0.006	0.057**	0.060***	0.002	0.064***	-0.019
	(0.5)	(2.1)	(3.4)	(0.2)	(3.4)	(-1.3)

Notes: Significant at *0.1, **0.05, ***0.01 level. The t-values are based on standard errors adjusted for clustering by persons and firms. θ_{F3} is significantly larger than θ_{F1} and θ_{D3}, but not θ_{F2}. θ_{F2} is significantly larger than θ_{F1} Sample: 3,731,548 person-months belonging to skilled workers in 116,249 firms in full sample, 2,474,843 person-months in 77,412 firms in the 50+ sample. Dependent variable: log daily wage in the given month relative to the national mean. Controls: person, job and firm characteristics, sector–year interactions, worker and firm fixed-effects

The estimated spillover effect might seem economically insignificant, but it is actually stronger than those we know from the literature. The study of Poole (2013) — which is closest to ours concerning method, sample characteristics, and industry coverage—estimated that at the average wage for a typical domestic worker, a 10 percentage points increase in the share of former MNE workers increased incumbents' wages by $23 per year. This amount could buy a little more than one Starbucks solo espresso a month in Rio de Janeiro in 2015. The comparable estimate for skilled incumbents in our sample is $139 a year, which could buy 5.2 cups of Starbucks espresso a month in Budapest at 2015 prices.[168]

Learning from ex-MNE peers is only one explanation for the effect we identify. A firm's effort to maintain its wage ladder after hiring a high-wage ex-MNE worker could occasionally motivate a firm to increase the wages of other employees. Still, we do not find this explanation convincing when spillover is observed in tens of thousands of firms. Why would so many domestic firms hire high-wage workers from MNEs if this decision implies further wage growth without an underlying rise in productivity? A positive selection of all workers to firms hiring from MNEs can also raise the average wage of coworkers with no MNE experience. However, our findings controlled for worker fixed effects and/or relating only to incumbents are free of this kind of bias. Last but not least, the finding that only skilled ex-

[168]The calculation is based on the estimated own effect (0.074), the mean monthly earnings of skilled domestic firm employees in 2011 (236,078 Ft) and an average exchange rate of 225 Ft/$ in 2011 (National Bank, http://mnbkozeparfolyam.hu/arfolyam-2011.html). We could find Starbucks solo espresso prices for 2015 on the websites of local shops in Rio and Budapest: $1.92 and $1.43, respectively.

MNE peers have an effect on skilled wages yields further support to the learning hypothesis.

4.7 Two notes on differences by skills and sectors

Throughout this paper, we focused on skilled workers mainly because we are interested in possible knowledge flows from foreign-owned to domestic firms, the traces of which are easier to find in the skilled labor market.[169] We nevertheless estimated all our models for less-skilled workers and found that the effects of interest are smaller and, in many cases statistically insignificant. Appendix D.1: Fig. D.1 illustrates this point. The figure compares the estimates of the wage gap model (Table 4.3, model A) to similar ones for unskilled and medium-skilled workers. The latter are very close to each other and amount to about 0.4 log points in the uncontrolled model, less than 0.1 in the panel regression with worker FE and less than 0.02 in the 2FE model.

Data available in the Labor Force Survey (Tables D.3, D.4 of Appendix D.1) furthermore suggest that a part of the MNE premium compensates unskilled workers for non-wage disamenities. Overtime work and afternoon and night shifts are about twice as likely to occur among low and medium-skilled MNE employees compared to their domestic counterparts. There is a smaller but similarly signed difference concerning work on Saturdays and Sundays. Furthermore, low skilled workers have a higher probability of becoming unemployed in foreign-owned than domestic firms. The data does not indicate ownership-specific differences of this kind among highly skilled workers.

Table 10 summarizes point estimates of the wage gap, lagged returns, and spillover effects from our preferred model specifications for manufacturing and all other sectors labeled 'services'. The foreign-domestic wage gap is more substantial in services than manufacturing, and the lagged returns are broadly similar or somewhat larger in services. By contrast, the spillover effects are estimated to be stronger in manufacturing. We do not go to the details of the between-sector differences. We only note that the returns to MNE experience are not restricted to the manufacturing sector heavily over-represented in the related literature.

[169] Skilled workers account for 25 percent of the total population observed in the source file. 15 per cent is unskilled (never worked in an occupation requiring nay kind of qualification) and 60 percent is classified as middling (worked in skilled jobs but not in ones requiring tertiary educational attainment).

Table 4.10: Selected estimates by sectors

	Manufacturing	Services
Contemporaneous MNE premium		
All firms, worker FE	0.152	0.236
New, incumbent firms, DiD	0.135	0.232
Lagged MNE premium in domestic firms		
Sending firm is MNE, $dL < 0.5$, OLS	0.135	0.133
Sending firm is MNE, $dL < 0.5$, firm FE	0.056	0.044
Overlapping cohorts estimate, DiD	0.027	0.072
Spillover effect, firms $L \leq 50$ employees		
On incumbents	0.088	0.057
On all workers with no MNE experience	0.069	0.05

Notes: All coefficients are significant at the 0.01 level. The coefficients were estimated separately for the two sectors

4.8 Discussion

We interpret the coincidence of an MNE premium, substantial wage loss from separation, lagged returns to MNE experience, and wage spillover as a signal of knowledge flows from FDI to domestic firms. In such a scenario, workers acquiring general and firm-specific skills in the modern environment of MNEs are expected to earn more than their domestic counterparts. The specific components in their skills imply that MNE workers lose a part of their wage advantage in case of involuntary separation. The general component in their skills gives rise to wage advantages in their new, domestic firm and tends to influence their peers' productivity. The simultaneity of these symptoms calls into question some alternative explanations, of which we discuss three ones.

First, the finding of a contemporaneous MNE premium even after controlling for worker fixed effects calls into question that the foreign-domestic wage gap is fully explained by the crowding of high productivity workers in foreign-owned firms. Similarly, in a comparison of domestic and foreign-owned start-ups, we find a sizable MNE premium even after controlling for their workers' pre-entry wages.

Second, intense human capital accumulation is admittedly not the only potential source of an MNE premium, with the most important alternative being efficiency wage setting. MNEs may try to prevent leakage of information through labor turnover by paying a premium above the market level (Fosfuri et al., 2001). Their limited knowledge of the local labor market and capital-labor relations may urge them to pay high wages and share a part of their revenues with workers. Furthermore, they may try to compensate their employees for a higher labor demand volatility (Fabbri et al., 2003) or a higher plant closure rate (Bernard & Sjoholm, 2003). The implications of skills accumulation versus efficiency wages for the foreign-domestic wage gap and the wage loss from separation are observationally identical. However, efficiency wages in MNEs do not imply that ex-MNE employees earn a premium over the receiving domestic firm's going wage rate and exert influence on the earnings of their peers.

Third, a set of findings like this is likely to emerge only if MNE workers accumulate both general and firm-specific knowledge. As outlined in Becker (1962) seminal paper, in the case of general skills acquired through on-the-job training, productivity and wages move in tandem. Workers accumulating a substantial stock of general skills in one firm are expected to earn higher-than-average wages in other firms. As far as general skills develop through informal communication between coworkers, their presence also tends to have a spillover effect. However, we do not expect that separation from an MNE induces a wage loss in this scenario.

If the acquired knowledge is purely firm-specific, and the risk of voluntary separation (motivated by factors other than between-firm wage differentials) is zero, then the firm pays the going market wage before, during and after the period of skills accumulation. These skills lose their value with separation without an impact on salaries. Pre-separation and post-separation wages are equal, post-separation wages do not exceed the host firm's average level, and they do not exert influence on the earnings of coworkers. In the likely case of non-zero risk of voluntary quits, the firm will share in the costs and benefits of training, which implies lower wages in the accumulation phase and higher wages afterward as long as the worker stays with her employer. In this case, post-training involuntary separations imply a wage loss, but we continue not to expect lagged returns and spillover effects.

The literature emanating from Becker's benchmark models has been trying to reconcile the theory of on-the-job training with a series of empirical observations inconsistent with the extreme scenarios. A series of empirical findings and ample everyday experience suggest that (i) most skills are general, or at least sector rather than firm-specific (ii) enterprises are willing to pay for general training, and (iii) involuntary separations typically imply a loss. Acemoglu and Pischke (1999) demonstrate that in a variety of market settings such as a compressed wage structure, substantial hiring costs, information asymmetry, and other labor market imperfections, general skills are rewarded as if they were partly specific. The "skill-weights" model of Lazear (2009) hypothesizes that skills are predominantly general, but firms attach different weights to their components. A worker who leaves a firm will have a difficult time finding another employer that can make use of all the skills he acquired at the sending firm. The limits of transferability impose a cost on mobile workers, so the workers are unwilling to bear the full cost of training, and the costs and benefits will be shared. Such a setting is likely to produce all of the four outcomes observed in our data.

4.9 Conclusions

We found that high skilled MNE workers earn substantially higher wages than their domestic counterparts. They lose a part of their wage advantage after leaving the foreign-owned sector but, even so, they earn more than their domestic sector colleagues with no MNE experience. Their presence in domestic firms exerts a positive effect on the wages of their peers, who had no contact with foreign-owned firms or had no recent outside work experience at all.

The direct and indirect wage returns to work experience in MNEs are large in Hungary, similar to less developed countries analyzed in the literature. The positive wage effects are not restricted to the manufacturing sector, which is in the focus of attention in the research on FDI. The estimates suggest that the effect of MNE experience on domestic sector wages is strongly affected by between-firm variance, that is, the higher-than-average wages of domestic firms connected with the MNEs via labor turnover.

Finally, the results draw attention to the difficulties of identifying a ‘pure’ ownership effect. The non-random selection of firms flaws the identification of the foreign-domestic wage gap from acquisitions. Thanks to a rich and big data set, we could compare how workers are selected to new MNEs and domestic firms, and identify a substantial wage differential between them. In the analysis of lagged returns and spillovers, we drew attention to trade-offs between model quality and unbiasedness of the samples on which the models can be estimated.

As we find substantial wage effects attributed to foreign ownership both in the short-run and long-run, even after controlling for potential biases as much as possible, we believe that the presence and significance of knowledge transfer from MNEs is beyond doubt. Therefore, we argue that FDI coming from more developed countries exert positive effects on the receiving countries’ labor markets both through direct, and indirect channels. Exploring whether these gains outweigh the potential drawbacks could be the focus of future research on the topic.

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A Appendix for Chapter 1

A.1 Additional tables and graphs

Table A.1: Wage variance decompositions based on AKM methods

Article	Country	Period (years)	Sample	Bias-c.	Var(w)	Var(θ) sh(%)	Var(ψ) sh(%)	Cov(θ,ψ) sh(%)	R(θ,ψ)
Gruetter and Lalive (2009)	Austria	1990-1997 (8)			0.224	66.3	37.0	-22.5	-0.27
Bonhomme et al. (2020)	Austria	2010-2015 (6)	BLM corrected	BLM	0.187	n.a.	11.7	19.6	n.a.
Lopes De Melo (2018)	Brazil	1995-2005 (11)			0.601	66.6	30.0	3.6	0.04
Alvarez et al. (2018)	Brazil	2008-2012 (5)			0.470	57.4	14.9	19.1	0.33
Engbom and Moser (2021)	Brazil	2010-2014 (5)	KSS corrected	KSS	0.453	29.4	17.0	15.2	0.34
Gerard et al. (2021)	Brazil	2002-2014 (13)	White male	KSS	0.449	36.3	16.4	22.8	0.47
Gerard et al. (2021)	Brazil	2002-2014 (13)	Non-white male	KSS	0.332	30.0	16.4	17.6	0.40
Gerard et al. (2021)	Brazil	2002-2014 (13)	White female	KSS	0.498	43.9	15.0	24.6	0.48
Gerard et al. (2021)	Brazil	2002-2014 (13)	Non-white female	KSS	0.324	44.4	14.0	18.4	0.37
Bagger and Lentz (2019)	Denmark	1985-2003 (19)			0.097	72.2	14.4	-2.1	-0.03
T. Sørensen and Vejlin (2013)	Denmark	1985-2003 (19)	AKM		0.111	33.1	4.6	-1.5	-0.06
T. Sørensen and Vejlin (2013)	Denmark	1985-2003 (19)	Match		0.113	50.3	5.5	-1.8	-0.05
K. L. Sørensen and Vejlin (2011)	Denmark	1980-2006 (27)			0.094	57.0	13.0	1.6	0.03
Abowd et al. (2002)	France	1976-1987 (12)			0.269	76.9	30.2	-27.2	-0.28
Abowd et al. (1999)	France	1976-1987 (12)			0.269	69.8	87.0	46.2	0.30
Goux and Maurin (1999)	France	1993-1995 (3)			0.151	79.3	19.6	1.3	0.01
Abowd and Kramarz (2004)	France	1976-1996 (21)			0.354	70.3	61.5	-31.8	-0.24
Andrews et al. (2008)	Germany	1993-1997 (5)	Bias corrected	AGSU	0.055	92.0	21.6	-13.2	-0.15
Goldschmidt and Schmieder (2017)	Germany	2008-2008 (1)			0.205	n.a.	26.7	20.8	n.a.
Card et al. (2013)	Germany	2002-2009 (8)			0.249	51.0	21.3	16.5	0.25
This study: Boza (2021)	Hungary	2003-2017 (15)			0.338	38.5	18.3	9.3	0.18
Macis and Schivardi (2016)	Italy	1982-1997 (16)			0.116	49.8	14.6	-1.1	-0.02
Iranzo et al. (2008)	Italy	1981-1997 (17)			0.110	43.9	13.1	2.1	0.04
Kline, Saggio, and Sølvsten (2020b)	Italy	1999-2001 (3)	KSS	KSS	0.184	60.8	13.0	16.0	0.28
Fanfani (2018)	Italy	1996-2001 (6)	Female		0.079	62.0	19.0	-6.3	-0.09
Fanfani (2018)	Italy	1996-2001 (6)	Male		0.141	78.7	9.9	5.0	0.09
Devicienti et al. (2016)	Italy	1996-2001 (6)			0.131	99.2	18.3	-4.6	-0.05
Bonhomme et al. (2020)	Italy	1996-2001 (6)	BLM corrected	BLM	0.167	n.a.	12.7	20.0	n.a.
Casarico and Lattanzio (2019)	Italy	1995-2015 (21)	Male		0.236	185.0	18.5	-5.0	-0.04
Casarico and Lattanzio (2019)	Italy	1995-2015 (21)	Female		0.172	187.3	22.1	0.0	0.00
Bonhomme et al. (2020)	Norway	2009-2014 (6)	BLM corrected	BLM	0.239	n.a.	11.8	16.8	n.a.
Card et al. (2018)	Portugal	2005-2009 (5)			0.275	64.1	22.7	13.0	0.17
Card et al. (2016)	Portugal	2002-2009 (8)	Male		0.307	57.5	19.9	11.3	0.17
Card et al. (2016)	Portugal	2002-2009 (8)	Female		0.263	60.8	17.2	9.8	0.15
Torres et al. (2018)	Portugal	1986-2009 (24)	No job effects		0.323	75.0	18.0	16.8	0.23
Torres et al. (2018)	Portugal	1986-2009 (24)	With job effects		0.323	38.0	16.1	9.7	0.20
Bonhomme et al. (2020)	Sweden	2000-2005 (6)	BLM corrected	BLM	0.164	n.a.	5.0	10.3	n.a.
Sorkin (2018)	US	1990-1999 (10)			0.670	51.0	14.0	10.0	0.19
Abowd and Kramarz (2004)	US	2000-2008 (9)			0.800	78.7	16.3	1.5	0.02
Song et al. (2019)	US	2007-2013 (7)			0.924	51.5	8.8	11.7	0.28
Woodcock (2015)	US	2007-2013 (7)	No match		0.410	71.0	19.5	-1.0	-0.01
Woodcock (2015)	US	2007-2013 (7)	Orth. match		0.410	70.7	19.8	-0.6	-0.01
Bonhomme et al. (2020)	US	2010-2015 (6)	BLM corrected	BLM	0.414	n.a.	6.2	15.0	n.a.
Lamadon et al. (2019)	US	2001-2015 (15)	AKM, BLM	BLM	0.450	72.4	3.2	13.1	0.43
Lamadon et al. (2019)	US	2001-2015 (15)	TV-AKM, BLM	BLM	0.450	73.5	3.3	13.4	0.43
Abowd et al. (2002)	US	1990-2000 (11)			0.278	81.6	19.2	-2.0	-0.03
Lachowska et al. (2020)	US	2002-2014 (13)	AKM , KSS	KSS	0.407	61.4	11.6	16.9	0.32
Lachowska et al. (2020)	US	2002-2014 (13)	TV-AKM, KSS	KSS	0.407	62.2	13.5	14.8	0.26

Notes: KSS bias-correction refers to the method of Kline, Saggio, and Sølvsten (2020b), BLM to Bonhomme et al. (2019) and AGSU to Andrews et al. (2008). Boza (2021) contains a more detailed version of the table.

Table A.2: Comparison of bias-corrected and standard results from the literature

Article	Country	Period (years)	Sample	Bias-c.	Var(w)	Var(θ) sh(%)	Var(ψ) sh(%)	Cov(θ,ψ) sh(%)	R(θ,ψ)
Bonhomme et al. (2020)	Austria	2010-2015 (6)		BLM	0.187	n.a.	11.7	19.6	n.a.
Bonhomme et al. (2020)	Austria	2010-2015 (6)			0.187	n.a.	18.7	4.7	n.a.
Gerard et al. (2021)	Brazil	2002-2014 (13)	Non-white F	KSS	0.324	44.4	14.0	18.4	0.37
Gerard et al. (2021)	Brazil	2002-2014 (13)	Non-white F		0.307	62.2	23.1	7.7	0.10
Gerard et al. (2021)	Brazil	2002-2014 (13)	Non-white M	KSS	0.332	n.a.	16.4	17.6	0.40
Gerard et al. (2021)	Brazil	2002-2014 (13)	Non-white M		0.339	50.8	23.0	11.4	0.17
Gerard et al. (2021)	Brazil	2002-2014 (13)	White F	KSS	0.498	43.9	15.0	24.6	0.48
Gerard et al. (2021)	Brazil	2002-2014 (13)	White F		0.469	59.2	19.7	18.0	0.26
Gerard et al. (2021)	Brazil	2002-2014 (13)	White M	KSS	0.449	36.3	16.4	22.8	0.47
Gerard et al. (2021)	Brazil	2002-2014 (13)	White M		0.449	52.2	20.6	18.0	0.28
Engbom and Moser (2021)	Brazil	2010-2014 (5)		KSS	0.453	29.4	17.0	15.2	0.34
Engbom and Moser (2021)	Brazil	2010-2014 (5)			0.453	34.0	18.1	13.5	0.27
Andrews et al. (2008)	Germany	1993-1997 (5)		AGSU	0.055	92.0	21.6	-13.2	-0.15
Andrews et al. (2008)	Germany	1993-1997 (5)			0.057	94.4	23.5	-18.0	-0.19
Bonhomme et al. (2020)	Italy	1996-2001 (6)		BLM	0.167	n.a.	12.7	20.0	n.a.
Bonhomme et al. (2020)	Italy	1996-2001 (6)			0.167	n.a.	23.1	-1.3	n.a.
Kline, Saggio, and Sølvsten (2020b)	Italy	1999-2001 (3)		KSS	0.184	60.8	13.0	16.0	0.28
Kline, Saggio, and Sølvsten (2020b)	Italy	1999-2001 (3)			0.184	71.8	19.5	4.2	0.06
Bonhomme et al. (2020)	Norway	2009-2014 (6)		BLM	0.239	n.a.	11.8	16.8	n.a.
Bonhomme et al. (2020)	Norway	2009-2014 (6)			0.239	n.a.	24.4	-7.7	n.a.
Bonhomme et al. (2020)	Sweden	2000-2005 (6)		BLM	0.164	n.a.	5.0	10.3	n.a.
Bonhomme et al. (2020)	Sweden	2000-2005 (6)			0.164	n.a.	14.6	-8.1	n.a.
Lachowska et al. (2020)	US	2002-2014 (13)		KSS	0.407	61.4	11.6	16.9	0.32
Lachowska et al. (2020)	US	2002-2014 (13)			0.407	63.0	11.8	16.7	0.31
Lachowska et al. (2020)	US	2002-2014 (13)		KSS	0.407	62.2	13.5	14.8	0.26
Lachowska et al. (2020)	US	2002-2014 (13)			0.407	63.7	14.0	14.5	0.24
Lamadon et al. (2019)	US	2001-2015 (15)		BLM	0.450	72.4	3.2	13.4	0.44
Lamadon et al. (2019)	US	2001-2015 (15)			0.450	75.0	9.0	5.2	0.10
Bonhomme et al. (2020)	US	2010-2015 (6)		BLM	0.414	n.a.	6.2	15.0	n.a.
Bonhomme et al. (2020)	US	2010-2015 (6)			0.414	n.a.	12.2	1.1	n.a.

Notes: KSS bias-correction refers to the method of Kline, Saggio, and Sølvsten (2020b), BLM to Bonhomme et al. (2019) and AGSU to Andrews et al. (2008). Boza (2021) contains a more detailed version of the table.

Table A.3: Decomposition of wage variance, based on Song et al. (2019)

Variance of log wages	0.338	
Between-firm Variance	**0.160**	47.5%
— *Var($\bar{\theta}$)*	*0.038*	11.3%
— *Var(ψ)*	*0.062*	18.3%
— Var($\bar{\lambda}$)	*0.003*	0.8%
— Var($\bar{X}\beta$)	*0.006*	1.7%
— *2Cov($\bar{\theta}$,ψ)*	*0.031*	9.3%
— 2Cov($\bar{\lambda}$,$\bar{\theta}$)	*0.014*	4.1%
— 2Cov($\bar{\lambda}$,ψ)	*-0.003*	-0.8%
— 2Cov($\bar{X}\beta$,$\bar{\theta}$)	*0.008*	2.4%
— 2Cov($\bar{X}\beta$,ψ)	*-0.002*	-0.7%
— 2Cov($\bar{X}\beta$,$\bar{\lambda}$)	*0.004*	1.1%
Within-firm Variance	**0.177**	52.5%
— Var(($\theta-\bar{\theta}$))	*0.092*	27.3%
— Var(($\lambda-\bar{\lambda}$))	*0.005*	1.5%
— Var(($X-\bar{X})\beta$)	*0.007*	2.1%
— Var(ε)	*0.049*	14.6%
— 2Cov(($\lambda-\bar{\lambda}$),($\theta-\bar{\theta}$))	*0.022*	6.6%
— 2Cov(($X-\bar{X})\beta$,($\theta-\bar{\theta}$))	*0.001*	0.2%
— 2Cov(($X-\bar{X})\beta$,($\lambda-\bar{\lambda}$))	*0.001*	0.2%
— 2Cov(ε,($\theta-\bar{\theta}$))	*0.000*	0.0%
— 2Cov(ε,($\lambda-\bar{\lambda}$))	*0.000*	0.0%
— 2Cov(ε,($X-\bar{X})\beta$)	*0.000*	0.0%
Number of Observations (1000)	66155	
Number of Firms (1000)	146	
Number of Workers (1000)	2462	

Notes: Decomposition is based on Equation 1.7.

Table A.4: Decomposition of wage variance, estimated on random sub-samples

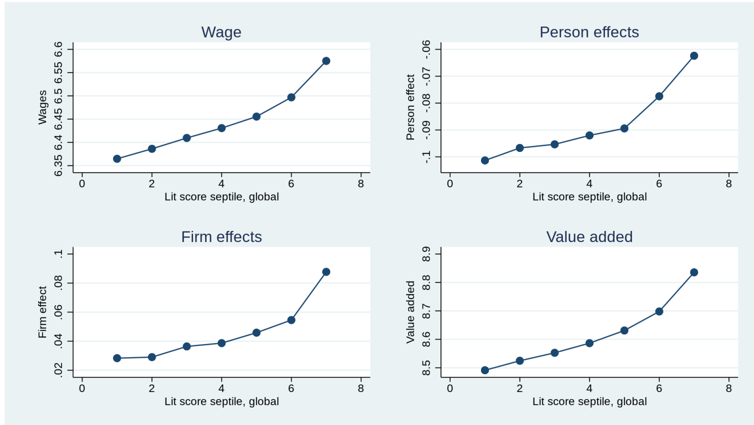
Estimation sample	Full	Sub	Sub	Full	Full	Alt.
Reporting sample	50%	20%	10%	20%	10%	50%
Variance of log wages	0.338	0.336	0.335	0.337	0.339	0.338
Ensemble decomp. (%)						
Contribution of XB	5.40	5.33	5.14	5.40	5.39	5.99
— Year	1.98	1.95	1.92	1.99	1.97	2.21
— age*, firm size, contract, tenure*	3.41	3.38	3.23	3.42	3.42	3.77
Contribution of individual heterogeneity	49.85	50.71	51.50	49.79	49.90	49.32
— Unobserved individual heterogeneity	29.00	29.22	29.49	28.94	29.06	28.58
— Observed individual (gender, quasi ed.)	17.62	18.16	18.64	17.62	17.61	17.61
— Birth year	0.32	0.38	0.39	0.33	0.32	0.23
— Region	2.91	2.96	2.97	2.90	2.91	2.90
Contribution of firm heterogeneity	22.21	21.49	20.96	22.25	22.16	22.43
— Unobserved firm heterogeneity	15.69	15.28	14.98	15.72	15.59	15.87
— Observed firm heterogeneity (ownership)	4.14	3.83	3.59	4.13	4.15	4.21
— Sector	2.38	2.37	2.39	2.40	2.43	2.35
Contribution of occupations	7.93	7.97	8.01	7.93	7.97	8.02
Residual variation	14.61	14.51	14.38	14.63	14.57	14.25
Correlations						
Corr(θ_i,ψ_j))	0.175	0.150	0.130	0.175	0.175	0.170
Corr(ε_i^I,ε_j^J)	0.138	0.111	0.088	0.136	0.138	0.137
Corr(θ_i,ψ_j) for inc. firms	0.31	0.293	0.275	0.311	0.310	0.307
Corr(θ_i,VA_j) for inc. firms	0.364	0.369	0.376	0.364	0.362	0.361
Corr(ψ_j,VA_j) for inc. firms	0.614	0.606	0.600	0.614	0.615	0.616
Between-within decomposition (%)						
Between-firm share	47.7	46.9	46.0	47.9	47.7	48.0
— Ind. segregation	11.6	11.5	11.3	11.2	11.2	11.2
— $Var(\psi_j)$)	18.2	18.0	17.8	18.5	18.5	18.7
— Sorting	9.1	9.0	8.9	9.2	9.1	9.1
Number of Observations (1000)	66155	24809	11526	26462	6616	66354
Number of Firms (1000)	144	115	84	133	93	144
Number of Workers (1000)	2462	932	438	985	246	2468

Notes: See Table 1.1. Column 1 is the main result of 1.1. Columns 2 and 3 are from AKM models re-estimated on randomly drawn 20% and 10% samples of the population of workers (without replacement). Columns 4 and 5 use the wage components estimated as in Column 1, but reported on random subsamples. Column 6 represents an AKM model estimated on data using a different sampling of monthly observations, using wage data from February, May, August and November, instead of January, April, July and October.

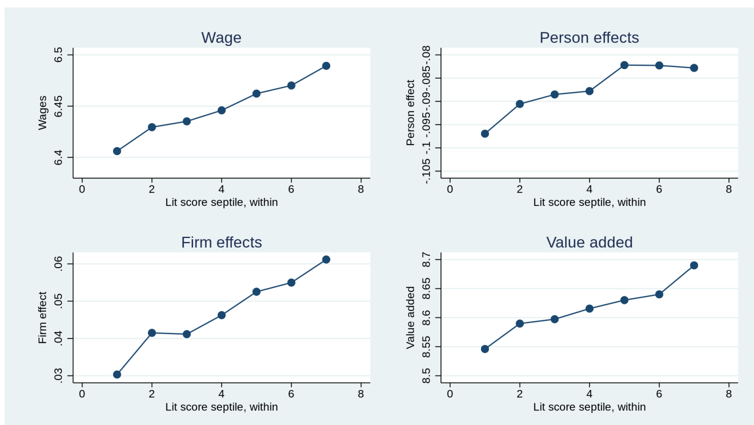
Table A.5: Cross-sectional rent-sharing elasticity estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV - Lag	IV - Lag	IV - Sales	IV - Sales
Outcome:	lnW	ψ_j	lnW	ψ_j	lnW	ψ_j
Ln(VA/L)	0.346***	0.153***	0.391***	0.172***	0.401***	0.173***
	(0.010)	(0.005)	(0.011)	(0.006)	(0.013)	(0.007)
Firm-years	394,585	363,196	280,761	263,104	394,531	363,147
R-squared	0.618	0.525	0.455	0.320	0.444	0.316
Number of units	45	44	45	44	45	44

Notes: *** significant at the 0.1% level. Cluster-robust standard errors in parentheses. Outcome is the mean of (log-)value added per worker of the firm in the given year for specifications (1), (3) and (5). For specifications (2), (4) and (6) the outcome is the estimated AKM firm effect of the employer. Controls include firm size and fixed effects for 45 sectors defined as the interaction of 3 ownership and 15 industry categories and year fixed effects. The instruments used are the one-year lags of logged productivity and the logarithm of sales per worker of the firms in the same year.



(a) Overall septiles



(b) Within-school septiles

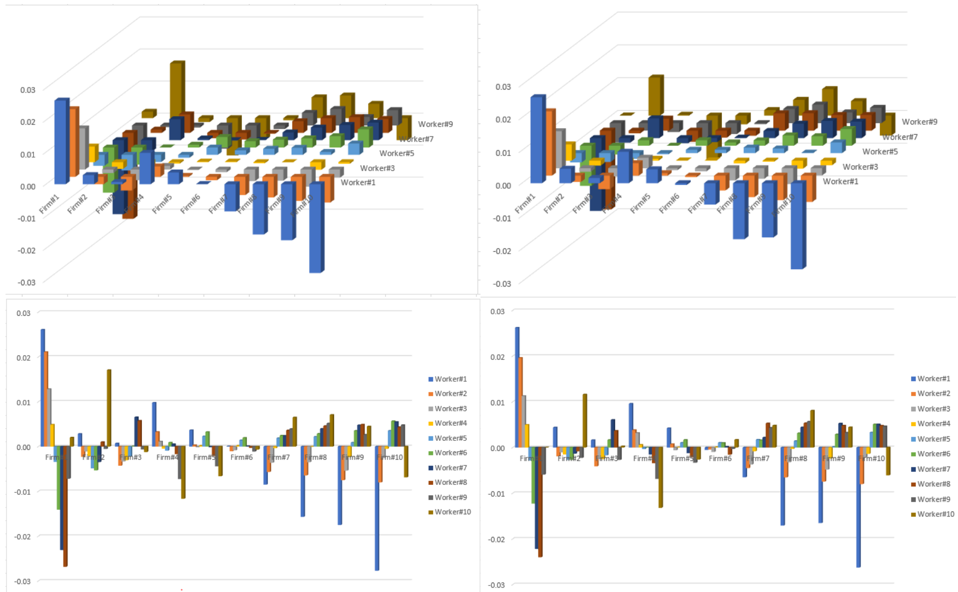
Figure A.1: Wage components and value added along 10^{th} grade literacy score septiles from NABC

Notes: The seven quartiles are created along the distribution of literacy scores in year the students took the test (top panel), or within the distribution of the given school-year (bottom panel). The figures relate to those students for whom we have a test score observation no sooner than 2008 and also at least one wage observation anytime in the panel. The value added measure is available only for incorporated firms and not for public institutions.

Table A.6: Decomposition of wage variance, with match effects

Variance of log wages	0.338	
Ensemble decomp. (and sub-shares) (%)		
Contribution of XB	**5.05**	
— Year	*2.04*	40.4
— age*, firm size, contract, tenure*	*3.01*	59.6
Contribution of match heterogeneity	**5.06**	
Contribution of individual heterogeneity	**51.79**	
— Unobserved individual heterogeneity	*30.38*	58.7
— Observed individual (gender, quasi ed.)	*18.15*	35.0
— Birth year	*0.38*	0.7
— Region	*2.88*	5.6
Contribution of firm heterogeneity	**23.70**	
— Unobserved firm heterogeneity	*16.68*	70.4
— Observed firm heterogeneity (ownership)	*4.55*	19.2
— Sector	*2.47*	10.4
Contribution of occupations	**4.50**	
Residual variation	**9.90**	
Correlations (and contr. to overall)		
Corr(θ_i,ψ_j)	0.176	10.0%
Corr(ε_i^I,ε_j^J)	0.131	4.5%
Corr(θ_i,ψ_j) for inc. firms	0.288	15.5%
Corr(θ_i,VA_j) for inc. firms	0.346	
Corr(ψ_j,VA_j) for inc. firms	0.608	
Between-within decomposition (%)		
Between-firm share	47.3	
— Ind. segregation	12.0	
— Var(ψ_j)	19.0	
— Sorting	10.0	
Number of Observations (1000)	71914	
Number of Firms (1000)	161	
Number of Workers (1000)	2660	

Notes: See Table 1.1. The first stage is estimated with match and occupation fixed effects as in Equation 1.16, with match effects decomposed according to Equation 1.17, and the firm and person effects decomposed according to 1.2 and 1.3.



(a) Match effects (b) Residuals

Figure A.2: The estimated match effects and residuals along firm and worker effect deciles

Notes: The left panel presents the mean value of $\tilde{\omega}_{ij}$ from Equation 1.17 by cells defined along 10 deciles of estimated firm effects and 10 deciles of estimated person effects. The right panel contains the mean values of ε_{ijt} from Equation 1.1 along the same distribution.

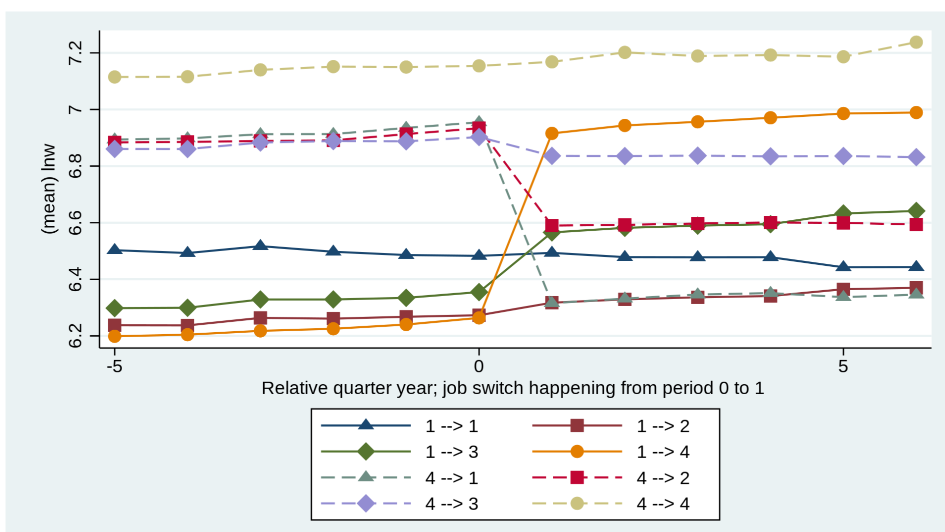


Figure A.3: Event study of Card et al. (2013)

Notes: Data points represent mean log wages of job-switchers in the 18 months before, and 18 months following a job-to-job transition (on a quarterly basis), categorized by the firm effect quartile the worker belonged to before and after the switch. Only switches originating or arriving in the bottom or the top quartiles are included in the graph.

Table A.7: Contribution of wage sorting, by sectors, regions and occupations

	N	Mean(w)	Var(w)	Corr.	Contr.
Occupation					
Managers	1692694	7.3	0.390	0.234	12.2
Professionals	5609251	7.0	0.305	0.243	14.1
Technicians and associate professionals	5520028	6.7	0.295	0.292	16.4
Office and management occupations	1930135	6.6	0.221	0.190	11.2
Commercial and services occupations	3176894	6.3	0.116	0.046	2.9
Agricultural and forestry occupations	223064	6.3	0.119	-0.007	-0.4
Industry and construction occupations	3540587	6.5	0.206	0.232	12.7
Machine operators, assembly workers, drivers	4655858	6.5	0.184	0.110	6.0
Elementary occ. requiring no qualification	4513468	6.2	0.109	0.061	4.3
Collapsed to occupation-years	70	6.6	0.120	0.107	2.6
Sector					
A -Agriculture, forestry and fishing	611855	6.5	0.182	-0.037	-1.8
D -Electricity, gas, steam and air conditioning	7317011	6.6	0.310	0.289	13.5
E -Water supply, sewerage, waste management	305221	7.1	0.345	0.198	7.2
F -Construction	492562	6.6	0.190	0.153	6.1
G -Wholesale and retail trade; repair of vehicles	873188	6.5	0.281	0.238	12.7
H -Transporting and storage	3092502	6.6	0.289	0.305	15.8
I -Accommodation and food service activities	2225119	6.6	0.246	0.220	9.7
J -Information and communication	679052	6.3	0.146	0.087	4.7
K -Financial and insurance activities	757030	7.2	0.396	0.136	7.1
L -Real estate activities	824052	7.2	0.389	0.172	6.7
M -Professional, scientific and technical activities	274265	6.5	0.259	0.184	10.3
N -Administrative and support service activities	670799	7.0	0.468	0.318	16.1
O -Public administration, defence, social security	1547679	6.4	0.224	0.224	12.5
Q -Human health and social work activities	174167	6.7	0.250	0.069	3.4
R -Arts, entertainment and recreation	417977	6.4	0.221	0.136	5.7
S -Other services, activities	175165	6.7	0.334	0.072	3.9
T -Activities of households as employers	240482	6.5	0.229	0.235	11.7
Collapsed to sector-years	148	6.7	0.084	0.716	31.2
Region					
Budapest	4891390	6.8	0.425	0.277	14.0
Central Hungary	3873678	6.7	0.362	0.236	12.1
Central Transdanubia	3929306	6.6	0.293	0.103	5.8
Western Transdanubia	3370532	6.6	0.289	0.111	6.1
Southern Transdanubia	2716328	6.5	0.287	0.125	6.4
Northern Hungary	3647330	6.6	0.280	0.098	5.3
Northern Great Plain	4442112	6.5	0.264	0.087	4.6
Southern Great Plain	3817505	6.5	0.264	0.077	4.2
Unknown	181013	6.4	0.245	0.152	8.2
Collapsed to region-years	63	6.6	0.024	0.557	14.3
All categories	30869194	6.6	0.325	0.173	9.1

Notes: The table reports the number of observations, mean log wage and the variance of log wages for given occupations, sectors or regions, alongside the correlation of estimated firm and worker effects in these cells with their respective contribution to $Var(w)$. The table relates only to the period of 2011-2017 (due to changes in occupational classifications). The last row of each panel corresponds to the between-unit estimations, where we collapsed data into the unit-year observations before estimating means, variances and correlations.

Table A.8: Decomposition of wage variance, occupation-sector effects

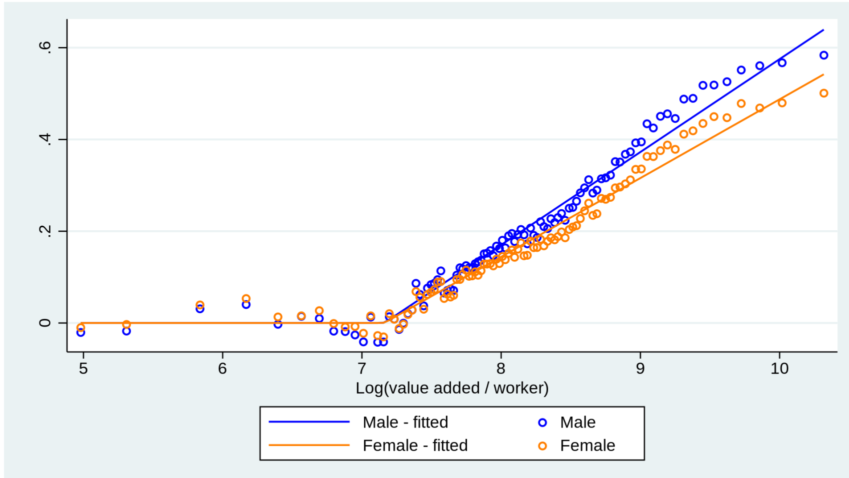
Variance of log wages	0.336	
Ensemble decomp. (and sub-shares) (%)		
Contribution of XB	**5.05**	
— Year	*1.92*	38.0
— age*, firm size, contract, tenure*	*3.13*	62.0
Contribution of individual heterogeneity	**49.69**	
— Unobserved individual heterogeneity	*28.80*	58.0
— Observed individual (gender, quasi ed.)	*17.62*	35.5
— Birth year	*0.35*	0.7
— Region	*2.90*	5.8
Contribution of firm heterogeneity	**22.03**	
— Unobserved firm heterogeneity	*14.92*	67.7
— Observed firm heterogeneity (ownership)	*3.94*	17.9
— Sector	*3.17*	14.4
Contribution of sector-occupations	**8.69**	
— Occupation	*8.85*	101.9
— Sector	*-0.72*	-8.2
— Unexplained	*0.55*	6.4
Residual variation	**14.54**	
Correlations (and contr. to overall)		
Corr(θ_i,ψ_j)	0.225	11.0%
Corr(ε_i^I,ε_j^J)	0.141	4.3%
Corr(θ_i,ψ_j) for inc. firms	0.326	15.9%
Corr(θ_i,VA_j) for inc. firms	0.365	
Corr(ψ_j,VA_j) for inc. firms	0.626	
Between-within decomposition (%)		
Between-firm share	46.4	
— Ind. segregation	10.9	
— Var(ψ_j)	15.9	
— Sorting	11.0	
Number of Observations (1000)	61358	
Number of Firms (1000)	102	
Number of Workers (1000)	2362	

Notes: See Table 1.1. In this model occupation categories are interacted with firm industries to form the third fixed effect of the first-stage estimation. These parameters are then decomposed into additive occupation and sector effects, with the residuals reflecting the importance of interaction terms.

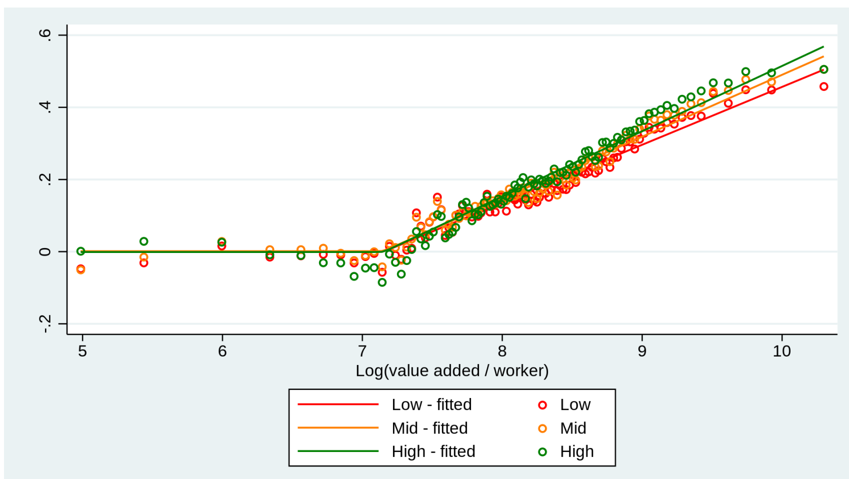
Table A.9: Ownership gaps decomposed, with sectoral controls.

	(1)	(2)	(3)	(4)
Difference:	Wage	Individual	Firm	Occupation
Foreign-owned firm	0.353***	0.122***	0.223***	0.010***
	(0.020)	(0.010)	(0.011)	(0.002)
State-owned firm	-0.039	-0.011	-0.020	-0.006
	(0.041)	(0.017)	(0.023)	(0.006)
Public institution	0.000			0.000
	(0.000)			(0.000)
Observations	43,281,722	43,281,722	43,281,722	43,281,722
R-squared	0.178	0.092	0.326	0.088
	(2a)	(2b)	(3a)	(3b)
Difference in:	Observed individual	Unobserved individual	Within individual	Between individual
Foreign-owned firm	0.049***	0.073***	0.131***	0.092***
	(0.005)	(0.006)	(0.006)	(0.009)
State-owned firm	-0.002	-0.008	0.014	-0.034
	(0.010)	(0.011)	(0.010)	(0.020)
Public institution	0.000			0.000
	(0.000)			(0.000)
Observations	43,281,722	43,281,722	43,281,722	43,281,722
R-squared	0.073	0.089	0.847	0.095

Notes: The parameters in the table are results from regression estimates of the effect of majority ownership dummies on wage components defined in Equations 1.11 (first panel) and 1.15 (bottom panel) as outcomes. The benchmark category consists of domestic, private-owned firms. The elements of X and Z are included as additional controls. Such variables include quadratic age, quadratic tenure, firm size, year, contract type and firm industry. Two-way clustered standard errors are in parentheses, with *** p<0.001, ** p<0.01, * p<0.05.



(a) Gender-firm effects (on dual connected set)



(b) Education-firm effects (on triple connected set)

Figure A.4: Re-scaled group-firm effects, versus log value added per worker of firms

Notes: Data points are mean estimated firm-group fixed effects corresponding a hundred percentiles of firm-year observations along the distribution of the logarithm of value added per worker for firms with balance sheet data available. Firm-gender and firm-education effects are both normalized to have a zero mean value in all categories for observations below a log value added of 7.15 – the threshold that provided the best fit for the kinked function presented on the graphs.

A.2 Relation to Card et al. (2016)

In this Appendix we demonstrate, through the example of gender sorting and differences in bargaining, the relation of our approach to the framework of Card et al. (2016). While in the latter, the gender-based sorting is captured either by $E(\Psi_{j\text{Male}}|\text{Male}) - E(\Psi_{j\text{Male}}|\text{Female})$ or by $E(\Psi_{j\text{Female}}|\text{Male}) - E(\Psi_{j\text{Female}}|\text{Female})$, in our setting the corresponding term would be $\frac{\partial \tilde{\psi}_j}{\partial G} = E(\tilde{\psi}_j|\text{Male}) - E(\tilde{\psi}_j|\text{Female})$, which has to be between the two measures of Card et al. (2016). This follows from the fact that $\tilde{\psi}_j$ is actually a weighted average of female and male effects of the given firm. Specifically, let us consider the model from Equation 1.19:

$$\Psi_{jg} = G\tilde{\beta}_g + \tilde{\psi}_j + \varepsilon_{jg}^G \tag{A.1}$$

It can be shown, that in such a simple model with only firm fixed effects and G being a dummy for two gender categories, the following holds:

$$\tilde{\psi}_j = s_{Mj}(\Psi_{j\text{Male}} - \tilde{\beta}_g) + (1 - s_{Mj})\Psi_{j\text{Female}} \tag{A.2}$$

Where s_M is the share of male workers in the given firm, $\frac{N_M}{N_M+N_F}$, while $\Psi_{j\text{Male}}$ and $\Psi_{j\text{Female}}$ correspond to the firm effects for workers of the given gender at the firm. For simplicity, let us assume that s_M is constant across firms.[170] Then:

$$\begin{aligned} E(\tilde{\psi}_j|\text{Male}) - E(\tilde{\psi}_j|\text{Female}) = &\\ &(s_M E(\Psi_{j\text{M}}|\text{M}) - s_M\tilde{\beta}_g + (1 - s_M)E(\Psi_{j\text{F}}|\text{M})) - \\ &(s_M E(\Psi_{j\text{M}}|\text{F}) - s_M\tilde{\beta}_g + (1 - s_M)E(\Psi_{j\text{F}}|\text{F})) = \\ s_M(E(\Psi_{j\text{M}}|\text{M}) - E(\Psi_{j\text{M}}|\text{F})) + (1 - s_M)(E(\Psi_{j\text{F}}|\text{M}) - E(\Psi_{j\text{F}}|\text{F})) & \end{aligned} \tag{A.3}$$

That is, in this particular setting, our proposed estimator for sorting, $\frac{\partial \tilde{\psi}_j}{\partial G}$ will be the weighted average of the two alternative estimations Card et al. (2016) propose, with the weights s_M and s_F. It also follows that the estimator for bargaining will be also linear combination, as

$$\tilde{\beta}_g = \frac{\partial(\Psi_{jg} - \psi_j)}{\partial G} \tag{A.4}$$

To demonstrate that the above argumentation holds, and to assess how severe is the simplifying assumption of s_M being constant is, we replicate the estimators of Card et al. (2016) in Appendix Table A.10.

[170] For the following arguments to hold without concern, it would be enough to assume that the expected value of s_M is the same for males and females and that it is independent of both $\tilde{\psi}_{j\text{Male}}$ and $\tilde{\psi}_{j\text{Female}}$. While any segregation by gender violates the former assumption, and the latter can be violated as well, these assumptions simplify the argumentation. Later, we will show that in our data this simplification has negligible importance.

Table A.10: Relation to Card et al. (2016), dual-connected set

	Diff in Ψ_{jg}	Sorting	Bargaining	Sort. sh	Barg. sh
(1) Male distribution, female effects	0.1059	0.1004	0.0055	94.78%	5.22%
(2) Female distribution, male effects	0.1059	0.0789	0.0270	74.53%	25.47%
(3) Obs. distribution, firm mean effects	0.1059	0.0890	0.0169	84.03%	15.97%
s_M(1)+(1-s_M)(2), with s_M=0.4678	0.1059	0.0890	0.0169	84.00%	16.00%
(3) with controls for XB	0.0628	0.0456	0.0172	72.59%	27.41%

Notes: first two rows are based on Card et al. (2016). The third row reports the decomposition of Equation A.3. The fourth row is a weighted average of Rows 1 and 2, with the weight given by the in-sample average of the male-share variable in the sample. The estimation sample is restricted to firms for which both the male and female firm effects fall into their respective connected set. The estimation for the final row controls for age, tenure, calendar year and firm size.

The first two rows are replications of the estimators of Card et al. (2016), while the third row is the fixed effect approach proposed in this paper. As we can observe the decomposition in row 3 is indeed between the two previous estimates. Row 4 is defined by Equation A.3, weighting rows 1 and 2 assuming a constant share of male workers across workplaces, $s_M = 46.78\%$, obtained as the mean of the within-firm share of male workers across the sample. The difference between rows 3 and 4 are of negligible magnitude, suggesting a small role of correlations between firm effects and gender ratios. We also note that the results of the first two rows are quite similar to the finding of Card et al. (2016), who present 6% or 31% share of the bargaining component, depending on the specification of choice. Therefore, our results also suggest an over-representation of male workers in firms with smaller gender gaps.[171]

An advantage of our approach, besides providing one estimator instead of two, is that it can be easily generalised to G variables of more than two categories. Also, we can easily incorporate the effect of X control variables, by estimating and subtracting $X\beta_X$ from the elements in Equation 1.19. The results, presented in row 5, suggests a 4.6% sorting parameter, which is comparable to the parameter in Table 1.6 (Panel A, Column 3, 3.0%). The source of the difference is either the slightly different sub-sample – this table refers to the dual-connected set of (integrated) firms – or, the differing assumptions of the AKM and G-AKM models, suggesting that assuming a common wage premium across firms is too restrictive compared to the model allowing for firm-specific gender gaps.

[171] Casarico and Lattanzio (2019) also reproduces the exercise of Card et al. (2016), and also report a weighted average of the male and female distribution based decompositions, with the weights of 0.5-0.5. They find around 70% importance of the sorting channel. The Online supplement to Lamadon et al. (2019) replicates these results as well, presenting almost identical sorting shares both with and without bias correction applied to the AKM estimations.

A.3 Variable definitions, estimation issues

Sample. Although we have monthly data, for computational convenience we use data from only every third month of the year, namely January, April, July and October.[172] We excluded partial months at the start or end of employment spells and used only months when workers were employed (insured) for all days in the given month, hence avoiding issues related to the imprecise measurement of wages in these months. We also excluded employers with less than 5 observed workers in the given year for two reasons. First, data from smaller firms is prone to be less reliable. Second, identification of the firm effects of small employers relies only on a small number of moves and thus estimations including them are more prone to limited mobility bias (Bonhomme et al., 2020).[173] We kept workers between the age of 17 and 65, as younger workers should be affected by compulsory schooling age, and by the age of 65 most Hungarians retire. We kept workers with standard contracted employment, including public servants and employees of public institutions (public workers) as well. Individual entrepreneurs, self-reliant farmers and other independent forms of employment are excluded.

Mobility. The connected set on which the estimated fixed effects are directly comparable has to be defined according to the algorithm of Weeks and Williams (1964), as noted by both Torres et al. (2018) and Gyetvai (2017). This three-way connected set for our main specification includes 91.9% of observations, 86.2% of firms, 92.1% of workers from the sample defined above. As our panel is only a 50% sample, limited mobility bias could not be neglected. However, we trust that having fifteen years of data in the same panel helps greatly in overcoming this issue. Furthermore, using quarterly data, we observe 60 time periods with within-year movements also contributing to the set of job switches used for identification of the firm effects.

Wages. Our wage variable is defined the following way. We calculated hourly wages by dividing monthly earnings by four times the reported weekly work hours. (If no value was reported, we imputed the most common value, 40 hours per week.) Then, within all calendar months wages were winsorized, that is values below the bottom and above the top percentile cut-offs were re-coded to the corresponding cut-off values. Finally, nominal wages were divided by a monthly consumer price index, and then taken the logarithm of.

Time-varying factors. Building upon the findings and specifications of Card et al. (2018) and Torres et al. (2018), we included in the main AKM estimation as time varying terms quadratic and cubic age terms, with the age profile assumed to be flat at the age of 40. That is we include the variables $(age-40)^2$ and $(age-40)^3$. We included tenure and quadratic tenure (measured in months) to capture within spell wage evolution and added dummies to control for calendar years, as even

[172] Using February, May, August and November did not alter meaningfully the results of main estimations. The re-estimation of our main model using these months is included in Appendix Table A.4.

[173] Song et al. (2019) also omit employer-year observations with fewer than 5 employees in the year. While our restriction is more strict, abandoning it did not affect results substantially. Bonhomme et al. (2020) applies a different restriction, by excluding firms with less than 10 mobility events over their 6 year observation period. Adapting this approach also did not alter the presented results to a relevant extent.

the baseline level of real wages may vary across subsamples. We also control for the (logarithmic) size of the firm. Finally, the type of contract is accounted by dummies, reflecting whether the individual has a private or a public contract of employment.

Time-invariant terms. Anonymous person identifiers are provided in the data. Occupational differences are captured by high-dimensional occupation categories, coming from the Hungarian equivalent of the ISCO occupation categorization system. The classification was substantially altered in 2008, resulting for different codes being used before and since 2011. To overcome this issue, we harmonised the two category sets by using clusters of codes in which all old categories has to correspond to exactly one of the codes in the new nomenclature. Using this crosswalk, we ended up with 332 occupation clusters, of which the average firm employs 6.7 different ones (in the given year). Finally, instead of the original firm identifiers, we assigned firms new ones if their ownership changed with regard to the majority of foreign or state capital in the firm, or if they changed their main reported sector of operation. This way, we allow firms to have different wage premiums during different ownership or management regimes. Therefore, ownership and sector will become truly time-invariant characteristics of firms defined this way.[174]

Firm characteristics. Time invariant firm characteristics are sector categories created from 2-digit codes of the Hungarian equivalent of the NACE system of industries, corresponding to 61 distinct categories, and dummies indicating the majority of ownership – with domestic private, foreign private, state owned firm and public institution being the possible employer categories.

Individual characteristics. Individual time-invariant characteristics in our models include gender, the year of birth capturing cohort effects and the residential districts that individuals lived in for the most years during the time span of our panel. (In the case of multiple modes, the latest residence was used.) Districts are Local Administrative Units (LAU-1), of which Hungary has a total of 175. Finally, dummies for low and high quasi-education categories are included. This education variable is implicitly inferred from the data, and corresponds to the highest educational requirement of the occupations – as defined in the categorization of the Statistical Office – we ever observe the given individual working in. Specifically, we define the low education category as those who only ever worked as machine operators, assembly workers, drivers or in other elementary occupations requiring no qualification (ISCO categories 8 and 9). The high category consists of those who worked at least once as a manager or as a professional in jobs, which require the autonomous application of higher educational degrees (ISCO categories 1 or 2). Everyone else forms the in-between, middling category. Appendix Table A.11 comprises the distributions of key categorical variables on the largest connected sample used for the majority of estimations presented in the study.

[174] In Torres et al. (2018), the authors argue that changes in these variables are not common or has no substantial effect in Portugal and treat these variables as time-invariant elements of the second-stage regressions, while in-fact some within-firm variation remains in their data. The (minor) drawback of our approach may be losing some efficiency of estimates with the addition of extra estimable firm unit parameters and the use of smaller units in cases, where similar effects would apply for the same firm even under different regimes.

Estimation. For estimating the AKM model we use the method of Correia (2017), implemented in *Stata* under the command *reghdfe*.

Table A.11: Descriptive statistics on categorical variables

Gender	Observations	Share	**Region of residence (mode)**	Observations	Share
Female	36,815,363	50.8%	Budapest	11,794,027	16.3%
Male	35,593,233	49.2%	Central Hungary	8,779,139	12.1%
Proxied education			Central Transdanubia	9,028,080	12.5%
Low ed.	8,700,436	12.0%	Western Transdanubia	7,949,449	11.0%
Mid ed.	39,054,621	54.0%	Southern Transdanubia	6,521,187	9.0 %
High ed.	24,634,902	34.0%	Northern Hungary	8,583,571	11.9%
Age category			Northern Great Plain	10,279,085	14.2%
17-25 years	6,109,979	8.4%	Southern Great Plain	8,947,871	12.4%
26-40 years	28,893,529	39.9%	Unknown	526,187	0.7 %
41-55 years	30,129,289	41.6%	**Occupation category**		
56-65 years	7,275,799	10.1%	Political/religional/ngo leader	516,957	0.7 %
Tenure category			Top manager	588,628	0.8 %
¡= 12 months	13,045,516	27.0%	Other manager	3,739,393	5.2 %
12-35 months	13,251,047	27.4%	Professional	11,732,497	16.2%
36-60 months	6,768,405	14.0%	Other white collar	17,787,811	24.6%
60+ months	15,328,702	31.7%	Skilled blue collar	18,626,922	25.7%
Ownership type			Assembler, machine op.	9,759,379	13.5%
Domestic, private	26,073,563	36.0%	Unskilled laborer	9,296,730	12.8%
Foreign owned	16,522,151	22.8%	Unknown	360,279	0.5 %
State owned	5,789,831	8.0%	**Year**		
Public inst.	24,023,051	33.2%	2003-2005	14,336,394	19.8%
Employment type			2006-2008	14,695,375	20.3%
Standard contract	56,872,100	78.5%	2009-2011	14,309,224	19.8%
Public servant	3,301,429	4.6%	2012-2014	14,104,661	19.5%
Public worker	12,235,067	16.9%	2015-2017	14,962,942	20.7%

Notes: The distributions refer to the connected sample of the main estimations in Table 1.1.

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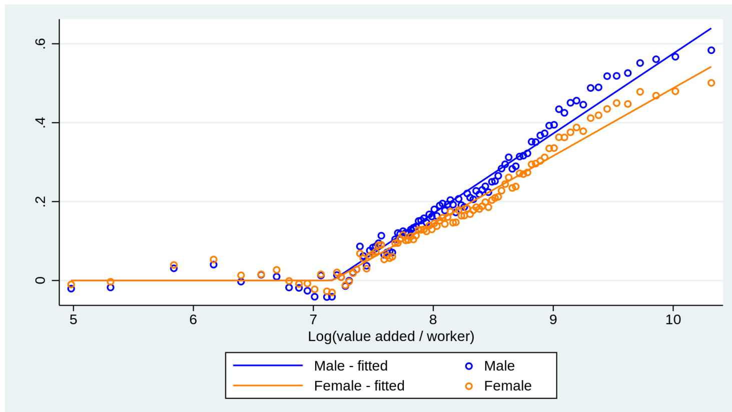
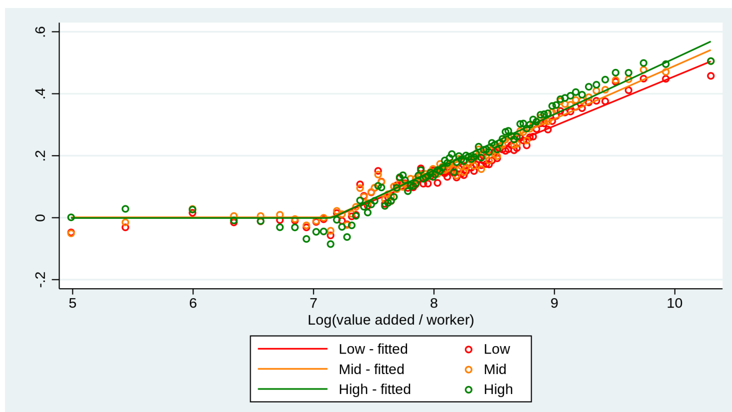
B Appendix for Chapter 2

B.1 Additional Tables and Graphs



(a) Gender-firm effects (on dual connected set)



(b) Education-firm effects (on triple connected set)

Figure B.1: Re-scaled group-firm effects, versus log value added per worker of firms

Notes: Data points are mean estimated firm-group fixed effects corresponding a hundred percentiles of firm-year observations along the distribution of the logarithm of value added per worker for firms with balance sheet data available. Firm-gender and firm-education effects are both normalized to have a zero mean value in all categories for observations below a log value added of 7.15 – the threshold that provided the best fit for the kinked function presented on the graphs.

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[175] Using February, May, August and November did not alter meaningfully the results of main estimations.

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B.3 Regression tables for figure results

Table B.1: Rent-sharing elasticites with IV: lagged productivity

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jt}	(3) Wi sector ψ_j	(4) Wi firm lnW	(5) Wi firm ψ_{jt}	(6) Wi match lnW	(7) Wi match ψ_{jt}
Log value added pw.	0.391***	0.178***	0.172***	0.162***	0.113***	0.133***	0.118**
	(0.011)	(0.007)	(0.006)	(0.025)	(0.022)	(0.027)	(0.030)
Observations	280,761	253,538	263,104	266,202	240,695	34,742,342	29,123,312
R-squared	0.455	0.299	0.320	-0.011	0.002	-0.003	-0.024
Number of units	45	44	44	44050	39783	2.768e+06	2.346e+06

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2. The models use the one-year lag of firm value added as an instrument for contemporaneous value added.

Table B.2: Rent-sharing elasticites with IV: bracketed sales

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jt}	(3) Wi sector ψ_j	(4) Wi firm lnW	(5) Wi firm ψ_{jt}	(6) Wi match lnW	(7) Wi match ψ_{jt}
Log value added pw.	0.399***	0.180***	0.171***	0.142***	0.097***	0.100***	0.092***
	(0.012)	(0.007)	(0.006)	(0.020)	(0.017)	(0.018)	(0.020)
Observations	212,748	194,224	201,229	203,590	185,881	28,911,192	24,353,730
R-squared	0.475	0.314	0.334	0.023	0.026	0.004	0.015
Number of units	45	44	44	33999	31098	2.308e+06	1.973e+06

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2. The instrument used is the mean of sales observations from the given year, one year before and one year after.

Table B.3: Rent-sharing elasticites by ownership

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jt}	(3) Wi sector ψ_j	(4) Wi firm lnW	(5) Wi firm ψ_{jt}	(6) Wi match lnW	(7) Wi match ψ_{jt}
VA—Private domestic	0.362***	0.174***	0.170***	0.248***	0.183***	0.231***	0.213***
	(0.014)	(0.009)	(0.008)	(0.018)	(0.016)	(0.022)	(0.024)
VA—Foreign owned	0.434***	0.188***	0.179***	0.129***	0.092***	0.099***	0.098***
	(0.014)	(0.009)	(0.008)	(0.018)	(0.017)	(0.020)	(0.022)
VA—State owned	0.292***	0.127**	0.131***	-0.001	0.009	0.014	0.021
	(0.058)	(0.036)	(0.031)	(0.024)	(0.017)	(0.021)	(0.024)
Observations	280,761	253,538	263,104	266,202	240,695	34,742,342	29,123,312
R-squared	0.464	0.303	0.323	-0.021	0.001	-0.011	-0.060
Number of units	45	44	44	44050	39783	2.768e+06	2.346e+06

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2.

Table B.4: Rent-sharing elasticites by sectors

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jt}	(3) Wi sector ψ_j	(4) Wi firm lnW	(5) Wi firm ψ_{jt}	(6) Wi match lnW	(7) Wi match ψ_{jt}
VA—Agriculture	0.255***	0.143***	0.107***	0.278***	0.215***	0.254***	0.254***
	(0.025)	(0.018)	(0.014)	(0.032)	(0.027)	(0.042)	(0.040)
VA—Manufacturing	0.364***	0.174***	0.165***	0.174***	0.125***	0.147***	0.137***
	(0.015)	(0.009)	(0.007)	(0.021)	(0.018)	(0.021)	(0.022)
VA—Trade	0.450***	0.217***	0.210***	0.189***	0.142***	0.176***	0.164***
	(0.020)	(0.011)	(0.009)	(0.030)	(0.024)	(0.027)	(0.028)
VA—Services	0.412***	0.170***	0.168***	0.110*	0.068	0.069	0.054
	(0.023)	(0.014)	(0.013)	(0.046)	(0.033)	(0.040)	(0.037)
Observations	280,761	253,538	263,104	266,202	240,695	34,742,342	29,123,312
R-squared	0.459	0.301	0.324	-0.009	0.007	-0.004	-0.025
Number of units	45	44	44	44050	39783	2.768e+06	2.346e+06

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2.

Table B.5: Rent-sharing elasticites by firm size

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jt}	(3) Wi sector ψ_j	(4) Wi firm lnW	(5) Wi firm ψ_{jt}	(6) Wi match lnW	(7) Wi match ψ_{jt}
10-25	0.404***	0.192***	0.187***	0.174***	0.121***	0.209***	0.190***
	(0.010)	(0.007)	(0.005)	(0.021)	(0.019)	(0.016)	(0.017)
25-100	0.405***	0.190***	0.184***	0.173***	0.119***	0.198***	0.180***
	(0.009)	(0.006)	(0.004)	(0.021)	(0.019)	(0.016)	(0.018)
100-500	0.394***	0.182***	0.175***	0.165***	0.115***	0.150***	0.143***
	(0.010)	(0.007)	(0.005)	(0.022)	(0.020)	(0.017)	(0.019)
500-5000	0.387***	0.176***	0.169***	0.156***	0.111***	0.101***	0.099***
	(0.012)	(0.008)	(0.007)	(0.023)	(0.021)	(0.021)	(0.022)
5000+	0.366***	0.157***	0.150***	0.142**	0.105**	0.055	0.060
	(0.034)	(0.017)	(0.016)	(0.034)	(0.027)	(0.038)	(0.040)
Observations	280,761	253,538	263,104	266,202	240,695	34,742,342	29,123,312
R-squared	0.455	0.301	0.323	-0.009	0.003	-0.007	-0.039
Number of units	45	44	44	44050	39783	2.768e+06	2.346e+06

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2.

Table B.6: Rent-sharing elasticites by (proxied) education

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
Unskilled	0.264***	0.155***	0.112**	-0.009	0.163***	0.168***	0.087*
	(0.025)	(0.012)	(0.030)	(0.005)	(0.015)	(0.011)	(0.030)
Middling	0.302***	0.166***	0.142***	-0.001	0.167***	0.171***	0.128***
	(0.011)	(0.006)	(0.024)	(0.002)	(0.009)	(0.006)	(0.028)
High educ	0.395***	0.208***	0.200***	0.015**	0.198***	0.195***	0.177***
	(0.009)	(0.008)	(0.023)	(0.005)	(0.008)	(0.007)	(0.026)
Observations	667,103	517,045	665,044	510,999	665,044	510,999	34,742,342
R-squared	0.597	0.305	0.600	0.019	0.201	0.309	-0.005
Number of units	45	44	56550	47570	45	44	2.768e+06
Diff. (high vs low)	0.131	0.0533	0.0880	0.0239	0.0357	0.0270	0.0900
p(Diff≠0)	8.92e-05	0.00130	0.000327	0.0160	0.0129	0.0136	0.00336

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. $\hat{\psi}_j$ refers to the firm-effects obtained from specification (4), and correspond to $\tilde{\psi}_j$ in Equation 2.14. The last column contains results of the within-match specification of Equation 2.4, with different parameters for members of different groups.

Table B.7: Rent-sharing elasticites by gender

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
Female	0.361***	0.161***	0.127***	-0.019***	0.240***	0.183***	0.119***
	(0.012)	(0.005)	(0.026)	(0.002)	(0.009)	(0.005)	(0.024)
Male	0.391***	0.188***	0.177***	0.013***	0.212***	0.176***	0.141***
	(0.012)	(0.007)	(0.026)	(0.002)	(0.012)	(0.007)	(0.032)
Observations	519,644	417,653	515,855	409,785	515,855	409,785	34,742,342
R-squared	0.458	0.321	0.224	0.188	0.243	0.300	-0.004
Number of units	45	44	54820	46048	45	44	2.768e+06
Diff.	0.0301	0.0264	0.0501	0.0316	-0.0286	-0.00698	0.0218
p(Diff≠0)	0.00982	0.000366	2.89e-05	6.09e-07	0.00319	0.175	0.205

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table B.6.

Table B.8: Rent-sharing elasticites by occupation

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
Top manager	0.466***	0.241***	0.294***	0.056***	0.209***	0.188***	0.229***
	(0.055)	(0.007)	(0.061)	(0.006)	(0.012)	(0.009)	(0.023)
Other manager	0.444***	0.218***	0.287***	0.045***	0.215***	0.186***	0.157***
	(0.017)	(0.008)	(0.028)	(0.007)	(0.016)	(0.009)	(0.024)
Professional	0.328***	0.219***	0.166***	0.034***	0.228***	0.205***	0.148***
	(0.013)	(0.010)	(0.027)	(0.008)	(0.017)	(0.011)	(0.025)
Other white collar	0.327***	0.206***	0.165***	0.024***	0.221***	0.198***	0.142***
	(0.012)	(0.007)	(0.024)	(0.004)	(0.012)	(0.008)	(0.025)
Skilled blue collar	0.290***	0.171***	0.167***	0.021***	0.170***	0.163***	0.135***
	(0.013)	(0.007)	(0.024)	(0.003)	(0.016)	(0.008)	(0.029)
Assembler, machine op.	0.240***	0.156***	0.105**	0.001	0.192***	0.173***	0.108**
	(0.023)	(0.012)	(0.030)	(0.007)	(0.020)	(0.011)	(0.033)
Unskilled laborers	0.220***	0.153***	0.104***	-0.003	0.150***	0.160***	0.126***
	(0.015)	(0.009)	(0.024)	(0.004)	(0.015)	(0.009)	(0.027)
Observations	997,044	936,993	994,798	933,795	994,798	933,795	34,557,250
R-squared	0.616	0.370	0.592	0.231	0.197	0.267	0.002
Number of units	45	45	56360	55199	45	45	2.762e+06
Diff. (Top vs. bottom)	-0.246	-0.0879	-0.191	-0.0593	-0.0586	-0.0283	-0.103
p(Diff≠0)	0.00124	4.75e-06	0.00713	5.09e-06	0.00121	0.0191	6.83e-05

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table B.6.

Table B.9: Rent-sharing elasticites by completed tenure

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
0-11 months	0.344***	0.135***	0.093	-0.003	0.253***	0.139***	0.100
	(0.014)	(0.006)	(0.043)	(0.004)	(0.010)	(0.006)	(0.046)
12-35 months	0.387***	0.163***	0.126*	0.013**	0.264***	0.151***	0.118*
	(0.016)	(0.006)	(0.043)	(0.003)	(0.010)	(0.006)	(0.047)
36-60 months	0.407***	0.169***	0.144**	0.016**	0.262***	0.152***	0.118*
	(0.014)	(0.007)	(0.043)	(0.004)	(0.011)	(0.007)	(0.047)
61+ months	0.410***	0.160***	0.145*	0.008*	0.264***	0.151***	0.102
	(0.012)	(0.008)	(0.045)	(0.003)	(0.010)	(0.008)	(0.048)
Observations	556,755	556,755	553,905	553,905	553,905	553,905	21,340,612
R-squared	0.498	0.303	0.264	0.264	0.315	0.263	0.004
Number of units	44	44	39708	39708	44	44	1.819e+06
Diff.	0.0655	0.0249	0.0515	0.0120	0.0110	0.0116	0.00200
p(Diff≠0)	0.000507	0.00192	0.000858	0.00476	0.185	0.0487	0.817

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table B.6.

Table B.10: Rent-sharing elasticites by worker age

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
Age 17-25	0.291***	0.150***	0.062	-0.005	0.221***	0.151***	0.122***
	(0.016)	(0.006)	(0.031)	(0.004)	(0.011)	(0.006)	(0.028)
Age 26-40	0.409***	0.186***	0.160***	0.016***	0.241***	0.165***	0.133***
	(0.011)	(0.006)	(0.029)	(0.002)	(0.010)	(0.006)	(0.029)
Age 41-55	0.390***	0.162***	0.163***	0.004*	0.220***	0.155***	0.107**
	(0.015)	(0.007)	(0.029)	(0.002)	(0.013)	(0.007)	(0.030)
Age 56-65	0.400***	0.162***	0.172***	-0.001	0.222***	0.160***	0.094**
	(0.015)	(0.008)	(0.028)	(0.003)	(0.013)	(0.007)	(0.030)
Observations	723,886	723,886	720,277	720,277	720,277	720,277	30,202,190
R-squared	0.461	0.315	0.148	0.168	0.268	0.281	0.003
Number of units	44	44	49222	49222	44	44	2.376e+06
Diff.	0.108	0.0113	0.110	0.00428	0.000944	0.00859	-0.0280
p(Diff≠0)	7.09e-05	0.169	1.01e-05	0.450	0.936	0.202	0.0223

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table B.6.

Table B.11: Rent-sharing elasticites by gender and ownership

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
VA— female — Private domestic	0.360***	0.170***	0.233***	-0.016***	0.133***	0.188***	0.219***
	(0.015)	(0.007)	(0.018)	(0.003)	(0.013)	(0.007)	(0.021)
VA— female — Foreign owned	0.405***	0.171***	0.107***	-0.014***	0.303***	0.191***	0.080**
	(0.015)	(0.008)	(0.018)	(0.002)	(0.012)	(0.008)	(0.023)
VA— female — State owned	0.298***	0.128***	-0.012	-0.017***	0.322***	0.146***	0.036
	(0.057)	(0.030)	(0.026)	(0.004)	(0.060)	(0.031)	(0.025)
Male — Private domestic	0.351***	0.180***	0.252***	0.009***	0.098***	0.171***	0.237***
	(0.015)	(0.008)	(0.018)	(0.001)	(0.016)	(0.009)	(0.023)
Male — Foreign owned	0.413***	0.189***	0.137***	0.014***	0.274***	0.179***	0.115***
	(0.014)	(0.009)	(0.019)	(0.002)	(0.014)	(0.009)	(0.019)
Male — State owned	0.291***	0.142***	0.000	0.007***	0.297***	0.134***	0.007
	(0.058)	(0.031)	(0.024)	(0.002)	(0.061)	(0.031)	(0.019)
Observations	519,644	417,653	515,855	409,785	515,855	409,785	34,742,342
R-squared	0.473	0.328	0.242	0.197	0.252	0.307	-0.011
Number of units	45	44	54820	46048	45	44	2.768e+06
p(Diff in Private domestic)	0.489	0.126	0.045	0.000	0.001	0.005	0.091
p(Diff in Foreign owned)	0.443	0.009	0.003	0.000	0.003	0.026	0.020
p(Diff in State owned)	0.554	0.029	0.187	0.000	0.008	0.023	0.104

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2.

Table B.12: Rent-sharing elasticites by gender and sectors

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jg}	(3) Wi firm lnW	(4) Wi firm ψ_{jg}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_j$	(7) Wi spell lnW
Female — Agriculture	0.234***	0.088***	0.244***	-0.023***	-0.006	0.111***	0.272***
	(0.028)	(0.015)	(0.031)	(0.003)	(0.026)	(0.015)	(0.043)
Female — Manufacturing	0.320***	0.148***	0.138***	-0.020***	0.181***	0.171***	0.135***
	(0.017)	(0.006)	(0.020)	(0.002)	(0.012)	(0.007)	(0.024)
Female — Trade	0.421***	0.204***	0.168***	-0.012***	0.265***	0.222***	0.137***
	(0.022)	(0.009)	(0.030)	(0.002)	(0.021)	(0.010)	(0.031)
Female — Services	0.395***	0.163***	0.080	-0.016***	0.326***	0.181***	0.063
	(0.022)	(0.012)	(0.048)	(0.003)	(0.022)	(0.013)	(0.032)
Male — Agriculture	0.264***	0.117***	0.285***	0.006***	-0.024	0.109***	0.248***
	(0.024)	(0.013)	(0.033)	(0.001)	(0.022)	(0.014)	(0.043)
Male — Manufacturing	0.357***	0.178***	0.186***	0.012***	0.162***	0.168***	0.153***
	(0.014)	(0.008)	(0.021)	(0.001)	(0.014)	(0.008)	(0.020)
Male — Trade	0.449***	0.228***	0.210***	0.017***	0.244***	0.215***	0.224***
	(0.018)	(0.009)	(0.030)	(0.002)	(0.018)	(0.009)	(0.024)
Male — Services	0.422***	0.187***	0.121*	0.012***	0.304***	0.175***	0.071
	(0.026)	(0.014)	(0.047)	(0.003)	(0.026)	(0.014)	(0.048)
Observations	519,644	417,653	515,855	409,785	515,855	409,785	34,742,342
R-squared	0.466	0.328	0.233	0.198	0.261	0.306	-0.005
Number of units	45	44	54820	46048	45	44	2.768e+06
p(Diff in Agriculture)	0.021	0.000	0.000	0.000	0.074	0.774	0.251
p(Diff in Manufacturing)	0.005	0.000	0.000	0.000	0.048	0.550	0.089
p(Diff in Trade)	0.029	0.002	0.000	0.000	0.037	0.220	0.001
p(Diff in Services)	0.025	0.001	0.000	0.000	0.018	0.228	0.800

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table 2.2.

Table B.13: Rent-sharing elasticites by gender and occupations

VARIABLES	(1) Wi sector lnW	(2) Wi sector ψ_{jgo}	(3) Wi firm lnW	(4) Wi firm ψ_{jgo}	(5) BW firm $\hat{w}_j$	(6) BW firm $\hat{\psi}_{jgo}$	(7) Wi spell lnW
Top manager — Male	0.418***	0.225***	0.257***	0.048***	0.186***	0.185***	0.228***
	(0.035)	(0.007)	(0.045)	(0.008)	(0.009)	(0.007)	(0.024)
Top manager — Female	0.581***	0.183***	0.417**	0.013	0.199***	0.167***	0.227***
	(0.106)	(0.011)	(0.107)	(0.012)	(0.021)	(0.020)	(0.022)
Other manager — Male	0.402***	0.218***	0.266***	0.052***	0.170***	0.178***	0.158***
	(0.018)	(0.009)	(0.029)	(0.008)	(0.017)	(0.010)	(0.027)
Other manager — Female	0.474***	0.195***	0.320***	0.025**	0.205***	0.188***	0.159***
	(0.015)	(0.008)	(0.027)	(0.007)	(0.011)	(0.008)	(0.023)
Professional — Male	0.333***	0.230***	0.177***	0.047***	0.199***	0.203***	0.148***
	(0.013)	(0.011)	(0.025)	(0.008)	(0.014)	(0.010)	(0.027)
Professional — Female	0.294***	0.191***	0.144***	0.016	0.203***	0.196***	0.149***
	(0.015)	(0.009)	(0.029)	(0.008)	(0.015)	(0.011)	(0.024)
Other white collar — Male	0.337***	0.224***	0.184***	0.041***	0.194***	0.199***	0.143***
	(0.012)	(0.008)	(0.024)	(0.006)	(0.010)	(0.007)	(0.028)
Other white collar — Female	0.297***	0.177***	0.148***	0.005	0.195***	0.190***	0.142***
	(0.012)	(0.007)	(0.024)	(0.004)	(0.011)	(0.008)	(0.024)
Skilled blue collar — Male	0.272***	0.178***	0.175***	0.041***	0.122***	0.148***	0.135***
	(0.013)	(0.008)	(0.024)	(0.004)	(0.016)	(0.009)	(0.031)
Skilled blue collar — Female	0.219***	0.142***	0.103**	-0.013*	0.140***	0.162***	0.133***
	(0.014)	(0.007)	(0.025)	(0.005)	(0.013)	(0.009)	(0.025)
Assembler, machine op. — Male	0.227***	0.157***	0.112**	0.011	0.150***	0.156***	0.108**
	(0.025)	(0.014)	(0.029)	(0.007)	(0.021)	(0.013)	(0.035)
Assembler, machine op. — Female	0.201***	0.139***	0.099**	-0.002	0.129***	0.156***	0.106**
	(0.017)	(0.010)	(0.028)	(0.008)	(0.013)	(0.010)	(0.030)
Unskilled laborers — Male	0.210***	0.154***	0.117***	0.010*	0.109***	0.150***	0.126***
	(0.013)	(0.008)	(0.024)	(0.004)	(0.011)	(0.009)	(0.029)
Unskilled laborers — Female	0.188***	0.137***	0.080**	-0.018*	0.128***	0.165***	0.127***
	(0.017)	(0.011)	(0.025)	(0.006)	(0.017)	(0.012)	(0.025)
Observations	1,358,474	1,355,730	1,357,197	1,354,486	1,357,197	1,354,486	34,557,250
R-squared	0.631	0.370	0.602	0.236	0.176	0.253	0.002
Number of units	45	45	57329	57183	45	45	2.762e+06
p(Diff in Top manager)	0.060	0.003	0.051	0.002	0.421	0.243	0.935
p(Diff in Other manager)	0.000	0.006	0.000	0.000	0.005	0.212	0.926
p(Diff in Professional)	0.001	0.000	0.010	0.000	0.622	0.239	0.963
p(Diff in Other white collar.)	0.001	0.000	0.003	0.000	0.920	0.058	0.953
p(Diff in Skilled blue collar)	0.002	0.001	0.000	0.000	0.254	0.189	0.916
p(Diff in Assemblers, machine op.)	0.262	0.220	0.334	0.117	0.298	0.989	0.887
p(Diff in Unskilled laborers)	0.0810	0.118	0.000	0.000	0.191	0.189	0.943

Notes: Robust standard errors in parentheses, using firm and year level multi-way clustering. *** p<0.001, ** p<0.01, * p<0.05. The models use the one-year lag of firm value added as an instrument for contemporaneous value added. For additional controls see Table 2.2. For the interpretation of specifications see Table B.6.

B.4 Differenced versus fixed effect stayer designs

Table B.14: Comparision of differenced stayer and within-match designs

Model type:		diff.	diff.	diff.	diff.	diff.	FE
Changes over:		5 years	3 years	1 year	3 years	1 year	*
Subsample:		6 years	4 years	2 years	6 years	6 years	2 years*
Outcome	Reg.						
$\ln w_{ijt}$	OLS	0.057	0.043	0.032	0.053	0.039	0.048
		(0.011)	(0.006)	(0.003)	(0.009)	(0.007)	(0.004)
$\ln w_{ijt}$	IV	0.114	0.084	0.069	0.096	0.093	0.123
		(0.018)	(0.010)	(0.009)	(0.013)	(0.012)	(0.012)
ψ_{jt}	OLS	0.053	0.040	0.034	0.050	0.041	0.046
		(0.012)	(0.007)	(0.004)	(0.010)	(0.007)	(0.005)
ψ_{jt}	IV	0.121	0.095	0.085	0.099	0.103	0.120
		(0.021)	(0.011)	(0.009)	(0.015)	(0.015)	(0.030)

Notes: Outcome variable is either change in wages (or firm-year effects) over the indicated time period or the deviation from the employment spell (match) mean – in the final, fixed effect column. The third row indicates the number of consecutive years we require the individuals in the sample to be observed. All parameters are significant at the 0.001 level, with standard errors being clustered at the firm and year level. IV refers to the change in sales (or deviation from spell-mean) over the given period.

In table B.14, we replicate classical stayer design parameter estimations in order to compare them to our within-match alternative. Specifically, we regress changes in the wages – or firm-year effects – of individuals over a period 5, 3 or 1 years on the change of productivity at their employer during the corresponding period. In these models, our sample is naturally restricted to individuals having wage observations at the same employer for 6,4 or 2 consecutive years. As the corresponding OLS estimates suggest – first three columns, first and third row –, the longer difference we use for estimation, the larger the estimated parameters become. This could reflect two things. On one hand, long run changes may capture less of transitory variation in productivity. However, the difference in parameters could be also driven by sample selectivity if firms share their rents to a larger extent with long-run incumbents. To assess this latter possibility, we re-estimate these models with also restricting the sample to workers being incumbent in their firm for at least 6 years. By comparing the parameters of columns 4 and 5 to those of columns 2 and 3, we can observe that the estimated elasticites are indeed higher in the subsample of long-time incumbents. Still, within this set of estimations the estimated effects rise as we focus on changes over more longer periods, suggesting that both focusing on less transitory effects and sample selection play a role in the previously observed patterns. Of the difference between the 1-year and 5-year change model roughly 30% could be attributed to selection in the OLS model.

In the second and fourth row we replicate our results with a simple – probably imperfect – instrument, the change of sales over the same time period. While we can observe similar patterns between the (considerably larger) parameters, the results are more consistent when being constrained to the subsample of stayers, indicating a relatively larger role of selection and a lower role of transitory effects in this setup – with the latter phenomena being (partly) captured by instrumentation.

Next, we compare these estimations to the specification formulated in Equation 2.4 of the main text. As we can observe, the parameters of our within-design – which incorporates variation relating to both short-run and long-run incumbents, although with larger weights allocated to the latter groups – generally fall between the 3-year and 5-year difference models, being somewhat more close to the 5-year ones. Therefore relying on this specification in our main text should not substantially alter the interpretation of differences between classical and novel model specifications. Finally, we also replicate the design of Juhn et al. (2018) and Lamadon et al. (2019) by regressing 5-year changes in wages or firm-year effects on 1-year change of productivity (with overlapping mid-points). The resulting parameters are 0.080 for wages (se: 0.029) and 0.137 for firm-year effects (se: 0.053), which are also comparable in magnitude to the parameter estimates in the main text.

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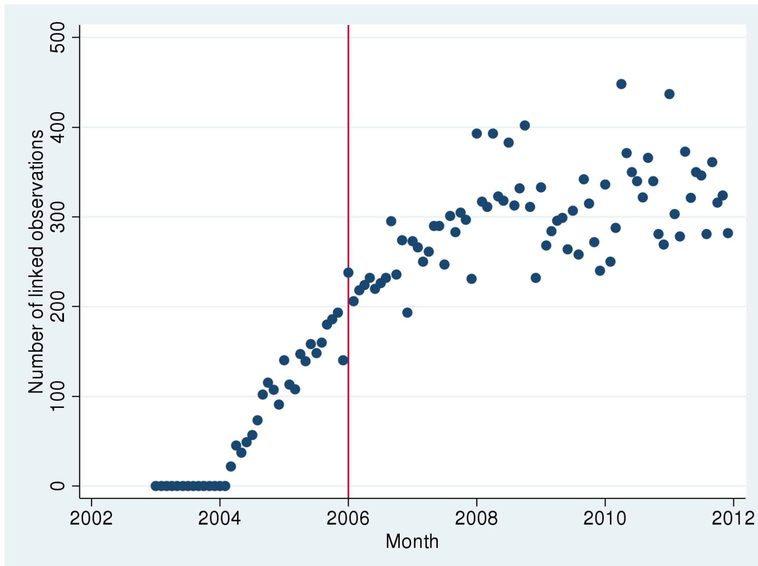
C Appendix for Chapter 3

C.1 Figures and tables

Figure C.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. The sub-sample used for the estimation is the same as in Table 3.2.

Table C.1: 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	964807	501200	964807	964807	943643	571443	964807	964807
N_i	616386	197435	616386	616386	616365	223021	616386	616386
N_j	105818	61121	105818	105818	84655	105778	105818	105818
R^2	0.327	0.860	0.190	0.200	0.443	0.612	0.052	0.086

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 and unskilled white-collar and blue-collar workers. Only the parameters for female workers are presented. For the list of additional controls, see Table 3.3. Standard errors are in parentheses and clustered at both firm-level and individual-level.
*Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

Table C.2: 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***	-0.0262	-0.0508**	-0.0080	-0.0397*	0.0378	-0.0111	-0.0458***
	(0.0223)	(0.0259)	(0.0183)	(0.0121)	(0.0171)	(0.0196)	(0.0079)	(0.0109)
$Skilled_W$	0.1146***	0.0620**	0.0046	0.0480**	-0.0026	0.0130	0.0072	0.0349*
	(0.0240)	(0.0202)	(0.0158)	(0.0156)	(0.0141)	(0.0205)	(0.0088)	(0.0136)
$Unskilled_W$	0.0558***	0.0244	0.0223*	0.0091	0.0249**	0.0022	-0.0026	0.0069
	(0.0126)	(0.0137)	(0.0096)	(0.0089)	(0.0086)	(0.0116)	(0.0056)	(0.0076)
$Skilled_B$	0.0507***	0.0159*	0.0236***	0.0112*	0.0156***	0.0132*	0.0079**	-0.0020
	(0.0060)	(0.0068)	(0.0044)	(0.0049)	(0.0038)	(0.0058)	(0.0030)	(0.0043)
$Unskilled_B$	0.0349***	0.0079	0.0287***	-0.0017	0.0278***	0.0117	0.0009	-0.0134**
	(0.0072)	(0.0070)	(0.0044)	(0.0063)	(0.0041)	(0.0076)	(0.0028)	(0.0051)
N	964807	501200	964807	964807	943643	571443	964807	964807
N_i	616386	197435	616386	616386	616365	223022	616386	616386
N_j	105819	61121	105819	105819	84655	105779	105819	105819
R^2	0.327	0.860	0.204	0.200	0.453	0.612	0.052	0.087

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry (Eq. (3.7)), without any and with two-way fixed effects, and the consecutive decomposition regressions on estimated firm and individual effects (Eq. (3.8) - (3.13) 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 and unskilled white-collar and blue-collar workers. For the list of additional controls, see Table 3.3. Standard errors are in parentheses and clustered at both firm-level and individual-level.

*Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

Table C.3: Co-worker gains and skill requirements

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

Note: Estimation results from the main regression on the logarithm of daily earnings upon job entry with two-way fixed effects (Eq. (3.7)). 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 3.3. 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 R^2 between 0.860 and 0.861. *Statistically significant at the .05 level; ** at the .01 level; *** at the .001 level.

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D Appendix for Chapter 4

D.1 Figures and tables

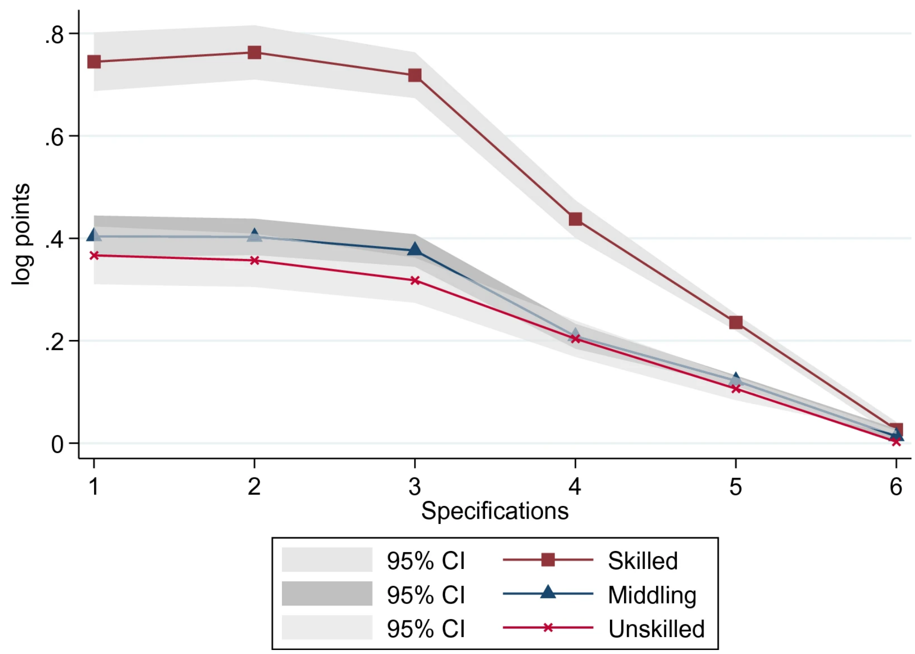

Figure D.1: Estimates of the foreign-domestic wage gap by skills.

Specifications: (1) sector–year interactions; (2) +person controls; (3) +job controls; (4) +firm controls; (5) +worker fixed effects; (6) + firm fixed effects. The confidence intervals are based on standard errors adjusted for clustering by persons and firms. On the sets of controls and the definition of skill levels, see Appendix D.2: "Data"

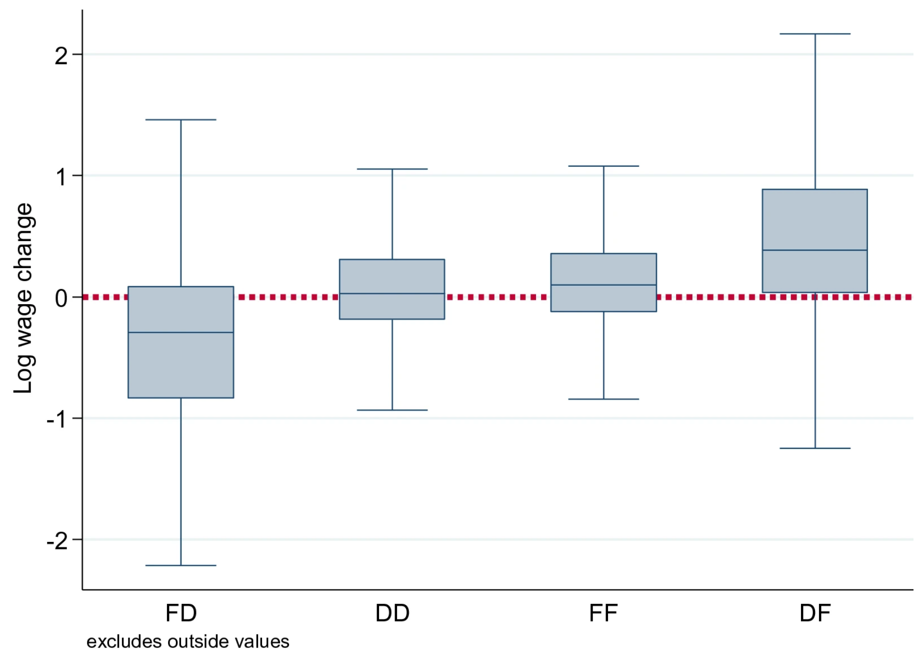

Figure D.2: Shifts between sectors and wage change.

The data relate to 307,874 shifts by skilled workers between ownership sectors in 2003–2011. F and D denote foreign-owned and domestic firms, respectively, in chronological order. The boxes display the interquartile ranges of log wage changes, with a horizontal line within the box indicating the median, and the whiskers showing the highest and lowest adjacent values. Heavy outliers are excluded. Wage change is measured as ln(w1/w0), where w1 and w0 are average earnings (normalized for the national mean) in the job spells after and before the shift, respectively

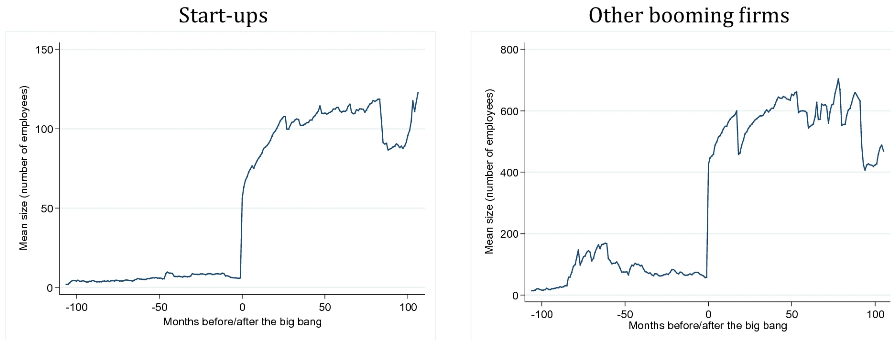

Figure D.3: The mean size of firms classified as newly established.

The data relate to 544 firms the size of which jumped from less than 5 to more than 50, or from less than 50 to more than 300 within a month (big bang). Firms changing majority owner after the big bang are excluded

Table D.1: Descriptive statistics

	Mean	**St. dev.**
Male	0.619	0.24
Age	37.9	10.40
Log health expenditures/national average wage	-2.080	1.80
Receives disability pension/payment	0.006	0.01
Receives care benefit	0.008	0.01
Log regional unemployment rate	-2.660	0.39
Central Hungary including Budapest	0.458	0.25
Tenure is unobserved	0.398	0.24
Tenure (months)	13.44	19.00
Top manager	0.051	0.05
Other manager	0.271	0.20
Professional	0.299	0.21
Other white collar	0.112	0.10
Skilled blue collar	0.025	0.02
Assembler, machine operator	0.169	0.14
Elementary occupation	0.012	0.01
Agriculture	0.025	0.02
Manufacturing	0.277	0.20
Construction	0.061	0.06
Trade	0.278	0.20
Finance	0.126	0.11
Energy	0.018	0.02
Other services	0.215	0.17
Foreign	0.397	0.24
Domestic	0.603	0.24
Firm size (log)	4.670	2.32
Fixed assets per worker (log)	7.920	1.81
Exporter	0.521	0.25

Notes: Skilled workers, estimation sample for the wage gap model (Eq. 4.1) Each variable covers 19,961,622 person months. The spells belong to workers employed at least once in a firm, the size of which exceeded the 10 workers limit at least once in 2003–2011. Public sector and state-owned firms are excluded. Note that other samples used in the paper have been drawn from this source file

Table D.2: Pooled OLS results for Eq. 4.1, specification 4

	Coefficient	**t-value**
Majority owner		
Foreign	0.437	20.4
Person controls		
Male	0.154	19.1
Age	0.032	15.2
Age squared/100	-0.033	13.8
Months spent non-employed in 2003–2011	-0.003	31.8
Receipt of disability payment	-0.373	23.3
Receipt of care allowance	-0.207	12.1
Health expenditures (log)	0.002	7.3
Job controls		
Tenure if observed	0.001	4.3
Tenure is unobserved	0.138	12.2
Spell lasting for 1 day	0.354	3.1
Top manager	*Ref.*	
Other managers	-0.062	1.6^n
Professional	-0.016	0.4^n
Other white collar	-0.298	9
Skilled blue collar	-0.607	28.8
Assembler, machine operator	-0.728	18.7
Laborer in elementary occupation	-0.821	18.8
Regional unemployment rate (log)	-0.063	5.2
Budapest	0.142	11.3
Firm controls		
Firm size (log)	0.086	7.3
Capital-labor ratio (log)	0.041	9.3
Exporter	0.185	9.4
Constant	-1.650	24
Adjusted R^2	0.479	
Number of observations	19,961,622	

Notes:Skilled workers, 2003–2011 Dependent variable: log daily earnings relative to the national mean. For the exact definition of the variables see Appendix D.2: "Data". The coefficients of 63 sector–year dummies are not shown. The standard errors are adjusted for clustering by persons and firms. All coefficients are significant at 0.01 level except ^{n} not significant at 0.1 level

Table D.3: Incidence of atypical work schedules in foreign and domestic enterprises

Level of education[a]	**Domestic**	**Foreign**	**Domestic**	**Foreign**
	Shift work[b]		*Overtime work*[b]	
Low	27.5	58.2	14.4	32.0
Middling	22.4	41.2	12	24.3
High	4.4	4.5	4.0	7.6
	Work in the afternoon[c]		*Work in the night*[c]	
Low	14.4	29.1	8.1	20.3
Middling	18.6	33.1	9.4	22.1
High	17.7	14.4	7.1	6.6
	Work on Saturdays[c]		*Work on Sundays*[c]	
Low	26.3	29.3	16.9	17.6
Middling	35.4	36.6	21.2	24.0
High	26.8	18.9	16.7	12.7

Notes: 2003–2011, percent
[a] Low=primary school attainment, High=college or university, Middling=rest
[b] Source: Wage Surveys, 2003–2011, private sector. Firms are classified on the basis of their majority owners. The data indicate the percentage share of employees receiving shift pay and overtime pay, respectively. Authors' calculation
[c] Source: Labor Force Surveys, 2003 Q1–2011 Q4, excluding public administration, education, health and social services. The data indicate the percentage share of employees working in the respective periods at least occasionally. Authors' calculation

Table D.4: The effect of ownership on the probability of becoming unemployed—logit odds ratios

	Educational attainment[a]		
	Low	**Middling**	**High**
Employer: MNE	1.199*** (2.57)	0.971 (0.50)	1.061 (0.89)
Female	0.916 (1.40)	1.029 (0.55)	1.149*** (2.44)
Age	1.012 (0.71)	0.941*** (3.85)	0.919*** (5.01)
Age squared	0.999** (2.06)	1.000*** (3.08)	1.000*** (4.25)
Tenure (years)	0.894*** (9.27)	0.886*** (13.9)	0.895*** (10.0)
Number of observations	82,638	205,597	227,074
Pseudo R^2	0.076	0.067	0.068
Wald chi^2 (51)	617.4***	958.0***	763.8***

Notes: Significant at the **0.5 and ***0.01 level Discrete time survival model, logit form, following Jenkins (1995). Estimated for the pool of 28 quarterly waves of the Labor Force Survey in 2003–2009. The estimation excludes the crisis period (2010 and 2011). Sample: employees. Dependent variable: 1 if the person was ILO-OECD unemployed in wave t+1 and 0 otherwise. The coefficients of 19 county dummies and 27 survey wave dummies are not shown [a]Low=primary school attainment, High=college or university, Middling=rest

Table D.5: On-the-job training: fraction participating among MNE and domestic firm employees

	Foreign	**Domestic**	**Ratio**
2003	0.102	0.065	1.57
2004	0.100	0.060	1.67
2005	0.043	0.024	1.80
2006	0.045	0.019	2.41
2007	0.033	0.019	1.73
2008	0.024	0.019	1.30
2009	0.020	0.013	1.59
2010	0.025	0.015	1.59
2011	0.021	0.014	1.61

Notes: High skilled employees working at least one hour in the reference week=1 Source: Authors' calculation using waves 45–80 of the LFS. Sample: ILO-OECD employed with college or university background. Key variables: participates in training of any kind outside the school system; the employer is majority or minority foreign-owned Note that the question on participation changed in 2005. Figures for 2003-2004 and for 2005-2011 are not directly comparable.

D.2 Data and key variables

Data

- Starting sample: 50 percent random sample drawn from Social Security Numbers (SSN, Hungarian TAJ) valid on January 1, 2003. SSN holders aged 5–74 were retained. Data held by the Pension Directorate (ONYF), the Tax Office (NAV), the Health Insurance Fund (OEP), the Office of Education (OH), and the Public Employment Service (NMH) were merged and anonymized by the National Information Service (NISZ). The original data consisted of payment records with start and end dates, a type-of-payment code and amounts received by the person. Employers were identified by ONYF and their annual financial data were provided by NAV. The data was transformed to a fixed format monthly panel data set by the Databank of the Institute of Economics of the Hungarian Academy of Sciences.

- Estimation sample: Workers employed with a labor contract at least once in a foreign or domestic private enterprise the maximum employment level of which exceeded the 10 workers limit at least once in 2003–2011. We removed workers and firms with less than two data points, zero wages and missing covariates. 98.5 percent of the workers belong to a single connected group.[178] Special subsamples have been selected for the study of new firms, lagged returns and spillovers.

- Data access: Data for the estimation samples and Stata do files are available on request. The original data set called Admin2 is also available via remote access to the Databank's servers. Write to adatkeres@krtk.mta.hu for requesting access to the data. Note that the size of the original data ranges between 60 and 120 Gbytes, depending on the amount of information stored in special modules that you want to merge to the base file. The files are in Stata16 format. R and Python codes are allowed.

Key variables

- Wage: The daily wage figure used in the paper was calculated as monthly earnings divided by the number of days covered by pension insurance ('working days' henceforth) in the given month. Multiple payments made by the same employer to the same person within a month were summed up. Working days belonging to these payments were also summed up but capped at 30 or 31 days. In the case of multiple job holders the wage figure belongs to the

[178]'When a group of persons and firms is connected, the group contains all the workers who ever worked for any of the firms in the group and all the firms at which any of the workers were ever employed. In contrast, when a group of persons and firms is not connected to a second group, no firm in the first group has ever employed a person in the second group, nor has any person in the first group ever been employed by a firm in the second group. From an economic perspective, connected groups of workers and firms show the realized mobility network in the economy. From a statistical perspective, connected groups of workers and firms block-diagonalize the normal equations and permit the precise statement of identification restrictions on the person and firm effects.' Abowd et al. (2006)

highest paying job. We normalized the wage figures by dividing them with the national average wage in the given month, as measured in the starting sample. Source: ONYF.

- Foreign-owned firm, MNE: dummy variable set to 1 for firms majority owned by one or more foreign owners. Ownership shares are measured as fractions of subscribed capital. Source: NAV.

Person controls

- Gender, age: Source: ONYF.
- Skill levels: Skill levels are inferred from the 'highest' occupational status held by the person in 2003–2011. The classification is based on one-digit occupational codes: 1 Top managers, 2 Other managers, 3 Professionals, 4 Other white collars, 5 Skilled blue collars, 6 Assemblers and machine operators, 7 Elementary occupations. Persons employed in occupations 1–3 at least once are classified as high skilled. Persons never employed outside occupations 6 and 7 are classified as low skilled. Other persons are classified as medium skilled. Source: ONYF.
- Total time spent non-employed: The number of months out of employment in 2003–2011. Source: ONYF.
- Disability payment: dummy variable, with 1 standing for any kind of transfer (pension or allowance) received on the basis of permanent disability (rokkant nyugdíj, rokkantsági járadék). Monthly data. Source: ONYF.
- Care allowance: dummy variable, with 1 standing for any kind of benefit received by the observed person on the basis of raising children (tgyás, gyed, gyes, gyet) or taking care of disabled relatives (ápolási segély). Monthly data. Source: OEP, ONYF.
- Health expenditures: Expenditures and costs registered by the National Health Insurance Fund (OEP). The items include total amount paid for OEP-supported medicine and the costs of OEP-supported services/treatment provided by district doctors, specialists and hospitals. We normalized the nominal figures by dividing them with the national average wage in the given month, as measured in the starting sample. Zero expenditures were replaced with 1 Ft (0.3 Euro cents per annum). Annual data. Source: OEP.

Job controls

- Tenure: Months elapsed since entry to the firm. Set to zero in the case of left-censored employment spells. A dummy stands for observations from left-censored spells.
- Spell lasting for 1 day: Hungarian firms often pay to individual subcontractors by formally employing them on the day of payment. This practice results in exorbitant 'daily wages' in some cases.

- Occupation: One-digit ISCO codes.
- Regional unemployment rate: seasonally adjusted ILO-OECD unemployment rate in the given month and NUTS-2 region. The worker's region is identified on the basis of his/her zip code in 2003. Source: author's calculation using the Labor Force Survey.

Firm controls

- Firm size: average number of employees. Annual data. Source: NAV.
- Capital-labor ratio: net value of fixed assets per worker. Annual data. Source: NAV.
- Exporter: non-zero exports revenues. Annual data. Source: NAV.
- Sector: NACE 2. Source: NAV.