A Fixed-effect Approach for Estimating Rent-sharing Elasticities

István Boza[a]

[a]Centre for Economic and Regional Studies, Budapest, Hungary

This paper combines recent advances in the literature of rent-sharing and firm-specific wage premia and proposes a novel rent-sharing estimation design utilizing time-varying firm effects. This design has three notable properties, which have not been jointly present in previous models in the literature. First, it controls for both observed and unobserved heterogeneity in the workforce composition of firms. Second, it could be used in specifications relying on within-firm identification of wage effects of inter-temporal productivity changes of the firm as well, not only in models relying on cross-sectional variation. Third, the design can achieve both above features while still incorporating information from the wage-variation of both long-term stayers at the firms and job-switchers as well. Accordingly, this approach intends to solve the selectivity issues inherently present in previous, state-of-the-art specifications of productivity-wage pass-through estimations. Our empirical application, using Hungarian administrative linked employer employee panel data, suggests that the selectivity problem of previous models is only a second-order issue. The estimated OLS elasticities range between 0.05-0.16 across the established specifications, while estimates relying on internal instruments, range between 0.12-0.18. A second set of results focuses on heterogeneity of firms with respect to their rent-sharing behaviour and the importance of design choice. 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.

Keywords: bargaining, productivity, rent-sharing, time-varying firm premia
JEL codes: J31, C13
Corresponding author: István Boza, bozaistvan@gmail.com
Address of affiliation: 1097 Budapest, Tóth Kálmán street 4., Hungary
Declarations of interest: none
* The author would like to thank for the technical support of the Databank of the Centre for Economic and Regional Studies and for all the comments and suggestions of Andrea Weber, Ana Rute Cardoso, Tímea Laura Molnár and participants of seminars at Central European University and CERS.

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. In this process, we provide a within-match, fixed effect alternative to the stayer designs that conventionally use (first) differences.[1] Then, we propose a novel solution for the selectivity issues inherent to both the stayer designs – and to a smaller extent to the AKM-based ones as well – by using firm-year effects as outcome variables in regressions on firm productivity. As this time-varying measure of firm premia – introduced by (Lachowska et al., 2020), among others – captures wage information of both long-term stayers and job-switchers, while also maintaining both the possibility of controlling for unobserved worker heterogeneity and the

[1] Despite some minor differences in identification, these model present similar behavior.

possibility to be be included in longitudinal models, we bridge multiple gaps between the existing modelling approaches. Accordingly, 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 our empirical results, using linked employer-employee data from Hungary, we find rent-sharing elasticities 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. Allowing for heterogeneous effects across sets of firms also reveal the important differences between cross-sectional and longitudinal estimation methods. 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.

The remainder of the paper is structured as follows. Section 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 2.5 presenting the main extension we apply to the simple model in order to assess these econometric challenges. Section 3 assesses the further issues of identification and the necessary data restrictions, and also introduces our models for assessing differences in rent-sharing behavior across groups of firms. Section 4 contains our results from the comparison of different specifications and heterogeneity across firm types. Section 5 concludes.

2 Relation of wages and productivity

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.[2] 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 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.[3] 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.[4]

An other branch of explanations relies on the bargaining power of workers ,

[2]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).

[3]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.

[4]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 elasticities.

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 differences, and hence will prefer to use the term *rent-sharing elasticity* in favor of *pass-through rate* throughout the paper.

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 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.[5] 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[6]. 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

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

[6]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).

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 et al. (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

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.

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{RENT}_{jt} + \beta \mathbf{X}_{ijt} + \theta_k + \omega_t + \varepsilon_{ijt} \tag{1}$$

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.[7] 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

[7]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.

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

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \lambda_{s(j)} + \omega_t + \varepsilon_{ijt} \tag{2}$$

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.[8] 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 elasticities.[9] 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 2.4. Making use of firm fixed effects, a very simple

[8] 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.

[9] Lamadon et al. (2022) uses short term productivity changes as an instrument for identifying long-term elasticities. 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.

longitudinal model would take the following form.

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \psi_j + \omega_t + \varepsilon_{ijt} \tag{3}$$

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.[10] 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 for a within-spell identification. Hence the simple model of Equation 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:

$$\ln W_{ijt} = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \mu_{ij} + \omega_t + \varepsilon_{ijt} \tag{4}$$

While these *stayer* models are often formulated in terms of (first) differences [11], 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).[12] 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,

[10] 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.

[11] 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.

[12] 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.

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 A, 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.

2.4 Threats to identification

As given sufficient cross-sectional or temporal variation in firm rents γ is identified, the estimation on any form of Equation 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 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.

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.[13] 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; Martins, 2009) or even the productivity measures of similar firms in other local labor markets

[13]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.

(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.[14] 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 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.

$$\ln w_{ijt} = \mathbf{X}_{ijt}\beta + \theta_i + \psi_j + \epsilon_{ijt}. \tag{5}$$

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

[14] 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.

therefore provides a clear estimation of the rent-sharing elasticity.[15] Equation 1 of our framework can also nest this approach, by substituting the outcome, W, with ψ_j firm effects.

$$\psi_j = \alpha + \gamma \ln \mathrm{VA}_{jt} + \beta X_{ijt} + \lambda_{s(j)} + \omega_t + \varepsilon_{ijt} \tag{6}$$

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 2.5 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 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

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

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 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 elasticities. 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 4, only with smaller weights than long run stayers.[16]

[16] 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

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.[17] 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.[18] 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.

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

$$\psi_{jt} = \alpha + \gamma \mathrm{lnVA}_{jt} + \beta X_{ijt} + \tilde{\psi}_j + \omega_t + \varepsilon_{ijt} \tag{7}$$

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

$$ln\ w_{ijt} = \boldsymbol{X_{ijt}\beta} + \theta_i + \psi_{jt} + \lambda_{k(ij)} + \epsilon_{ijt} \tag{8}$$

, 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

sample is 9 years – a formidable length.

[17] 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.

[18] Juhn et al. (2018) discusses a specification in which (at least in some industries) rent-sharing elasticities are somewhat larger for individuals who have at least one year of tenure at the given firm.

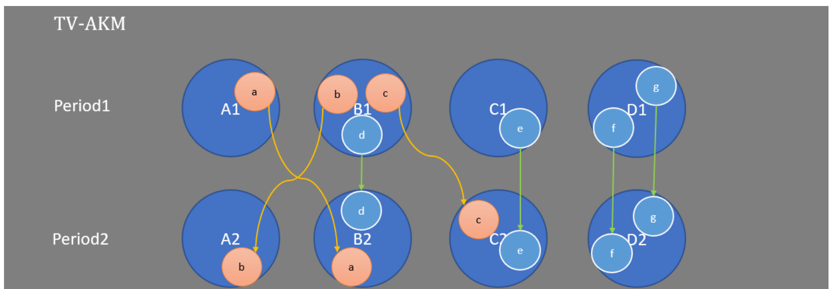
the given firm. Lachowska et al. (2020) labels this specification TV-AKM and discusses its stability and contribution to overall wage dispersion, while also proposing rent-sharing as a possible reason for the emergence of such, time-varying components. As discussed by the authors, the identification 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.[19] 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 effects of this model ψ_{jt} can be used in within-firm, cross-temporal comparisons. Accordingly, as Equation 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.

The difference between the three different specifications – stayer, AKM and TV-AKM designs – is illustrated in Figure 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 2.4.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

[19]"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)

Equation 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 7.[20]



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 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.

[20]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.

2.6 Comparison of conventional and novel methods

Table 1: Conventional and novel approaches for estimating the rent-sharing elasticity

Setting	Classic CS	Classic L.	Stayer	CC(H)K	Boza
Equation	(2)	(3)	(4)	(6)	(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 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 2.5 – 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 2.4. 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.

3 Empirical strategy

The main aim of our empirical exercise is to estimate both the models established in previous literature (Equations 2, 3, 4 and 6) and the novel approach proposed in Equation 7, using the same general framework –Equation 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 2.4 and summarized in Table 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.[21] 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.

3.1 Sources of data, definition of variables

Our estimations use data from the Databank of the Research Centre for Economic and Regional Studies[22]. 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 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.

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

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

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[23] or the firm-year effects defined in Equation 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).[24] A minor, although important extension in our approach compared to Equation 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 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 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

[23]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.

[24]The sample restrictions and variable choices for the AKM models are presented in Appendix B.

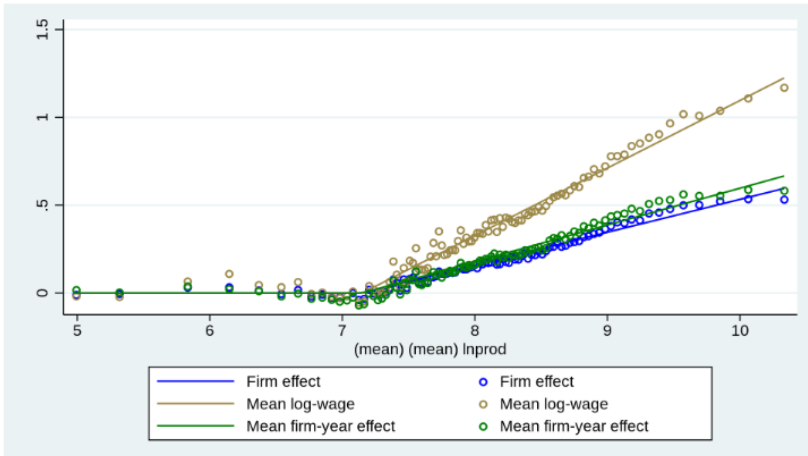


Figure 2: The relation between wage and productivity measures

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.

firms may have no rents (in given years) to share with their employees.[25] The kink-points using the three different wage-measures coincide almost perfectly, and as Figure 2 illustrates we will exclude observations corresponding to around 15% of firm-year observations.

3.2 Estimation and inference issues

For our main estimation, we will report estimations based on the Equation 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, 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, 3, 4 and 6 and 7 – and one alternative versions for both Equations 4 and 6 in which we substitute wages or firm-effects

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

(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.[26] 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.[27]

As discussed in Section 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.

$$\mathrm{VA}_{jt} = \mathrm{VA}_{jt}^{LR} + \mathrm{VA}_{jt}^{SR} + \mathrm{me}_{jt} + \epsilon_{jt} \tag{9}$$

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, 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.[28]

[26] 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.

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

[28] 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.

$$
\begin{aligned}
\gamma_{IV} &= \frac{cov(\mathrm{VA}_{t-1}, w_t)}{cov(\mathrm{VA}_{t-1}, \mathrm{VA}_t)} \\
&= \frac{cov(\mathrm{VA}_{t-1}, \alpha + \gamma \mathrm{VA}_t + \varepsilon_t)}{cov(\mathrm{VA}_{t-1}, \mathrm{VA}_t)} \\
&= \frac{cov(\mathrm{VA}^{LR}_{t-1} + \mathrm{me}_{t-1} + \epsilon_{t-1}, \gamma \mathrm{VA}^{LR}_t + \gamma \mathrm{me}_t + \gamma \epsilon_t t)}{cov(\mathrm{VA}^{LR}_{t-1} + \mathrm{me}_{t-1} + \epsilon_{t-1}, \mathrm{VA}^{LR}_t + \mathrm{me}_t + \epsilon_t)} \\
&= \\
&\frac{cov(\mathrm{VA}^{LR}_{t-1}, \gamma \mathrm{VA}^{LR}_t)}{cov(\mathrm{VA}^{LR}_{t-1}, \mathrm{VA}^{LR}_t)} \\
&= \gamma
\end{aligned}
\tag{10}
$$

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.[29] If the same argument holds for the temporary productivity components – that is $Corr(\mathrm{VA}^{SR}_{jt}, \mathrm{VA}^{SR}_{jt-1}) = 0$ – , 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.[30]

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 et al. (2020) proposes an appealing method. While our computational infrastructure does not allow (yet) for adapting this correction, having 15 years of quarterly data may help at least partially overcome the limited mobility bias problem.[31] Nevertheless, when interpreting and comparing

[29] 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 midpoint. Gürtzgen (2009), on the other hand, proposes higher order lags as valid instruments.

[30] 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.

[31] The data infrastructure we have access to unfortunately does not allow us (yet) to effectively adapt the bias-correction method of Kline et al. (2020).

the rent-sharing elasticities, especially when making across group comparisons as in the exercises of Section 4.2, we have to bear in mind that the reported standard errors may be underestimated.

4 Results

4.1 Comparison of conventional and novel methods

Table 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.[32] 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.[33]

[32]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.

[33]Appendix A 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: 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.	TV-AKM	Stayer	Stayer^a
Equation:	Eq. 2	Eq. 6^a	Eq. 6	Eq. 3	Eq. 7	Eq. 4	Eq. 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.[34] This pattern also suggests a substantial role of the worker composition of firms in defining the cross-sectional relation of productivity and wages.[35] 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.[36]

[34]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.

[35]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.

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

By estimating the novel specification proposed in Section 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 effects is not significantly different from the one using conventional firm fixed effects.[37] 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 elasticities. 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.

Card et al. (2016).

[37] 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.

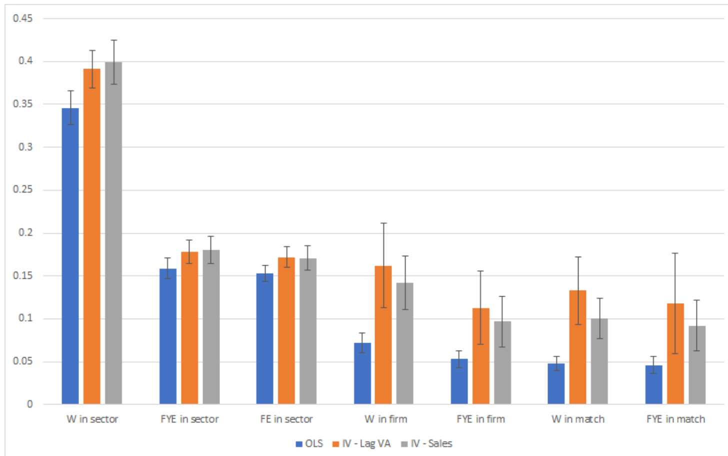


Figure 3: Rent sharing elasticities from conventional and novel specifications (OLS and 2 IVs)

Notes: Regression estimations are presented in Table 2 and Appendix Tables C1 and C2. 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 3, presenting these estimates alongside the OLS results of Table 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 elasticities – 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.

4.2 Heterogeneous effects across firms

To assess the heterogeneity of the wage-productivity relation across sectors, we modify Equation 1 the following way.

$$\ln W_{h(j)ijt} = \alpha + \Sigma_{h\in H}\gamma_h \mathrm{I}_{h(j)}\ln\mathrm{VA}_{jt} + \beta X_{ijt} + \theta_k + \varepsilon_{ijt} \tag{11}$$

In this form I_h represents subsets of firms based on majority ownership category, industry, size or combinations of these.[38] 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.

In the following, we estimate models of the form proposed in 11 for the three main specifications of Section 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.[39] 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 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.

[38] 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.

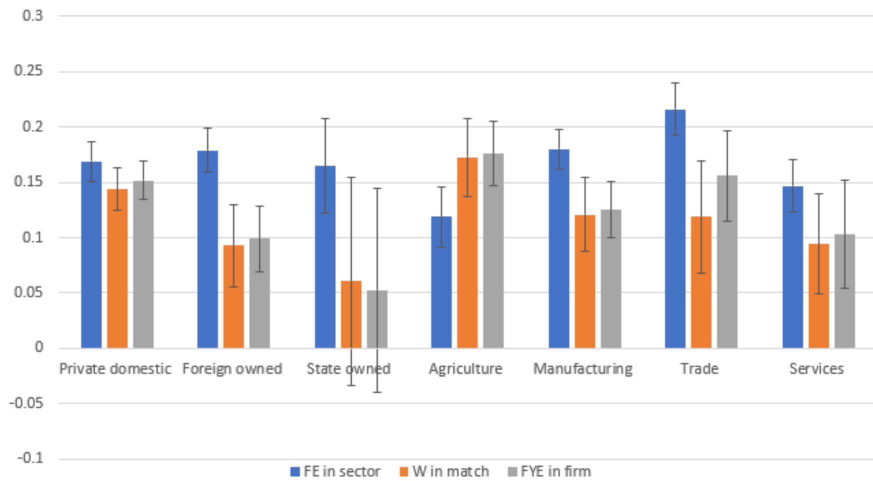
[39] Technically as many instruments are included as the number of interacted parameters of interests, which we generate by interacting the lagged productivity measure with the corresponding firm types, similarly as Juhn et al. (2018).

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.[40]

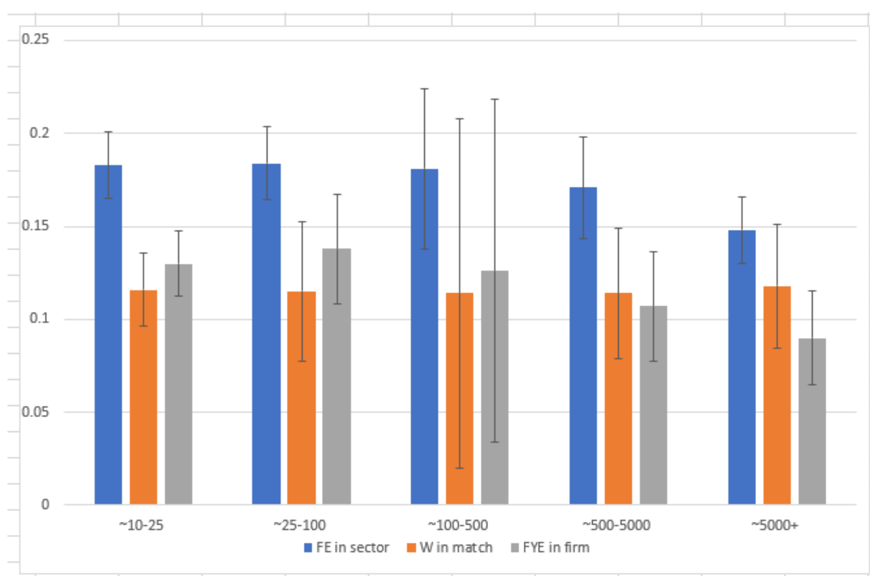
The bottom panel of Figure 4 also presents results based on average firm size. Interestingly, in stayer-focused models – the middle columns – a strong downward trend is apparent as we focus on larger and larger firms, while we find no such pattern or only a very weak one in models using AKM or TV-AKM 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.[41]

[40]Appendix Table C6 contains estimates using industry-ownership interactions, revealing that the low parameter estimates in services are indeed, in part, driven by virtually zero rent-sharing in the state owned service firms.

[41]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 4: Rent-sharing elasticities across sets of firms – IV estimates

Notes: In all set of columns, the first estimation relates to the heterogeneous parameter estimates based on Equation 6, the second to those based on Equation 4, and the third column relates to the model we propose in Equation 7. Regression estimations are presented in Appendix Tables C3, C4 and C5.

5 Discussion

As our study makes both methodological and empirical contributions, we consider two 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 elasticities from assuming a bargaining power of workers and a rent-sharing behavior of employers.

References

Abowd, J. M., Kramarz, F., & Margolis, D. N. (1999). High wage workers and high wage firms. *Econometrica*, *67*(2), 251–333. https://doi.org/10.1111/1468-0262.00020

Abowd, J. M., & Lemieux, T. (1993). The Effects of Product Market Competition on Collective Bargaining Agreements: The Case of Foreign Competition in Canada. *The Quarterly Journal of Economics*, *108*(4), 983–1014. https://doi.org/10.2307/2118457

Allan, David Maré & Corey, C. (2021). Do Workers Share in Firm Success? Pass-through Estimates for New Zealand. www.iza.org

Arai, M., & Heyman, F. (2009). Microdata evidence on rent-sharing. *Applied Economics*, *41*(23). https://doi.org/10.1080/00036840701721620

Bagger, J., Bent Jesper, C., & Mortensen, D. T. (2014). *Wage and Labor Productivity Dispersion : The Roles of Total Factor Productivity , Labor Quality , Capital Intensity , and Rent Sharing.*

Barth, E., Bryson, A., Davis, J. C., & Freeman, R. (2016). It's where you work: Increases in the dispersion of earnings across establishments and individuals in the United States. *Journal of Labor Economics*, *34*(S2). https://doi.org/10.1086/684045

Bonhomme, S., Holzheu, K., Lamadon, T., Manresa, E., Mogstad, M., & Setzler, B. (2020). How Much Should We Trust Estimates of Firm Effects and Worker Sorting? *SSRN Electronic Journal*. https://doi.org/10.2139/ssrn.3625745

Boza, I. (2021). Wage Structure and Inequality : The role of observed and unobserved heterogeneity. *KRTK-KTI WORKING PAPERS*, (31).

Card, D., Cardoso, A. R., Heining, J., & Kline, P. (2018). Firms and Labor Market Inequality: Evidence and Some Theory. *Journal of Labor Economics*, *36*(S1), S13–S70. https://doi.org/10.1086/694153

Card, D., Cardoso, A. R., & Kline, P. (2016). Bargaining, Sorting, and the Gender Wage Gap: Quantifying the Impact of Firms on the Relative Pay of Women. *Quarterly Journal of Economics*, *131*(2), 633–686. https://doi.org/10.1093/qje/qjv038

Card, D., Devicienti, F., & Maida, A. (2014). Rent-sharing, holdup, and wages: Evidence from matched panel data. *Review of Economic Studies*, *81*(1), 84–111. https://doi.org/10.1093/restud/rdt030

Carlsson, M., Messina, J., & Skans, O. N. (2016). Wage Adjustment and Productivity Shocks. *Economic Journal*, *126*(595), 1739–1773. https://doi.org/10.1111/ecoj.12214

Correia, S. (2017). Linear Models with High-dimensional Fixed Effects: An Efficient and Feasible Estimator. *Working Paper.*

Correia, S. (2018). IVREGHDFE: Stata module for extended instrumental variable regressions with multiple levels of fixed effects. https://ideas.repec.org/c/boc/bocode/s458530.html

Criscuolo, C., Hijzen, A., Koelle, M., Schwellnus, C., Barth, E., Chen, W.-h., Fabling, R., Fialho, P., Garloff, A., Grabska, K., Kambayashi, R., Lankester, V., Stadler, B., Skans, O. N., & Murakozy, B. (2021). The firm-level link between productivity dispersion and wage inequality : A symptom of low job mobility ? *OECD Economics Department Working Papers*, (1656).

Friedrich, B., Laun, L., Meghir, C., & Pistaferri, L. (2019). Earnings Dynamics and Firm-Level Shocks. *SSRN Electronic Journal*, (2175). https://doi.org/10.2139/ssrn.3375653

Guiso, L., Pistaferri, L., & Schivardi, F. (2015). Insurance within the Firm. *https://doi.org/10.1086/432136*, *113*(5), 1054–1087. https://doi.org/10.1086/432136

Gürtzgen, N. (2009). Rent-sharing and collective bargaining coverage: Evidence from linked employer-employee data. *Scandinavian Journal of Economics*, *111*(2). https://doi.org/10.1111/j.1467-9442.2009.01566.x

Gyetvai, A. (2017). *High Wage Workers, High Wage Firms, and High Wage Occupations* (Doctoral dissertation No. *2*).

Hildreth, A. K. (1998). Rent-sharing and wages: product demand or technology driven premia? *Economics of Innovation and New Technology*, *5*(2-4), 199–226. https://doi.org/10.1080/10438599800000005

Hildreth, A. K., & Oswald, A. J. (1997). Rent-sharing and wages: Evidence from company and establishment panels. *Journal of Labor Economics*, *15*(2). https://doi.org/10.1086/209835

Juhn, C., McCue, K., Monti, H., & Pierce, B. (2018). Firm performance and the volatility of worker earnings. *Journal of Labor Economics*, *36*(S1), S99–S131. https://doi.org/10.1086/694167/SUPPL_FILE/16097DATA.ZIP

Kline, P., Petkova, N., Williams, H., & Zidar, O. (2019). Who Profits from Patents? Rent-Sharing at Innovative Firms. *The Quarterly Journal of Economics*, *134*(3), 1343–1404. https://doi.org/10.1093/QJE/QJZ011

Kline, P., Saggio, R., & Sølvsten, M. (2020). Leave-Out Estimation of Variance Components. *Econometrica*, *88*(5), 1859–1898. https://doi.org/10.3982/ecta16410

Lachowska, M., Mas, A., Saggio, R., & Woodbury, S. (2020). *Do Firm Effects Drift? Evidence from Washington Administrative Data* (tech. rep.). National Bureau of Economic Research. Cambridge, MA. https://doi.org/10.3386/w26653

Lamadon, T., Mogstad, M., & Setzler, B. (2022). Imperfect Competition, Compensating Differentials, and Rent Sharing in the US Labor Market. *American Economic Review*, *112*(1), 169–212. https://doi.org/10.1257/aer.20190790

Macis, M., & Schivardi, F. (2016). *Exports and Wages: Rent Sharing, Workforce Composition, or Returns to Skills?* (Tech. rep.). http://www.journals.uchicago.edu/t-and-c

Manning, A. (2021). Monopsony in Labor Markets: A Review. *ILR Review*, *74*(1), 3–26. https://doi.org/10.1177/0019793920922499

Martins, P. S. (2009). Rent sharing before and after the wage bill. *Applied Economics*, *41*(17). https://doi.org/10.1080/00036840701736164

Portugal, P. (2020). The sources of wage variability in Portugal: a binge reading survey. *Banco de Portugal Economic Studies*.

Reizer, B. (2019). Wage structure , Employment and Efficiency, 1–73.

Song, J., Price, D. J., Guvenen, F., Bloom, N., & Von Wachter, T. (2019). Firming Up Inequality. *The Quarterly Journal of Economics*, *134*(1), 1–50. https://doi.org/10.1093/qje/qjy025

Sorkin, I. (2018). Ranking firms using revealed preference. *Quarterly Journal of Economics*, *133*(3), 1331–1393. https://doi.org/10.1093/QJE/QJY001

Torres, S., Portugal, P., Addison, J. T., & Guimarães, P. (2018). The sources of wage variation and the direction of assortative matching: Evidence from a three-way high-dimensional fixed effects regression model. *Labour Economics*, *54*, 47–60. https://doi.org/10.1016/j.labeco.2018.06.004

Van Reenen, J. (1996). The Creation and Capture of Rents: Wages and Innovation in a Panel of U. K. Companies. *The Quarterly Journal of Economics*, *111*(1), 195–226. https://doi.org/10.2307/2946662

Weeks, D. L., & Williams, D. R. (1964). A Note on the Determination of Connectedness in an N-Way Cross Classification. *Technometrics*, *6*(3), 319–324. https://doi.org/10.1080/00401706.1964.10490188

Appendices

Appendix A - Differenced versus fixed effect stayer designs

In table A1, 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 elasticities 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.

Table A1: 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 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 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. (2022) 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.

Appendix B - Estimation of the AKM model

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.[42] 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 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).[43] 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. 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 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

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

[43] 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.

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/ categories. 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 industry will become truly time-invariant characteristics of firms defined this way.[44]

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 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.

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

[44] 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.

Appendix C - Regression tables for figure results

Table C1: Rent-sharing elasticities 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}
lnprod	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. For the interpretation of specifications see Table 2. The models use the one-year lag of firm value added as an instrument for contemporaneous value added.

Table C2: Rent-sharing elasticities 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}
lnprod	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. For the interpretation of specifications see Table 2. The instrument used is the mean of sales observations from the given year, one year before and one year after.

Table C3: Rent-sharing elasticities 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. For the interpretation of specifications see Table 2.

Table C4: Rent-sharing elasticities 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. For the interpretation of specifications see Table 2.

Table C5: Rent-sharing elasticities 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}
VA— 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)
VA— 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)
VA— 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)
VA— 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)
VA— 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. For the interpretation of specifications see Table 2.

Table C6: Rent-sharing elasticities by sectors and 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—Agriculture — private owned	0.240***	0.145***	0.101***	0.288***	0.220***	0.256***	0.260***
	(0.028)	(0.022)	(0.016)	(0.038)	(0.030)	(0.046)	(0.046)
VA—Manufacturing — private owned	0.343***	0.174***	0.170***	0.233***	0.175***	0.217***	0.198***
	(0.016)	(0.010)	(0.009)	(0.023)	(0.020)	(0.026)	(0.029)
VA—Trade — private owned	0.370***	0.181***	0.174***	0.283***	0.224***	0.271***	0.259***
	(0.015)	(0.013)	(0.010)	(0.024)	(0.022)	(0.024)	(0.026)
VA—Services — private owned	0.396***	0.172***	0.171***	0.237***	0.159***	0.218***	0.193***
	(0.031)	(0.017)	(0.015)	(0.022)	(0.020)	(0.030)	(0.034)
VA—Agriculture — foreign owned	0.345***	0.135**	0.140***	0.209***	0.079	0.196**	0.093
	(0.042)	(0.034)	(0.030)	(0.036)	(0.045)	(0.062)	(0.081)
VA—Manufacturing — foreign owned	0.381***	0.171***	0.160***	0.134***	0.096***	0.107***	0.107***
	(0.020)	(0.012)	(0.009)	(0.021)	(0.018)	(0.020)	(0.021)
VA—Trade — foreign owned	0.534***	0.250***	0.242***	0.097*	0.079**	0.092**	0.101**
	(0.032)	(0.015)	(0.014)	(0.035)	(0.022)	(0.029)	(0.027)
VA—Services — foreign owned	0.481***	0.189***	0.182***	0.134*	0.085	0.066	0.051
	(0.026)	(0.021)	(0.021)	(0.048)	(0.042)	(0.049)	(0.055)
VA—Agriculture — state owned	0.217**	0.119***	0.100***	0.041	0.188*	0.292*	0.243*
	(0.061)	(0.023)	(0.022)	(0.254)	(0.084)	(0.116)	(0.112)
VA—Manufacturing — state owned	0.333***	0.180***	0.156***	0.073**	0.084**	0.088***	0.099***
	(0.030)	(0.017)	(0.018)	(0.023)	(0.022)	(0.020)	(0.021)
VA—Trade — state owned	0.486***	0.205***	0.218***	0.309	-0.222	-0.497**	-0.223
	(0.041)	(0.040)	(0.039)	(0.191)	(0.145)	(0.150)	(0.174)
VA—Services — state owned	0.274**	0.108*	0.121**	-0.012	-0.002	0.000	0.009
	(0.078)	(0.046)	(0.040)	(0.019)	(0.006)	(0.009)	(0.015)
Observations	280,761	253,538	263,104	266,202	240,695	34,742,342	29,123,312
R-squared	0.473	0.307	0.329	-0.020	0.003	-0.011	-0.061
Number of units	45	44	44	44050	39783	2.768e+06	2.346e+06

Robust standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05

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. For the interpretation of specifications see Table 2.