The pass-through of monopsony power to wages

Boris Hirsch[*], Elke J. Jahn[†], Alan Manning[‡], and Michael Oberfichtner[§]

***Abstract*:** There is ample empirical evidence that the labour supply to an individual employer is not perfectly elastic with respect to wages. This implies employers have potential monopsony power. This paper aims to estimate the extent to which potential monopsony power passes through to lower wages. Using German administrative data, we find that the average pass-through is nearly 40%. We also show that the pass-through is lower where collective bargaining and work councils are present. When estimating the pass-through along the wage distribution, we find that monopsony power is most severe in low-wage labour markets.

***Keywords*:** Monopsony, imperfect labour markets, pass-through to wages

***JEL classification*:** J42, J31

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[*] Boris Hirsch, Leuphana University of Lüneburg, IWH, and IZA, Universitätsallee 1, 21335 Lüneburg, Germany, email: hirsch@leuphana.de

[†] Elke J. Jahn, Institute for Employment Research, University of Bayreuth, and IZA, Regensburger Str. 104, 90478 Nuremberg, Germany, email: elke.jahn@iab.de

[‡] Alan Manning, London School of Economics and Centre for Economic Performance, Houghton Street, London WC2A 2AE, United Kingdom, a.manning@lse.ac.uk

[§] Michael Oberfichtner, Institute for Employment Research, Regensburger Str. 104, 90478 Nuremberg, Germany, email: michael.oberfichtner@iab.de

1 Introduction

The last two decades saw a renewed interest in monopsony as a relevant way of thinking about wage formation. This observation is hardly surprising against the background of falling labour shares, increasing wage inequality, and the erosion of organised labour, all of these being suggestive of a widening power imbalance between workers and employers.

The general idea of this literature, summarised in Manning (2011, 2021a), is that the wage elasticity of labour supply to an individual employer is not infinite as it would be under perfect competition. Workers' responsiveness to wages is limited for several reasons including search frictions, mobility costs, job differentiation, and employer concentration. The empirical evidence is highly supportive of this idea (see the meta-analysis of 1,320 estimates of the wage elasticity of labour supply to the individual employer from 53 studies by Sokolova and Sorensen, 2021).

One obvious shortcoming of the literature is that "evidence of upward sloping labor supply is not sufficient to infer monopsonistic outcomes" (Hirsch and Schumacher, 2015: 987). In other words, the labour supply elasticity only measures employers' *potential* monopsony power, and it needs to be established that employers' monopsony power really translates into lower wages paid to workers. It is this paper's aim to estimate the pass-through of monopsony power to wages based on rich administrative data for West Germany over the years 1985–2018.

We proceed in two steps. In a first step, we obtain an estimate of the wage elasticity of labour supply to the individual employer. This provides us with one measure of the employer's *potential* monopsony power. In the textbook model of monopsony pioneered by Robinson (1933), the labour supply elasticity would determine the *actual* mark-down of the employer's wage below the marginal revenue product of labour. But it is possible that the employer is unable to exercize all its monopsony power i.e. the pass-through to the mark-down will be less than one.

In a second step, we will therefore relate the wage paid by the employer to the implied mark-down under monopsony to quantify the pass-through of monopsony power to wages. To capture the employer's wage or its "wage policy", we rely on the employer wage effect of a two-way fixed-effects wage decomposition *à la* Abowd, Kramarz, and Margolis (1999, AKM henceforth) that provides a suitable approximation of the West German wage structure (Card et al., 2013). The AKM decomposition splits up individual workers' wages into a permanent worker-specific and a permanent employer-specific component, where the latter can be interpreted as a pure wage premium from working for this rather than another employer corrected for worker sorting (Card et al., 2018; Hirsch and Müller, 2020).

Our approach is similar to Webber (2015) that documents a significantly negative correlation between employer-level estimates of the labour supply elasticity and worker earnings (rather than employers' "wage policy" that we use). Unlike Webber (2015) we use an instrumentation strategy that copes with attenuation originating from the noisy first-step estimate of monopsony power and thus allows us to give the estimated pass-through parameter a causal interpretation as the employer's *actual* rather than its *potential* monopsony power.

Our main finding is that the pass-through is far from zero as it would be under perfect competition. In our baseline estimate, the average pass-through is around 40%. This result remains robust in a specification with employer fixed effects that rests identification on within-employer variation in monopsony power and wages. One possible reason why pass-through may be less than 100% is unions. For a subgroup of employers in our data, we have information on whether these are bound by collective agreements and covered by workplace co-determination through works councils, which allows us to check whether the pass-through differs by industrial relations. Our finding will be that the pass-through is substantially muted where the traditional German system of industrial relations, which combines collective bargaining with works councils, is present. In contrast, the pass-through is significantly higher when any of these are absent.

We also estimate the pass-through at different quantiles of the (conditional) wage distribution. Our core finding will be that the pass-through is monotonously decreasing when moving up the distribution from about 55% at the first decile to 25% at the ninth decile. This result is in line with the notion that monopsony is most severe in low-wage labour markets and less of a problem in high-wage labour markets where organised labour tends to be strong but also individual workers may have more bargaining power.

The remainder of this paper is organised as follows. Section 2 describes in detail our approach and Section 3 our data. Section 4 presents and discusses our results, and Section 6 concludes.

2 Approach

Our approach to quantify the pass-through of monopsony power to wages involves two steps. In the first step, we obtain a measure of employers' monopsony power by estimating the wage elasticity of labour supply to the individual employer from worker data on job durations. In the second step, we relate the mark-down of the wage below the marginal revenue product of labour that would prevail under monopsony to the wage that is actually paid by the employer, that is we check the extent to which differences in monopsony power translate into different wages.

2.1 Measuring monopsony power

As a measure of an employer's monopsony power, we use the elasticity of this employer's labour supply with respect to its wage. Consider the employer pays some wage w reflecting its "wage policy". Then, in a steady state the labour supply to this employer $L(w)$ is given by

$$L(w) = R(w)/s(w) \tag{1}$$

where $R(w) > 0$ denotes the number of recruits and $0 < s(w) < 1$ the separation rate of incumbent workers (where we omit other factors affecting recruitment and separations for notational convenience). It is natural to assume that a higher wage makes it easier to recruit new and retain incumbent workers, that is $R' > 0$ and $s' < 0$, so that the employer can raise the labour supply by increasing its wage and thus possesses some discretion in wage setting.

From equation (1), the wage elasticity of labour supply to this employer ε_{Lw} is the difference of the wage elasticity of recruitment ε_{Rw} and the wage elasticity of the separation rate ε_{sw}:

$$\varepsilon_{Lw} = \varepsilon_{Rw} - \varepsilon_{sw} \tag{2}$$

Equation (2) simplifies further by observing that in many models of imperfect competition in the labour market, notably the canonical Burdett and Mortensen (1998) model, the recruitment and the separation rate elasticities are the same in absolute value. Intuitively, this equality holds because one employer's wage-related hire is another employer's wage-related quit (see Manning, 2003: 96–100, for the details). This is a neat result because it allows us to estimate the labour supply elasticity from the wage elasticity of the separation rate of incumbent workers as

$$\varepsilon_{Lw} = -2\varepsilon_{sw} \tag{3}$$

and thus circumvents the problem of how to estimate the wage elasticity of recruitment, which would require us to observe employers' recruitment pools or workers' choices among potential employers, an information that is typically absent in data.[1]

To estimate the separation rate elasticity of workers at a specific employer, we model the separation rate of worker i holding a job at employer j as an exponential model

[1] This theoretical result is further backed up empirically by some evidence that estimated recruitment elasticities are similar in magnitude to estimated separation rate elasticities (Falch, 2017; Hirsch et al, 2022).

$$s_{ij}\left[t \middle| \log w_{ij}(t), \mathbf{x}_i(t)\right] = \exp[\theta_j \log w_{ij}(t) + \mathbf{x}_i(t)'\boldsymbol{\beta}_j] \tag{4}$$

where t is job duration, $\mathbf{x}_i(t)$ is a vector of worker characteristics, all of which potentially time-varying. Note that the exponential model (4) assumes a constant baseline hazard and thus no duration dependence. This modelling decision is driven by the idea that job tenure is a bad control; including it tends to understate the separations elasticity as one effect of lower separations rate is higher job tenure. (Manning, 2003: 103; Hirsch et al., 2018).[2]

Importantly, the exponential model (4) is employer-specific and thus controls by construction for permanent employer observables and unobservables.[3] In the separation equation (4), θ_j thus represents the wage elasticity of the separation rate of workers holding jobs at employer j that, based on equation (3), gives the labour supply elasticity to this firm we are interested in as: $\varepsilon_{Lw,j} = -2\theta_j$. Note, however, that estimation of the separation equation (4) is only viable when observing enough jobs at each employer so that we will have to focus on large, not too short-lived employers in our data, a restriction we share with Webber (2015) who also focusses on large employers.

To obtain credible estimates of how workers' separation decisions vary with the wage paid by their employers, it is crucial that the included worker characteristics control for workers' alternative wage because their labour supply to their current employer depends on their wage relative to the wage they could earn when working for another employer (Manning, 2003: 101–103). At the same time, degrees of freedom are limited because we have to estimate an exponential model for each employer in our data. Hence, parsimony of covariates is imperative.

As a control for the alternative wage, we therefore add to standard worker demographics

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2 In line with this expectation, we obtain lower elasticity estimates when, in a check of robustness, estimating semi-parametric Cox models that allow for an arbitrary baseline hazard and thus permit all kinds of duration dependence. That said, controlling for job tenure in Cox models only results in a level effect for the estimated elasticities and hardly matters for the estimated pass-through parameter of monopsony power to wages. Results are available upon request.

3 Note that a robustness check shows that our results do not change when adding time-varying employer characteristics capturing workforce composition, i.e. the shares of females, non-Germans, low-skilled and high-skilled workers, and workers on full-time hours. Results are available upon request.

capturing the worker's age, sex, nationality, and education, the worker wage effect from an AKM two-way fixed-effects decomposition. In the AKM framework, the worker wage effect represents the worker's permanent skills and other factors that are rewarded equally across all employers (Card et al., 2013). It thus suits ideally as a parsimonious catch-all measure of the worker's alternative wage. Identification of θ_j thus rests on the fact that different workers among the employer's workforce earn different wages relative to their alternative wage and the accompanying variation of these workers' job separations.[4]

2.2 Measuring the pass-through of monopsony power to wages

With an estimate of the labour supply elasticity, the second step involves quantifying the pass-through of monopsony power to the wage paid by the employer. As a starting point, consider the textbook model of monopsony by Robinson (1933) where the pass-through is complete and the employer's monopsony power directly translates into its wage.

In this polar case, the profit-maximising wage w^* representing the "wage policy" adopted by an employer is:

$$w^* = \frac{\varepsilon_{Lw}}{\varepsilon_{Lw}+1} MRPL \tag{5}$$

where $MRPL$ denotes the marginal revenue product of labour and $\frac{\varepsilon_{Lw}}{\varepsilon_{Lw}+1}$ gives the mark-down of the wage below the marginal revenue product. Taking logs on both sides of equation (5), we obtain:

$$\log w^* = \log \frac{\varepsilon_{Lw}}{\varepsilon_{Lw}+1} + \log MRPL \tag{6}$$

4 Rather than controlling for the AKM worker wage effect in the separation equation (4), we could also rest identification of θ_j on the difference between a worker's current wage and this worker's alternative wage by including the difference between these two as a single variable in the model. We did so in a check of robustness, and this specification gave similar results. Results are available upon request.

Therefore, in the polar case of a complete pass-through the employer's wage is unit-elastic in the mark-down. In contrast under perfect competition, the employer is forced to pay its workers the marginal revenue product of labour and the wage is thus unrelated to the mark-down.

Hence, the elasticity of the wage paid by the employer with respect to the mark-down is a direct measure of the pass-through of monopsony power to wages, with zero corresponding to a perfectly competitive and one corresponding to a perfectly monopsonistic outcome. We estimate this pass-through parameter by regressing the wage paid by the employer on the log mark-down. As a measure of the employer's wage, or its "wage policy", we use the AKM employer wage effect. In the AKM framework, the employer wage effect gives the log wage premium enjoyed by every worker holding a job at this employer adjusted for observed and unobserved worker quality. In other words, the AKM employer wage effect represents a pure wage premium corrected for worker sorting (Card et al., 2018; Hirsch and Müller, 2020).

Since the wage mark-down is obtained from the estimates of the separation rate elasticities from the employer-specific exponential models (4), it will be subject to estimation error and a noisy measure of monopsony power. A regression of the AKM employer wage effect on the log wage mark-down will thus be subject to attenuation and biased toward zero or, in other words, biased in favour of competitive outcomes. To deal with this problem, we use an alternative measure of monopsony power that can serve as an instrument for the wage mark-down in that it has a strong correlation with the wage mark-down and is derived from a different set of statistics so that any measurement error in the two variables is unlikely to be correlated.

For this purpose, we follow Hirsch et al. (2022a) and use the share of the employer's hires from non-employment (as opposed to employment) as an instrument for the wage mark-down. As they and Manning (2003: 44–49) demonstrate, in the canonical Burdett and Mortensen (1998) model this share is a sufficient statistic for how competitive is the employer's labour market. The intuition is that the share captures direct competition in the hiring process among employers for workers, which is present when employers hire from employment but absent when they hire from non-employment. So the larger the share of hires from non-employment, the less often employer faces direct competition from other employers when hiring workers and the lower thus competition is.

Using the share of hires from non-employment as an instrument for the wage mark-down not only addresses attenuation, but it also permits identification of the causal effect running from employers' monopsony power to their wages, provided that the share is a credible exclusion restriction with no impact on wages other than affecting competition in the labour market. To ensure this, we include a rich set of controls when regressing the AKM employer wage effect on the instrumented log wage mark-down. Specifically, we control workforce composition (shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours), two-digit industry, and location (in local labour markets).[5] These controls, in particular, ensure that large shares of hires from non-employment do not simply reflect other factors than competition, such as local deprivation or sector-specific market tightness, that may influence employers' wage policies.[6]

3 Data and institutional background

Our linked employer–employee data combine two administrative German data sets for the period 1985–2018: the Integrated Employment Biographies (IEB) and the Establishment History Panel (BHP), which are both provided by the Institute for Employment Research (IAB). The information in these data is used to calculate social security contributions and thus is highly reliable and especially suited for analyses on wages and job durations.

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[5] We distinguish 204 self-contained local labour markets, based on commuting links, that come from an updated classification by Kosfeld and Werner (2012).

[6] A popular measure of labour market competitiveness in recent research has been the Herfindahl-Hirschman Index (HHI); see, for example, Azar, Marinescu and Steinbaum (2022) Marinescu, Ouss and Pape (2021) inter alia. We do not use this measure because it is computed at the level of the market (defined in some way) while we need a measure at the level of the plant.

In these data, we consider the universe of West German plants (not companies) from the manufacturing, construction, and services sectors that employed at least 100 full-time workers for at least 10 years during our observation period.[7] Our focus on large, long-lasting employers is dictated by our approach that builds on employer-specific exponential models that we estimate on an inflow sample of all jobs started with these employers during 1985–2018. This is also the reason for restricting our analysis to West German (excluding Berlin) employers. Although our data contain observations for East German workers and employers from 1992 onward, restricting analysis to the post-unification period would markedly reduce the period of observation. Further, including East German observations for the 1990s would be questionable anyway because of the long transition from a socialist planned economy to a market economy during these years.

The data on job durations (at daily frequency), job transitions, wages, and worker characteristics (age, education, sex, and nationality) stem from the IEB (for details on the IEB, see Jacobebbinghaus and Seth, 2007). The IEB comprises all wage and salary employees registered with the German social security system, in total about 80% of employment. The IEB dates back to 1975, but does not include bonus payments before 1985 which we use as the start of our sample. We restrict our analysis to full-time workers as the data lack detailed information on the number of hours worked. We drop jobs with top-coded wages (14.9% of our observations) because any imputation of wages would attenuate our estimates of the separation rate elasticities and thus bias our findings towards more monopsony power of employers. Information on workers' education is provided by employers on non-mandatory basis so is for this reason inconsistent or missing for some workers. To alleviate this problem, we follow the imputation procedure proposed by Fitzenberger et al. (2006). After applying this procedure, we have to drop just 0.8% of jobs because of missing or inconsistent information on education.

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[7] Note that we exclude the temporary work industry because the vast majority of the workers employed by temporary work agencies are working at user plants on which we lack any information.

Information on employers in the BHP also consists of data from the German social security system aggregated at the level of the plant at June 30 of a year (for details on the BHP, see Ganzer et al., 2020). It contains information on employers' workforce composition, industry, and location in local labour markets.

To these data on workers and their employers, we merge the AKM worker and employer wage effects calculated by Bellmann et al. (2020) for the IEB based on full-time workers aged 18–60. Following the methodology from Card et al. (2013), Bellmann et al. provide separate AKM effects for the five time intervals 1985–1992, 1993–1998, 1999–2004, 2005–2010, and 2011–2018; we will refer to these as AKM estimation periods.

In part of our analysis, we will further use information from the IAB Establishment Panel (for details, see Ellguth et al., 2014) that can be merged to our data for all those plants in the administrative data that took part in this survey. Starting in 1993, the IAB Establishment Panel has surveyed West German plants from all sectors that employ at least one worker subject to social security on June 30 of the survey year and is representative of the population of these plants. Crucial for our purpose, the survey asks about employers' industrial relations (IR) regime, specifically whether the employer is covered by collective bargaining and workplace co-determination.

The traditional German IR model combines collective bargaining between a union and an employers' association or a single firm and workplace co-determination through a works council (Oberfichtner and Schnabel, 2019). Collective bargaining in Germany predominantly concerns wages, but also determines job classifications, working time, and working conditions. The norms stipulated in the collective agreement are generally minimum terms that employers bound by the agreement cannot undercut but only improve upon (for further details, see Hirsch and Schnabel, 2014). At the end of our observational window in 2018, 56% of workers in West Germany worked in the 29% of plants covered by collective agreements (Ellguth and Kohaut, 2019).

Beyond collective bargaining, the second backbone of the traditional German IR model is workplace co-determination through works councils, the German counterpart of workplace unions in other countries. Works councils are mandatory but not automatic in all plants with at least five permanent workers, for setting up a works councils requires an election procedure initiated by the plant's workforce. Although works councils are generally forbidden to bargain over wages, which are settled in collective agreements with unions, they have far-reaching information, consultation and co-determination rights, in particular on so-called "social matters" that include remuneration arrangements, health and safety measures, and the regulation of working time (for further details, see Addison 2009). Unsurprisingly, there exists clear evidence that works council existence positively affects wages (e.g. Addison et al., 2010; Hirsch and Müller, 2020). At the end of our observational window in 2018, 42% of workers in West Germany worked in the 9% of plants with a works council (Ellguth and Kohaut, 2019).

As an instrument for the wage mark-down obtained from our separation rate elasticity estimates, we will use the share of the employer's hires from non-employment which requires us to distinguish employment and non-employment as labour market states. Consequently, a job may either start after a job-to-job move has taken place (that is the old job was with an employer with a different plant identifier), or following a previous spell in registered unemployment or no spell in the data at all. This latter possibility means that the worker has been unemployed without receiving benefits or was employed without being covered to by social security (e.g. as a self-employed) and is thus not included in our data. Although we cannot disaggregate this category of unknown origin in our data, information from other German data sets suggests that the vast majority of workers in this category have indeed started new jobs from non-employment (e.g. Hirsch et al., 2018).

In our merged data, we set up an inflow sample of all jobs started by workers aged 18–59 years during 1985–2018. If the job still lasts when the worker turns 60, we consider the job duration as right-censored because later job separations may be separations into retirement and since Bellmann et al. (2020) do not provide AKM effects for older workers. We also ignore separations into non-employment if the worker is recalled by the same employer within three months. Similarly, in classifying job-to-job moves we allow a gap of up to three months between two subsequent employment spells with different employers. Furthermore, in our data plant identifiers change in some cases for administrative reasons. To avoid any impact of changing identifiers on worker transitions and job durations, we consider all job durations as right-censored when identifiers change spuriously as identified in Hethey-Maier and Schmieder (2013).

4 Results

4.1 Baseline estimate of the pass-through parameter

Our estimation sample consists of 23,736,680 jobs held by 14,697,437 workers at 24,532 employers. In the first step, we fit equation (4) for the separation rate of jobs at each of these employers. These regressions provide us with estimates of individual employers' labour supply elasticity (3) and thus their potential market power.

By construction, these models control for permanent employer observables and unobservables. As worker characteristics we include dummies for education (distinguishing low-skilled, medium-skilled, and high-skilled workers), age (linearly and squared), dummies indicating female and non-German workers, the AKM worker wage effect controlling for the worker's alternative wage, and a set of dummies for the AKM estimation periods (1985–1992, 1993–1998, 1999–2004, 2005–2010, and 2011–2018).[8]

The exponential models did not converge for eleven employers, so we are left with

[8] Low-skilled workers are workers with neither a vocational nor an academic degree, medium-skilled workers those with a vocational degree, and high-skilled workers those with an academic degree.

24,521 estimates of the labour supply elasticity to the individual employer (calculated using equation (3) from the employer-specific coefficient of the log wage in the estimated separation rate equation (4) as $\varepsilon_{Lw,j} = -2\theta_j$). The distribution of the labour supply elasticities is shown in Figure 1. The average elasticity is 2.33 and the median elasticity 2.30, which is well within the range of estimates reported in Sokolova and Sorensen (2021) and also consistent with previous estimates for Germany using (almost) the same data and a similar methodology involving region-specific separation equations (Hirsch et al., 2022a).

Observe that our estimates show substantial variability, and part of this variability undoubtedly comes from estimation error. Note further that a small fraction of the estimated labour supply elasticities (5.8% of estimates) is even negative which means that the wage mark-down (5) is no longer defined. We therefore cannot make use of these estimates, and to cope with positive outliers too, we decided to symmetrically trim our data and drop both the top 6% and bottom 6% of estimates. After trimming, we are left with 21,363 elasticity estimates (with a hardly changed average elasticity of 2.30 and median elasticity of 2.33). Using equation (5) for the wage mark-down, these numbers imply that workers would receive about 70% of the marginal revenue product of labour from the typical employer in the polar case of pure monopsony, which suggests that it has substantial but not implausibly large monopsony power.

In our second step, we estimate the pass-through of monopsony power to actual wages. To that end, we regress the AKM employer log wage effect (which captures the wage paid by the employer corrected for worker sorting) on the log wage mark-down and a rich set of employer controls. As instrument for the log wage mark-down, we use the employer's share of hires from non-employment, which is an alternative measure of monopsony power that does not contain the same kind of measurement error as our mark-down estimates. The included employer characteristics control for workforce composition (shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours), two-digit industry, and location (at NUTS 3 level). Controlling for these is important for the share of hires from non-employment to serve as a credible exclusion restriction unlikely to affect employers' wage policy through other channels than labour market competition.

Note that our data include up to five AKM employer wage effects because separate AKM wage decompositions were performed for any of the AKM estimation periods (i.e. 1985–1992, 1993–1998, 1999–2004, 2005–2010, and 2011–2018). In the 2SLS regressions, we thus use 92,121 observations belonging to 21,363 employers and take account for repeated measurements for the same employer by clustering standard errors at employer level.

Table 2 reports the core results of our baseline 2SLS regression of the AKM employer wage effect on the instrumented log wage mark-down. As expected, there is a significant relationship between the two measures of monopsony power. A 10 percentage point higher share of hires from non-employment predicts a 3.5 log points larger wage mark-down, and the *F* statistic of 244.6 signifies a strong first stage. In contrast to perfect competition where wages solely reflect marginal productivity and are thus unrelated to monopsony power, we obtain a significant effect of the log wage mark-down on the AKM employer wage effect. A 10 log points narrower mark-down raises the wage significantly by 4.2 log points on average. In other words, the average pass-through of employers' monopsony power to their wages is 42%, suggesting that employers exercize some but not all of their monopsony power.

4.2 Controlling for employer fixed effects

One concern with our baseline estimate of the pass-through parameter is that, despite the rich set of employer controls for workforce composition, sector, and location, the share of an employer's hires from non-employment may affect an employers' wage policy through other channels than labour market competition, thereby casting doubt on its suitability as an exclusion restriction. Obviously, this issue would be less of a concern when resting identification on within-employer variation over time and thus controlling for all permanent employer characteristics in the 2SLS regression. Yet, doing so raises the bar higher still in terms of data requirements because we have to arrive at a time-varying measure of monopsony power first.

We therefore deviate from the analysis in the previous subsection and estimate, in the first step, separate labour supply elasticities to each employer in each of the five AKM estimation periods (i.e. in each of the time intervals 1985–1992, 1993–1998, 1999–2004, 2005–2010, and 2011–2018) in which we observe the employer. We do so by setting up separate inflow samples for each period on which we then fit the employer-specific exponential models (4). As before, this first step uses data on 23,736,680 jobs held by 14,697,437 workers at 24,532 employers. We could not estimate any of these models for two employers, so we end up with 101,942 elasticity estimates for 24,530 employers.

Figure 2 shows the distribution of these elasticity estimates which looks rather similar to the distribution we had before when estimating a pooled employer-level elasticity for the entire observation period. The median elasticity is 2.29 and thus the same to the previous specification. The mean elasticity of 2.54, though, is a bit higher, which is due to some large positive outliers. Overall, the estimates' variability is much higher in this specification, which is hardly surprising as we have to estimate a much larger number of models and individual estimates thus rely on substantially reduced numbers of jobs. In consequence, a much larger fraction of the estimates (16.5% or 16,849 estimates) are negative, meaning that the wage mark-down (3) is not defined. With symmetric trimming of the top 17% and bottom 17% estimates, we thus have to drop about one-third of the estimates. After trimming, the median elasticity is still 2.29 and the mean elasticity is 2.32, which are identical numbers compared to the baseline specification.

Regressing the AKM employer wage effect on the instrumented log wage mark-down and the same employer controls as before, which is the first specification in Table 3, we see that a 10 log points smaller wage mark-down raises the wage by 5.6 log points. This estimate of the pass-through of 56% is somewhat higher than but not far from our baseline estimate of 42%. As before, the first stage is strong and shows a significant negative relation of our two measures of monopsony power. A 10 percentage points higher share of hires from non-employment predicts a 1.8 log points wider wage mark-down, and the first-stage *F* statistic is 192.4.

Adding employer fixed effects to our 2SLS regression, we still obtain a strong first stage with an *F* statistic 71.2. A rise in the share of hires from non-employment by 10 percentage predicts a widening of the wage mark-down by 1.5 log points. Turning to the second-stage estimates, a widening of the mark-down by 10 log points leads to a drop of the employer's wage by 4.8 log points on average. Hence, we estimate an average pass-through of 48% that comes very close to our baseline estimate of 42%, when permanent employer unobservables are controlled for.

4.3 Differences in the pass-through by industrial relations

So far, we found a sizeable average pass-through and thus a significant deviation from perfect competition, which is robust to the inclusion of employer fixed effects. Still, this pass-through of around 40% means that employers are unable to fully exercise their monopsony power. One possible constraint is organised labour that we expect to increase workers' and decrease employers' influence in wage formation (see also Dobbelaere et al., 2020, and the references therein).

In a next step, we will therefore check for the subset of employers in our data that took part in the IAB Establishment Panel how industrial relations influence pass-through. In doing so, we distinguish employers where the traditional German IR model combining collective bargaining with a union and workplace co-determination through works councils is present from employers where any of these two is missing.

Since the IAB Establishment Panel just surveys a small fraction of the employers included in our administrative data and since it started in 1993, this part of our analysis is only possible on a substantially reduced data set that comprises the years 1993–2018. To maximise the number of observations available for the analysis, we return to the less noisy estimates of employers' monopsony power from Section 4.1 that pool all the AKM estimation periods when fitting the first-step employer-specific exponential models.

Out of the 21,363 employers in this sample, 5,132 employers are contained in the IAB Establishment Panel and thus enter our second-step AKM employer wage regressions. For 3,936 or about three-quarters of these employers, the traditional German IR model combining collective bargaining and workplace co-determination exists, whereas for the remaining 1,448 employers at least one of its components is missing. At first sight, these numbers, which are in the same order of magnitude as those reported by Oberfichtner and Schnabel (2019), seem to suggest that the traditional German IR model is pervasive (see Jäger, Noy and Schoefer, 2022a,b for summaries of this model). Yet, bear in mind that our sample only includes large employers where collective bargaining and workplace co-determination is more common than in medium-sized and small employers (Oberfichtner and Schnabel, 2019).

Table 4 presents the estimates of the pass-through parameter for the two subgroups of employers where the traditional German IR model is present and where it is not and for the pooled sample for comparison to our baseline estimates in the larger sample from Section 4.1. Note first that, despite the substantial drop in observations, the first stage is strong in all specifications so that 2SLS regressions are reliable. In the pooled sample, a 10 log points narrower wage mark-down yields a 2.1 log points higher wage on average so that the estimated pass-through of 21% is smaller than our baseline estimate of 42% in the larger sample.

Turning to differences by industrial relations, for employers covered by the traditional German IR model a 10 log points narrower wage mark-down raises wages by 1.5 log points on average[9]. For uncovered employers, the average rise is 4.7 log points and thus three times as

9 This might be thought inconsistent with the findings of Jäger, Schoefer, and Heining (2021) which find that giving worker representatives seats in the supervisory board of large companies, does not affect rent sharing. We are studying worker representation at plant level which may have different effects – see Dobbelaere et al (2020).

large as for covered employers. Both these estimates are significant, and they are also significantly different.[10] Hence, we obtain marked differences in the pass-through of monopsony power to wages that is just 15% where organised labour is strong but nearly 50% where it is weak. This finding lends strong support to the notion that organised labour is an important constraint on employers' monopsony power and mutes the pass-through of monopsony power to wages (this is consistent with the results for Norway reported in Dodini, Salvanes and Willen, 2022). That said, even with strong organised labour, a significant monopsonistic element in wage formation remains.

We also find that pass-through is less than 100% in the absence of collective bargaining suggesting that even non-union plants do not exercise all their monopsony power. There are a number of possible reasons for this. One is that there remains some measurement error in our estimates (e.g. because the errors in the two measures are correlated); then the IV strategy does not fully deal with attenuation bias. Or it could be that firms do not want to exercise all monopsony power because a large gap between wages and marginal products encourages workers to find ways to reduce their marginal product (see Langella and Manning, 2021).

4.4 Controlling for labour productivity and pass-through to other outcomes

In a next step, we check whether our estimate of the pass-through parameter is robust to controlling for the labour productivity, that is the $MRPL$ in equation the wage-setting equation (6) under monopsony, and investigate the pass-through of employers' monopsony power to other outcomes than wages. Specifically, we will re-estimate our baseline specification when controlling for log value added per worker, and we will further estimate the pass-through to the labour costs per worker, the value added per worker, and the labour share.

[10] We formally tested for differences in the pass-through estimates by industrial relations in fully interacted models and found that they differed significantly at the 1% level.

The balance sheet information required for this exercize is not included in our administrative data, but we merge our administrative data with Orbis data provided by Bureau van Dijk. Orbis is a commercial database that provides balance sheet information from almost 160 different sources and covers several 100 million companies at an annual basis for more than two decades. For Germany Orbis covers all firms that are subject to legal reporting obligations (because their total assets or sales exceed certain thresholds defined by law).

Since Orbis and our administrative data do not contain a common plant identifier, firm information from Orbis and individual (plant) information from the administrative data cannot be merged directly. To overcome this problem, we use a record linkage based on firm and plant names, their legal forms, addresses, numbers of employees, and main industrial affiliations for the period 2012–2016 put forward by Egger et al. (2022).[11] The matching success rate for German firms in Orbis is high in general – well above 80% for firms with more than five employees – and it increases further with firm size. Since not all employers in our administrative data are included in the Orbis data and since the crosswalk is not available for our entire observation period but only for the years 2012–2016, the following analysis only uses information on the labour costs per worker, the value added per worker, and the labour share (defined as the labour costs divided by value added) for a subset of employers from our administrative data. To ensure that our results are not driven by outliers, we only keep observations within the 1% trimmed range of these three variables. Akin to Section 4.3, we use the less noisy estimates of employers' monopsony power from Section 4.1 that pool all the AKM estimation periods when fitting the first-step employer-specific exponential models.

Table 5 investigates the robustness of our earlier conclusions to controlling for our measure of worker productivity, log value-added per worker. The first column of Table 5 reports estimates of our basic pass-through equation for the 5,930 employers (out of the 21,363

[11] See Antoni et al. (2018) and Egger et al. (2022) for details on this crosswalk.

employers in our original sample) which can be matched to the ORBIS data. The estimated pass-through rate is 29%, similar to our earlier estimates. The second column then controls for the log of value-added per worker, our measure of productivity. As expected this is positively related to the wage but the pass-through estimate is only slightly lower at 23%.

It is also possible that productivity itself responds to market power. This possibility is investigated in Table 6 where we estimate outcomes at plant-level. The first column estimates the pass-through from monopsony power to the average plant-level wage; the estimated pass-through is higher at 0.662 than we found using individual wage data. The second column changes the dependent variable to log value-added per worker. We find that firms with less market power are more productive. But the impact of labour market competitiveness is larger on wages than productivity so the labour share rises as the labour market becomes more competitive – this is shown formally in the third column of Table 6.

Overall, our results on pass-through are robust to controlling for plant-level productivity but there is some evidence productivity itself may be affected by labour market competitiveness.

4.5 Differences in the pass-through along the wage distribution

This section investigates whether the pass-through parameter varies along the wage distribution. We again rely on the pooled elasticity estimates from Section 4.1 and now run quantile IV regressions at each decile of the conditional distribution of the AKM employer wage effects. These regressions use the quantile IV estimator by Chernozhukov et al. (2015) that uses a control variable approach where, similar in spirit to 2SLS, the control variable dealing with endogeneity is estimated in a first-stage OLS regression that includes all second-stage variables and an exclusion restriction. As before, we instrument the log wage mark-down by the share of hires from non-employment, and control for workforce composition, two-digit sector, location at NUTS3 level, and the AKM estimation period. For inference, we rely on a block bootstrap at employer level with 200 replications.

Table 7 provides the second-stage coefficients of the log wage mark-down at all deciles of the conditional distribution of employer' wages. These are also plotted, along with 95% confidence bands, in Figure 3. In stark contrast to perfect competition where no pass-through is expected, we see a significant pass-through of monopsony power to wages at all deciles of the wage distribution. At the median, the pass-through is 39% and thus very close to our baseline estimate of the average pass-through of 42%.

The estimated pass-through declines monotonically from 55% at the first to 26% at the ninth decile. In other words, employers' monopsony power plays a much bigger role in low-wage than in high-wage labour markets, though it significantly affects wages along the entire distribution. This is consistent with our earlier result that the pass-through is lower when organised labour is present as employers with collective bargaining and works councils pay higher wages (e.g. Gürtzgen, 2009; Addison et al., 2010; Hirsch and Müller, 2020).

5 Conclusions

Using administrative data for West Germany comprising the years 1985–2018, this paper has estimated the pass-through of employers' monopsony power to their wages. To measure monopsony power, we estimated in a first step the wage elasticity of the labour supply to an individual employer by means of separate exponential models for workers' job durations with each employer. In a second step, we regressed the AKM employer wage effect on the estimated mark-down of wages below the marginal revenue product of labour that would prevail if employers made full use of their monopsony power. To remove attenuation bias and to allow for causal interpretation, we instrumented the wage mark-down by the share of hires from non-employment, which is an alternative measure of monopsony power capturing competition among employers in recruiting workers.

We found a significant average pass-through of employers' monopsony power to wages of about 40%, which is in stark contrast to perfect competition where monopsony power would leave the wages employers pay unaffected. This estimate of pass-through is robust to controlling for employer fixed effects and to controlling for plant-level productivity. We found lower pass-through of 15% for employers bound by collective wage agreements and co-determined by works councils compared to employers lacking any of these two backbones of traditional German industrial relations where the pass-through is about 50%. And that pass-through declines as we move up the wage distribution, from about 55% at the first decile to 25% at the ninth decile. This can potentially explain why the minimum wage is effective at raising wages in low-wage labour markets without harming employment.

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Figures

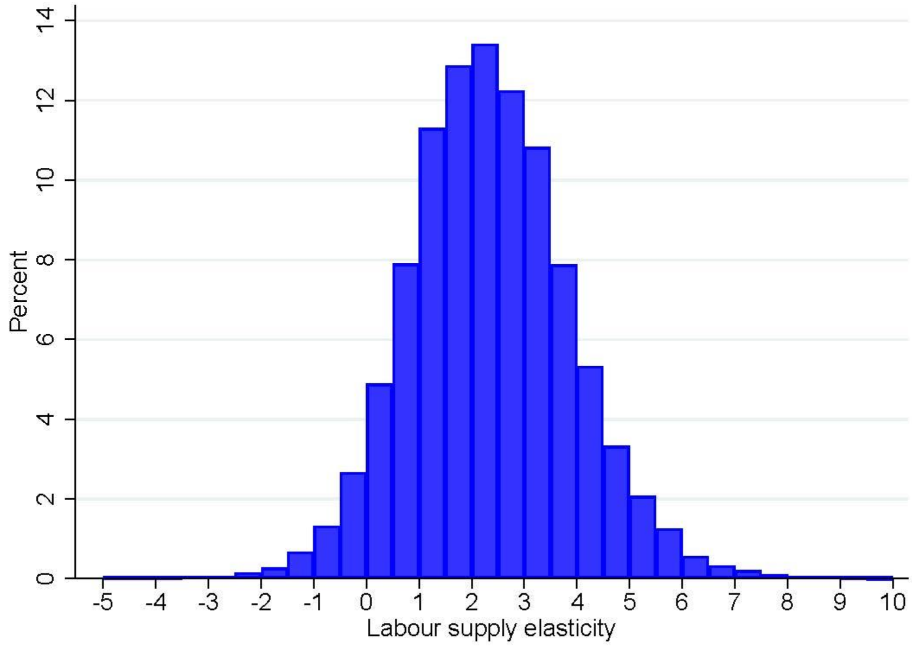

Figure 1: Histogram of the estimated labour supply elasticities to individual employers (time-constant estimates pooled over all AKM estimation periods)

Note: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020). We obtain the estimates of the labour supply elasticity to an individual employer as minus two times the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4). These exponential models control for the AKM worker wage effect; the worker's age, sex, nationality, and education; and the AKM estimation period.

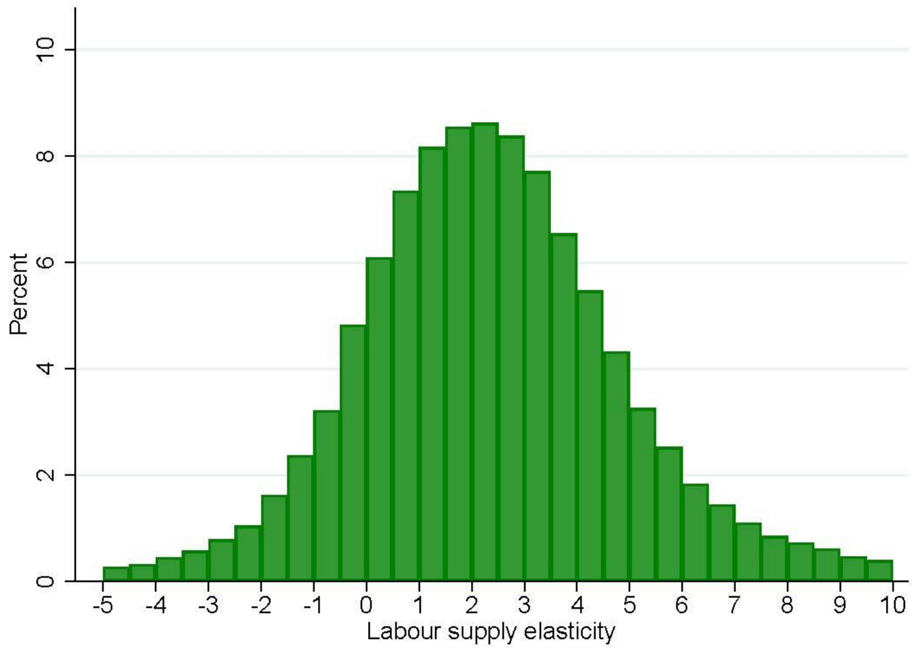

Figure 2: Histogram of the estimated labour supply elasticities to individual employers (time-varying estimates for every AKM estimation period)

Note: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020). We obtain the estimates of the labour supply elasticity to an individual employer as minus two times the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4) for every AKM estimation period. These exponential models control for the AKM worker wage effect and the worker's age, sex, nationality, and education.

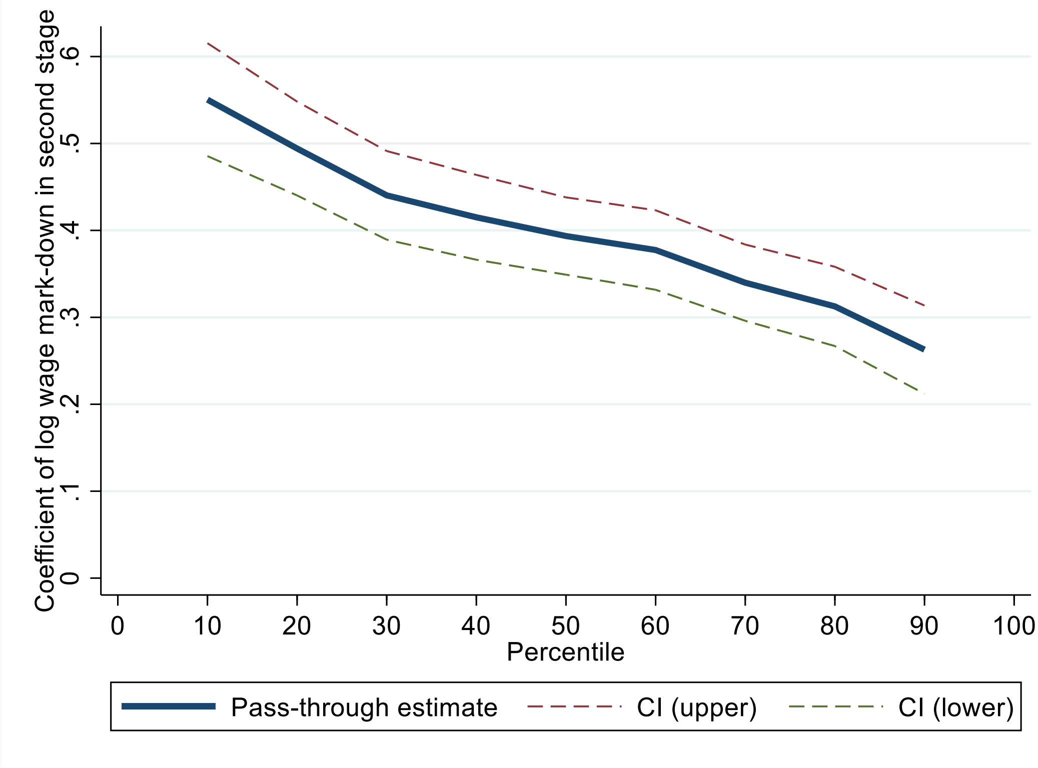

Figure 3: Estimates of the pass-through parameter along the distribution of employers' wages

Note: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020). The figure shows the second-stage coefficients of the log wage mark-down from quantile IV regressions for every decile using Chernozhukov et al.'s (2015) quantile IV estimator, along with 95% confidence bands from a block bootstrap at employer level with 200 replications. Further details are in the note to Table 5.

Tables

Table 1: Descriptive statistics (means)

	Mean	SD
Worker-level data		
Age	35.684	10.604
Male	0.693	0.461
Non-German	0.134	0.341
Low-skilled	0.157	0.364
Medium skilled	0.758	0.428
High skilled	0.085	0.279
Worker AKM effect	4.351	0.327
Observations	124,783,499	
Plant-level data		
Share female worker	0.346	0.230
Share foreign worker	0.094	0.100
Share full-time worker	0.822	0.142
Share low-skilled worker	0.168	0.116
Share medium-skilled worker	0.708	0.143
Share high-skilled worker	0.124	0.147
Labour supply elasticity	2.323	1.132
Share of hires from non-employment	0.516	0.120
Plant AKM effects	0.187	0.216
Observations	92,121	

Notes: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020).

Table 2: Baseline estimate of the pass-through parameter (based on time-constant estimates of monopsony power)

2SLS regression for the AKM employer wage effect	
Coefficient of instrumented log wage mark-down in second stage	0.419 (0.032)
Coefficient of share of hires from non-employment in first stage	–0.349 (0.022)
First-stage *F* statistic	244.6
Observations	92,121
Employers	21,363

Notes: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020). The dependent variable is the AKM employer wage effect. The key regressor is the plant's log wage mark-down, which we calculate from the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4). These exponential models control for the AKM worker wage effect; the worker's age, sex, nationality, and education; and the AKM estimation period. We instrument the employer's log wage mark-down by its share of hires from non-employment. The 2SLS regressions further control for the shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours; two-digit industry; location at NUTS 3 level; and the AKM estimation period. Standard errors clustered at employer level in parentheses.

Table 3: Estimates of the pass-through parameter based on time-varying estimates of monopsony power

2SLS regression for the AKM employer wage effect	(1)	(2)
Coefficient of instrumented log wage mark-down in second stage	0.557	0.476
	(0.045)	(0.073)
Coefficient of share of hires from non-employment in first stage	–0.175	–0.112
	(0.013)	(0.018)
First-stage *F* statistic	192.4	46.2
Employer fixed effects	No	Yes
Observations	67,281	67,281
Employers	23,416	23,416

Notes: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020). The dependent variable is the AKM employer wage effect. The key regressor is the employer's log wage mark-down, which we calculate from the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4) for each of the AKM estimation periods. These exponential models control for the AKM worker wage effect and the worker's age, sex, nationality, and education. We instrument the employer's log wage mark-down by its share of hires from non-employment. The 2SLS regressions further control for the shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours; two-digit industry; location at NUTS 3 level times AKM estimation period; and the AKM estimation period. Standard errors clustered at employer level in parentheses.

Table 4: Estimates of the pass-through parameter by industrial relations (based on time-constant estimates of monopsony power)

2SLS regression for the AKM employer wage effect	(1) All plants	(2) Traditional IR	(3) No traditional IR
Coefficient of instrumented log wage mark-down in second stage	0.214 (0.041)	0.145 (0.040)	0.466 (0.116)
Coefficient of share of hires from non-employment in first stage	–0.448 (0.067)	–0.415 (0.078)	–0.478 (0.108)
First-stage *F* statistic	45.0	28.0	19.7
Observations	8,686	6,613	2,073
Employers	5,132	3,936	1,448

Notes: IEB, BHP, IAB Establishment Panel, 1993–2018, and the AKM wage effects provided by Bellmann et al. (2020). The subgroup of employers with traditional IR are bound by a collective wage agreement and workplace co-determination through a works council, whereas for employers without traditional IR at least one of these two is absent. The dependent variable is the AKM employer wage effect. The key regressor is the employer's log wage mark-down, which we calculate from the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4). These exponential models control for the AKM worker wage effect; the worker's age, sex, nationality, and education; and the AKM estimation period. We instrument the employer's log wage mark-down by its share of hires from non-employment. The 2SLS regressions further control for the shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours; two-digit industry; location at NUTS 3 level; and the AKM estimation period. Standard errors clustered at employer level in parentheses.

Table 5: Estimates of the pass-through parameter when controlling for log value added per worker (based on time-constant estimates of monopsony power)

2SLS regression for the AKM employer wage effect	(1)	(2)
Coefficient of instrumented log wage mark-down in second stage	0.289	0.233
	(0.055)	(0.046)
Coefficient of log value added per worker in second stage		0.118
		(0.005)
Coefficient of share of hires from non-employment in first stage	–0.317	–0.320
	(0.036)	(0.036)
First-stage *F* statistic	77.9	78.1
Observations	20,565	20,565
Employers	5,930	5,930

Notes: IEB, BHP, and BVD, 2012–2016, and the AKM wage effects provided by Bellmann et al. (2020). The dependent variable is the AKM employer wage effect. The key regressor is the employer's log wage mark-down, which we calculate from the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4) for each of the AKM estimation periods. These exponential models control for the AKM worker wage effect and the worker's age, sex, nationality, and education. We instrument the employer's log wage mark-down by its share of hires from non-employment. The 2SLS regressions further control for the shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours; two-digit industry; location at NUTS 3 level; the AKM estimation period. Standard errors clustered at employer level in parentheses.

Table 6: Estimates of the pass-through parameter to other outcomes (based on time-constant estimates of monopsony power)

2SLS regression for the logarithm of …	(1) Labour costs per worker	(2) Value added per worker	(3) Labour share
Coefficient of instrumented log wage mark-down in second stage	0.662 (0.126)	0.482 (0.146)	0.180 (0.087)
Coefficient of share of hires from non-employment in first stage	–0.317 (0.036)	–0.317 (0.036)	–0.317 (0.036)
First-stage *F* statistic	77.8	77.8	77.8
Observations	20,565	20,565	20,565
Employers	5,930	5,930	5,930

Notes: IEB, BHP, BVD, 2012–2016, and the AKM wage effects provided by Bellmann et al. (2020). The dependent variables stem from BVD. The key regressor is the employer's log wage mark-down, which we calculate from the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4). These exponential models control for the AKM worker wage effect; the worker's age, sex, nationality, and education; and the AKM estimation period. We instrument the employer's log wage mark-down by its share of hires from non-employment. The 2SLS regressions further control for the shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours; two-digit industry; location at NUTS 3 level; and the AKM estimation period. Standard errors clustered at employer level in parentheses.

Table 7: Estimates of the pass-through parameter (based on time-constant estimates of monopsony power) along the wage distribution

Quantile IV regressions for the AKM employer wage effect	
Coefficient of instrumented log wage mark-down in second stage at the…	
1st decile	0.550 (0.033)
2nd decile	0.494 (0.028)
3rd decile	0.440 (0.026)
4th decile	0.415 (0.025)
5th decile	0.394 (0.023)
6th decile	0.377 (0.023)
7th decile	0.340 (0.022)
8th decile	0.313 (0.023)
9th decile	0.263 (0.026)
Observations	92,121
Employers	21,363

Notes: IEB and BHP, 1985–2018, and the AKM wage effects provided by Bellmann et al. (2020). The dependent variable is the AKM employer wage effect. The key regressor is the employer's log wage mark-down, which we calculate from the estimated coefficient of the log wage in an employer-specific exponential model of workers' job durations (4). These exponential models control for the AKM worker wage effect; the worker's age, sex, nationality, and education; and the AKM estimation period. We instrument the employer's log wage mark-down by its share of hires from non-employment. The quantile IV regressions rely on Chernozhukov et al.'s (2015) quantile IV estimator that uses a control variable approach where the control variable dealing with endogeneity is estimated in a first-stage OLS regression that includes all second-stage regressors and the instrument. Further controls are the shares of female, non-German, low-skilled and high-skilled workers, and workers on full-time hours and dummies for two-digit industry, location at NUTS 3 level, and the AKM estimation period. Standard errors in parentheses come from a block bootstrap at employer level with 200 replications.