Identifying rent-sharing using firms' energy input mix*

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Abstract: We present causal evidence on the rent-sharing elasticity of German manufacturing firms. We develop a new firm-level Bartik instrument for firm rents that combines the firms' predetermined energy input mix with national energy carrier price changes. Instrumental variable estimation yields a rent-sharing elasticity of approximately 0.20 implying that a 10 percent change in rents leads to a 2 percent change in wages. Rent-sharing induced by energy price variation is asymmetric and driven by energy price increases, such that, on average, workers do not benefit from energy price reductions but are harmed by price increases. Reduced-form evidence shows that a 10 percent increase in firm-level energy prices depresses firm-level wage growth by 0.34 percent.

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1 Introduction

Studies documenting that equally productive workers are paid different wages by different employers are legion (Abowd et al. 1999, Card et al. 2013, Card et al. 2016, Song et al. 2019, Card et al. 2018). Persistent firm wage differentials indicate imperfect labor markets where employers and workers possess market power in the wage formation process and employment rents accrue to workers and employers. Recent decades witnessed a strong revival of studies acknowledging labor market imperfections when analyzing the role of firms and worker-firm bargaining in shaping wage inequality (e.g., Card et al. 2018, Manning 2011). Worker-firm rent-sharing (or more generally, the pass-through from productivity to wages) offers a potentially important explanation for between-firm wage differences of workers with similar skills and within comparable occupations.[1]

Many early studies estimating sector-level relationships between performance and wages (e.g., Christofides and Oswald 1992, Blanchflower et al. 1996) may suffer from general equilibrium effects. In particular, variation in sector-level performance directly impacts sector-level market wages and, thus, workers' outside options.[2] Outside options are part of the right-hand side of the standard bargaining solution and are typically unobserved in empirical studies. Hence, unobserved variation in outside options may directly bias estimates of the rent-sharing elasticity. The same problem occurs in firm-level studies using aggregate (usually sector- or region-level) variation in firm performance. Idiosyncratic variation in individual firms' performance, however, leaves workers' outside options unaffected. In a recent survey of the rent-sharing literature, Card et al. (2018) conclude that there is a particularly strong need for causal estimates of rent-sharing elasticities based on *firm-level* productivity shocks. Our article provides exactly such evidence.

Of course, endogeneity concerns are also crucial at the firm level. These include, for instance, unobserved differences in worker quality, affecting wages and firm performance, or reversed causality when efficiency wages drive firm performance. Finding exogenous idiosyncratic

[1] As we will discuss, our evidence is more consistent with a bargaining/rent-sharing model in the spirit of Abowd and Lemieux (1993) and subsequent work (see the survey by Card et al. 2018), and we thus place our paper predominantly in that literature strand. However, the monopsony literature has also studied the pass-through of productivity to wages in the context of an upward-sloping labor supply curve (e.g., Lamadon et al. 2022).

[2] Outside options are often modeled in terms of unemployment benefits instead of outside wages. However, Jaeger et al. (2020) show that unemployment benefits are unlikely to define workers' outside options.

firm-level productivity shocks to address those concerns is notoriously difficult, and thus credible studies are scarce. Moreover, in a standard production function setting with a constant output elasticity of labor and without frictions in labor and product markets, even firm-level demand shocks will cause adjustments in employment that leave rents per worker and wages unaffected (Abowd and Lemieux 1993). In this and similar settings, instruments for revenue function shifters will be weak. Thus, the challenge is to find an instrument that acts in an environment with sufficient imperfections in product and labor markets.

Two seminal studies are Van Reenen (1996) and Kline et al. (2019). These authors use major innovations and patents as instruments for firm performance and find considerable rent-sharing elasticities of approximately 0.5.[3] These innovation-based studies are prototypical for a setting with product market imperfections and workers possessing firm-specific human capital. By focusing on innovations, both papers zoom in on a selected subsample of the economy and concentrate on a specific process generating rents. Recently, Garin and Silverio (2024) and Acemoglu et al. (2022) use firm-specific variation in foreign demand and estimate wage pass-through elasticities of approximately 0.14-0.19. By the nature of their research design, these studies focus on exporters only.

Our contribution is to complement the sparse causal evidence on rent-sharing by estimating causal rent-sharing elasticities in a novel and important setting across a broad sample of firms. Specifically, we estimate rent-sharing elasticities in the context of energy price changes and study the relatively energy-intensive German manufacturing sector from 2003 to 2017. Understanding the economic effects of changing energy prices is increasingly important, particularly in the context of the green transition, which makes our research setting highly relevant for studying rent-sharing. In addition, one major advantage of utilizing energy cost shocks as an instrument, instead of using a revenue shifter (such as innovation outcomes), is that these cost shocks are arguably mostly unobserved by workers *during our sample period* and are thus less likely to have a direct effect on worker effort.[4] Additionally, innovation success and timing are at least partly under the firms' control, whereas for instance, the oil price is not.

Using exogenous firm-level cost shocks instead of revenue shifter to instrument firm rents

[3] See Section 2 for further details on the literature.

[4] Clearly, this may differ for the more recent radical increases in energy costs, such as those caused by the war between Russia and Ukraine.

constitutes a novel research design. Therefore, our analysis offers new insights into how cost shocks influence rent-sharing and examines whether their magnitude aligns with existing findings based on revenue shifter. The impact of cost shocks on firm rents will be particularly strong if firms face adjustment frictions and cannot fully pass on higher costs to consumer prices. The German manufacturing sector is an ideal case study: It accounts for a substantial share in economic output and is characterized by a significant rise in energy prices, high exposure to international competition, which restricts firms' ability to pass on increased costs to higher prices, and the presence of strong labor market imperfections.[5] Our setting leverages these strong rigidities in the German labor market, making employment adjustments in response to input cost shocks costly and thus incomplete. Recent findings of a decline in firm responsiveness to productivity shocks observed in US data (Decker et al. 2020) imply that such a setting might also be increasingly relevant in the US. In fact, Kline et al. (2019) conclude that US workers in innovating firms, who are the most difficult to replace, capture the largest fraction of rents.

To identify rent-sharing elasticities, we develop a new Bartik instrument for firms' value-added labor productivity from firms' energy consumption by energy carriers (in kilowatt-hours). The instrument combines the (predetermined) firm-level energy carrier mix with economy-wide energy price changes for each energy carrier. A price change of a certain energy carrier affects firms more the more intensively they use this carrier.[6] In our setting, identification comes from energy shares as opposed to price changes, and, thus, our identifying assumption is that, conditional on covariates, initial firm-level energy carrier shares are exogenous to future wage changes. Our firm-level perspective addresses the issue of correlated shocks influencing market-level wages (i.e., outside options) that plagues analyses using productivity shocks at the sector or regional level. To address any biases from unobserved heterogeneity in firm characteristics, we estimate our model in first differences. Our new instrument passes recently developed plausibility checks for Bartik instruments (Goldsmith-Pinkham et al. 2020, henceforth GPSS) and can be used in many other research contexts, including studies on the determinants of technology adoption, R&D investments, or exporting.

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5 Section 3 will provide a detailed account of the institutional background.

6 Other studies use changes in observed energy costs as an instrument for firm performance (e.g., Blanchflower et al. 1996, Arai and Heyman 2009). In contrast, we do not rely on total energy costs, as these are at least partly under the control of the firm and thus endogenous.

First-stage F values of approximately 30 indicate that our instrument is a strong predictor of changes in firms' labor productivity. The IV estimator yields a rent-sharing elasticity of approximately 0.20, implying that a 10-percent increase in firms' labor productivity increases wages by 2 percent. Leveraging firm-level information on workforce composition and limited additional linked employer-employee data combined with a difference-in-differences setting, we find no effects of the instrument on workforce composition, hours worked per worker, or worker tenure. As increased worker turnover would have reduced average tenure, this provides us with suggestive evidence that our rent-sharing estimates are primarily based on repeated observations of the same worker-firm matches. Our rent-sharing elasticity is close to the trade-based estimates of Garin and Silverio (2024) and Acemoglu et al. (2022) but substantially below the innovation-based estimates of Van Reenen (1996) and Kline et al. (2019) that mark the upper end of the distribution of rent-sharing elasticities in the literature (see Figure A.1).

Finally, one distinctive feature of our setting is that we utilize positive and negative shocks, allowing us to analyze asymmetric rent-sharing. Disentangling whether the results differ for positive or negative shocks is key, for instance, because the green transition will likely lead to rising energy prices in the future. If our results were driven by energy price reductions, only, they might be less informative in that context. However, we find that our results are exclusively generated by increases in energy prices, implying that our rent-sharing parameter is driven by loss-sharing, i.e. reduced wage growth following an erosion of existing rents. Hence, energy price reductions do not benefit workers, but price increases reduce their wage growth. Specifically, reduced-form evidence indicates that a 10 percent increase in firm-level energy prices reduces firm-level wage growth by 0.34 percent.

The remainder of our study is structured as follows: Section 2 discusses the related literature. Section 3 summarizes key aspects of the German institutional background. Section 4 presents our firm-level data on productivity, wages, and energy use. Section 5 discusses theory, our empirical strategy, and the novel Bartik instrument. Section 6 presents the results, scrutinizes the plausibility of our new instrument, and discusses effect heterogeneity. Section 7 concludes the paper.

2 Related Literature

A robust finding in the literature is that wages vary with firm performance. However, depending on specifications and data sets, quantitative estimates of rent-sharing elasticities vary widely. A concern that may partly explain the wide range of estimates is that, despite the considerable progress in the literature, many studies still lack plausibly exogenous variation in firm performance to estimate rent-sharing elasticities. In the following, we thus restrict our review to studies that identify the rent-sharing elasticity using arguably exogenous productivity shocks varying at the firm level as opposed to studies using aggregate variation. Hence, we do not consider studies employing variation in firm performance shared by many firms in a certain labor market (e.g., same sector or region), as such variation directly affects workers' outside options.[7] We also do not discuss structural approaches, as such studies typically do not employ exogenous variation in firm performance.[8]

Figure A.1 summarizes estimates based on firm-level variation in rents and shows that the range of estimates is substantial, varying from 0.03 to 0.58.[9] Notably, two of the most convincing studies, i.e., Van Reenen (1996) and Kline et al. (2019), report by far the highest rent-sharing estimates. Recently, Acemoglu et al. (2022) and Garin and Silverio (2024) provide evidence using arguably exogenous firm-level variation in foreign demand to instrument for the rents of Danish and Portuguese exporters, respectively. These studies find pass-through elasticities of approximately 0.14-0.19.[10]

How can we rationalize the considerable variation in estimates occurring even within this narrow group of studies using credible research designs? One potential context-specific explanation highlighting the rent-generating process in innovating firms (i.e., Van Reenen 1996 and Kline et al. 2019) is that innovating firms may implicitly or explicitly condition

[7] Card et al. (2018) review sector-level studies. Berger et al. (2022) and Fuest et al. (2018) are recent examples of studies using regional variation in business taxes. Cho and Krueger (2022) analyze oil extraction companies in the US and use world-market price changes in crude oil to instrument for firm rents and thereby resort to a type of variation that is common to all oil extraction companies in the market.

[8] For instance, Lamadon et al. (2022) estimate the relationship between productivity and wages in an event-study setting but without resorting to exogenous variation in productivity in their main specification (they use arguably exogenous variation for the construction sector subsample). Similarly, Friedrich et al. (2019) define residuals from firm-level productivity regressions as productivity shocks without employing a causal identification strategy.

[9] Considering that value-added-based estimates yield rent-sharing elasticities that are on average twice as high as quasi-rent-based approaches (Card et al. (2018)), the quasi-rent-based elasticity of 0.29 in Van Reenen (1996) enters Figure A.1 as a 0.58 elasticity.

[10] We report the results for firms with managers without business degrees from Acemoglu et al. (2022).

substantial wage hikes for their core workforce on successful innovations. Hence, estimates are influenced by strong wage increases for innovators in innovating firms (Kline et al. 2019).[11]

The results based on trade shocks hitting exporting firms (Acemoglu et al. 2022 and Garin and Silverio 2024) might be specific to firms selecting themselves into exporting, which pertains to relatively productive firms that invested a fixed entry cost, creating rents that are subject to bargaining. Moreover, Acemoglu et al. (2022) add another layer by showing that managers with a business degree do not share gains from exporting with their workers. We therefore argue that the substantial range of estimates in the literature points to a context-specificity of rent-sharing elasticities, i.e., rent-sharing elasticities vary depending on which type of firms and which rent-generating processes are analyzed. Consequently, additional causal evidence using different and relevant rent-changing processes is key to understanding this context-specificity. Unlike prior studies that focus on innovation and export demand settings and examine revenue shifters, our setting includes a broad population of manufacturing firms and leverages a cost shifter driven by changing energy prices. This enables us to examine rent-sharing (and loss-sharing) within the highly relevant context of changing energy prices.

There are further studies using instrumental variable approaches. Carlsson et al. (2014) use physical total factor productivity (TFPQ) as an instrument for labor productivity but acknowledge that investments in firm TFPQ may be a consequence of rising wages (for a case in point, see Nguyen 2019 on the productivity effects of the minimum wage). Arai and Heyman (2009) use multiple instruments that are, however, all choice variables for the firm. For instance, total energy costs (as opposed to our shift-share instrument for energy usage) reflect firms' input decisions and are therefore endogenous. Other instruments, such as foreign sales or pricing, are also at least partly controlled by the firm. Some studies rely on (dynamic) panel estimators in which lagged differences and levels of firm performance provide valid technical instruments if the panel model is dynamically complete. However, these studies either have to rely on sector-level wage information (Hildreth and Oswald 1997), demanding assumptions to distinguish between permanent and transitory shocks plus the notoriously critical timing assumptions for using productivity lags as instruments (Guiso

[11] Highlighting the role of implicit contracts, Howell and Brown (2023) show that ex-ante financially constraint firms pass through windfall cash gains from R&D grants primarily to their incumbent workers.

et al. 2005), or specification tests directly reject the panel model's dynamic completeness (Gürtzgen 2009).[12]

Finally, a few studies discuss asymmetric sharing, meaning that wages respond differently to positive versus negative productivity shocks (i.e., gain-sharing and loss-sharing). Acemoglu et al. (2022) find rent-sharing for positive shocks only. This is in line with Arai and Heyman (2009) and Cho and Krueger (2022). On the other hand, Garin and Silverio (2018) report symmetric rent-sharing in their Portuguese data. Asymmetries in rent-sharing likely depend on the context and can be decisive for a study's implications. For instance, in our setting, responses of wages to rising energy prices are much more relevant than responses to falling energy prices. This is because decarbonizing our economies will most likely lead to an era of rising energy prices.

3 Institutional Background

To motivate our rent-sharing setting, it is key to understand the German institutional background. Most importantly, German labor legislation and industrial relations (see Jaeger et al. 2022 for a recent overview) impose limits on employer discretion in layoffs and wage setting. Firstly, all establishments with at least ten regularly employed workers (at least five workers before 2004) are subject to the German Employment Protection Act (*Kündigungsschutzgesetz*). This legislation is relatively rigid, for instance requiring justifications for dismissals and the application of social criteria in determining layoffs. Secondly, works councils reinforce employment protection. Workers in German plants with at least five employees have the legal right to elect a works council. Despite the legal mandate, works councils are not universal, as eligible employees in many smaller establishments often choose against forming one.[13] Once established, works councils hold various legal rights under the Works Constitution Act (*Betriebsverfassungsgesetz*), including information,

[12] Saez et al. (2019) analyze a nation-wide payroll tax cut for young workers in Sweden employing a difference-in-differences setting. Without reporting a rent-sharing elasticity, they find that young workers' wages did not rise disproportionally and that the resulting decline in firms' total wage costs is shared with all workers in the firm. The variation used in their setting is similar to ours in the sense that input costs change differently across firms because of different initial conditions. Similarly, Carbonnier et al. (2022) analyze tax credits in France and also find evidence for rent-sharing. A specific feature of their study is that the treatment intensity varies with firms' initial wage level and, thus, pay strategies.

[13] Mueller and Stegmaier (2020) discuss employer resistance to works council formation and the (social) costs for workers that discourage the establishment of works councils.

consultation, and codetermination rights. Importantly, the works council must be consulted before layoffs, and if it opposes a dismissal and the employee brings the dismissal to court, the dismissal is suspended until a labor court decision. To avoid prolonged court cases, this right often leads to substantial severance payments in practice, further increasing termination costs beyond Germany's already restrictive employment protection legislation.

In addition to works councils, sector-level collective wage bargaining agreements form the second pillar of the German industrial relations system. These agreements are negotiated between employer associations and sector unions, setting minimum standards for wages and certain working conditions. Employers who are members of an employer association are typically bound by these agreements, although they can opt out by withdrawing from the association. For employers bound by collective agreements, all workers in the plant are covered, regardless of individual union membership. Finally, employers can negotiate individual firm-level agreements with unions (*Haustarifvertrag*), which sometimes include a temporary ban on dismissals.

Even if covered by these institutions, individual employers have discretion regarding wages and employment. Many employers bound by a collective bargaining agreement pay more than the agreed minimum wage, often referred to as a 'wage cushion' (Jung and Schnabel 2011), which gives employers the opportunity to reduce wages or forego collective wage increases in the face of adverse conditions. Furthermore, collective agreements may include 'opening clauses' (see Ellguth *et al.* 2014a) that permit individual firms to deviate from collectively agreed standards while remaining covered. These clauses are typically invoked during periods of economic hardship. When these clauses are applied, works councils represent workers and often make wage concessions to avoid layoffs, sometimes also referred to as 'pacts for employment'. Of course, collective agreements can also be terminated. For example, Volkswagen used to be covered by firm-level bargaining agreements with the metalworkers' union, which Volkswagen terminated after 30 years in 2024 in response to adverse economic conditions partly driven by rising energy prices and the transition to electric vehicles. Given the rigid employment protection law (EPL), many German firms avoid hiring regular workers during expansions and seek further flexibility by renting workers from temporary work agencies (Hirsch and Mueller 2012). In the face of adverse conditions, these temporary agency workers can be sent back to their agency and the firm's

core workforce can be retained (Hirsch 2016) - a pattern that we will also document in our context.

The sample of firms used in our study, i.e. manufacturing firms with at least 20 employees, faces particularly strong EPL, as these firms exceed certain size thresholds and are more frequently covered by works councils.[14] Furthermore, collective wage bargaining between employers and unions is more prevalent in these firms than in the overall private sector economy. As our data (see Section 4) do not contain information on organized labor, we use representative data from the IAB establishment panel (see Ellguth *et al.* 2014b for details on the data) to calculate coverage by collective bargaining agreements and works councils. According to this data, 39 percent of manufacturing plants with at least 20 employees are covered by either a sector-level or firm-level collective bargaining agreement, and 36 percent have a works council. In the overall population of German private sector plants, only about one-quarter are covered by a collective bargaining agreement, and less than 10 percent of eligible plants have a works council.

4 Data

We use annual administrative panel data on German manufacturing plants from 2003 to 2017.[15] The data are supplied by the statistical offices of Germany and consist of two complementary data sets. One is a firm-level data set called the "cost structure survey", which contains information on firms' outputs and inputs, including information on sales, employment in full-time equivalents (FTEs), investment, labor costs, and intermediate input expenditures for a representative and periodically rotating 40% sample of all German manufacturing firms with more than 19 employees (firm data, henceforth).[16] We use this data set to calculate firm-level average wages (total deflated wage bill divided by FTE), deflated value-added and other variables used in our regression analysis. While FTE values are measured as of September 30th, all other variables correspond to the full calendar year.

[14] Consequently, larger manufacturing firms rely more on temporary agency workers than the average firm (Mueller 2014).

[15] Data source: RDC of the Federal Statistical Office and Statistical Offices of the Federal States, DOI: 10.21242/42221.2018.00.01.1.1.0, 10.21242/62111.2014.00.03.1.1.0, and 10.21242/43531.2020.00.03.1.1.0.

[16] We follow Bräuer et al. (2023) in calculating capital stocks based on available information on firm depreciation and investment using a perpetual inventory method where the first capital stock is derived from observed capital depreciation and assumptions on the depreciation rate.

Importantly, the labor input of temporary agency workers is part of intermediate inputs and the costs for these workers are intermediate input expenditures. Hence, firm-level average wages and employment pertain to the firm's core workforce.

The other data set is a census of all manufacturing *plants* with more than 19 employees containing detailed information on plants' total energy consumption in terms of quantities (energy data, henceforth). The data report plants' energy consumption by multiple energy source categories. For our analysis, we focus on the five main categories, electricity, heavy fuel oil, light fuel oil, natural gas, and hard coal, as official price data are available only for these carriers. These five main carriers account for more than 95% of the average firm's energy consumption. We retain firms in the sample that use additional energy carriers. Our results hold when reducing the sample to firms that exclusively use the five main energy carriers. Our energy data report quantity information by energy source in kilowatt-hours (kWh), allowing us to aggregate across the different carriers and to calculate the shares of each energy carrier in total energy consumption.

We merge national energy price data from the Federal Ministry of Economic Affairs and Climate Action (BMWK) with our data. From these data, we calculate energy prices per kWh using conversion tables from the BMWK.[17] Note that our energy data refer to the plant level, whereas our firm data contain firm-level information. We combine both data sets using a link between the unique plant- and firm-level identifiers provided by the statistical offices of Germany. We focus on single-plant manufacturing firms, which account for 90% of all manufacturing firms in our data.[18]

We clean the data by excluding the top and bottom one percent in revenue over production inputs and wages, value-added over revenue, and total consumed kWh over capital for each year and two-digit industry. We further exclude recycling industry firms from our analysis because these firms generate additional energy from sources other than those reported in our energy data (i.e., recycling). Similarly, we exclude manufacturers of coke and refined petroleum products because energy price changes also directly impact their output prices.

[17] The price data can be accessed via the webpage of the BMWK. We use the update of 05.03.2021. Prices for electricity and gas are provided in euros per kWh. For heavy fuel oil, light fuel oil, and coal, prices are given per ton, hectoliter, and coal units, respectively.

[18] We exclude multi-plant firms because they may operate plants outside the manufacturing sector, whereas our energy data exclusively covers manufacturing plants. Including multi-plant firms could lead to inaccurately combining firm-wide output and input measures with energy consumption data limited to manufacturing plants, thereby compromising the accuracy of our analysis.

We present and discuss further summary statistics of the data in Section 6.1.

5 Empirical Strategy

5.1 The Bargaining Model

Rent-sharing is often motivated by bargaining models, in which firms and workers bargain over a joint surplus (e.g., Abowd and Lemieux 1993). To identify the rent-sharing elasticity, we follow existing work and derive a structural relationship between wages and value-added per employee from a bargaining model. Value-added equals sales ($P_{it}Q_{it}$) minus energy expenditures ($P^E_{it}E_{it}$) and other intermediate inputs ($P^S_{it}S_{it}$), such as raw materials or intermediate services: $VA_{it} = P_{it}Q_{it} - P^E_{it}E_{it} - P^S_{it}S_{it}$. We consider that risk-neutral workers bargain collectively with firms. Firms' objective is to maximize short-run profits, $\Pi_{it} = VA_{it} - W_{it}N_{it}$, with W_{it} and N_{it} denoting wages and labor, whereas workers bargain over wages and employment to maximize the objective function $(W_{it} - W_{ot})N_{it}$, where W_{ot} denotes workers' outside option. We can write the bargaining equation as:

$$max[\phi log(W_{it} - W_{ot})N_{it}) + (1 - \phi)log(\Pi_{it})], \tag{1}$$

where $0 < \phi < 1$ is an exogenously defined Nash-bargaining parameter (i.e., workers' relative bargaining power). We are agnostic as to whether bargaining power originates from, for instance, hiring and training costs on the firm's side or search costs on the worker's side. For bargaining to exist, it must hold that $W_{it} > W_{ot}$. The first-order condition for W_{it} implies (see Appendix C.1.1):

$$W_{it} = (1 - \phi)W_{ot} + \phi\frac{VA_{it}}{N_{it}} = W_{ot} + \phi\frac{QR_{it}}{N_{it}}, \tag{2}$$

where $QR_{it} = VA_{it} - W_{ot}N_{it}$ denotes (short-run) quasi-rents. Hence, the wage that workers earn equals the sum of their outside option and their share of value-added or quasi-rents per worker. This standard rent-sharing equation motivates our structural equation that we bring to the data:

$$log(W_{it}) = \beta_0 + \gamma log(\frac{VA_{it}}{N_{it}}) + \boldsymbol{X}_{it}\beta + \eta_{it}, \tag{3}$$

where the vector $\boldsymbol{X}_{it}$ includes control variables we discuss further below: firms' capital intensity to control for differences in rents due differences in capital intensity, energy consumption over labor, and various fixed effects. η_{it} is an error term that consists of a white noise error term (ϵ_{it}) and, eventually, a common component reflecting unobserved outside options of the firm's workforce (we explain how we account for outside options below). Note that in Eq. (3), we specify the empirical model in terms of value-added rather than quasi-rents. Finally, we estimate the model in logs. This monotonic transformation produces a coefficient different from a specification in levels but is consistent with the existing literature (e.g., Card et al. 2018) and ensures the comparability of our results with prior studies.

5.2 Rent-Sharing vs. Monopsony

Before we discuss the empirical specification and identification strategy, it is helpful to connect our rent-sharing analysis to the recent literature on monopsony. Estimating Eq. (3) yields a coefficient, γ, which captures the pass-through of productivity changes to wages. We interpret this elasticity as a rent-sharing parameter in context of our bargaining model, consistent with prior work (see the survey by Card et al. 2018). Recently, however, this pass-through has also been studied in monopsony settings (e.g., Lamadon et al. 2022). In monopsony models, a nonzero coefficient γ in Eq. (3) can emerge as firms adjust along an upward-sloping labor supply curve in response to productivity shocks.

For understanding the wage effects of productivity (or energy price) shocks, or explaining wage variation more generally, distinguishing between monopsony and rent-sharing is not strictly necessary—both frameworks attribute wage differences (or changes) to differences (or changes) in productivity. However, the distinction carries significant implications for aspects beyond the scope of this study, such as efficiency, unemployment, and policy design.[19]

We believe that interpreting our findings through the lens of rent-sharing is more appropriate in our context for three reasons. First, the structure of collective bargaining agreements

[19] The definition of rent-sharing in our standard bargaining model differs from that of Lamadon et al. (2022), who define worker rents as the utility premium a worker gains in her current job compared to an alternative job. Furthermore, in the monopsony model by Lamadon et al. (2022), rents for infra-marginal workers vary with productivity shocks, as firms adjust wages for all workers when moving along the labor supply curve. In contrast to Lamadon et al. (2022), in our standard bargaining model, wages exceed the marginal revenue product of labor even for the marginal worker (if there are rents). By using the first-order condition for labor in our model, it can be demonstrated that the outside option equals the marginal product of labor. Eq. (2) shows that wages exceed the outside option.

between unions and employer associations and the role of firm-level works councils provide a natural framework for rent-sharing dynamics between firms and their core workforce (see Section 3). Second, our empirical results align more closely with the predictions of a bargaining model. Specifically, we find that firms respond to energy price shocks (which predominantly manifest as price increases) by adjusting wages without changing employment (they adjust, however temporary agency workers, which are part of intermediates). This behavior is consistent with a rent-sharing framework, where workers bargain with firms over wages and employment, and where workers accept lower wage growth in exchange for job security (see Section 3). Wage pass-through with constant employment is inconsistent with a monopsony interpretation as, in a monopsony model, wages change because firms adjust labor inputs by moving along their upward-sloping labor supply curve. Finally, we find that firm-level labor shares increase with markups in our data (Table C.1). Appendix C.1.2 formally shows that this relationship cannot be be explained by a standard monopsony model and instead requires a rent-sharing mechanisms, which further supports our rent-sharing interpretation.[20]

5.3 Identification

We estimate Eq. (3) in first differences ($\triangle x_{it} = x_{it} - x_{it-1}$) and include various fixed effects to control for unobserved heterogeneity, including workers' outside options. Estimating the model in first differences ensures that level-differences in wages pertaining to time-invariant firm characteristics, such as location or sector are taken out of the error term.

As German workers tend to be geographically immobile,[21] we assume that changes in workers' outside options have a strong regional component that we capture by including *region* $\times$ *year* fixed effects. The year fixed effects also absorb any macroeconomic effects on wage changes. We further include industry fixed effects to capture industry wage trends[22] and even add firm fixed effects controlling for firm-specific wage trends. As mentioned above, capital intensity enters the wage equation with its contemporaneous value, and the

[20] The theoretical analysis follows Mertens and Mottironi (2023).

[21] E.g., Fackler and Rippe (2017) show that less than 4 percent of workers move outside a 40 km radius around their home over a five-year time span.

[22] Replacing sector fixed effects with *region* $\times$ *industry* fixed effects to control for *region* $\times$ *industry*-specific wage trends yields similar results. In a robustness check (Table B.4), we also test different combinations of fixed effects

first difference transformation therefore includes the contemporaneous first difference of capital intensity.[23] Finally, as energy price changes hit firms differently depending on their energy intensity, we control for firms' predetermined energy intensity (kWh per FTE). The equation we estimate is:

$$\triangle log(W_{it}) = \beta_0 + \gamma \triangle log(\frac{VA_{it}}{N_{it}}) + \boldsymbol{X}_{it}\beta + \epsilon_{it}. \tag{4}$$

Wages and labor productivity are both defined in terms of FTE. Defining wages in terms of FTE instead of the number of workers accounts for variations in hours worked. Any measurement error in FTE would introduce a spurious correlation between value-added per FTE and wages in OLS regressions but will be addressed by our IV regressor (see below). The vector $\boldsymbol{X}_{it}$ includes the remaining control variables (capital and energy intensity and industry and region-year fixed effects).

The coefficient of interest, γ, measures workers' relative bargaining power, i.e., the rent-sharing elasticity. OLS estimates of γ will most likely be biased, for instance, due to reverse causality (e.g., efficiency wages, Katz 1986) or simultaneity (e.g., firm amenities or management practices, Bender et al. 2018). To address endogeneity, we employ an instrumental variables approach. We define a firm-level Bartik instrument $(\triangle EI_{it})$ as the weighted sum of time shifts of the logarithm of national energy carrier prices (in Euro/kWh, $\triangle pe_{st}$), where the weights are the firm-level shares (e_{is0}) of each energy source $s \in S = \{electricity, naturalgas, lightoil, heavyoil, hardcoal\}$ in firms' energy consumption (in kWh). We fix the shares at their initial value (i.e., when firms are first observed in the data) to guarantee that adjustments in the energy mix do not impact our results.[24] Formally, the Bartik instrument is:

$$\triangle EI_{it} = \sum_{s=1}^{S} \triangle pe_{st} e_{is0}. \tag{5}$$

To have a valid instrument, two conditions must hold. First, $\triangle EI_{it}$ has to be a relevant instrument for labor productivity. Our productivity measure captures value-added, i.e., sales minus intermediate inputs, per FTE. Energy costs are intermediate inputs and should thus be negatively correlated with labor productivity. As we discuss in the next section, the first-stage

[23] Including capital intensity alongside with value-added per worker resembles the rent-sharing specification in Card et al. (2014). We replicate our main results using lagged differences of capital intensity and find very similar results.

[24] We also run a version using previous-year weights and obtain nearly identical results.

coefficient on the instrument is highly statistically significant and has the expected sign.[25]

The second condition is that the instrument is strictly exogenous conditional on covariates; that is, the instrument affects wages only through value-added per FTE. Estimating the model in first differences controls for unobserved permanent heterogeneity between firms. One particularly important aspect of the German energy market is that fees of gas and electricity networks vary regionally and over time due to differences in legislation, network coverage, investments, age, and quality of the regional gas and electricity network. We address this by including region $\times$ year fixed effects to ensure that we only compare firms operating in the same region-year cell.[26]

Despite their popularity, an in-depth analysis of Bartik instruments (shift-share instruments) and their identifying assumptions has been undertaken only recently by Borusyak et al. (2022) and GPSS. Whereas Borusyak et al. (2022) focus on a setting where many shocks are as good as randomly assigned, GPSS consider situations where initial shares are exogenous to the change in the dependent variable. Both papers propose different tests for Bartik IV's, and it is therefore important to clarify whether the identification comes from shares or shocks. The 'shocks' setting in Borusyak et al. (2022) requires a large number of randomly shocked energy carriers and rests on the asymptotic properties of the distribution of these shocks. With just five carriers, we are clearly not in the 'shocks' setting.[27] In the formulation of GPSS, our setting is best described by its identification coming from energy shares as opposed to price changes.[28] That is, differences in initial firm-level energy carrier shares create a differential exposure of firms to economy-wide price changes. Therefore, our identifying assumption is that, conditional on covariates, initial energy carrier shares are exogenous to

[25] Although larger German firms tend to use derivatives to hedge against price volatility in commodity markets, the share of firms doing so is small. The Deutsches Aktieninstitut (2012) uses the survey by Bodnar and Gebhardt (1999) and shows that only 7% of firms with less than 100 million euros in revenue use derivatives to hedge against raw materials and commodity price volatility. One-third of firms with more than 100 million euros in revenue hedge against this risk, which supports earlier results of Bodnar and Gebhardt (1999). If relevant at all, hedging should work against finding a strong first stage in our IV regression.

[26] Electricity price differences may also depend on the amount of electricity used, e.g., because of tax benefits for electricity-intensive firms (German Renewable Energy Sources Act, EEG). Note that our instrument would not be directly affected by these price differences, as we use changes in log prices instead of price levels. Recap that we also control for firmsâ^{TM} predetermined total energy intensity.

[27] Although we observe too few shocks to base identification on them, we view nationwide energy carrier price changes as exogenous to individual firms' wage formation.

[28] GPSS frame their study within the canonical Bartik setting, where locations are regional entities (e.g., commuting zones), have different industry shares, and are hit by an aggregate shock affecting regions differently because of their differing industry composition. We have establishments instead of regional entities and energy carrier shares instead of industries. Our energy price shocks affect establishments differently because of their different energy mix.

wage *changes*, such that wage *changes* are only affected by the instrument via its impact on productivity *changes*.[29] GPSS propose a series of diagnostics on the validity of the instrument that we run after presenting our baseline results.

Generally, however, implicit contracts or unobserved changes in workplace amenities pose potential threats to analyses of rent-sharing. We mentioned earlier that revenue shifters like innovation success might influence wages directly due to implicit contracts conditioning wage increases on innovation success. However, it is highly implausible that implicit (or explicit) contracts would link wage changes to energy price fluctuations. Additionally, energy price changes are unlikely to affect workplace amenities and, through this channel, wages.

Another potential concern is that the effect of our Bartik IV on wages may depend on the production function. Under a Cobb-Douglas production function, an energy price shock is akin to a total cost shock. In contrast, in a CES production function, wage responses also depend on the elasticity of factor substitution. However, in our framework, where we estimate a rent-sharing elasticity, this is not an issue. In this setting, the substitution elasticity influences the extent to which rents (i.e., value-added per FTE or quasi-rents) respond to energy price shocks. Since value-added per FTE is the endogenous variable that we instrument, factor substitution impacts only our first stage and does not introduce endogeneity concerns for the second stage of our IV regressions.[30]

6 Empirical Results

This section starts by presenting basic descriptive evidence on the firm data and energy use in Section 6.1. Section 6.2 presents our main results, followed by a discussion of associated adjustment mechanisms in Section 6.3. In Section 6.4, we discuss further potential identification threats and conduct a battery of tests recently proposed by GPSS to scrutinize the Bartik instrument. Section 6.5 provides an extensive heterogeneity analysis. Finally,

[29] Firms may anticipate price changes. Note, however, that anticipation effects pose a threat to identification only if they are systematically related to future wage changes. We argue that technological preconditions rooted in firms' idiosyncratic production processes coupled with uncertainty about future energy prices impose narrow limits on the firms' capability to adjust their short-run energy mix to future price changes. Section 6.1 shows that firms indeed rarely adjust their energy carrier mix. By fixing the energy shares to the first year in which we observe the firm, we minimize any potential impact of anticipation effects on our results.

[30] For example, if firms can easily substitute energy with other intermediates, we would expect a weaker relationship between rents (value-added per FTE) and energy price shocks.

Section 6.6 concludes by presenting results on asymmetric rent-sharing.

6.1 Descriptive Results on Energy Use

Table 1 presents summary statistics for our sample with almost 97,000 firm-year observations. We observe not only substantial variation in wages and productivity but, importantly, also considerable heterogeneity in the amount of energy use and the composition of energy carriers. Firms at the 10th percentile of the energy consumption per worker distribution use just approximately 5,650 kWh per FTE, whereas this number is more than 20 times larger at the 90th percentile. To illustrate firm-level heterogeneity in the energy mix, we consider the example of electricity. Firms at the 10th percentile of the electricity share distribution cover only approximately 20 percent of their total energy consumption with electricity, whereas firms at the 90th percentile almost exclusively use electricity (89 percent). Hence, changes in electricity prices impact firms very differently. Also on average, energy expenditures represent an important cost component in our sample of manufacturing firm. The average ratio of energy expenditures to primary input costs (labor and capital) is 0.06, increasing to 0.14 for firms in the 90th percentile.

Figure 1 illustrates that energy prices increased between 2003 and 2017. This trend is further reflected in the average firm-specific price increase, as measured by the Bartik IV, which indicates a positive annual growth rate of 3.1 percent at the firm level (Table 1). This is important for interpreting our results, because rent-sharing will be driven by *loss-sharing* in our context (we will provide evidence on asymmetric sharing).

If firms could immediately adapt their energy consumption mix to changes in relative prices, our instrument would be weak (yet, we will find a strong first stage). The left panel of Figure 1 shows the log price development (normalized to one in 2003) for our main energy carriers.[31] Overall, energy prices increased substantially over our observation period, and there is significant heterogeneity in the timing of price changes across carriers. While the electricity price rose steadily, prices for light oil and gas increased until 2008, decreased through the Great Recession, then rose rapidly again until 2011, and started to fall afterward. Overall, the electricity price increased the most over the full observation period.

[31] For the sake of clarity, we dropped the quantitatively unimportant carriers heavy oil and coal from Figure 1.

Table 1: Descriptive Statistics

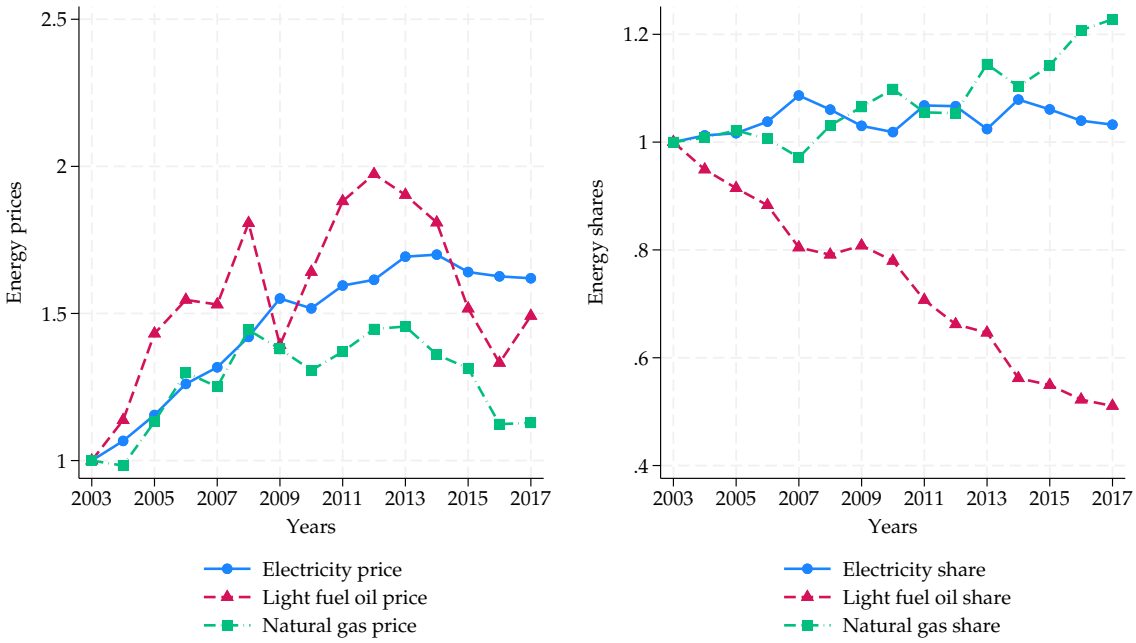
	Mean	SD	P10	P25	P50	P75	P90
Log(Wage bill per FTE)	10.51	0.320	10.08	10.31	10.54	10.74	10.90
Log(value-added per FTE)	10.85	0.495	10.26	10.54	10.84	11.15	11.47
Log(FTE)	4.425	0.944	3.303	3.689	4.290	5.017	5.756
Number of employees	148.3	226.4	30	43	77	157	326
Log(Capital stock per FTE)	11.14	0.886	10.03	10.59	11.17	11.72	12.29
Markup	1.12	0.29	0.83	0.91	1.05	1.24	1.49
kwh (in 1,000) per FTE	77.23	469.2	5.651	9.981	18.94	43.73	113.8
Share of energy source							
in total kWh used							
Electricity	0.513	0.250	0.203	0.314	0.481	0.702	0.893
Natural gas	0.292	0.288	0.000	0.000	0.229	0.546	0.716
Light fuel oil	0.136	0.238	0.000	0.000	0.000	0.179	0.572
Heavy fuel oil	0.001	0.030	0.000	0.000	0.000	0.000	0.000
Hard coal	0.001	0.021	0.000	0.000	0.000	0.000	0.000
Bartik IV (previous year)	0.032	0.084	-0.059	-0.021	0.029	0.089	0.135
Bartik IV (fixed year)	0.031	0.088	-0.059	-0.022	0.030	0.092	0.136
$\triangle$Log(Wage bill per FTE)	0.003	0.125	-0.128	-0.052	0.005	0.059	0.130
Electricity producer (D)	0.085	0.279	0.000	0.000	0.000	0.000	0.000
Electricity receiver (D)	0.035	0.183	0.000	0.000	0.000	0.000	0.000
Electricity supplier (D)	0.062	0.242	0.000	0.000	0.000	0.000	0.000
Coal user (D)	0.005	0.072	0.000	0.000	0.000	0.000	0.000
$\geq$ 10 GWh of electricity (D)	0.086	0.280	0.000	0.000	0.000	0.000	0.000
Export status (D)	0.813	0.390	0.000	1.000	1.000	1.000	1.000
R & D	0.326	0.469	0.000	0.000	0.000	1.000	1.000
kWh in total (in millions)	17.4	185	0.25	0.54	1.5	5.5	18.8
Energy expenditures / value-added	0.075	0.233	0.011	0.020	0.039	0.080	0.158
Energy expenditures / labor and capital costs	0.061	0.070	0.011	0.019	0.037	0.074	0.136
N			96,397				

Notes. AFiD Panel, 2003–2017, single-plant firms. 96,397 plant-year observations, 22,513 single-plant firms. (D) indicates dummy variables. We approximate capital costs as 8% of the capital stock.

The right panel of Figure 1 shows corresponding energy carrier shares in firm-level energy use. Gas consumption became more important, which can be rationalized by its relatively moderate price increase. Note that we observe a strong reduction in the importance of light oil without any recovery after its sharp price decline and that electricity maintained its relative importance, despite having experienced the strongest price increase. This does not support the notion that firms can flexibly adjust (or substitute) their energy inputs in response to changes in relative prices. Therefore, our tentative conclusion from this aggregate evidence is that firm-level responses to relative price changes are incomplete.

We further scrutinize this conclusion using firm-level data and analyze the relative importance of the between-firm versus within-firm variation in firm-level energy carrier shares. If firms frequently adjust their energy mix, the within-firm variation will be large.

Figure 1: Development of main energy carrier prices and shares relative to 2003



Notes. AFiD Panel 2003–2017 and Federal Ministry for Economic Affairs and Energy. Log price changes (left panel) and changes in average firm-level energy carrier shares in total energy consumption in kWh (right panel). Year 2003 is normalized to unity. Nominal prices. Prices were converted to kWh per Euro using conversion tables from the Federal Ministry for Economic Affairs and Energy.

If, in turn, firms' energy mix is rather stable, e.g., because technological necessities prevent firms from substituting among different energy carriers, the between-firm component will dominate. Table 2 shows that for each carrier, the between-firm standard deviation is almost as large as the overall standard deviation. For instance, the standard deviation in the firm-level share of electricity is 0.250, whereas the between-firm standard deviation is 0.246. Hence, firms are very different in their energy carrier mix (between-firm) but do not strongly change their energy carrier composition over time (within-firm).

6.2 Baseline Regression Results

Table 3 documents our main results where we ran all regressions in first differences and fixed the energy carrier weights of the Bartik instrument to the first year of observation for each firm. Column 1 starts with reduced-form estimates that project logged wages (in first differences) on the Bartik instrument. To the best of our knowledge, this result provides the first causal evidence of the effect of firm-level energy price changes on wages. An increase in energy prices by 10 percent translates into wage losses of approximately 0.23 percent. The

Table 2: Decomposition of the standard deviation of firm-level energy carrier shares

	Electricity share	Light oil share	Natural gas share	Heavy oil share	Hard coal share
	(1)	(2)	(3)	(4)	(5)
Overall	0.250	0.238	0.288	0.030	0.036
Between-firm	0.246	0.235	0.281	0.023	0.029
Within-firm	0.082	0.071	0.084	0.013	0.011
Observations: 96,397. Firms: 22,513.					
Average number of years a firm is observed: 4.28 years.					

Notes. AFiD Panel, 2003–2017, single-plant firms. The table shows the overall standard deviation ("overall"), the standard deviation of firm-level averages ("between-firm"), and the standard deviation of within-firm deviations from firm-level averages ("within-firm"). Exact formulas are given in the STATA manual for the *xtsum* command.

coefficient is precisely estimated with a t-statistic of approximately 2.5.

Column 2 presents the first-stage IV results. We obtain a reassuringly high first-stage F-statistic of approximately 30, and the instrument enters the first-stage estimation with the expected negative sign. Columns 3 and 4 report OLS and IV rent-sharing regressions as specified in Eq. (4). OLS yields a rent-sharing elasticity of 0.14, indicating that a 10-percent increase in labor productivity is associated with approximately 1.4 percent higher wages. This is somewhat higher than the estimates in Jaeger et al. (2021), who report an OLS-based elasticity of 0.084 in German social security data. Our IV estimator yields an estimated coefficient of 0.21 that is statistically significant at the 1 percent level.[32] Our estimates are closer to the upper end of the value-added-based estimates surveyed in Card et al. (2018) and Figure A.1 and very close to the trade-based studies by Acemoglu et al. (2022) and Garin and Silverio (2024). The key takeaway from Table 3 is that rising energy prices depress wages and that energy price-induced changes in firm rents yield a fairly substantial and precisely estimated rent-sharing elasticity.[33]

[32] Recently, Lee et al. (2021) argued that in single-instrument IV settings, second-stage t testing needs to be corrected. Lee et al. (2021, Table 3a) display correction factors for the second-stage standard errors such that the usual critical values for t tests can be used. The correction factor depends on the first-stage F-statistic. In our case, this factor is approximately 1.2 at the 5 percent significance level, yielding corrected standard errors of approximately 0.080 for the IV specification. As corrected t-ratios remain above 1.96, we conclude that our rent-sharing elasticity is statistically significantly different from zero at the 5 percent level. Moreover, Adao et al. 2019 propose an adjustment procedure for standard errors for Bartik IVs. We do not apply this procedure, as it was derived for a Bartik setting where identification comes from shocks instead of shares.

[33] We replicate Table 3 using lagged differences in capital intensity as control in Table B.1. Despite the inevitable reduction in sample size, findings are very similar. The rent-sharing elasticity reduces to 0.16, which is within the 95% confidence interval of our baseline estimate. The first stage on labor productivity remains large and highly statistically significant.

Table 3: Main results - reduced form, first stage, and rent-sharing regressions

	Reduced form	First stage IV	OLS rent-sharing	IV rent-sharing
	(1)	(2)	(3)	(4)
Bartik instrument	-0.023***	-0.113***		
	(0.008)	(0.020)		
Value-added per FTE			0.144***	0.205***
			(0.003)	(0.067)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	–	fixed
R-squared	0.213	0.146	0.304	0.288
1^{st} stage F-Stat of instrument	–	–	–	31.54
Firm-year observations	96,397	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513	22,513

Notes. AFiD Panel, 2003–2017, single-plant firms. Column 1 reports reduced form results from an OLS regression of logged wages (deflated wage bill per FTE) on our Bartik instrument. Column 2 reports results from an OLS regression of logged labor productivity on our Bartik instrument, which corresponds to the first stage regression of column 4. Columns 3 and 4 report OLS and IV results from our rent-sharing regressions that project logged wages (deflated wage bill per FTE) on logged labor productivity. In column 4, we instrument logged labor productivity with our Bartik instrument. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table 4: Adjustment mechanisms

	Log value-added	Log Sales	Log Intermediates	Log FTE	Temp. worker cost over labor costs	Log Energy (kWh)	Log Markups
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bartik instrument	-0.113***	-0.089***	-0.080***	-0.000	-0.010***	-0.029*	-0.017**
	(0.020)	(0.014)	(0.017)	(0.005)	(0.002)	(0.017)	(0.008)
Industry fixed effects	yes	yes	yes	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes	yes	yes	yes
R-squared	0.169	0.242	0.177	0.530	0.089	0.105	0.073
Firm-year observations	96,397	96,397	96,397	96,397	96,397	96,397	96,397
Number of firms	22,513	22,513	22,513	22,513	22,513	22,513	22,513

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reports results from regressing various firm outcomes on our firm-level energy price shock (our Bartik instrument). Markups are estimated based on firms' intermediate input decision using the approach of De Loecker and Warzynski (2012), assuming a Cobb-Douglas production function, and using a cost share approach. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

6.3 Adjustment Mechanisms

Our results reveal a strong negative first-stage relationship between energy price changes and value-added per FTE. This section investigates the underlying mechanisms that account for the strong connection between energy price changes and labor productivity. Theoretically, the impact of energy price shocks on labor productivity depends on firms' production function and their ability to adjust along various dimensions. For instance, in a gross output production function, the effect of higher energy prices on labor productivity is shaped by the substitution elasticity between inputs and by firms' capacity to modify input quantities and pass on increased input costs to higher output prices. In an extreme scenario where firms can perfectly adjust inputs and fully pass on input price increases to output prices, we should not observe any effect of higher energy prices on labor productivity. The relevance of adjustment frictions in shaping rent-sharing has also been emphasized in recent research by Garin and Silverio (2024). Given the notable rigidity of the German labor markets (cf. Section 3), the positive association between labor productivity and energy price shocks is therefore not surprising.

To shed light on the specific adjustment processes within firms, we present a series of reduced-form regressions in Table 4, regressing various firm outcomes on our instrument. In Column 1, we observe a substantial negative effect of energy prices on value-added, with a 10% increase in energy prices leading to a 1.1% reduction in value-added. Columns 2 and 3 demonstrate that higher energy prices significantly decrease both sales and intermediate inputs. Conversely, the coefficient on employment (FTE) in Column 4 is indistinguishable from zero, suggesting that firms face limitations in downward employment adjustment.[34] As previously discussed, the zero-employment effect aligns with a bargaining model in which workers collectively negotiate wages and employment and prioritize job security at the expense of lower wage growth.[35]

Instead of reducing their core workforce, firms respond to rising energy prices by

[34] We provide suggestive evidence that firms did also not respond in terms of workforce compositions in section 6.4.2. Note, however, that the rigidity of labor as shown in Table 4 is a desirable feature as it facilitates the identification of the rent-sharing elasticity through repeated observations of the same workforce within firms.

[35] We do not interpret our findings through the lens of a monopsony model. However, it is worth noting that within a monopsony framework with a Cobb-Douglas production function, the zero-employment effects would imply an extremely inelastic labor supply curve. Under a CES production function, the response of labor to energy prices is less informative on the labor supply elasticity due to substitution effects.

slightly reducing their expenditures on temporary agency workers (which are part of their intermediate input costs) relative to their labor expenditures (Column 5). Temporary agency work offers greater flexibility compared to regular employment, and firms take advantage of this flexibility when scaling down in response to higher energy prices. This firm behavior has also been documented for the German manufacturing sector as a response to the 'Great financial crises' (Mueller 2014). More recently, De Leon et al. (2024) reported similar findings for the U.S., demonstrating that plants respond to productivity shocks not by adjusting their payroll employment but by changing their use of temporary or leased workers.

Column 6 reveals that firms reduce their energy consumption in response to higher energy prices, with a 10% increase in prices leading to a 0.29% decrease in energy consumption. Finally, Column 7 demonstrates that firms experience a decline in markups in response to higher energy prices.[36] This suggests that firms are unable to fully pass on the increased costs to consumers, which decreases their rents. However, the estimated impact on markups is relatively small. Specifically, a 10% increase in energy prices leads to a 0.17% reduction in markups, with average markups being 1.12 (Table 1).

In summary, our findings indicate a significant negative impact of higher energy prices on firms' output. The adverse effect on labor productivity can be attributed to the relatively inflexible nature of labor inputs. Instead, firms slightly reduce their more adaptable temporary agency worker input. Additionally, firms' product market rents decline through a reduction in markups in response to higher energy prices. This reflects the inability of firms to fully pass on higher energy costs to higher product prices.

6.4 Checks for Identification

6.4.1 Scrutinizing the Bartik Instrument

As discussed above, our IV strategy may be invalid if firms anticipate energy price changes. To solve this problem, we reported results based on fixed energy shares when constructing the Bartik instrument. Comparing those results with a specification that uses previous years'

[36] Markups are estimated based on firms' intermediate input decision using the approach of De Loecker and Warzynski (2012): $\mu_{it} = \theta_{it}^{M} \frac{z_{it} M_{it}}{P_{it} Q_{it}}$, where μ_{it}, $\theta_{it}^{M_{it}}$, $z_{it} M_{it}$, and $P_{it} Q_{it}$ are the markup, intermediate input output elasticity, intermediate input expenditures, and sales, respectively. We assume a Cobb-Douglas production function and estimate the output elasticity using a cost share approach.

shares sheds light on whether there are any sizeable anticipation effects. Appendix Table B.2 provides the corresponding results. Neither our first-stage IV results, nor our second-stage IV, nor our reduced-form estimates show any substantial differences between these two specifications. Therefore, we do not find any evidence for anticipation effects, which is in line with the limited within-firm variability in the energy carrier mix discussed above.

Bartik instruments combine individual instruments with a specific weight matrix, making the Bartik estimator a black box in the sense that it is not obvious which instruments drive the results. Our Bartik instrument is the sum of products of firm-level energy carrier shares and national price shifts for five energy carriers. High-weight instruments have a strong impact on the estimation outcome, and thus, GPSS propose that researchers identify and discuss these instruments in particular. Based on Rotemberg (1983), they show how to decompose the Bartik estimator into a weighted combination of just-identified IV estimators. The resulting Rotemberg weights attached to these just-identified estimators are informative about the importance of the individual instruments, i.e., the specific energy carrier, for the overall Bartik estimate.

Following GPSS, we present graphical evidence on the Rotemberg weights (Figure B.1), in which the x-axis is the first-stage F-statistic and the y-axis is the second-stage estimate associated with each just-identified IV regression. Circles represent positive weights, and triangles represent negative weights. The size of the Rotemberg weights is reflected by the size of the circles and triangles. Finally, the dashed horizontal line depicts the point estimate based on the combined Bartik instrument (our baseline regression). Figure B.1 shows that electricity is by far the most important instrument. Reassuringly, the point estimate from the just-identified regression based on the electricity share is closely resembling the overall Bartik estimate. Whereas light and heavy oil are also relatively close to the overall estimate, natural gas and hard coal yield somewhat counterintuitive results but only have very small Rotemberg weights compared to electricity and will therefore not impact the results. In the following, we therefore keep all energy carriers for transparency and discuss identifying assumptions with a focus on electricity shares.

First, GPSS propose testing whether initial-period shares predict initial-period firm characteristics, as finding strong correlates with the high-weight carrier helps in considering potential confounders. In Appendix Table B.3, we closely follow GPSS and regress initial-

period energy carrier shares on a set of initial-period firm characteristics separately for all five energy carriers. The results for electricity show statistically significant but economically very small coefficients for productivity, capital intensity, and employment. For example, the coefficient on value-added per FTE means that firms with a 10-percent higher productivity have a 0.1-percentage-point lower electricity share in energy consumption, which is very small when evaluated at the firm-level mean of 51 percent for the electricity share (see Table 1). We conclude that there are no economically significant correlations between firm characteristics and initial energy carrier shares.

As stated previously, the identifying assumption is that, conditional on covariates, initial energy carrier shares are uncorrelated with wage changes. For settings with a pretreatment period, GPSS propose checking pretrends for the instruments with the highest Rotemberg weight. As we do not have a pretreatment period (i.e., a period with constant energy prices), we cannot directly apply this test. However, we can go one step further by including firm fixed effects in our first-difference IV specification. Firm fixed effects control for unobserved firm-specific wage trends that may be correlated with our instrument. Adding firm fixed effects reduces the sample size. The rent-sharing coefficient is 0.273 in this specification (Appendix Table B.4). This is above the rent-sharing elasticity of our baseline estimate but still within its 95 percent confidence interval. Appendix Table B.4 additionally shows that our main results are robust to alternative multi-dimensional fixed-effects specifications.

Based on Kolesar et al. (2015), GPSS propose comparing results from maximum likelihood estimators with those of two-stage least squares (2SLS) estimators. They argue that obtaining similar results further increases confidence in the identifying assumptions. Appendix Table B.5 provides results from the proposed limited information maximum likelihood (LIML) estimator (Column 3) and the 2SLS estimator (Column 2).[37] Instead of the combined Bartik IV, the instruments enter individually in this case. Reassuringly, the point estimates are almost identical and closely in line with our baseline 2SLS estimates (Column 1). We therefore find no evidence for misspecification. Having multiple instruments also enables us to perform overidentification tests. Test statistics in Appendix Table B.5 again do not suggest misspecification (p-values > 0.2). Finally, in Appendix Table B.6, we report the first-stage

[37] GPSS additionally recommend using bias-corrected 2SLS and HFUL estimators. Both estimators have difficulty dealing with the high-dimensional fixed effects structure we apply. However, according to GPSS, having at least one of the maximum likelihood estimators (here LIML) yielding similar results to 2SLS provides considerable assistance in testing for misspecification.

coefficients of the overidentified 2SLS model. The coefficients of all energy carriers have the expected negative sign. The main carriers (electricity, natural gas, light oil) are highly significant, with t-values between 3.6 and 5.1.

Having conducted a battery of plausibility tests for Bartik instruments, we are confident that our Bartik instrument works well. In sum, we find i) no evidence for anticipation effects, ii) that the most commonly used energy carrier, electricity, has the highest Rotemberg weight, enters the first stage with the expected sign, and individually yields a rent-sharing elasticity that closely mirrors that of the combined instrument, iii) that our results are robust to controlling for firm-specific wage trends, and iv) no evidence for misspecification because the LIML estimator perfectly matches our 2SLS results and overidentification tests do not reject the null hypothesis.

6.4.2 Testing for Changes in Workforce Composition

Another challenge to identification is unobserved changes in workforce composition in reaction to energy price shocks. We argued that the strict German employment protection legislation makes labor adjustments costly, implying that changes in the composition of the workforce in response to energy price changes are unlikely. Moreover, we always use FTE instead of headcounts, which mostly accounts for potential changes in firms' average working time. To nevertheless test for workforce adjustments, we first use information on workforce characteristics from our main data which report firms' share of female employers, part time workers, and working owners in the total workforce. Appendix Table B.7 reports reduced form results from regressing these variables on our Bartik instrument. We find that there are no statistically significant adjustment in these workforce characteristics in response to energy price shocks.

To further test for unobserved adjustments in hours worked and workforce composition, we additionally resort to the Structure of Earnings Survey (SES), which is a linked-employer-employee data set provided by the statistical offices. The SES contains plant-worker-level information on hours worked and further worker characteristics, including tenure. This survey is conducted every four years starting in 2006 and contains information on approximately 60,000 randomly drawn plants.[38]

[38] The sample is representative of the population of German employees and contains plants of all size classes

Unfortunately merging the SES plants to our main data, consisting of the merge of the cost structure survey and the energy data, yields only a few 100 matches which is insufficient for using the SES in our main analysis. The match quality becomes even worse when following firms over multiple years in the SES. Recall that our main data come from a rotating survey that is drawn anew every four years. Because the dates of drawing the SES and our main data set are not synchronized, the overlap between the two data sets is extremely small. Ultimately, this data limitation reflects the trade-off between leveraging rich data to construct exogenous firm-level energy cost shocks for Germany's entire manufacturing sector and using large-scale linked employer-employee data to better capture workforce characteristics. Nevertheless, we exploit the SES as much as possible and use the small sample of plants reporting in multiple survey years to provide additional tests on within-plant adjustments in workforce composition, hours worked, and tenure in response to energy cost shocks over the four-year windows of the SES.

To use the employer-employee data in the SES, we combine our energy data, i.e., the census of all manufacturing plants with more than 19 employees, with the SES and focus on plants that report at least twice in the SES. This amounts to 1,400 plants and 2,000 plant-year observations (some plants report in all three years, while some plants only do so in two years), for which we observe 160,000 worker-plant matches. Table B.8 reports summary statistics for our SES sample.[39] We regress four-year changes in average tenure, average hours per worker, the share of workers in complex tasks and the share of workers having a university degree on 4-year differences of our energy shock (Bartik instrument).[40] The average energy shock over these four-year windows is 0.13, which almost exactly equals four times the Bartik instrument of our baseline regression (see Table 1). As in our baseline regression, we control for industry and region $\times$ year fixed effects.

By focusing on incumbent workers, some researchers attempt to control for unobserved

from all regions and industries in Germany. The SES is stratified according to size class, industry, and region and does not follow workers over time.

[39] Note that compared to our main sample, the SES contains much larger firms. Unfortunately, it is impossible to combine our energy carrier data with richer linked-employer-employee data.

[40] The Statistical Office classifies the complexity of tasks into five categories: (1) leading personnel with supervision tasks and specific knowledge typically acquired through a university degree; (2) workers in complex and diverse tasks that require completed vocational training and several years of experience; (3) difficult tasks that require completed vocational training and only limited or no experience; (4) mainly simple tasks that do not require completed vocational training but require skills that can be learned within two years; and (5) exclusively simple tasks that do not require completed vocational training and for which the required skills can be learned within three months.

worker quality. However, focusing on incumbent workers yields a potentially selected sample, such that the issue of churning on unobserved worker quality cannot be convincingly addressed by simply examining stayers without modeling the decision to stay or move. In any case, our employer-employee data do not have a panel dimension at the worker level. Fortunately, however, they contain workers' plant tenure, allowing us to test for increased churning: if energy cost hikes lead to increased worker churning, average plant-level tenure should decline. Appendix Table B.9 reports the results from the employer-employee SES data. Reassuringly, we cannot find any evidence for adjustments in tenure. Moreover, working time and skill composition are also unaffected by energy shocks. Table B.9 also reports results for hourly wages, showing negative but insignificant effects. This is consistent with the longer-run effects in our main sample presented in Section 6.5.[41]

Overall, we find no evidence that energy shocks led to adjustments in workforce composition or working time based on our main data and limited evidence from the supplementary SES data. While we note that our SES data results cover a small sample and should thus be interpreted with some caution, the insignificant results for tenure suggest no increase in worker churning, as higher churning would reduce average tenure. Together with our analysis of adjustment mechanisms, we conclude that the composition of a firm's core workforce (excluding temporary agency workers) remained largely stable in response to an energy price shock. This also suggests that our main results are unlikely to obscure any significant reshuffling of workers.

6.5 Heterogeneity and Additional Analyses

This section runs a series of heterogeneity analysis and additional robustness tests to study rent-sharing and the response of firms to energy shocks in different contexts and for different samples.

[41] We expect that longer-run adjustment in wages can rebound after 4-years, such that workers only experience an uncompensated short-run loss in wages. Such rebound effects are less likely with respect to workforce compositions or average tenure.

6.5.1 Rent-Sharing and Firm Size

Recently, it has been argued that large firms tend to share rents to a lesser extent with their workers (e.g., Mertens 2023, Wong 2021). The IV estimates in Table 5 support this view: we find IV estimates of 0.26 for small firms and 0.17 for larger firms. Hence, rent-sharing elasticities appear to be approximately 50 percent larger in small firms. However, while the difference in rent-sharing elasticities is highly statistically significant in our OLS regressions, large standard errors in our IV regression, particularly for the small firm sample, render the IV-coefficient difference statistically insignificant.

To scrutinize whether the rent-sharing elasticity is decreasing over the full firm size distribution, we run our main OLS and IV regressions on various samples where we manipulate firm size by excluding an increasing fraction of large firms.[42] Table A.1 (OLS) and Table A.2 (IV) show that rent-sharing elasticities indeed almost monotonically decrease with firm size (we note again that differences are not statistically significant for IV-regressions).

Table 5: Rent-sharing, OLS and 2SLS regressions, small and large firms

	Small firms		Large firms	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Value-added per FTE	0.164***	0.263**	0.110***	0.173**
	(0.005)	(0.132)	(0.005)	(0.070)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Reference year of instrument	–	fixed	–	fixed
R-squared	0.345	0.311	0.317	0.295
1^{st} stage F-Stat of instrument	–	9.523	–	18.63
Firm-year observations	57,436	57,436	38,118	38,118
Number of firms	16,330	16,330	7,896	7,896

Notes. AFiD Panel, 2003–2017, single-plant firms. Columns 1 and 2 report OLS and IV regressions of our baseline model for small firms. Columns 3 and 4 report OLS and IV regressions of our baseline model for large firms. We define small (large) firms as firms having less then (at least) 100 employees. Wages are defined as (deflated) wage bill per FTE. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

There are several explanations for differences in rent-sharing elasticities between large and small firms. Larger firms account for a large share of the labor market and can exploit their dominance to drive down wages, also reducing the pass-through from profits to wages

[42] Our sample is not sufficiently large to estimate separate regressions for fine-grained firm size categories.

(Azar et al. 2020, Gouin-Bonenfant 2022). Furthermore, workers in large, high-paying firms might favor nonmonetary work amenities over higher wages conditional on receiving high wages (Lamadon et al. 2022). This reduces the incentives for workers to bargain for higher wages in high-paying (large) firms. Also consistent with our results, Howell and Brown (2023) show that small, ex ante financially constraint firms pass through windfall cash gains to their incumbent workers. Finally, differences in rent-sharing by firm size can also partly be rationalized by the German industrial relations system (cf. Section 3). Collective wage bargaining reduce the firms' scope to cut wages in response to cost shocks. However, collective agreements do not only limit the scope for adjusting wages downward: as larger firms usually pay higher wages, they face a lower pressure to raise wages if productivity rises because high-wage firms can effectively "hide" behind industry-wide wage standards.[43] Gürtzgen (2009) confirms weaker rent-sharing in firms with an industry-wide collective wage agreement.

6.5.2 Longer-Run Changes

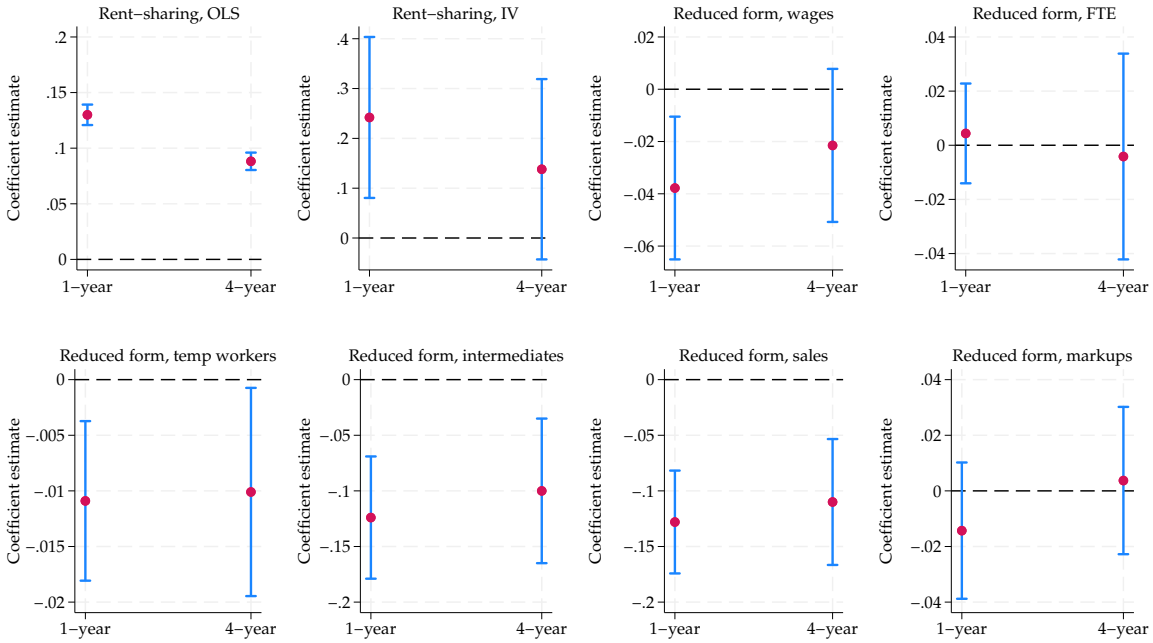
Our results are based on 1-year differences. To study longer-run adjustments, we replicate our rent-sharing regressions, reduced-form wage regressions, and adjustment mechanism regressions (Tables 3 and 4) using 4-year changes in the dependent variables. This approach reduces our sample size to 29,008 observations due to the rotating sampling scheme. To facilitate a direct comparison between short-run responses and longer-run adjustments, we also re-estimate the baseline specification in 1-year changes for this smaller sample. Figure 2 presents the coefficient estimates with 90% confidence bands.

While the smaller sample size reduces the precision of our estimates, the results for the specification with 1-year changes remain consistent with our earlier findings. Coefficient estimates for longer-run changes are qualitatively similar to the short-run results but smaller in absolute magnitude. Apart from the OLS rent-sharing specification, these longer-run coefficients are not statistically significantly different from the 1-year changes. However, the longer-run results for the IV rent-sharing and reduced-form wage regressions are also not statistically different from zero. This pattern aligns with the longer-run results for hourly

[43] For an in-depth discussion of collective wage agreements in Germany and their effect on wages, see Hirsch and Mueller (2020).

wages from the SES data, suggesting that wage adjustments (and rent-sharing) in response to energy shocks are stronger in the short run. Notably, employment adjustments involving the core workforce remain indistinguishable from zero over the longer run, while the effect on relative expenditures on temporary agency workers remains negative. This supports our earlier finding that firms primarily adjust through more flexible temporary agency workers rather than through changes to payroll employment in response to energy price shocks. Results for sales and intermediate inputs are consistent across the short- and longer-run specifications, revealing a strong negative firm response to energy price increases.

Figure 2: Longer-run changes



Notes. Binned scatter plots, AFiD Panel 2003–2017. Coefficient estimates and 90% confidence bands for rent-sharing IV regressions that regress changes in wages on changes in labor productivity, where we instrument productivity with our Bartik instrument, and for reduced form regressions that regress wages, FTE, expenditures for temporary agency workers over expenditures for payroll employment, intermediate input expenditures, sales, and markups on the Bartik instrument. Wages are defined as deflated wage bill over FTE. 1-year refers to our baseline specification in 1-year changes; 4-year refers to the same specification but where we replace the dependent variable by 4-year changes. Same sample of firms used throughout. 29,008 firm-year observations.

6.5.3 Energy-Intensive Firms

The behavior of energy-intensive firms is of key interest to this paper and much of the discussion on the green transition. Table 6 zooms in on these firms and provides results of our rent-sharing IV regressions and reduced-form wage regressions for energy-intensive firms.

In Column 1, we classify firms as energy-intensive based on their industries and restrict the sample to the five most energy-intensive industries, measured by the average firm-level energy consumption (kWh) per FTE. These industries are: food products and beverages, pulp, paper, and paper products, chemicals and chemical products, other non-metallic mineral products, and basic metals (energy-intensity statistics by industry are provided in Appendix Table A.3). Focusing on firms in these energy-intensive industries, we observe more pronounced rent-sharing elasticities (0.278) and reduced-form coefficients (-0.035) compared to our baseline results (0.205 and -0.023, respectively).

In Columns 2, we define energy-intensive firms by excluding firms below the median energy intensity within industries and years. We find statistically significant rent-sharing and reduced-form coefficients, which again exceed our baseline results. Overall, the findings in Table 6 indicate that our results are somewhat stronger for energy-intensive firms, supporting our expectation that energy-intensive firms react more strongly to energy price changes. However, we note that the larger coefficients are not statistically different from our baseline results.

Table 6: Rent-sharing and reduced form results for energy-intensive firms

	Energy-intensive industries	Energy-intensive firms ($\geq$ 50th pctl.)
	(1)	(2)
Panel A: Rent-sharing IV	0.278**	0.324***
	(0.122)	(0.091)
1^{st} stage F-Stat of instrument	11.39	18.89
Panel B: Reduced form (wages)	-0.035***	-0.043***
	(0.014)	(0.012)
Industry fixed effects	yes	yes
Region $\times$ year fixed effects	yes	yes
Reference year of instrument	fixed	fixed
Firm-year observations	26,845	48,300

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reports regressions for our rent-sharing IV and reduced form (wage) regressions as shown in Table 3 for different samples. In Column 1 we focus on the five most energy-intensive industries (food products and beverages, pulp, paper, and paper products, chemicals and chemical products, other non-metallic mineral products, basic metals). Column 2 restricts the sample to firms with above-median energy quantity consumption per FTE. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. All regressions include industry and region-year fixed effects. Wages are defined as deflated wage bill over FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

6.5.4 Excluding Small Energy Price Changes

We re-estimate our rent-sharing (IV) and reduced-form wage regressions, progressively excluding firms with the smallest absolute changes in the Bartik instrument. Table 7 presents the results, showing the impact of excluding firms with absolute changes in the Bartik instrument below the 10th, 20th, 50th, 80th, and 90th percentiles of the instrument distribution.

Our findings indicate that there are no significant differences in rent-sharing elasticities or reduced-form wage effects between the baseline results and those obtained from samples that exclude firms with the smallest 10 to 20 percent of absolute energy price changes. However, when we restrict the sample to the 20 or 10 percent of firms experiencing the largest energy price changes, we observe considerably higher estimates. Specifically, the rent-sharing elasticity increases to approximately 0.6, while the reduced-form regressions yield coefficients of 0.09 and 0.11 for the top 20 percent and top 10 percent of firms, respectively.

Due to the large standard errors of the rent-sharing elasticities, these estimates are not statistically significantly different from one another. However, the reduced-form results between the samples in Columns 1 and 5 are statistically significantly different at the 10 percent level (based on Welch's t-test).

Overall, our results suggest that firms experiencing the largest energy price changes pass on a higher share of these price changes to their workers' wages. This is an intuitive result, as more severe cost shocks incentivize firms (and potentially workers) to engage in more intensive bargaining over wage growth. Section 6.6.2 further explores this in the context of asymmetric responses to rising and falling energy prices.

6.5.5 Excluding the Financial Crisis

Our period of analysis covers the financial crisis. This might raise concerns that our results are confounded by the changes in the economic environment during that time. For instance, Figure 1 shows that electricity and gas prices grew in tandem until 2008 and diverged after 2009. Therefore, one might be concerned that factors correlated with firms' wage dynamics during the crisis and firms' energy carrier composition might confound our findings.

Table 7: Rent-sharing IV and reduced form wage regressions when excluding smaller changes in the Bartik instrument

	> 10th pctl	> 20th pctl	> 50th pctl	> 80th pctl	> 90th pctl
	(1)	(2)	(3)	(4)	(5)
Panel A: Rent-sharing IV	0.202***	0.206***	0.231***	0.609	0.558*
	(0.067)	(0.067)	(0.080)	(0.371)	(0.292)
1^{st} stage F-Stat of instrument	31.6	31.71	22.7	2.50	3.87
Panel B: Reduced form (wages)	-0.023***	-0.025***	-0.041***	-0.090**	-0.110**
	(0.008)	(0.009)	(0.017)	(0.039)	(0.045)
Industry fixed effects	yes	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	fixed	fixed	fixed
Firm-year observations	86,692	77,067	47,718	14,134	8,909

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reports regressions for our rent-sharing IV and reduced form (wage) regressions as shown in Table 3 for different samples. In Column 1 we exclude shocks being smaller in absolute terms than the 10th percentile of the Bartik IV distribution. In Columns 2 to 5, we raise this threshold to the 20th, 50th, 80th, and 90th percentile, respectively. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. All regressions include industry and region-year fixed effects. Wages are defined as deflated wage bill over FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

We do not believe that this is a particular issue for our analysis as we already showed that our results hold after controlling for firm fixed effects, which absorbs firm-specific wage trends (Table B.4). Moreover, firm size, productivity, and capital intensity exhibit only weak correlations with firms' energy carrier shares (Table B.3). Nonetheless, in Table 8, we reproduce our results excluding the years of the financial crisis. Although this reduces the sample size, we only observe minimal changes in our findings. Particularly, excluding the years 2007-2009 yields an instrumental variable estimate of the rent-sharing elasticity of 0.21, which closely aligns with the baseline estimate of 0.205.

6.6 Asymmetric Effects: Energy Price Increases and Reductions

We conducted our analysis in the context of predominantly increasing energy prices (Figure 1), which suggests that loss-sharing—specifically, wage adjustments in response to reductions in profits or labor productivity—is the primary driver of our results. To validate this hypothesis, we conclude our paper with a series of analyses that separate the sample into firms experiencing positive and negative changes in their energy prices.

Disentangling the effects of positive and negative shocks is particularly important in our

Table 8: Excluding the Financial Crisis

	OLS baseline	IV baseline	OLS w/o 2007-2009	IV w/o 2007-2009
	(1)	(2)	(3)	(4)
Value-added per FTE	0.144***	0.205***	0.139***	0.210***
	(0.003)	(0.067)	(0.004)	(0.080)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Firm-year observations	96,397	96,397	77,662	77,662
R-squared	0.304	0.288	0.305	0.284
Number of Firms	22,513	22,513	22,053	22,053

Notes. AFiD Panel, 2003–2017, single-plant firms. The table compares our baseline rent-sharing regressions with a specification that excludes the years of the financial crisis (2007-2009). All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Wages are defined as deflated wage bill over FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

context, as energy prices are expected to continue rising in many countries over the coming decades (e.g., due to the green transition). If our results were instead driven by rent-sharing following energy price reductions, they would be less relevant for understanding firms' responses in the likely scenario of sustained energy price increases.

6.6.1 Baseline Results

We start with evidence on reduced-form wage regressions. The first column of Table 9 shows results for the subsample of observations with energy price reductions (positive shocks). The regressions include the same set of controls and fixed effects as our baseline specification. We find a small and insignificant coefficient, highlighting that energy price reductions do not translate into wage increases in this sample. The second column demonstrates that an energy price increase (negative shock) of 10 percent leads to wage reductions of 0.34 percent. Hence, the negative reduced-form estimate for the full sample of -0.023 (Table 3) is driven by negative price shocks and masks considerable heterogeneity. To further scrutinize our results, Figure 3 shows the corresponding binned scatter plots from our regression of log wage changes on changes in firms' energy prices (Bartik instruments). We find no evidence of a relationship between wage and energy price changes for falling energy prices (Panel A). However, consistent with our regression results, Panel B shows a clear negative correlation between wage and energy price changes for rising energy prices.

Asymmetric rent-sharing is sometimes discussed with a specific focus on downward nominal

Table 9: Asymmetric effects - reduced form and rent-sharing

	Reduced form		Rent-sharing	
	Falling price	Rising price	Falling price	Rising price
	(1)	(2)	(3)	(4)
Bartik instrument	0.003	-0.034***		
	(0.015)	(0.012)		
Value-added per FTE			-0.033	0.269***
			(0.186)	(0.091)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	fixed	fixed
R-squared	0.244	0.227	0.197	0.251
Estimator	OLS	OLS	2SLS	2SLS
1^{st} stage F-Stat of instrument	–	–	5.544	22.77
Firm-year observations	33,712	62,185	33,712	62,185
Number of firms	16,912	20,291	16,912	20,291

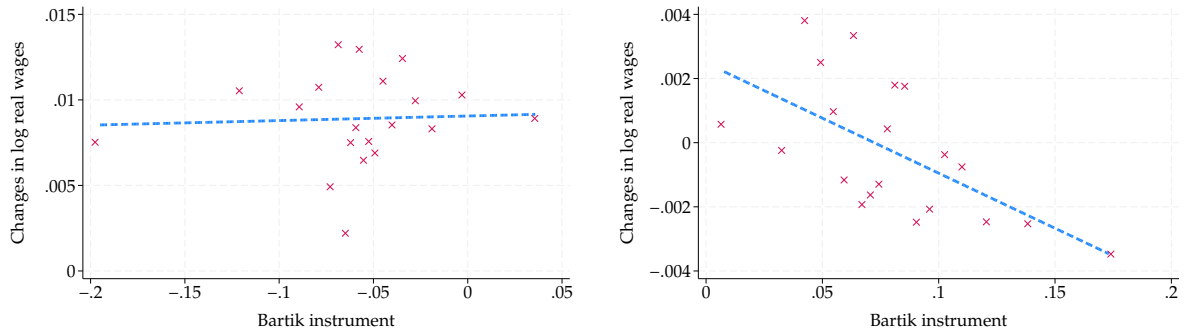
Notes. AFiD Panel, 2003–2017, single-plant firms. Columns 1 and 2 report reduced form results from OLS regressions of logged wages on our Bartik instrument, where we split the sample into firms experiencing an increase (decrease) in their energy price. Columns 3 and 4 report IV results from our rent-sharing regressions that project logged wages on logged labor productivity. In columns 3 and 4, we instrument logged labor productivity with our Bartik instrument. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Wages are defined as deflated wage bill over FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

wage rigidity. Panel B of Figure 3 illustrates a decline in real wages among workers employed at firms facing substantial increases in energy prices. Appendix Figure A.2 replicates Figure 3 using nominal wages to highlight that this does not imply negative nominal wage growth. Hence, our findings do not contradict the concept of downward nominal wage rigidity.

Our rent-sharing IV results are depicted in Columns 3 and 4 of Table 9. In line with the reduced-form results, energy price reductions yield an insignificant IV elasticity of -0.03. In contrast, rising energy prices are associated with a substantial rent-sharing elasticity of 0.27, which is larger than our baseline results for the full sample (0.205) and statistically significant at the 1 percent level. We conclude that firms partly pass through energy price increases to wages, yielding reduced wage growth for workers in firms facing stronger increases in energy prices. Conversely, workers, on average, do not benefit from energy price reductions.[44]

[44] We report 1- and 4-year changes for our rent-sharing IV regressions, reduced-form wage regressions, and firms' responses to energy price shocks with respect to FTE, temporary agency worker expenditures, intermediates, sales, and markups separately for firms experiencing increasing and decreasing energy prices in online Appendix Figure A.3. In the short run, firms facing rising and falling energy prices show no notable differences in their responses. However, in the longer run (4-year changes), we observe weak evidence of negative employment adjustments for firms experiencing rising energy prices. Additionally, there are clear

Figure 3: Wage and energy price changes



A: Falling energy prices **B: Rising energy prices**

Note: Binned scatter plots, AFiD Panel 2003–2017. Panel A (B) plots changes in log real wages against changes in our firm-level Bartik instrument for falling (rising) energy prices. Consistent with our regression analysis, we control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity as well as for industry and region-year fixed effects. Wages are defined as deflated wage bill over FTE. 33,712 firm-year observations with falling energy prices; 62,185 firm-year observations with increasing energy prices.

6.6.2 Discussion: Mechanism

Wage reductions due to loss-sharing (i.e., wage reductions following an erosion of existing rents) can be rationalized in our context, where dismissals are costly, and workers and firms share rents over which they bargain (cf. Section 3). However, through the lens of our standard bargaining model, the absence of rent-sharing for falling energy prices is surprising, as bargaining would typically imply that workers and firms also share increases in productivity or rents. Nonetheless, several mechanisms could rationalize this asymmetry in rent-sharing, extending beyond our simple bargaining model.

For instance, firms could closely monitor costs and sales, while workers may lack awareness of actual gains in firm rents, particularly when these gains are driven by energy price changes. Due to such information asymmetries, workers might miss the opportunity to bargain for higher wages in response to energy price reductions, whereas firms negotiate for lower wage growth in response to energy price increases. These asymmetries may be smaller in energy-intensive firms, where workers might have a better understanding of how energy price changes affect firm performance.

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negative effects on sales and intermediate input expenditures only for firms with increasing energy prices. In this reduced sample, rent-sharing IV coefficients are statistically significant (at the 10% level) only in the short run and only for firms experiencing rising energy prices. We argue in Section 6.6.2 that information asymmetries and differences in the magnitude of energy price shocks (that may also drive results in Figure A.3) appear to be important in understanding firms' asymmetric responses to rising and falling energy prices.

To investigate this potential explanation, Table 10 presents results from re-estimating the reduced-form wage regressions for firms experiencing rising and falling energy prices. The analysis is conducted by i) progressively excluding smaller price shocks in absolute terms, as in Table 7, and ii) focusing on energy-intensive firms (above-median energy consumption per FTE). We do not include rent-sharing IV regressions in Table 10 due to the limited sample size, which results in weak first-stage F-statistics. However, we provide these results in Appendix Table A.4 for transparency.

Panel A of Table 10 presents results for rising energy prices across all firms. Reduced-form coefficients tend to increase slightly with the size of the shock, although these effects are not statistically significant. Panel B examines falling energy prices across all firms, where coefficients increase sharply with the size of the shock. However, the statistical power to precisely detect these effects in our data is limited due to small sample sizes. Only the coefficient in Panel B, Column 5 approaches statistical significance at the 10% level. Panels C and D additionally zoom in on energy-intensive firms. For rising energy prices, the results become much stronger. For falling energy prices, the effects also intensify, except for the top 10% sample. When excluding below-median energy price changes (Panel D, Column 3), we even observe a statistically significant negative coefficient. This suggests that when shocks are sufficiently large and energy is a sufficiently important input, wage growth may indeed increase in response to falling energy prices.

Overall, our findings provide some evidence that small price reductions (and consequently small increases in rents) do not trigger bargaining processes, whereas larger energy price reductions may lead to bargaining and increased wage growth, particularly among energy-intensive firms. As discussed, this could be attributed to information asymmetries between firms and workers. Other mechanisms could also explain the limited pass-through of energy price reductions to wage growth. For instance, firms and workers might anticipate future energy price increases even after reductions. Investigating these and other mechanisms of (asymmetric) rent-sharing remains a critical avenue for future research that goes beyond the scope of our data.

Table 10: Reduced form wage regressions when excluding smaller changes in the Bartik instrument, separately for firms with increasing and falling energy prices

	> 10th pctl	> 20th pctl	> 50th pctl	> 80th pctl	> 90th pctl
	(1)	(2)	(3)	(4)	(5)
	Panel A: Rising energy prices, all firms				
Reduced form (wages)	-0.035***	-0.046***	-0.041**	-0.059*	-0.041
	(0.013)	(0.014)	(0.017)	(0.031)	(0.061)
R-squared	0.228	0.230	0.243	0.294	0.327
Firm-year observations	57,746	52,272	36,755	14,475	6,111
	Panel B: Falling energy prices, all firms				
Reduced form (wages)	-0.004	-0.007	-0.035	-0.067	-0.102
	(0.015)	(0.017)	(0.023)	(0.047)	(0.063)
R-squared	0.247	0.240	0.287	0.309	0.332
Firm-year observations	28,342	24,394	10,697	3,986	2,798
	Panel C: Rising energy prices, energy-intensive firms				
Reduced form (wages)	-0.051***	-0.061***	-0.040*	-0.100**	-0.195**
	(0.018)	(0.020)	(0.024)	(0.045)	(0.088)
R-squared	0.256	0.261	0.267	0.326	0.367
Firm-year observations	28,098	25,534	18,074	7,170	2,942
	Panel D: Falling energy prices, energy-intensive firms				
Reduced form (wages)	-0.012	-0.025	-0.083**	-0.101	-0.020
	(0.022)	(0.024)	(0.032)	(0.070)	(0.090)
R-squared	0.281	0.279	0.334	0.379	0.383
Firm-year observations	14,332	12,399	5,350	1,758	1,149

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reports regressions for our reduced form (wage) regressions for different samples. In Column 1 we exclude shocks being smaller in absolute terms than the 10th percentile of the Bartik IV distribution. In Columns 2 to 5, we raise this threshold to the 20th, 50th, 80th, and 90th percentile, respectively. Additionally, Panel A and B include all firms, given these selection criteria, whereas Panel C and D additionally reduce the sample to firms with an energy consumption above the median (defined by energy consumption (kWh) per FTE within industries and years). All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. All regressions include industry and region-year fixed effects. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

7 Conclusions

This study presents causal evidence on the wage pass-through in German manufacturing firms that is based on firm-level variation in rents (labor productivity) induced by energy price variation at the firm level. We develop a novel Bartik instrument for labor productivity combining the predetermined firm-level energy mix with nationwide changes in the prices of various energy carriers and present extensive evidence on the validity of our new instrument. Our IV estimator yields a rent-sharing coefficient (productivity-wage pass-through) of approximately 0.20, implying that a 10-percent increase in firms' labor productivity increases wages by approximately 2 percent. The productivity differential between firms at the 10th

and 90th percentiles of the log labor productivity distribution amounts to 230 percent (1.2 log points). Evaluating the estimated pass-through parameter of 0.2 at this 90-10 productivity gap yields a between-firm wage variability of 46 percent.

A novel and important feature of our study is that rent-sharing induced by energy price variation is primarily driven by energy price increases (i.e., loss-sharing). This provides new insights on how firms transfer profit losses into lower wage growth. While wages decline in response to rising energy prices, firms, on average, do not share gains from lower energy prices in the form of higher wage growth. Only for the subset of energy-intensive firms that experience relatively large energy price declines, we find evidence that energy price reductions lead to wage growth. It is possible that the energy price reductions during our sample period were too small or short-lived to initiate wage bargaining processes, whereas energy price increases were substantial and passed through as reduced wage growth.

In an environment of rising energy prices, for instance, due to carbon taxes, loss-sharing has direct implications for wage growth and inequality. Our results imply that workers in firms facing rising energy prices will see their relative wages decline. However, wage-pass through is incomplete, and also firms bear a significant burden, possibly diminishing their ability to allocate rents to investment and innovation.

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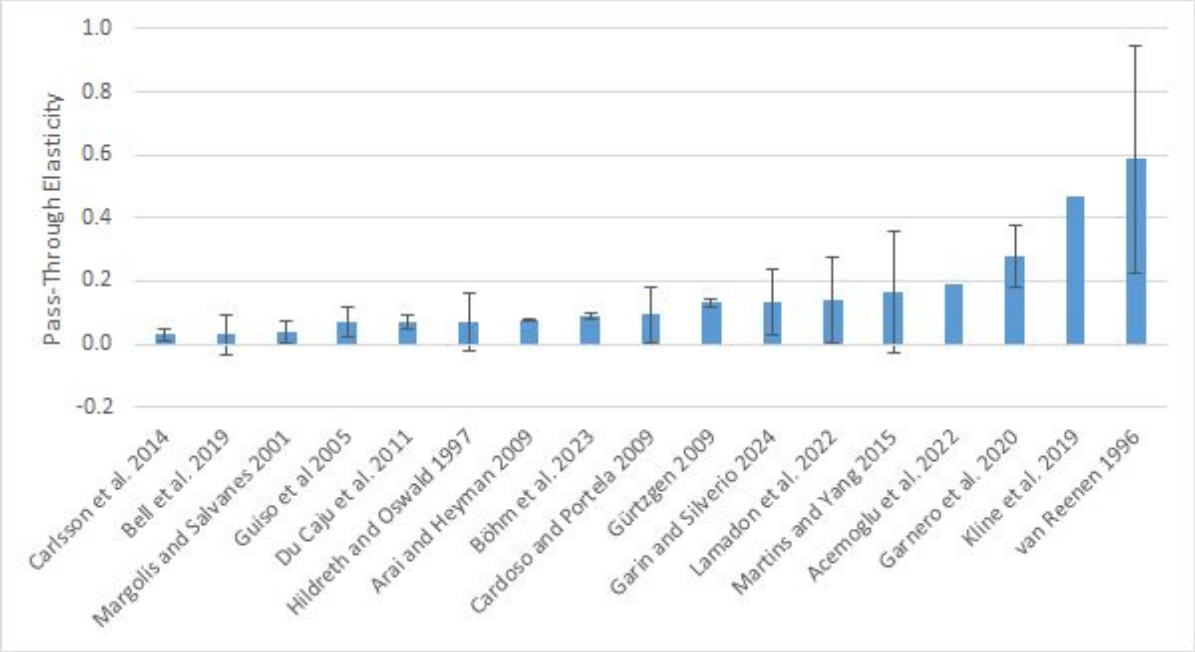
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Appendix (possibly online Appendix)

A.1 Additional Material

Figure A.1: Causal estimates of pass-through elasticities



Notes. The graph shows estimates and 95%-confidence bands (if available) of pass-through elasticities from studies that use firm-level variation in rents and interpret their estimates causally. Studies differ in their identification strategies and the type of treatment effects estimated. If applicable, results for permanent shocks and manufacturing sub-samples have been selected. All estimates are converted to a value-added based specification; i.e., estimates derived from a quasi-rent specification (e.g., Van Reenen (1996)) are multiplied with 2 as suggested by Card et al. (2014). Confidence bands are not available for the elasticities reported in Acemoglu et al. (2022) and Kline et al. (2019). We list precise references for the estimates below.

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Table A.1: Rent-sharing by firm size, OLS regressions

	<50	<100	<150	<250	All firms
	(1)	(2)	(3)	(4)	(5)
Value-added per FTE	0.179***	0.164***	0.159***	0.153***	0.144***
	(0.006)	(0.005)	(0.004)	(0.004)	(0.003)
Industry fixed effects	yes	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes	yes
Firm-year observations	29,279	57436	71,121	82,359	96,397
R-squared	0.394	0.345	0.330	0.316	0.304
Number of Firms	9,983	16,330	18,901	20,835	22,513

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reproduces our baseline rent-sharing regressions using OLS for firms of different size. Column headings indicate the number of employees in the firm sample. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table A.2: Rent-sharing by firm size, IV regressions

	<50	<100	<150	<250	All firms
Panel A: Rent-Sharing IV (second stage)	(1)	(2)	(3)	(4)	(5)
Value-added per FTE	0.350	0.263**	0.213**	0.214***	0.205***
	(0.451)	(0.132)	(0.089)	(0.080)	(0.067)
Industry fixed effects	yes	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	fixed	fixed	fixed
Firm-year observations	29,279	57,436	71,121	82,359	96,397
R-squared	0.303	0.311	0.319	0.301	0.288
Number of Firms	9,983	16,330	18,901	18,864	22,513
1^{st} stage F-Stat of instrument	0.935	9.523	19.57	23.07	31.54
Panel B: Value-added per FTE on instrument (first stage)	(1)	(2)	(3)	(4)	(5)
Bartik IV	-0.010	-0.073***	-0.096***	-0.10**	-0.113***
	(0.015)	(0.024)	(0.022)	(0.021)	(0.020)
Industry fixed effects	yes	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	fixed	fixed	fixed
Firm-year observations	29,279	57,436	71,121	82,359	96,397
R-squared	0.293	0.176	0.162	0.154	0.146
Number of Firms	9,983	16,330	18,901	18,864	22,513

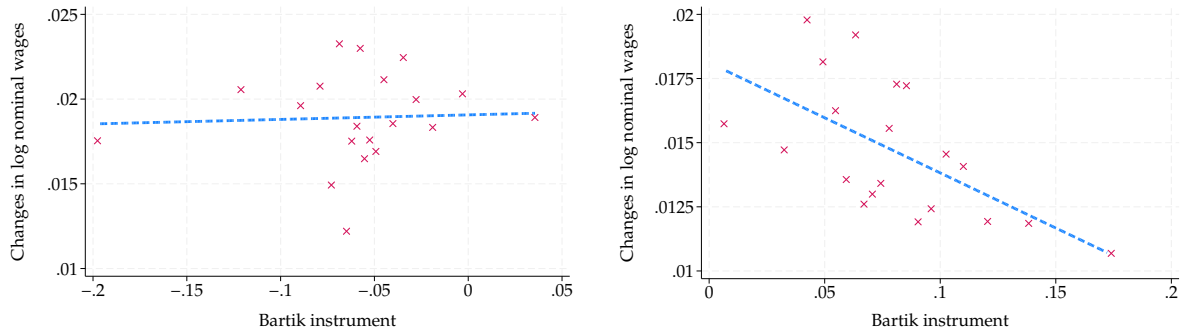
Notes. AFiD Panel, 2003–2017, single-plant firms. The table reproduces our baseline rent-sharing regressions using IV for firms of different size. Panel A repoirts the second stage of our IV regressions, regressing log real wages on log value-added per FTE instrumented with our Bartik instrument. Panel B reports the first-stage regressions (value-added per FTE on the Bartik instrument). Column headings indicate the number of employees in the firm sample. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table A.3: Average firm-level energy intensity by industry

	Energy (kWh) over FTE	Energy (kWh) over deflated sales
Industry	(1)	(2)
15 - Food products and beverages	92,097	0.379
16 - Tobbaco	81,549	0.575
17 - Textiles	59,261	0.506
18 - Apparel, dressing, and dyeing of fur	11,102	0.114
19 - Leather and leather products	22,785	0.201
20 - Wood and wood products	50,074	0.230
21 - Pulp, paper, and paper products	257,628	0.839
22 - Publishing and printing	32,420	0.203
24 - Chemicals and chemical products	155,482	0.475
25 - Rubber and plastic products	51,211	0.331
26 - Other non-metallic mineral products	169,277	1.084
27 - Basic metals	140,254	0.733
28 - Fabricated metal products	31,784	0.273
29 - Machinery and equipment	15,274	0.121
30 - Electrical and optical equipment	9,488	0.058
31 - Electrical machinery and apparatus	13,003	0.099
32 - Radio, television, and communication	18,548	0.104
33 - Medical and precision instruments	8,796	0.069
34 - Motor vehicles and trailerst	30,305	0.188
35 - Transport equipment	19,805	0.159
36 - Furniture manufacturing	16,173	0.147

Notes. AFiD Panel, 2003–2017, single-plant firms. Average firm-level energy (kWh) consumption over FTE and over deflated sale, by industry.

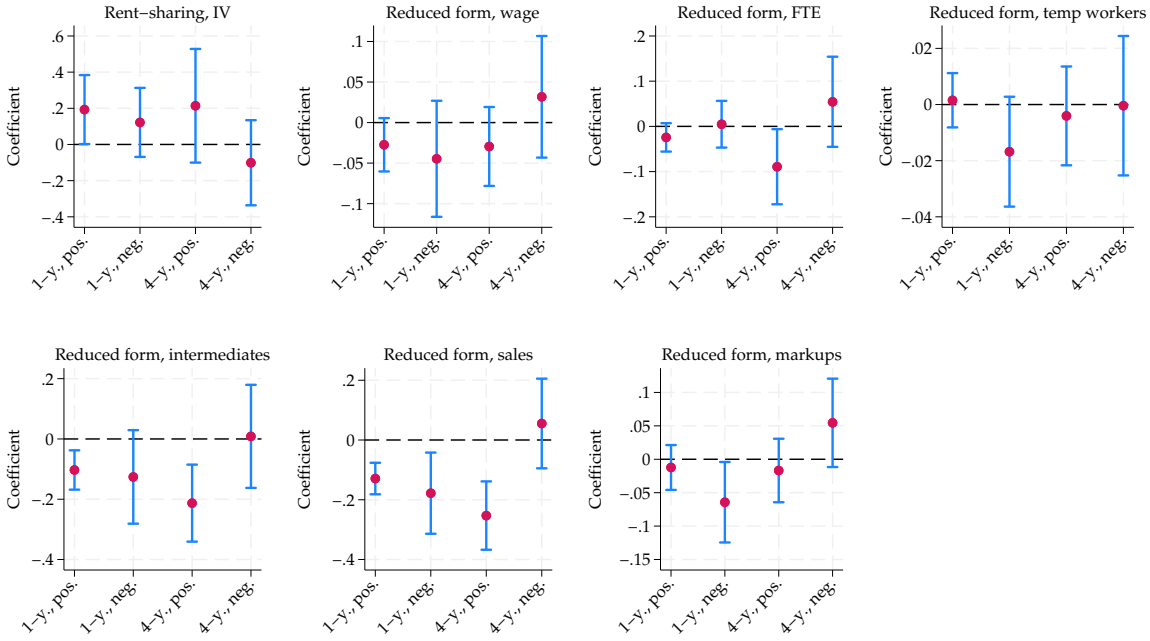
Figure A.2: Nominal wage and energy price changes



A: Falling energy prices **B: Rising energy prices**

Note: Binned scatter plots, AFiD Panel 2003–2017. Panel A (B) plots changes in log nominal wages against changes in our firm-level Bartik instrument for falling (rising) energy prices. Consistent with our regression analysis, we control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity as well as for industry and region-year fixed effects. Wages are defined as nominal wage bill per FTE.

Figure A.3: Longer-run changes, separately for firms experiencing increasing and falling energy prices



Notes. Binned scatter plots, AFiD Panel 2003–2017. Coefficient estimates and 90% confidence bands for rent-sharing IV regressions that regress changes in wages on changes in labor productivity, where we instrument productivity with our Bartik instrument, and for reduced form regressions that regress wages, FTE, expenditures for temporary agency workers over expenditures for payroll employment, intermediate input expenditures, sales, and markups on the Bartik instrument. 1-year (1-y.) refers to our baseline specification in 1-year changes; 4-year (4-y.) refers to the same specification but where we replace the dependent variable by 4-year changes. Separate estimates for firms experiencing positive (pos.) and negative (neg.) energy price shocks. Same sample of firms used throughout. Wages are defined as deflated wage bill per FTE. 22,706 firm-year observations with increasing energy prices; 5,546 firm-year observations with decreasing energy prices.

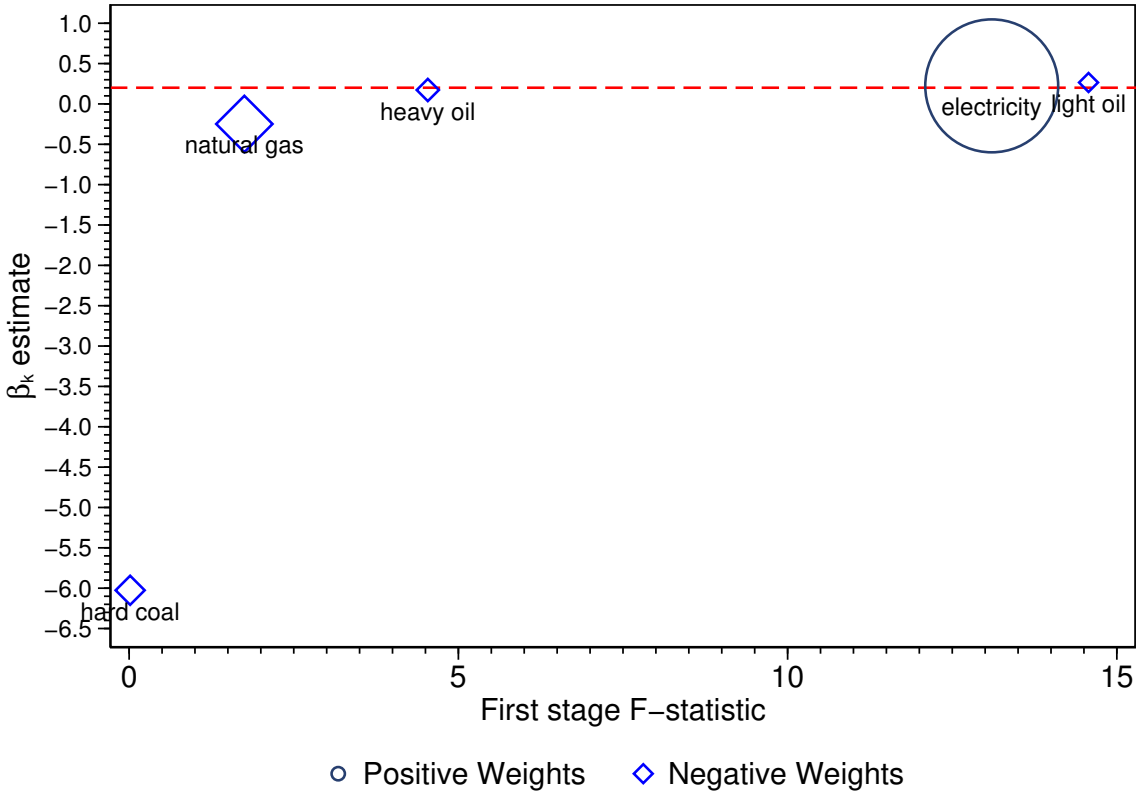
Table A.4: Rent-sharing IV regressions when excluding smaller changes in the Bartik instrument, separately for firms with increasing and falling energy prices

	> 10th pctl	> 20th pctl	> 50th pctl	> 80th pctl	> 90th pctl
	(1)	(2)	(3)	(4)	(5)
	Panel A: Rising energy prices, all firms				
Rent-sharing (IV)	0.284***	0.340***	0.448**	0.462	1.602
	(0.0998)	(0.104)	(0.211)	(0.282)	(7.122)
1^{st} stage F-Stat of instrument	19.34	19.90	6.0	3.45	0.04
Firm-year observations	57,746	52,272	36,755	14,475	6,111
	Panel B: Falling energy prices, all firms				
Rent-sharing (IV)	0.0476	0.100	0.308	0.436	0.466
	(0.188)	(0.208)	(0.202)	(0.332)	(0.343)
1^{st} stage F-Stat of instrument	4.94	3.97	4.78	2.01	2.19
Firm-year observations	28,342	24,394	10,697	3,986	2,798
	Panel C: Rising energy prices, energy-intensive firms				
Rent-sharing (IV)	0.283***	0.318***	0.285*	0.523*	1.059
	(0.103)	(0.109)	(0.173)	(0.296)	(0.976)
1^{st} stage F-Stat of instrument	16.95	16.58	6.2	3.54	1.10
Firm-year observations	28,098	25,534	18,074	7,170	2,942
	Panel D: Falling energy prices, energy-intensive firms				
Rent-sharing (IV)	0.213	0.486	1.003	4.088	-0.232
	(0.374)	(0.544)	(0.933)	(28.62)	(1.313)
1^{st} stage F-Stat of instrument	1.07	0.90	1.09	0.02	0.12
Firm-year observations	14,332	12,399	5,350	1,758	1,149

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reports regressions for rent-sharing IV regressions for different samples. In Column 1 we exclude shocks being smaller in absolute terms than the 10th percentile of the Bartik IV distribution. In Columns 2 to 5, we raise this threshold to the 20th, 50th, 80th, and 90th percentile, respectively. Additionally, Panel A and B include all firms, given these selection criteria, whereas Panel C and D additionally reduce the sample to firms with an energy consumption above the median (defined by energy consumption (kWh) per FTE within industries and years). All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. All regressions include industry and region-year fixed effects. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

B.1 Specification Tests

Figure B.1: Heterogeneity of β_k



Notes. AFiD Panel, 2003–2017, single-plant firms. The y-axis depicts the estimated beta coefficient of the second stage IV regression using GMM, where the instrument is the product of the shifts and shares of a single instrument $s \in S = \{electricity, naturalgas, lightoil, heavyoil, hardcoal\}$. The x-axis depicts the corresponding first stage F-statistic of this regression. The size of the points are scaled by the size of the Rotemberg weight (Goldsmith-Pinkham et al. 2020). Circles have a positive and squares have a negative Rotemberg weight. The red dashed line is the estimated rent-sharing elasticity of our baseline IV regression.

Table B.1: Main results, controlling for lagged changes in capital intensity

	Reduced form	First stage IV	OLS rent-sharing	IV rent-sharing
	(1)	(2)	(3)	(4)
Bartik instrument	-0.018*	-0.111***		
	(0.010)	(0.022)		
Value-added per FTE			0.163***	0.160**
			(0.004)	(0.079)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	–	fixed
R-squared	0.110	0.124	0.228	0.228
1^{st} stage F-Stat of instrument	–	–	–	25.04
Firm-year observations	67,677	67,677	67,677	67,677
Number of Firms	18,227	18,227	18,227	18,227

Notes. AFiD Panel, 2003–2017, single-plant firms. Column 1 reports reduced form results from an OLS regression of logged wages on our Bartik instrument. Column 2 reports results from an OLS regression of logged labor productivity on our Bartik instrument, which corresponds to the first stage regression of column 4. Columns 3 and 4 report OLS and IV results from our rent-sharing regressions that project logged wages on logged labor productivity. In column 4, we instrument logged labor productivity with our Bartik instrument. All regressions are in first differences and control for firms' logged predetermined energy intensity and lagged changes in firms' logged capital intensity. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table B.2: Main results with previous year Bartik instrument weights

	OLS reduced form	OLS first stage	IV rent-sharing
	(1)	(2)	(3)
Bartik instrument	-0.022**	-0.112***	
	(0.009)	(0.021)	
Value-added per FTE			0.192***
			(0.071)
Industry fixed effects	yes	yes	yes
Region × year fixed effects	yes	yes	yes
Reference year of instrument	previous	previous	previous
R-squared	0.213	0.146	0.294
1^{st} stage F-Stat of instrument			27.95
Firm-year observations	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513

Notes. AFiD Panel, 2003–2017, single-plant firms. Column 1 reports reduced form results from regressing logged wages on our Bartik instrument. Column 2 reports results from regressing logged labor productivity on our Bartik instrument, which corresponds to the first stage regression of column 3. Column 3 reports IV results from our rent-sharing regression that projects logged wages on logged labor productivity. We instrument logged labor productivity with our Bartik instrument. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. The energy carrier weights for the Bartik instrument are defined in the previous year for every firm. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table B.3: Relationship between energy shares and firm characteristics

	Electricity	Light oil	Natural gas	Hard coal	Heavy oil
	(1)	(2)	(3)	(4)	(5)
Value-added per FTE	-0.010**	-0.016***	0.020***	-0.001***	0.001*
	(0.004)	(0.004)	(0.004)	(0.000)	(0.000)
Capital stock per FTE	0.028***	-0.022***	-0.007***	0.001**	0.001***
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
Full-time equivalent (FTE)	0.016***	-0.036***	0.015***	0.002***	0.001***
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
Region fixed effects	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
R-squared	0.122	0.108	0.118	0.048	0.029
Observations	22,513	22,513	22,513	22,513	22,513

Notes: OLS regressions of the energy carrier shares in firm-level energy use on various economic indicators (all in logs). Each column represents a separate cross-sectional regression using the first year per firm, only. Clustered standard errors at the firm level are in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table B.4: Rent-sharing, OLS and IV regressions with additional fixed effects

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Value-added per FTE	0.143***	0.138***	0.140***	0.219***	0.273***	0.212*
	(0.003)	(0.004)	(0.005)	(0.071)	(0.079)	(0.126)
Firm fixed effects	no	yes	yes	no	yes	yes
Industry fixed effects	no	yes	no	no	yes	no
Region × year fixed effects	yes	yes	no	yes	yes	no
Region × industry fixed effects	yes	no	no	yes	no	no
Region × industry × year fixed effects	no	no	yes	no	no	yes
Reference year of instrument	–	–	–	fixed	fixed	fixed
R-squared	0.329	0.407	0.557	0.305	0.336	0.543
1^{st} stage F-Stat of instrument				27.95	23.63	8.958
Firm-year observations	96,043	92,289	69,101	96,043	92,289	69,101
Number of Firms	22,233	18,420	14,928	22,233	18,420	14,928

Notes: AFiD Panel, 2003–2017, single-plant firms. Firm-fixed effects are added to our baseline rent-sharing regressions from Table 3. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Wages are defined as deflated wage bill per FTE. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table B.5: Rent-sharing, IV regressions, overidentified models

	Baseline IV	Overidentified IV	
	(1)	(2)	(3)
Value-added per FTE	0.205***	0.202***	0.211***
	(0.067)	(0.063)	(0.073)
Industry fixed effects	yes	yes	yes
Region × year fixed effects	yes	yes	yes
Reference year of instrument	–	fixed	fixed
R-squared	0.304	0.289	0.284
Estimator	2SLS	2SLS	LIML
1^{st} stage F-Stat of instrument	–	6.953	6.953
Over Ident	–	5.177	5.172
Over Ident (p-V)	–	0.270	0.270
Firm-year observations	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513

Notes. Column 1 reports our baseline rent-sharing regression from Table 3. Columns 2 and 3 report results from rent-sharing regression using an overidentified IV approach. All regressions are in first differences and control for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Wages are defined as deflated wage bill per FTE. The five instruments used in the IV regressions are the products of energy shares and price changes of electricity, natural gas, light fuel, heavy fuel, hard coal. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table B.6: First Stage regressions with energy carriers as separate instruments (overidentified model)

	(1)
Electricity	-0.194***
	(0.054)
Light oil	-0.103***
	(0.021)
Natural gas	-0.150***
	(0.037)
Heavy oil	-0.242**
	(0.108)
Hard coal	-0.067
	(0.094)
Industry fixed effects	yes
Region × year fixed effects	yes
Reference year of instrument	fixed
R-squared	0.146
Firm-year observations	96,397
Number of Firms	22,513

Notes. AFiD Panel, 2003–2017, single-plant firms. Column 1 reports the first stage regression corresponding to the overidentified IV models in Table B.5. The regression is in first differences and includes controls for firms' logged predetermined energy intensity and changes in firms' logged capital intensity. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level, respectively.

Table B.7: Firm-level evidence on adjustments in workforce composition

	Female worker share	Part time worker share	Share of working owners
	(1)	(2)	(3)
Bartik instrument	0.023	-0.174	-0.030
	(0.368)	(0.380)	(0.046)
Industry fixed effects	yes	yes	yes
Region × year fixed effects	yes	yes	yes
Reference year of instrument	fixed	fixed	fixed
R-squared	0.061	0.098	0.057
Firm-year observations	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513

Notes: The table reports regressions of workforce characteristics (first differences) on the Bartik instrument. All regressions control for firms' logged predetermined energy intensity, changes in firms' logged capital intensity, and industry and region-year fixed effects. Clustered standard errors at the firm level are in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table B.8: Descriptive Statistics - SES data

	Plant level							
	Mean	SD	P10	P25	P50	P75	P90	N
Bartik IV (fixed year, 4-year diff.)	0.132	0.054	0.069	0.100	0.130	0.169	0.194	2,025
Number of employees	1,173	2,293	86	218	567	1,291	2,324	2,025
Average tenure	14.04	5.418	6.88	10.25	14.09	17.66	20.93	2,025
Average hours per worker (annual)	1,885	130.7	1,743	1,808	1,881	1,977	2,051	2,025
Percentage workers complex tasks	32.56	23.44	6.897	14.29	27.14	46.22	68.97	2,025
Percentage workers college degree	14.71	15.30	0	3.774	10	20.55	36.36	2,017
	Worker level							
	Mean	SD	P10	P25	P50	P75	P90	N
Tenure	14.58	11.28	1	5	12	23	31	160,392
Hours worked (annual)	1,866	312.4	1,623	1,825	1,955	2,076	2,086	160,392
Dummy for working in a complex task	0.331	0.471	0	0	0	1	1	160,392
Dummy for college degree	0.153	0.360	0	0	0	0	1	160,392

Notes. SES data 2006, 2010, 2014. Plants entering the regression analysis of Table B.9. Worker-level statistics refer to statistics based on the observed worker-firm matches (160,392 in total).

Table B.9: Worker-level evidence on adjustments in workforce composition

	Log avg tenure	Log avg hours	Share workers complex tasks	Share workers college degree	Log hourly wage
	(1)	(2)	(3)	(4)	(5)
Bartik instrument for SES (4-year diff.)	0.165	0.033	-0.922	1.622	-0.006
	(0.189)	(0.025)	(9.666)	(3.493)	(0.004)
Industry fixed effects	yes	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes	yes
Reference year of instrument	fixed	fixed	fixed	fixed	fixed
R-squared	0.266	0.309	0.237	0.231	0.310
Firm-year observations	2,018	2,025	2,025	2,016	2,025
Number of Firms	1,415	1,420	1,420	1,416	1,420

Notes. SES data (2006, 2010, 2014). The table reports results from regressing workforce characteristics, average working hours, and hourly wages (4-year differences) on the Bartik instrument. Columns 1-5 use a Bartik instrument based on 4-year differences to accommodate the survey structure of the SES. The complexity of tasks is divided into five categories following the definition of the Statistical Offices: (1) leading personnel with supervision tasks and specific knowledge typically acquired through a university degree; (2) workers in complex and diverse tasks that require completed vocational training and several years of experience; (3) difficult tasks that require completed vocational training and only limited or no experience; (4) mainly simple tasks that do not require completed vocational training but require skills that can be learned within two years; and (5) exclusively simple tasks that do not require completed vocational training and for which the required skills can be learned within three months. Robust standard errors are reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

C.1 Theoretical Derivations and Additional Theoretical Results

C.1.1 Derivations for the Bargaining Model from the Main Text

The notation follows the main text. We start from the bargaining equation (C.1) from the main text:

$$max[\phi log(W_{it} - W_{ot})N_{it}) + (1-\phi)log(\Pi_{it})]. \tag{C.1}$$

The first-order condition with respect to labor and wages are, respectively:

$$W_{it} = MRPL_{it} + \frac{\phi}{1-\phi}\frac{\Pi_{it}}{N_{it}}, \tag{C.2}$$

$$W_{it} = W_{ot} + \frac{\phi}{1-\phi}\frac{\Pi_{it}}{N_{it}}, \tag{C.3}$$

where $MRPL_{it}$ denotes the marginal revenue product of labor. These equations imply that wages exceed marginal revenue products of labor, whenever profits, Π_{it}, are positive and workers have non-zero bargaining power, ϕ. Rewriting Eq. (C.3) yields Eq. (2) from the main text:

$$\frac{1}{1-\phi}W_{it} = W_{ot} + \frac{\phi}{1-\phi}\frac{VA_{it}}{N_{it}}, \tag{C.4}$$

$$W_{it} = (1-\phi)W_{ot} + \phi\frac{VA_{it}}{N_{it}} = W_{ot} + \phi\frac{VA_{it} - W_{ot}N_{it}}{N_{it}} = W_{ot} + \phi\frac{QR_{it}}{N_{it}}, \tag{C.5}$$

where $QR_{it} = VA_{it} - W_{ot}N_{it}$ denotes short-run quasi-rents.

C.1.2 Rent-sharing, Monopsony, Markups, and Labor Shares

The notation follows the main text. We will now derive expressions for the labor share to study the relationship between markups and the labor share in our bargaining model and under a standard monopsony model. The key insight will be that this relationship is informative about the presence of rent-sharing because a positive association between changes in a firm's labor share and its price over cost markup (as we find in the data) cannot be rationalized within a standard monopsony model but requires that firms share rents.

Production function. Consider that firms produce output (Q_{it}) using the Cobb-Douglas production function (results also hold for a more general production function, such as a translog production function):

$$Q_{it} = K_{it}^{\theta^K} N_{it}^{\theta^N} M_{it}^{\theta^M} A_{it}. \tag{C.6}$$

K_{it}, N_{it}, and M_{it}, denote capital, labor, and intermediate (including energy) inputs. A_{it} is total factor productivity. θ denotes the output elasticity corresponding to each input. To simplify the analysis, we assume that input markets are flexible and that intermediate and capital input prices are exogenous to firms (there can be suppliers with market power).

Bargaining model. We start from the bargaining model from the main text. Reformulating Eq. (2) from the main text yields:

$$W_{it}\left(1-\phi\frac{QR_{it}}{W_{it}N_{it}}\right) = W_{it}\left(1-\frac{\phi}{1-\phi}\frac{\Pi_{it}}{W_{it}N_{it}}\right) = W_{it}\psi_{it} = MRPL_{it}, \tag{C.7}$$

where $\psi_{it} = \left(1-\frac{\phi}{1-\phi}\frac{\Pi_{it}}{W_{it}N_{it}}\right)$ is the wage markdown (or markup), that is the wedge between the wage and the marginal revenue product of labor. Reformulating Eq. (C.7) and expanding it with $\frac{N_{it}}{Q_{it}}$ yields:

$$LS_{it} = \frac{W_{it}N_{it}}{P_{it}Q_{it}} = \frac{\theta^N}{\mu_{it}\psi_{it}}, \tag{C.8}$$

where LS_{it} is the labor share in sales, μ_{it} is the price over marginal cost markup, and $\theta^N = \frac{\partial Q_{it}}{\partial N_{it}}\frac{N_{it}}{Q_{it}}$ is the output elasticity of labor. Using the first-order condition for intermediates, we can write a similar expression for the intermediate share:

$$\frac{P_{it}^M M_{it}}{P_{it}Q_{it}} = \frac{\theta^M}{\mu_{it}}, \tag{C.9}$$

where, for simplicity and as mentioned above, we abstract from intermediate input market imperfections (hence, there is no equivalent to ψ_{it} in Equation C.9). To study the relationship between the markup and the labor share, it is useful to express ψ_{it} again in terms of Eq. (C.7):

$$\psi_{it} = 1-\frac{\phi}{1-\phi}\frac{\Pi_{it}}{W_{it}N_{it}} = 1-\frac{\phi}{1-\phi}\left(\frac{P_{it}Q_{it}}{W_{it}N_{it}} - 1 - \frac{P_{it}^M M_{it}}{W_{it}N_{it}}\right). \tag{C.10}$$

Inserting Equations C.8 and C.9 yields:

$$\psi_{it} = \frac{1}{1-\phi} - \psi_{it}\frac{\phi}{1-\phi}\left(\frac{\mu_{it}-\theta^M}{\theta^N}\right). \tag{C.11}$$

$$\frac{1}{\psi_{it}} = \left(1-\phi+\phi\left(\frac{\mu_{it}-\theta^M}{\theta^N}\right)\right). \tag{C.12}$$

Inserting Eq. (C.18) into Eq. (C.8) yields an expression for the labor share in terms of workers' bargaining power:

$$LS_{it} = \phi + \frac{\theta^N - \theta^N\phi - \theta^M\phi}{\mu_{it}}. \tag{C.13}$$

Finally, the derivative of this expression with respect to the markup yields:

$$\frac{\partial LS_{it}}{\partial \mu_{it}} = -\frac{\theta^N}{\mu_{it}^2} + \frac{\phi\theta^N}{\mu_{it}^2} + \frac{\phi\theta^M}{\mu_{it}^2}, \tag{C.14}$$

where all variables are positively valued. This derivative is positive if workers bargaining power (ϕ) is sufficiently strong:

$$\phi > \frac{\theta^N}{\theta^N + \theta^M}. \tag{C.15}$$

Hence, in our standard bargaining model, labor shares can increase in markups (as we document below).

Monopsony model. Under a monopsony model, firms unilaterally maximize profits (or minimize costs) without bargaining with workers. We write the profit function of the firm as: $\Pi = VA_{it} - W_{it}(L_{it})L_{it}$, where wages are now a function of labor quantities. The first-order condition of the profit-maximization problem yields:

$$W_{it}(1+\xi^N) = MRPL_{it}, \tag{C.16}$$

where ξ^N is the inverse labor supply elasticity. Following the same derivation steps as in the bargaining model, we can write the labor share as:

$$LS_{it} = \frac{W_{it}N_{it}}{P_{it}Q_{it}} = \frac{\theta^N}{\mu_{it} + \mu_{it}\xi^N}, \tag{C.17}$$

Taking the derivative of the labor share with respect to the markup yields:

$$\frac{\partial LS_{it}}{\partial \mu_{it}} = -\frac{\theta^N}{(\xi^N + 1)\mu_{it}^2}, \tag{C.18}$$

which is an unambiguously negative expression. Hence, under a standard monopsony model, firms' labor shares decline as markups increase.

Results on the firm-level correlation between markups and labor shares. Table C.1 presents the results of regressing log labor shares on log markups in first differences. As outlined in the main text, markups are estimated based on firms' intermediate input decisions using the approach of De Loecker and Warzynski (2012) (Eq.C.9), assuming a Cobb-Douglas production function and employing a cost-share approach to estimate the output elasticity (θ^M). Column 1 reports the baseline specification without firm fixed effects, while Column 2 incorporates firm fixed effects. Columns 3 and 4 replicate the same regressions without applying the log transformation. All specifications, yield a positive, highly statistically significant correlation. Based on our previous derivations, we conclude that this finding is consistent with a bargaining model and the presence of rent-sharing in the German manufacturing sector.[45]

[45] Importantly, we do not argue that this invalidates the existence of monopsony; an upward-sloping labor supply curve may still be present. However, a standard monopsony model alone cannot explain the observed positive correlation between firms' markups and labor shares. Instead, this pattern aligns with a bargaining model in which firms and workers share product market rents.

Table C.1: Labor shares and markups (firm-level OLS regressions in first differences)

	Log labor shares	Log labor shares	Labor shares	Labor shares
	(1)	(2)	(3)	(4)
Log markups	0.162***	0.138***		
	(0.008)	(0.009)		
Markups			0.031***	0.026***
			(0.008)	(0.008)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Firm fixed effects	no	yes	no	yes
Reference year of instrument	fixed	fixed	fixed	fixed
R-squared	0.174	0.289	0.163	0.271
Firm-year observations	96,397	92,289	96,397	92,289
Number of Firms	22,513	18,420	22,513	18,420

Notes: OLS regressions of labor shares (wage bill divided by sales) on markups in logs (Columns 1-2) and without taking logs (Columns 3-4). All regressions are estimated in first differences. Markups are estimated based on firms' intermediate input decision using the approach of De Loecker and Warzynski (2012) (Equation C.9), assuming a Cobb-Douglas production function, and using a cost share approach to estimate the output elasticity (θ^M). ***/**/* denotes statistical significance at the 1%/5%/10% level.