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Identifying Rent-sharing Using Firms' Energy Input Mix*

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. Reduced-form evidence shows that higher energy prices depress wages. Instrumental variable estimation yields a rent-sharing elasticity of approximately 0.20. Rent-sharing induced by energy price variation is asymmetric and driven by energy price increases, implying that workers do not benefit from energy price reductions but are harmed by price increases. The rent-sharing elasticity is substantially larger in small (0.26) than in large (0.17) firms.

Keywords: Bartik instrument, energy prices, rent-sharing, wage inequality

JEL classification: C26, J30, P18

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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. Although a number of early studies emphasized the importance of employer wage setting power (Robinson 1933, Slichter 1950), most economists have long relied on the workhorse model of competitive labor markets to explain wage inequality. However, 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 processes offer a potentially important explanation for between-firm wage differences of workers with similar skills and within comparable occupations. Understanding how workers and firms share employment rents is thus key to this literature.

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.[1] 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

[1] 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.

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 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.[2] These innovation-based studies are prototypical for a setting with

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

product market imperfections and workers possessing firm-specific human capital. However, by focusing on innovations, both papers zoom into a small and selected subsample of the economy and concentrate on a very specific process generating rents. Recently, Garin and Silverio (2018) and Acemoglu et al. (2022) use firm-specific variation in foreign demand and estimate rent-sharing elasticities of approximately 0.15-0.20. By the nature of their research design, these studies focus on exporters only. The wide range in estimates, even among studies with credible research designs, suggests that rent-sharing elasticities are inherently heterogeneous and context specific. Hence, studies combining relevant and widely applicable rent-changing processes with broad samples of firms are key to the literature. Providing this combination is one of our contributions.

We estimate rent-sharing elasticities in the context of energy price changes and study the entire German manufacturing sector. 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 cost shocks as an instrument, instead of using a revenue shifter (such as innovation outcomes), is that cost shocks are largely unobserved by workers and are thus less likely to have a direct effect on worker effort. Additionally, innovation success and timing are at least partly under the firms' control, whereas for instance, the oil price is not.

The German economy is a particularly interesting case, as the manufacturing sector accounts for a large share of economic output, energy prices rose sharply, and the labor market is characterized by strong imperfections (e.g., Dobbelaere et al. 2020, Hirsch and Mueller 2020).

Our setting leverages the 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 by energy carriers. A price change of a certain energy carrier affects firms more the more intensively they use this carrier.[3] We assume that firms are price takers in energy markets and that economy-wide energy price changes for individual energy carriers do not directly affect workers' wages. 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 or worker characteristics, we estimate our model in first differences and conduct additional analyses using firm-worker-level data, demonstrating that our results are unlikely to be confounded by selective worker turnover. 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

[3] 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.

research contexts, including studies on the determinants of technology adoption, R&D investments, or exporting.

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 additional linked employer-employee data and 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, we conclude 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 (2018) 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). Confirming recent studies (e.g., Mertens (2021), Wong (2021)), we show that the rent-sharing elasticity is substantially larger in small (0.26) than in large (0.17) firms.

Finally, our setting utilizes both 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 be 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. Hence, energy price reductions do not benefit workers, but price increases reduce their wage growth.

The remainder of our study is structured as follows: Section 2 discusses the related literature. Section 3 presents our firm-level data on productivity, wages, and energy use. Section 4 discusses our empirical strategy and the novel Bartik instrument. Section 5 presents the results, scrutinizes the plausibility of our new instrument, and discusses effect heterogeneity. Section 6 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.[4] We also do not discuss structural approaches, as such studies typically do not employ exogenous variation in

[4] 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.

firm performance.[5]

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.[6] It is remarkable that two of the most convincing studies, i.e., Van Reenen (1996) and Kline et al. (2019), report by far the highest rent-sharing estimates. More recently, Acemoglu et al. (2022) and Garin and Silverio (2018) also provide convincing evidence by using arguably exogenous firm-level variation in foreign demand to instrument for the rents of Danish and Portuguese exporters, respectively. These studies find rent-sharing elasticities of approximately 0.15-0.2.[7]

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 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). The results based on trade shocks hitting exporting firms (Acemoglu et al. 2022 and Garin and Silverio 2018) might be specific to firms selecting themselves into exporting, which pertains to rather

[5] 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.

[6] 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.

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

productive firms and may require a fixed-cost investment 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 any gains from exporting with their workers. We therefore argue that the substantial range of estimates 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, estimates pertaining to the broadest possible set of firms and to highly relevant contexts are key.

There are further studies using instrumental variable approaches. For instance, 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 et al. 2005), or specification tests directly reject the panel model's dynamic completeness (Gürtzgen 2009).[8]

Finally, a few studies discuss asymmetric rent-sharing, meaning that wages respond differently to positive versus negative productivity shocks. Acemoglu et al. (2022) find rent-sharing for positive shocks only. This is in line with Arai and Heyman (2009)(although the findings differ in magnitude).[9] 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 in decarbonizing our economies, we will most likely face an era of rising energy prices.

3 Data

We use annual administrative panel data on German manufacturing plants from 2003 to 2017. 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

[8] 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.

[9] Cho and Krueger (2022) find rent-sharing for positive shocks.

input expenditures for a representative and periodically rotating 40% sample of all German manufacturing firms with more than 19 employees (firm data, henceforth).[10] We use this data set to calculate firm-level average wages, value-added and other variables used in our regression analysis.

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 a plant's 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 them. 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 category in kilowatt-hours (kWh), allowing us to aggregate across the different source categories 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.[11] Note that our energy data refer to the plant level, whereas our firm data contain firm-level information. We combine both data sets using

[10] We follow Bräuer et al. (2019) 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.

[11] 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.

a link between the unique plant- and firm-level identifiers provided by the statistical offices of Germany. Given this data structure, we focus on single-plant manufacturing firms, which account for 90% of all manufacturing firms in our data.

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. We present and discuss further summary statistics of the data in Section 5.1.

4 Empirical Strategy

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 rely on a structural relationship between wages and quasi rents per worker that is implied by the first-order conditions of a standard bargaining model (see Card et al. 2018):

$$W_{it} = \phi \frac{QR_{it}}{N_{it}} + W_{ot}, \tag{1}$$

where w_{it}, QR_{it}, and N_{it}, denote the wage, quasi rent, and labor inputs at firm i in period t. This equation motivates our structural equation that we bring to the data:

$$log(W_{it}) = \beta_0 + \gamma log(\frac{VA_{it}}{N_{it}}) + X\beta + \eta_{it}, \tag{2}$$

where γ measures the empirical rent-sharing elasticity and β_0 captures workers' outside options. The vector X includes firms' capital-labor ratios and various fixed effects discussed below. η_{it} is an error term. In our application, we replace quasi rents with value-added (VA_{it}) and control for capital-labor ratios, as quasi rents are defined as value-added minus capital costs and competitively priced total labor costs (we assume competitive capital markets and therefore a common interest rate). As highlighted in Card et al. (2018), a value-added-based estimate of γ equals the elasticity of wages with respect to quasi rents per worker multiplied by the value-added over quasi rents ratio. Hence, when comparing our results to quasi-rent-based results, one has to rescale γ with the value-added over quasi rents ratio. Following Card et al. (2014), we assume a value-added over quasi rents ratio of 2.

We estimate the model (2) in first differences ($\triangle x_{it} = x_{it} - x_{it-1}$) and control for various fixed effects to control for unobserved heterogeneity, including workers' outside options:

$$\triangle w_{it} = \beta_0 + \gamma_1 \triangle vadn_{it} + \beta X + \eta_{it}, \tag{3}$$

where wages (w_{it}) equal the logarithm of the wage bill per FTE and $vadn_{it}$ measures the logarithm of value-added per FTE. As German workers tend to be geographically immobile[12], we assume that changes in workers' outside

[12] E.g., Fackler and Rippe (2017) show that less than 4 percent of workers move

options have a strong regional component that we capture by including $region \times year$ fixed effects in the vector of control variables X. We further include industry fixed effects to capture industry wage trends and control for firms' predetermined energy intensity and capital intensity.[13]

The coefficient of interest, γ_1, measures workers' relative bargaining power, i.e., the rent-sharing elasticity. OLS estimates of γ_1 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. We set the shares to 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.[14] Formally, the Bartik instrument is:

$$\triangle EI_{it} = \sum_{s=1}^{S} \triangle pe_{st} e_{is0}. \tag{4}$$

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

outside a 40 km radius around their home over a five-year time span.

[13] 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 A.3), we even add firm fixed effects controlling for firm-specific wage trends.

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

correlated with labor productivity. As we discuss in the next section, the first-stage coefficient on the instrument is highly statistically significant and has the expected sign.[15]

The second condition is that the instrument is strictly exogenous conditional on covariates. We include industry fixed effects to control for industry-specific components of wage changes. We observe national prices for electricity and gas and must account for differences in the fees of gas and electricity networks, which vary regionally and over time due to differences in legislation, network coverage, investments, age, and quality of the regional gas and electricity network. Therefore, our region $\times$ year fixed effects ensure that we only compare firms operating in the same region in the same year.[16]

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

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15 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.

16 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. Our results are robust to controlling for firms' predetermined total energy consumption

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.[17] In the formulation of GPSS, our setting is best described by its identification coming from energy shares as opposed to price changes.[18] 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 wage *changes*, such that wage *changes* are only affected by the instrument via its impact on productivity *changes*.[19] GPSS propose a series of diagnostics on the validity of the instrument that we run after presenting our baseline results.

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[17] 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.

[18] 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.

[19] 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 5.1 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 further reduce any potential impact of anticipation effects on our results.

5 Empirical Results

5.1 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.

If firms could immediately adapt their energy consumption mix to changes in relative prices, our instrument would be weak. The left panel of Figure 1 shows the log price development (normalized to one in 2003) for our main energy carriers.[20] 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.

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

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. 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 barely change their energy carrier composition over time (within-firm).

5.2 Baseline Regression Results

Before we move to our baseline estimates for the rent-sharing elasticity, we present evidence for the direct reduced-form effect of energy price changes on wages and then discuss the first-stage IV results. A discussion of potential threats to identification as well as a battery of tests recently proposed by GPSS to scrutinize the Bartik instrument will follow in Section 5.3.

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 on the Bartik instrument. These results provide 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 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. Quantitatively, the negative effect of energy price changes on labor productivity is substantial. An increase in energy prices by 10 percent reduces labor productivity by 1.1 percent.

Columns 3 and 4 report OLS and IV rent-sharing regressions as specified in Equation (3). 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.[21] Our estimates are

[21] 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

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 (2018). 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.

5.3 Checks for Identification

5.3.1 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' shares sheds light on whether there are any sizeable anticipation effects. Appendix Table A.1 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

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.

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 (Appendix Figure A.2), 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 A.2 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 small Rotemberg weights compared to electricity. In the following, we therefore 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 A.2, we regress initial-period energy carrier shares on 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 strong differences in firm characteristics by 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 A.3). This is above the rent-sharing elasticity of our baseline estimate but still within its 95 percent confidence interval.

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 A.4 provides results from the proposed limited information maximum likelihood (LIML) estimator (Column 3) and the 2SLS estimator (Column 2).[22] Instead of being combined in one Bartik IV, the instruments enter individually. 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 A.4 again do not suggest misspecification (p values > 0.2). Finally, in Appendix Table A.5, we report the first-stage 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) 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.

[22] 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.

5.3.2 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 FTEs instead of headcounts, which mostly accounts for potential changes in firms' average working time. 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.[23]

Although our main data and the SES plants can be merged via unique plant identifiers, there are severe limitations to the analysis of these merged data preventing us from using them as our main data set. 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 small, particularly when researchers seek to follow plants in the SES over multiple survey years. 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,

[23] The sample is representative of the population of German employees and contains plants of all size classes from all regions and industries in Germany. The SES is stratified according to size class, industry, and region. Importantly, it does not follow workers over time.

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 main data 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 A.6 reports summary statistics for our SES sample. 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).[24] 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 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

[24] 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.

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 A.7 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.

Overall, we thus find no evidence for energy shock-induced adjustment processes in terms of workforce composition and working time. Moreover, as increased worker churning would have reduced average tenure, our insignificant results for tenure indicate that there is no increased worker churning either. This implies that our findings of stable workforce compositions and unchanged working time are not masking any significant reshuffling of workers or changes in workforce compositions. These results do not come as a surprise, as the strict German employment protection legislation makes labor a difficult-to-adjust input factor.

5.4 Asymmetric Effects

We utilize positive and negative rent shocks to study asymmetric rent-sharing. Disentangling whether the results differ for positive and negative shocks is particularly important in our context because energy prices will most likely rise in coming decades in many countries (e.g., due to the green transition). If our results were exclusively driven by energy price reductions, they would be less informative in that context. Asymmetric rent-sharing is sometimes discussed with a specific focus on downward wage rigidity. In our context, this could be misleading, as we study wage growth instead of wage levels. A positive rent-sharing elasticity induced by energy price increases implies that wage *growth* is reduced in our setting, which may well be in line with downward rigidity in wage *levels*.

We start with reduced-form evidence. The first column of Table 5 shows reduced-form results for the subsample of observations with energy price reductions (positive shocks). The regressions include the same set of controls and fixed affects as our baseline specification.[25] We find a small and insignificant coefficient, highlighting that energy price reductions do not transmit into wages. 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 2 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.

Our full IV results are depicted in Columns 3 and 4 of Table 5. 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.

[25] Here, we fix the energy carrier shares to those of the first year the firm is observed in the data. The results are very similar when using the previous year's carrier shares.

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 do not benefit from energy price reductions.

5.5 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 2021, Wong 2021). The IV estimates in Table (4) 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. This implies that workers in large firms obtain a smaller fraction of overall firm rents. Consequently, to arrive at the same wage premium as their small-firm counterparts, productivity needs to be approximately 50 percent higher in large firms.

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.[26] Table A.8 (OLS) and Table A.9 (IV) show that rent-sharing elasticities indeed almost monotonically decrease with firm size.

There are several explanations for these 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 (Azar

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

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. Finally, the difference in rent-sharing can also partly be explained by the German industrial relations system. Wages in the German economy, and in particular in the manufacturing sector, are often bound to collective wage agreements between employer associations and unions. Collective agreements are much more common in large firms, e.g., because the transaction cost advantages of such contracts over individualized bargaining increase with firm size. These agreements set minimum wages and thereby 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.[27] Gürtzgen (2009) confirms weaker rent-sharing in firms with an industry-wide collective wage agreement.

6 Discussion and Conclusions

This study presents causal evidence on the rent-sharing elasticity of German manufacturing firms that is based on firm-level variation in rents induced by energy price variation. We develop a novel Bartik instrument for firm rents combining the predetermined firm-level energy mix with nationwide

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

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 elasticity 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 0.2 elasticity at this 90-10 productivity gap yields a between-firm wage variability of 46 percent.

We document that rent-sharing elasticities monotonically decrease with firm size. Firms having fewer (more) than 100 employees have an elasticity of 0.26 (0.17). Hence, workers in large firms obtain a smaller fraction of the overall firm-level rents, and to arrive at the same wage premium as their small-firm counterparts, productivity needs to be 1.5 times higher in large firms. This is on the order of magnitude of one standard deviation of labor productivity in our data. We discussed potential reasons for the smaller rent-sharing elasticity in large firms, including market power, worker preferences for nonwage amenities, and the centralized German wage setting system.

Finally, we show that rent-sharing induced by energy price variation is asymmetric and driven by energy price increases. Energy price reductions do not lead to wage gains, whereas energy price increases are passed through to reduced wage growth. In an environment of rising energy prices, for instance, due to carbon taxes, this 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. It is generally expected that increasing energy costs will reallocate market shares away from energy-intensive producers. Our findings imply that part of this reallocation effect will come from workers leaving energy-intensive firms.

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Tables

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
Log(Capital stock per FTE)	11.14	0.886	10.03	10.59	11.17	11.72	12.29
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
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.

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.

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 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' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

Table 4: 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. All regressions are in first differences and control for firms' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

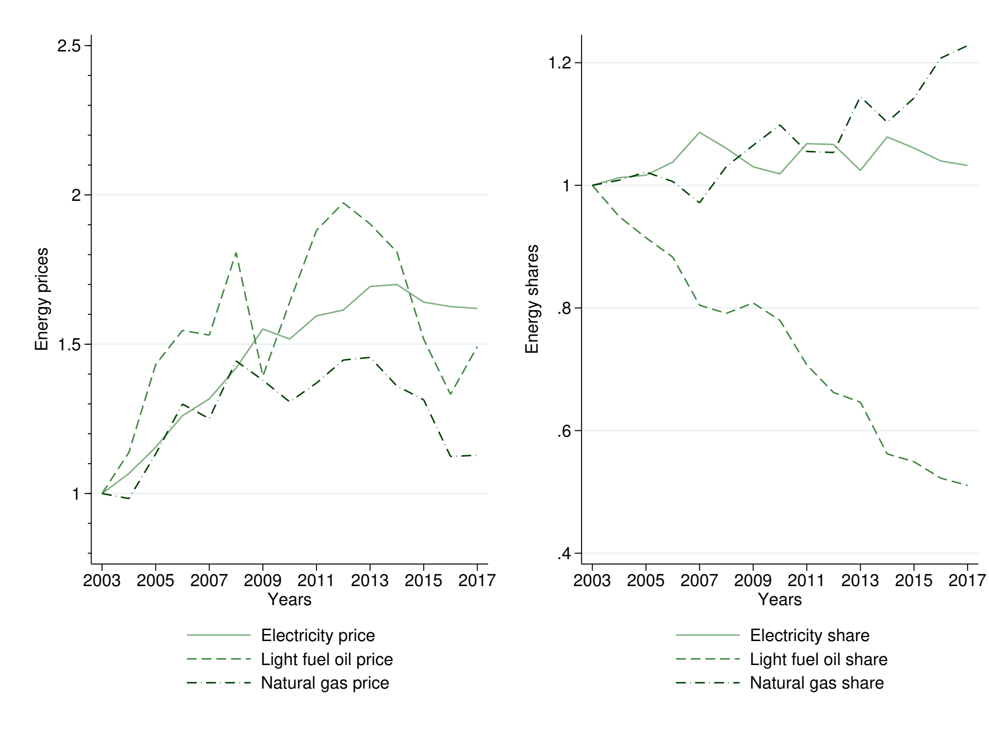
Table 5: Asymmetric effects - reduced form and rent-sharing

	Reduced form		Rent-sharing	
	falling price	rising price	falling price	rising price
	(1)	(2)	(3)	(4)
Bartik instrument	0.003	-0.034		
	(0.01)	(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' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

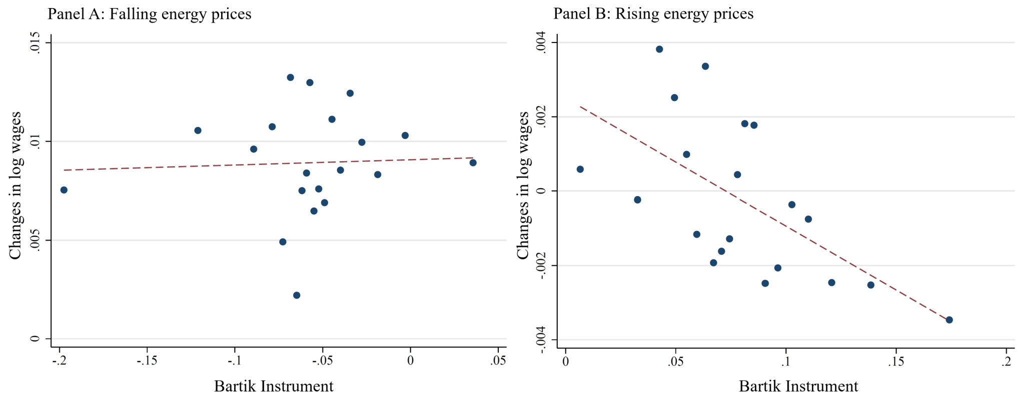
Figures

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. Prices were converted to kWh per Euro using conversion tables from the Federal Ministry for Economic Affairs and Energy.

Figure 2: Wage and energy price changes

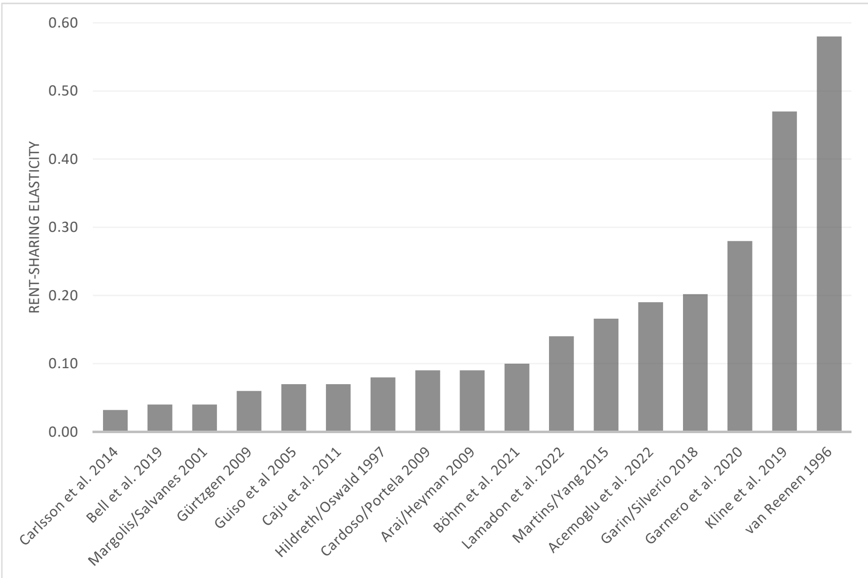


Notes. Binned scatter plots, AFiD Panel 2003–2017. Panel A (B) plots changes in log wages against changes in our firm-level Bartik instrument for falling (rising) energy prices. Consistent with our regression analysis, we control for firms' predetermined energy intensity and capital intensity as well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

Appendix

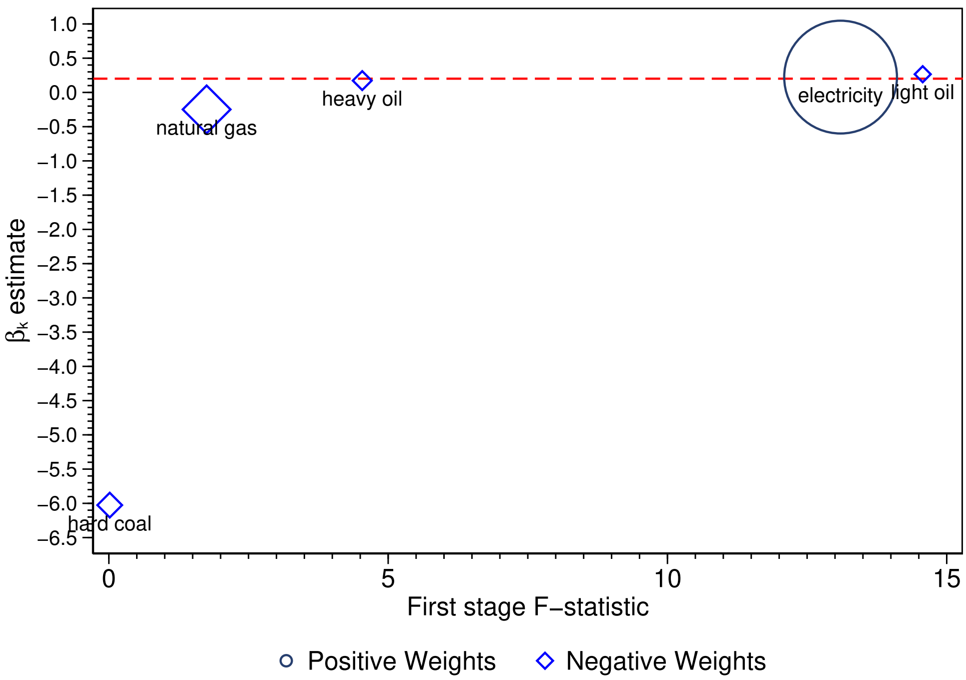
A Additional Material

Figure A.1: Causal estimates of rent-sharing elasticities



Notes. The graph shows estimates of rent-sharing 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. 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.

Figure A.2: 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 A.1: 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' predetermined energy intensity and capital intensity. The energy carrier weights for the Bartik instrument are defined in the previous year for every firm. Standard errors are clustered at the firm level and reported in parentheses.

Table A.2: Relationship between energy shares and firm characteristics

	Electricity	Light oil	Natural gas	Hard coal	Heavy oil
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.

Table A.3: Rent-sharing, OLS and IV regressions with firm fixed effects

	OLS	IV
	(1)	(2)
Value-added per FTE	0.138	0.273
	(0.004)	(0.079)
Firm fixed effects	yes	yes
Region × year fixed effects	yes	yes
Reference year of instrument	–	fixed
R-squared	0.407	0.336
1^{st} stage F-Stat of instrument		23.63
Firm-year observations	92,289	92,289
Number of Firms	18,420	18,420

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' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

Table A.4: 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' predetermined energy intensity and capital intensity. 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.

Table A.5: 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 $\times$ 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 A.4. The regression is in first differences and includes controls for firms' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

Table A.6: 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 A.7. Worker-level statistics refer to statistics based on the observed worker-firm matches (160,392 in total).

Table A.7: Adjustment of workforce composition, linked employer-employee data (SES)

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

Notes. SES data 2006, 2010, 2014. The table shows the results of regressing our Bartik instrument on firm-level workforce characteristics using 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.

Table A.8: 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)
Year fixed effects	yes	yes	yes	yes	yes
Region 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' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

Table A.9: Rent-sharing by firm size, IV regressions

	<50	<100	<150	<250	All firms
	(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)
Year fixed effects	yes	yes	yes	yes	yes
Region 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

Notes. AFiD Panel, 2003–2017, single-plant firms. The table reproduces our baseline rent-sharing regressions using IV for firms of different size. We instrument logged labor productivity with our Bartik instrument. Column headings indicate the number of employees in the firm sample. All regressions are in first differences and control for firms' predetermined energy intensity and capital intensity. Standard errors are clustered at the firm level and reported in parentheses.

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