

Competition, Management, and Productivity: Evidence from Protected German Coal Mines^{*}

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Abstract

Policies designed to protect domestic industries are resurging worldwide. We study a German policy that required steel producers to source coking coal and coke exclusively from domestic mines, effectively reducing import competition for coal mines. Using a difference-in-differences design that exploits differences in mines' pre-policy exposure to coke production, closely tied to the geological suitability of their coal deposits for coking, we find that the policy increased quantity-based total factor productivity (TFPQ) by 4–5%, raised markups by 13–14%, and reduced marginal costs by 3–4%. Using matched manager–mine data, we rationalize the efficiency gains through a managerial channel. More-exposed mines recruited more STEM-trained managers and managers with higher estimated ability. Such managerial hirings were followed by increased mechanization, investment, and TFPQ as well as lower marginal costs.

Keywords: Managers, Productivity, Technology Adoption, Industrial Policy

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1 Introduction

Competition is often viewed as an important driver of productive efficiency. A large literature in industrial organization and international economics has found that stronger competition can improve firm performance by disciplining producers, reducing slack, and reallocating resources toward more efficient firms (Olley and Pakes, 1996; Pavcnik, 2002; Amiti and Konings, 2007; De Loecker et al., 2016; Brandt et al., 2017; Backus, 2020). Yet governments frequently shield domestic firms from foreign competition through protective policies aimed at supporting infant industries, maintaining strategic capacity, or stabilizing mature sectors (e.g., Juhász et al., 2024). Such protection can weaken competitive discipline and reduce efficiency. This, however, need not hold if competition affects productivity not only through effort, selection, and reallocation, but also through the allocation of managerial talent. Stronger competition can improve management quality (Bloom and Van Reenen, 2007; Van Reenen, 2011; Bloom et al., 2015, 2016b), but it may also compress rents, raise downside risk, and reduce the attractiveness of firms to high-quality managers (Schmidt, 1997; Goldfarb and Xiao, 2011; Bao et al., 2026). Protective policies may therefore affect productivity not only by changing competitive pressure in product markets, but by altering which firms can attract managerial talent.

In this paper, we show that reduced competition can raise establishment-level productivity when declines in competition are accompanied by improvements in management quality. Our empirical setting is the vertical relationship between the German coal and steel industries during the 1960s and 1970s. We study the *Hüttenvertrag*, a protective policy that required downstream German steel producers to source coking coal and coke, a fuel produced from coking coal in cokeries, exclusively from domestic suppliers. This sourcing mandate sharply curtailed access to foreign suppliers and thereby reduced the potential import competition faced by domestic producers of coking coal and coke. Using detailed production data as well as matched manager–mine data, we estimate the policy’s effects on mine-level productivity, managerial hiring and management-team composition and provide evidence consistent with managerial upgrading as an important adjustment channel.

We exploit cross-mine variation in pre-policy exposure to coke production. We measure exposure using the pre-policy share of a mine’s coal output sent to coke production and, alternatively, an indicator for whether the mine operated an attached cokery. Approximately half of the mines operated a cokery. Exposure was closely related to predetermined geological differences in the suitability of coal deposits for coking. The difference-in-differences design compares changes in outcomes across mines with different levels of pre-policy exposure over

time. Event-study estimates show no differential pre-policy trends in the main outcomes.

The German coal mining industry provides an ideal setting to study the interaction of competition, management, and productivity. First, the industry underwent substantial technological change and consolidation during our study period. These changes made managerial decisions central to mine performance and managers' choices of employer consequential for their career prospects. Second, the restriction on foreign sourcing constituted a sizable change in competitive conditions because little competition remained in the highly concentrated domestic market. Third, we digitize historical records to construct a dataset combining rich information on managers with detailed mine-level data on production and technology use. We observe physical inputs and outputs at coal mines, allowing us to estimate quantity-based total factor productivity (TFPQ) (Foster et al., 2008). This measure enables us to isolate changes in productive efficiency from confounding price variation associated with market power, buyer power, or product quality.¹ Finally, coal is relatively homogeneous, further limiting concerns about confounding variation in product quality or demand shocks across product groups.

We begin by documenting that the policy generated persistent gains in productive efficiency at more-exposed mines relative to less-exposed mines. TFPQ increased by 4–5%, while marginal costs declined by 3–4%. The absence of a statistically detectable response in average wages, together with the TFPQ gains, suggests that the decline in marginal costs primarily reflected improved productive efficiency rather than lower labor costs. By 1980, the estimates imply an approximately 20-percentage-point increase in the probability of continued operation at more-exposed mines relative to less-exposed mines. The productivity and survival gains were concentrated among mines with low pre-policy TFPQ, and the distributional estimates indicate a narrowing of within-industry productivity dispersion. At the same time, output prices increased by 10% and markups by 13–14%, indicating that the efficiency gains were not passed through to downstream buyers. A comparison with the Belgian coal industry provides complementary descriptive evidence consistent with aggregate productivity gains in Germany rather than a redistribution of activity across German mines.

More-exposed mines also reorganized production towards more extraction-focused work and upgraded their technological infrastructure. The share of underground shifts allocated to coal extraction increased by 2–3 percentage points (or 4–5%). The share of production extracted using mechanized methods increased by 4–5 percentage points (7–8%) and the share of installed electrical machinery power by 4–5 percentage points (6–8%).

¹This approach differs from most studies of import competition, which rely on revenue-based productivity measures (Amiti and Konings, 2007; Topalova and Khandelwal, 2011; Bloom et al., 2016a; Brandt et al., 2017; Chen and Steinwender, 2021).

Using rich matched manager–mine data, we identify the managerial channel as a key mechanism underlying the productivity gains. We show that, at mines operating cokerries, the policy (i) induced the hiring of new managers shortly after the policy implementation, and (ii) persistently altered the composition of management teams. These mines hired more managers with STEM backgrounds, higher ability, and experience at more productive prior mines. Several newly hired managers, however, were not recruited from the management boards of other coal mines, implying also a substantial inflow of external managers. Exploiting the timing of these hires in an event-study design, we find that newly hired managers increased mechanization and markups while reducing marginal costs. Using a causal mediation analysis following [Schenk \(2025\)](#), we show that these managerial changes account for the majority of the policy-induced increases in productivity. In contrast to the importance of changes in the manager board, we find little evidence for within-manager improvements due to the policy. Alternative mechanisms, including returns to scale and changes in excess input use, also appear to play a negligible role.

Related Literature. Our paper contributes to three strands of the literature. First, we connect research on how competition affects management ([Schmidt, 1997](#); [Raith, 2003](#); [Bloom et al., 2015](#); [Hau et al., 2020](#); [Antón et al., 2023](#); [Bao et al., 2026](#)) with work on how management affects productivity and organizational performance ([Ichniowski et al., 1995](#); [Bennedsen et al., 2007](#); [Bloom and Van Reenen, 2007](#); [Sraer and Thesmar, 2007](#); [Bloom and Van Reenen, 2010](#); [Crossland and Hambrick, 2011](#); [Quigley and Hambrick, 2015](#); [Bloom et al., 2016b, 2020](#); [Fenizia, 2022](#); [Rubens, 2023](#); [Adhvaryu et al., 2022](#); [Koren and Orban, 2025](#); [Koren et al., 2025](#); [Demirer and Karaduman, 2026](#)). Competition may improve efficiency by disciplining managers, but it may also reduce profits and managerial compensation, potentially making firms less attractive to high-quality managers. Its net effect on productivity is therefore theoretically ambiguous. We show that reduced competition can increase productivity by reshaping managerial composition, especially in establishments with low initial productivity. This finding differs from earlier work documenting improvements in management following increases in competition (e.g., [Bloom et al., 2015](#); [Hau et al., 2020](#); [Kotia, 2026](#)). [Van Reenen \(2011\)](#) has pointed out that management quality is a channel that links competition and productivity through within-firm and within-manger changes in managerial practices and its between-firm transferability. We empirically distinguish between these two channels and find the allocation of managers across establishments to be most important in our setting.

Second, we contribute to the literature on import competition and its effects on productivity and technology adoption (Grossman, 1982; MacDonald, 1994; Amiti and Khandelwal, 2013; Autor et al., 2013; Bloom et al., 2016a; Autor et al., 2020; Amiti and Heise, 2024). This literature often finds that greater import competition and trade liberalization increase revenue-based productivity through access to cheaper inputs (Amiti and Konings, 2007; Goldberg et al., 2010; Halpern et al., 2015), stronger incentives to innovate (Aghion et al., 2005; Bloom et al., 2016a), and market-share reallocation and firm exit (Pavcnik, 2002; Trefler, 2004). However, the evidence is not uniformly positive, with some work also finding that the relationship is context-dependent (Shu and Steinwender, 2019). In our paper, we show that reduced competition from imports can increase productivity through changes in management. Our design uses an establishment-level measure of exposure to the import competition shock, exploiting variation within a single industry rather than relying on conventional measures derived from import flows. We also estimate quantity-based productivity at the mine level, which helps distinguish changes in productive efficiency from price variation associated with product-market power, buyer power, or product quality. More broadly, our findings suggest that managerial composition is an important margin through which changes in competitive conditions affect productivity.

Finally, we connect to the growing literature on industrial policy,² which has focused primarily on place-based policies (Becker et al., 2010; Kline and Moretti, 2014; Ehrlich and Seidel, 2018; Heblich et al., 2026; Siegloch et al., 2025), subsidy-intensive interventions (Criscuolo et al., 2019; Cerrato, 2024; Barwick et al., 2025; Siegloch et al., 2025), and policies targeting infant rather than declining industries as in our case (Juhász, 2018; Kim et al., 2021; Manelici and Pantea, 2021; Lane, 2022; Barwick et al., 2024; Incoronato and Lattanzio, 2024). Yet the mechanisms through which industrial policies operate are often unclear, and the extent to which findings from particular settings generalize more broadly remains uncertain (Juhász et al., 2024). Our paper identifies a mechanism that may be relevant across a wide range of industries: industrial policy can alter the allocation of managerial talent across firms and thereby affect productivity.

The remainder of the paper proceeds as follows. Section 2 describes the industry, the policy, and the data. Section 3 presents the empirical strategy. Section 4 reports the main results, and Section 5 examines the managerial mechanism underlying the effects. Section 6 reports the robustness checks, and Section 7 concludes.

²While some studies define industrial policy as a means to transform economic activity or reshape the industrial landscape (e.g., Juhász et al., 2024), the policy we study aims to sustain an existing industry and foster improvements within it.

2 German Coal Mining Industry and Data

2.1 Industry and Policy Background

Coal Mining and Coke Production in Germany: 1960s and 1970s. Coal was a major source of energy for heating, transportation, and electricity generation, as well as the principal input into coke production. Coke, in turn, is an essential intermediate input in steelmaking and is produced by processing suitable grades of coal in cokeries. Panel A of Figure 1 reports the allocation of German coal consumption across end uses in 1960 and 1970. Coke production accounted for 40% of total coal consumption. Approximately half of all coal mines operated a vertically integrated cokery, and by 1969, mine-owned cokeries produced roughly 80% of German coke (Gatzka, 1990).

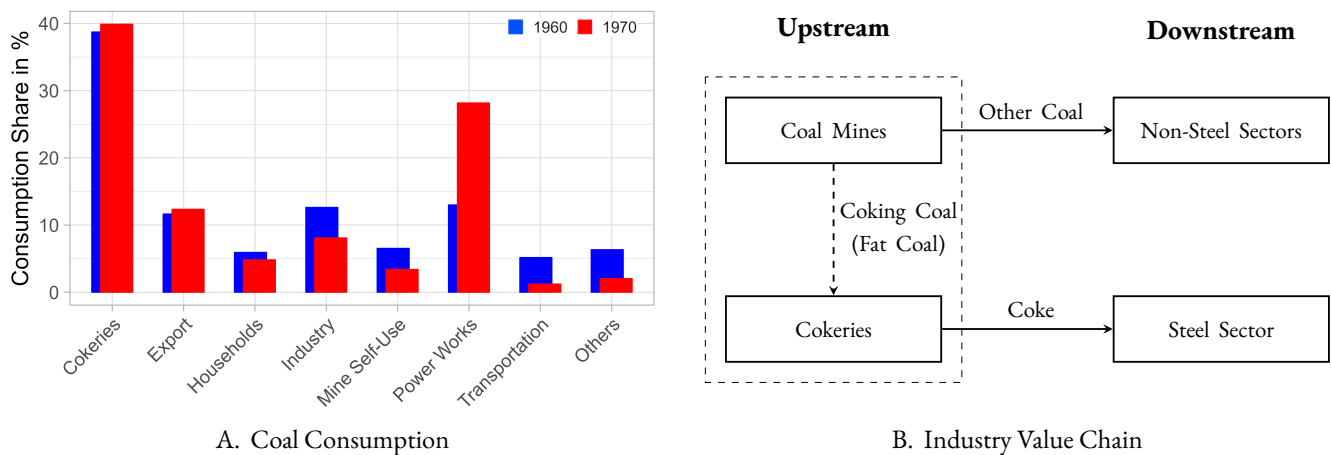


Figure 1: Coal Consumption and Industry Structure

Notes: Panel A reports the distribution of German coal consumption across end uses in 1960 and 1970. Panel B depicts the vertical structure of the coal and steel industries; the dashed frame indicates the typical vertical integration of coal mines and cokeries. The data in Panel A are drawn from the statistical sections of the *Yearbooks of the German Mining Industry*.

Panel B summarizes the industry's vertical structure. Mines could either sell extracted coal directly to downstream users or process coal suitable for coking, typically fat coal, into coke in a vertically integrated cokery for sale to the steel industry. Mines and their affiliated cokeries were not vertically integrated with steel producers, following the Allied-mandated post-WW II breakup of the large coal–steel conglomerates in 1953 (Allied Control Council, 1950). At its peak in the late 1950s, the West German hard-coal industry employed over 500,000 workers, making it the largest coal industry in continental Western Europe. Germany also possessed some of Europe's richest hard-coal deposits (Gebhardt, 1957; Böhmert, 1988; Nonn, 2001).

Coal Mining under Pressure. Already beginning in the late 1950s, the German coal industry faced two major sources of competitive pressure. The government first responded to low-priced imported coal by introducing import quotas and tariffs in 1957 (Böhmert, 1988; Fischer, 2026a). A second challenge arose from imported oil. Improved diplomatic relations with oil-producing countries and declining transportation costs increased its availability and reduced its price, making oil an increasingly important substitute for coal (Nonn, 2001; Fritzsche and Wolf, 2026; Fischer, 2026a). As shown in Appendix Figure A1, oil increasingly displaced coal in the German energy mix during the 1960s. Between the mid-1950s and the mid-1970s, oil consumption increased tenfold, while coal consumption fell by half despite continued growth in aggregate energy consumption. This substitution margin was confined to non-coking uses of coal. Steel production continued to require coke, which oil could not replace because it could not generate the carbon monoxide levels required in blast-furnace operations.

Against this backdrop, German coal output declined by 25% between 1960 and 1970, while both employment and the number of mines fell by approximately half (Jopp, 2021, 2023). In response, the government adopted a series of industrial and social policies in the late 1960s and 1970s to slow structural adjustment and limit mass layoffs. A principal intervention was the 1969 coal and steel agreement, the *Hüttenvertrag*, which required German steel producers to source coking coal and coke domestically, thereby largely foreclosing foreign sourcing and reducing import competition faced by domestic mines and cokeries.

The *Hüttenvertrag*: Domestic Sourcing and Import Competition. Unlike non-coking coal, coking coal and coke were relatively insulated from the shift to oil, which could not replace them in steel production. By the late 1960s, however, domestic producers of coking coal and coke faced increasing competitive pressure from lower-priced foreign suppliers. Figure 2 shows that the available foreign price benchmarks for coking coal remained below the German list price throughout the late 1960s and 1970s.³ Although the recognized high quality of German coking coal partly mitigated this price disadvantage (Federal Reserve System, 1923; Der Spiegel, 1971), lower foreign prices led German steel producers to consider substituting imported coke and, especially, coking coal for domestic inputs (Gatzka, 1990). Foreign sourcing therefore represented a relevant outside option for German steel producers.

In 1969, the government introduced the *Hüttenvertrag* (coal and steel agreement) to prevent lower-priced foreign inputs from displacing domestic supplies (Farrenkopf, 2021; Gatzka, 1990; Zydek, 1973). The agree-

³For 1965–1968, we use the U.S. coking-coal price as the foreign benchmark because a consistent world-market price series is unavailable for these years.

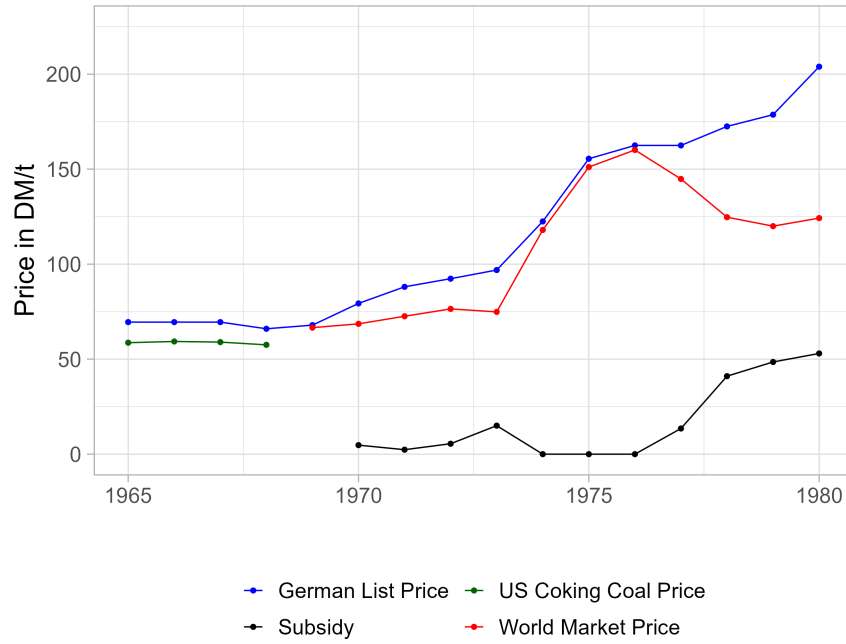


Figure 2: Coking Coal Prices and the *Hüttenvertrag* Subsidy

Notes: The figure reports the German list price of coking coal, the U.S. coking-coal price, the world-market price, and the per-tonne subsidy under the *Hüttenvertrag*. The U.S. price is used as the foreign benchmark for 1965–1968 because a consistent world-market price series is unavailable for these years. All series are measured in Deutsche Marks (DM) per tonne. Data come from [Federal Statistical Office of Germany \(1970\)](#) and [Gatzka \(1990\)](#).

ment required German steel producers to purchase coking coal and coke from domestic mines. It also prohibited domestic mines from purchasing coking coal or coke at world-market prices and reselling these inputs to steel producers in place of domestic production, thereby closing a potential route around the domestic-sourcing requirement. All mines owned by *Ruhrkohle AG*, which accounted for approximately 85% of the German coal market, were covered by the agreement. Moreover, the contract had a 20-year term, making the sourcing restriction persistent. Taken together, these provisions largely removed foreign suppliers from steel producers' feasible sourcing set and thereby reduced the potential import competition faced by domestic producers of coking coal and coke.

Because domestic coking coal and coke remained more expensive than foreign alternatives, the state almost fully compensated the difference between German and world-market prices ([Engels et al., 1988](#); [Neu, 1995](#)). This compensation limited the cost to steel producers of complying with the domestic-sourcing requirement without restoring access to foreign suppliers. As Figure 2 shows, the per-tonne subsidy became quantitatively important in the second half of the 1970s, when it exceeded 25% of the German list price. At the same time,

restricting access to foreign suppliers weakened steel producers’ outside option in an already highly concentrated domestic coal market. The contract therefore included safeguards intended to constrain domestic mines’ ability to exercise market power.⁴ Thus, the central policy margin relevant to our analysis was a durable restriction on foreign sourcing rather than an explicit expansion of aggregate steel demand. This protection was most directly relevant to mines supplying coking coal or operating cokerries, motivating the measures of policy exposure used in the empirical analysis.

Role of Managers. Coal-mine managers (*Grubendirektoren* and *Bergwerksdirektoren*) were senior employees who directed mines but did not own them. A mine in our sample employed, on average, two such managers per year. They delegated day-to-day operations to second-tier operational directors (*Betriebsdirektoren*) while retaining supervisory authority over operating decisions, the allocation of labor and machinery, and investment.

Managerial decisions were particularly consequential during our sample period, which coincided with rapid technological change in German coal mining. Between 1960 and 1980, the share of mechanized production nearly doubled, the share of machinery power supplied by electricity rose from 54% to 89%, and the share of mechanized mining points increased from 26% to 96%.

Managers also differed substantially in their educational backgrounds. Some were trained in economics, business, or law, while approximately half held a diploma or higher degree in engineering. More than 30% held a mining-specific qualification, such as a degree in mining engineering, and approximately 25% held a PhD. Managers were also mobile across establishments: roughly one-third of managerial hires involved a move from another mine. Together, this heterogeneity in expertise and cross-mine mobility suggests that managerial movements may have facilitated knowledge transfer across establishments. These features motivate our analysis of managerial composition as a potential channel underlying policy effects.

2.2 Data

Production and Price Data. We digitized annual mine-level production records for 1960–1980 from the *Establishment Statistics* of *Statistik der Kohlenwirtschaft e.V.* and the *Yearbooks of the German Mining Industry*.⁵

⁴The agreement tied coke prices to the list prices charged to German non-steel customers and foreign customers required coal firms to incorporate potential price differences to independent cokerries, permitted the government to monitor cost data when necessary (Gatzka, 1990), and provided for arbitration.

⁵Postwar production data from the *Yearbooks of the German Mining Industry* have also been used in previous research; see, for example, Jopp (2021), Jopp (2023), Fischer (2026a), and Fischer (2026b).

We restrict the sample to mines owned by firms that signed the *Hüttenvertrag*. The excluded mines did not operate cokeries and were located primarily in the smaller and more remote coal districts of Ibbenbüren, Aachen, and Saar. These districts lie outside the Ruhr district, Germany’s main coal-producing region. We also drop observations for mines subject to partial closure under the *Rationalisierungsverband des deutschen Steinkohlenbergbaus*, a closure program introduced in 1963, to avoid attributing program-induced output reductions to the *Hüttenvertrag*. These restrictions leave 87 of the 101 German coal mines, accounting for around 80% of total German coal production during the study period. We further exclude mine-year observations covering only part of a year, primarily closure years, because employment is typically measured at the end of the calendar year. Figure 3 displays the locations of the sample mines by cokery status and Table B1 gives summary statistics.

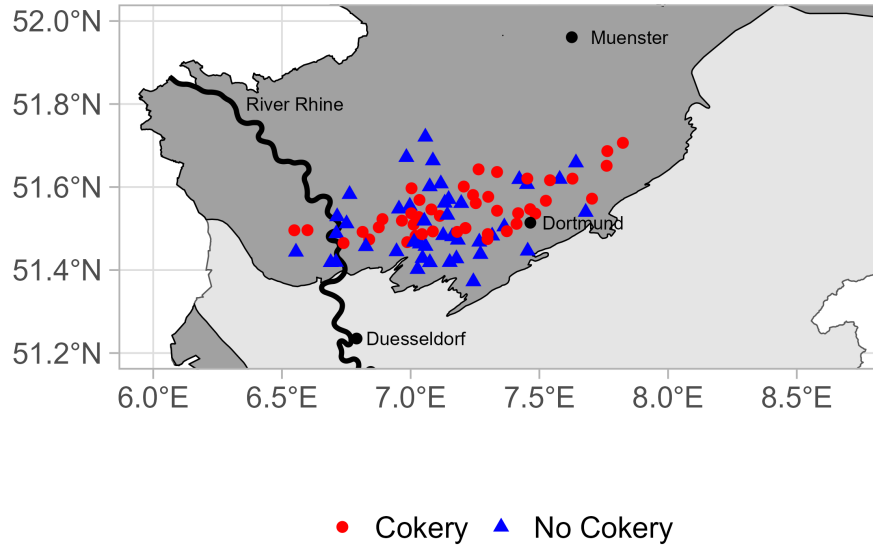


Figure 3: Locations of Sample Mines by Cokery Status

Notes: The figure shows the locations of the sample mines by cokery status. The dark-gray areas indicate where coal deposits occur at depths of up to 2,000 meters. The locations of the cities Düsseldorf, Dortmund, and Münster are shown for reference. The administrative boundaries are those of the German state of North Rhine-Westphalia (NRW) in the west of Germany bordering the Netherlands and Belgium. The shapefiles for the NRW boundaries, coal deposits, and the Rhine are from [Information und Technik Nordrhein-Westfalen \(2023\)](#), [Geologischer Dienst Nordrhein-Westfalen \(2023\)](#), and [Generaldivision Wasserstraßen und Schifffahrt \(2021\)](#), respectively.

The dataset contains detailed measures of physical inputs and outputs to estimate mine-level TFPQ and track changes in the production process around the policy. Input prices, including average wages, are also available. We observe mechanization and electrification rates to study technology adoption. Additional mine-year outcomes include worker flows, turnover, and measures of mine infrastructure. Finally, we collect annual output prices by coal and coke type from the *Statistical Yearbooks of Germany*. We construct a mine-year output

price by weighting coal- and coke-type-specific industry-year prices by mine-year-level production shares. We also observe whether each mine operates an attached cokery. For these cokeries, the data include physical inputs and outputs, employment, the number of ovens, and total capacity measured in tonnes.

Manager Data. We compile annual rosters of mine and cokery managers for every establishment-year in our sample from the *Yearbooks of the German Mining Industry*. The resulting matched manager–mine dataset records each manager’s name and educational background, including field of study and degree. Appendix Table B2 summarizes the characteristics of the 313 managers in our sample. All managers are male; 26% hold a PhD, and 51% have a STEM background. The average manager is observed for 9 years. The roughly 100 managers who move from, to, and between mines have characteristics similar to those of the full sample and are the focus of our switcher analysis. Because the records often do not specify managers’ responsibilities within integrated mine-cokery establishments, we treat all managers as responsible for the entire entity.

3 Empirical Strategy

3.1 Empirical Specification

We first focus on identifying the pooled policy effects before digging into the mechanisms at a later stage. We study the policy effects by comparing cokery and non-cokery mines over time. Specifically, we estimate the consequences of the policy using the difference-in-differences specifications below:

$$Y_{it} = \beta Exposure_i \times Post_t + \alpha_i + \lambda_t + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the outcome variable for coal mine i in year t . $Exposure_i$ is a measure of the magnitude of exposure to the policy for each coal mine, and $Post_t$ is an indicator that equals one after 1969. α_i and λ_t refer to mine- and year-fixed effects, respectively. The coefficient β is the primary parameter of interest.

Since the foreign-sourcing restriction applied specifically to coking coal and coke purchased by steel producers, mines whose pre-policy operations were more closely tied to these products experienced a larger reduction in exposure to foreign competition. We construct two different specifications for $Exposure_i$. First, we calculate a mine’s share of quarried coal that is used for coke production ($[\%Coking\ Coal]_i$). We use the weighted average

over all pre-policy years to account for long-run exposure. Using only pre-policy years takes into account that post-policy values might already be a policy outcome. Second, we employ a binary treatment based on whether a mine has a cokery attached or not pre-policy ($\mathbb{1}[\text{Attached Cokery}]_i$). Table 1 summarizes both exposure measures and their variation. Half of all mines have a cokery attached. The average share of produced coal going into coke production is 25.5%. We winsorize the continuous exposure measures at the 95th percentile to avoid distant outliers to largely drive our results.

Table 1: Exposure Measures

	N	$\mathbb{1}[> 0]$	Min	p25	Mean	p75	Max
$[\% \text{Coking Coal}]_i$	87	44	0	0	0.255	0.514	0.844
$\mathbb{1}[\text{Attached Cokery}]_i$	87	44	0	0	0.506	1	1

Selection Into Treatment. Since policy exposure might not be randomly assigned, we examine whether it is systematically related to observable pre-policy mine characteristics. Specifically, we regress each standardized exposure measure on a common set of characteristics capturing productivity, scale, production technology, operating conditions, infrastructure, and geology. We use only pre-policy observations to avoid conditioning on characteristics that may themselves respond to the policy. Appendix Figure A2 reports the resulting coefficient estimates, with the dependent exposure measure indicated in each panel heading. The estimates provide little evidence that exposure is systematically related to pre-policy TFPQ, mine size, or production technology. The notable exception is the fat coal indicator, which captures access to the coal type most suitable for coke production. Because fat coal deposits are predetermined by geology, we interpret this pattern as suggestive evidence that geological endowments largely determine cokery status.

Two additional features limit concerns about selection into treatment. First, the median (mean) establishment year among sample mines was 1875 (1879), approximately nine decades before the policy’s introduction in 1969, and only two sample mines were established after World War II. These dates limit concerns that mine entry or location choices responded to the market conditions and available coal deposits in the coal fields surrounding the policy’s introduction. Second, consistent with the substantial fixed costs of cokery construction, no sample mine constructed a new cokery after the policy was introduced. Thus, post-policy entry into attached-cokery status does not arise in our sample.

Dynamic Effects. To provide evidence that mines with different exposure magnitudes had similar trends before the policy, we implement event studies by replacing $Post_t$ with a series of year dummies in Equation (1):

$$Y_{it} = \sum_{\tau=1960, \neq 1969}^{1980} \beta_{\tau} \times Exposure_i \times 1[Year = \tau]_t + \alpha_i + \lambda_t + \varepsilon_{it}. \quad (2)$$

The coefficients β_{τ} capture differences in the outcome variable among mines with different exposure magnitudes in year τ relative to the omitted year of policy introduction, 1969. We will plot the estimates of β_{τ} to exhibit the evolution of corresponding dependent variables relative to the policy introduction. We will rescale the y-axis to make the effect size of both exposure analyses comparable. To do so, we multiply the estimated coefficients of each regression with the mean of the exposure variable of mines with strictly positive exposure. Hence, estimates of the continuous exposure measure can be interpreted as the effect of moving a mine from zero exposure to the mean exposure of mines with strictly positive exposure.

Identification. A causal interpretation of the estimated policy effects requires three assumptions. First, the parallel-trends assumption requires that, absent the policy, outcomes at mines with different levels of pre-policy exposure would have evolved in parallel. We assess the plausibility of this assumption using the pre-policy coefficients in Equation (2). The absence of discernible differential pre-trends supports, but does not establish, the identifying assumption.⁶ As Callaway et al. (2024) show, identification with a continuous treatment such as $[\%Coking\ Coal]_i$ generally requires stronger restrictions on counterfactual trends across exposure levels than a binary-treatment design. We therefore also estimate the model using $1[Attached\ Cokery]_i$. Although this specification identifies a coarser treatment contrast, it provides a complementary robustness check that relies less on variation across the full distribution of continuous exposure.

Second, the stable unit treatment value assumption (SUTVA) requires that a mine’s potential outcomes do not depend on the exposure or post-policy responses of other mines. Because cokery capacity was largely predetermined and constructing or expanding a cokery took several years, strategic changes in cokery status around the policy introduction are unlikely. This predetermined status does not, however, rule out economic spillovers. In particular, less-exposed mines may have responded by expanding their production of coking-suitable coal for sale to independent cokeries. We therefore interpret the estimated coefficients as the effects of greater policy

⁶The absence of differential pre-trends also mitigates concerns that policy exposure was selected in response to pre-policy changes in outcomes, although it cannot rule out such concerns in general.

exposure relative to lower exposure, rather than as the policy’s total industry-wide effects. If such spillovers affected less-exposed mines in the same direction as more-exposed mines, but by less, they would attenuate the estimated exposure gradient toward zero. Under these conditions, our estimates provide conservative estimates of the differential policy effects. We examine this possibility in Section 6.

Third, identification requires the absence of contemporaneous shocks whose incidence varies systematically with policy exposure. Aggregate shocks common to the coal industry are absorbed by year fixed effects; the relevant threat is another shock arising around 1969 that differentially affected mines according to $[\%Coking\ Coal]_i$ or $\mathbb{1}[Attached\ Cokery]_i$. Although the German coal industry was subject to several consolidation and subsidy policies from the 1950s through the 1980s (e.g., [Fischer, 2026a](#)), we are not aware of another contemporaneous intervention that specifically targeted mines according to their exposure to coke production.

3.2 Measure of Productivity, Markups, and Marginal Costs: Production Approach

As a main outcome of our analysis, we obtain quantity-based total factor productivity, i.e., TFPQ, by estimating a production function of coal mines using the control function approach, and subsequently estimate markups and marginal costs following [De Loecker and Scott \(2026\)](#).

Production Function Estimation. We assume that mines produce coal Q_{it} with a Leontief production technology using labor L_{it} , material M_{it} , and capital K_{it} .^{7,8}

$$Q_{it} = \min\{\kappa_{it}M_{it}, F(L_{it}, K_{it}; \beta) \exp(\omega_{it})\} \exp(\epsilon_{it}), \quad (3)$$

where κ_{it} is the inverse of a fixed per-unit input requirement for material, $F(\cdot)$ is a technology function across labor and capital with parameters β , and ω_{it} is the unobserved Hicks-neutral productivity shock. We allow for an unobserved term ϵ_{it} , which captures unobserved measurement error in output or unanticipated shocks to production.

⁷The production technology is assumed to be the same for fat and non-fat coal mines, as suggested by the mining science literature ([Fritzsche, 2013](#)). The extraction process is identical for both types of coal, only the final use differs.

⁸We follow [Fischer \(2026a\)](#) and [Fischer \(2026b\)](#) in assuming that each mine minimizes costs independently, even though mines are part of a multi-mine firm. This is motivated by two institutional features: First, mines operated with independent capital stocks, staff, and managerial boards and barely cooperated across mine borders. Second, most mines belonging to the quasi-monopolist *Ruhrkohle AG* used to operate as independent or smaller-scale businesses well into the 1960s.

Herein, capital is a dynamic input, while labor and material are flexible inputs.⁹ We assume capital is chosen in period $t - 1$, while labor and material are decided in period t . We take advantage of mine-level physical data for both input and output information, which leaves our specification free from the input/output price bias (Klette and Griliches, 1996; De Loecker and Goldberg, 2014). The Leontief function ensures that identification concerns in the presence of multiple flexible inputs do not apply (Gandhi et al., 2020). This benefit facilitates a consistent measure of the quantity-based total factor productivity, which purely reflects coal mines' efficiency levels, and purges out demand-side factors embodied in prices. Specifically, the output is measured in tonnes of coal. Labor is given by the number of workers (excluding workers in refinement plants), capital by the machine power in kW, and material by the amount of pitwood in cubic meters. Labor and capital are substitutable in the production process, but both are non-substitutable with pitwood, which is typically used for roof support in underground mining and is a key input for coal extraction.

To identify the coefficients of the production function, we follow the two-stage estimation procedure of Akerberg et al. (2015). Taking the logarithm of the production function in Equation (3), the estimating equation turns out to be:

$$q_{it} = f(l_{it}, k_{it}; \beta) + \omega_{it} + \epsilon_{it}, \quad (4)$$

where non-substitutable material is not part of the to-be-estimated production function directly. However, the Leontief structure of the production function implies a fixed-proportion rule in equilibrium between material and other inputs, i.e., $\kappa_{it} M_{it} = F(L_{it}, K_{it}; \beta) \exp(\omega_{it})$, which allows us to derive a simple control for unobserved productivity: $\omega_{it} = \ln(\kappa_{it}) + m_{it} - f(l_{it}, k_{it}; \beta)$. Substituting this into Equation (4) gives us the first-stage estimating equation: $q_{it} = \ln(\kappa_{it}) + m_{it} + \epsilon_{it}$.

Specifically, the first stage of the estimation procedure projects coal output on material input and a mine-time-specific material mix, κ_{it} , where we approximate it by a nonparametric function of inputs, wages, and other controls following De Loecker and Scott (2026). The first stage specification is given by:

$$q_{it} = \varrho(l_{it}, m_{it}, k_{it}, \mathbf{z}_{it}) + \epsilon_{it}, \quad (5)$$

⁹Labor is assumed to be a flexible input. Although there was almost full union coverage in this industry, firms could flexibly adapt their labor input choice. The supply of workers was high given the decline of the industry and rising unemployment. Layoffs were facilitated through the state's industry-specific social programs such as early retirement (from workers in their early 50s onwards) and reschooling programs to which workers could be sent. Moreover, a common practice was the flexible reshuffling of workers across mines of the same firm, given the geographical clustering of the industry. The average turnover rate, i.e., the share of workers leaving a mine in a given year, is 19%.

where $\varrho(\cdot)$ is a nonparametric function. We collect the treatment variables ($Exposure_i, Exposure_i \times Post_t$), a vector of dummies for the years since a new manager hiring ($Manager_{it}$), mine age, average wage, exit probability ($Exit_{it}$),¹⁰ longitude and latitude, and year fixed effects in the vector $\mathbf{z}_{it}$. The first stage allows us to recover the expected output $\hat{q}_{it}$ and the measurement error ϵ_{it} .

In the second stage, we can compute the quantity-based productivity using $\omega_{it}(\boldsymbol{\beta}) = \hat{q}_{it} - f(l_{it}, k_{it}; \boldsymbol{\beta})$ given any values of $\boldsymbol{\beta}$. We specify a Cobb-Douglas functional form for $f(\cdot)$, which leads to $f(l_{it}, k_{it}; \boldsymbol{\beta}) = \beta^L l_{it} + \beta^K k_{it}$, where $\boldsymbol{\beta} = (\beta^L, \beta^K)$. To identify production function coefficients and to allow the policy, new manager hiring, and mine exit likelihoods to affect productivity, we consider an endogenous first-order Markov process, $g(\cdot)$, to govern the law of motion for productivity following De Loecker (2011, 2013):

$$\omega_{it} = g(\omega_{it-1}, \mathbf{E}_{it,t-1}) + \xi_{it}, \quad (6)$$

where ξ_{it} denotes innovation in the productivity process and $\mathbf{E}_{it,t-1}$ is a vector capturing all endogenous impacts on the productivity process, including the lagged treatment $Exposure_i, Post_{t-1}, Exposure_i \times Post_{t-1}, Manager_{it-1}, Exit_{it-1}$ as well as the non-lagged treatment $Post_t, Exposure_i \times Post_t$. By including predicted exit probabilities in the law of motion, we account for the decline and the selection process in the industry, which the policy actively targeted. Note that the endogenous productivity process nests the typical exogenous process as a special case. We let productivity be a non-parametric function of (i) its lagged value, (ii) the linear elements in $\mathbf{E}_{it,t-1}$, and (iii) interactions of lagged productivity and $\mathbf{E}_{it,t-1}$.¹¹

Therefore, given $\boldsymbol{\beta}$ we can recover the innovation to productivity, $\xi_{it}(\boldsymbol{\beta})$, by nonparametrically regressing $\omega_{it}(\boldsymbol{\beta})$ on $\omega_{it-1}(\boldsymbol{\beta})$ and $\mathbf{E}_{it,t-1}$, and form moments to identify the parameters, using a generalized method of moments (GMM) estimator:

$$\mathbb{E} \left(\xi_{it}(\boldsymbol{\beta}) \begin{bmatrix} l_{it-1} \\ k_{it} \end{bmatrix} \right) = 0. \quad (7)$$

The instruments' exclusion restrictions are derived from the previously established timing assumptions regarding labor and capital inputs. These assumptions posit that productivity innovation is orthogonal to the current

¹⁰In the spirit of Collard-Wexler and De Loecker (2015), this is the predicted likelihood of exit coming from a regression of a dummy for exit in the next year on the three logged inputs.

¹¹We also considered an even more flexible specification of the law of motion for productivity, where we allow for non-linearities and interactions between the elements in $\mathbf{E}_{it,t-1}$, and found similar results.

capital and lagged labor.

We report the estimated production function coefficients in columns (1) and (2) of Appendix Table B3, first using a naive OLS specification and then applying the GMM procedure outlined above. The estimates indicate that coal production is labor-intensive, consistent with the industry pattern that labor costs account for the majority of variable costs (Pierenkemper and Fremdling, 2018) and findings in other coal mining literature (Rubens, 2023; Delabastita and Rubens, 2025). Moreover, we cannot reject constant returns to scale, suggesting that increasing returns to scale are unlikely to constitute an important mechanism behind potential productivity changes. In column (3), we allow the production function to vary by coal-mine type, distinguishing mines with and without cokeres, in the spirit of Collard-Wexler and De Loecker (2015). Consistent with historical descriptions of the production processes across different coal and mine types (Fritzsche, 2013), we find no statistically significant differences in production function parameters across mine types. This provides further suggestive evidence that the production technologies for coking coal and other coal were broadly comparable. We therefore use the production function estimates from column (2) in the subsequent analysis.

Markups and Marginal Costs. Having obtained the production function estimates, we next compute markups using the production approach (De Loecker and Warzynski, 2012; De Loecker and Scott, 2026). Specifically, under the assumption that coal mines minimize costs, the markup is given by $\mu_{it} \equiv P_{it}/\lambda_{it} = (\alpha_{it}^L/\beta^L + \alpha_{it}^M)^{-1}$, where λ_{it} denotes marginal cost, P_{it} is the output price, β^L is the output elasticity of labor, and $\alpha_{it}^X = P_{it}^X X_{it}/(P_{it} Q_{it})$ denotes the expenditure share of flexible input $X \in \{L, M\}$ in revenue, with P_{it}^X denoting its input price. In the data, however, observed output may contain measurement error or unanticipated shocks. Denoting observed output by $\tilde{Q}_{it} = Q_{it} \exp(\epsilon_{it})$, using $\tilde{Q}_{it}$ directly would lead to mismeasured revenue shares. We therefore construct corrected input revenue shares as $\hat{\alpha}_{it}^X = P_{it}^X X_{it}/(P_{it} \tilde{Q}_{it} / \exp(\epsilon_{it}))$, and use these corrected shares to compute markups. Finally, since output prices P_{it} are observed in the data, we recover time-varying, firm-level marginal costs as $\lambda_{it} = P_{it}/\mu_{it}$.

4 Effects of the Protective Policy on Firm Performance

4.1 Productivity

We start our analysis by estimating the policy effect of the *Hüttenvertrag* on within-mine productivity dynamics. Figure 4 presents the dynamic effects of the policy on TFPQ for different exposure specifications. TFPQ exhibits parallel pre-trends prior to the policy. In contrast, after its introduction, productivity increases immediately and persistently by more than 5% for mines with high exposure to the policy. The pooled effect is sizeable, implying a 0.3 standard deviation increase in productivity. The productivity gains continue to increase for several years after the policy announcement, potentially suggesting that underlying drivers of productivity growth take time to materialize. We study the mechanism in Section 5.

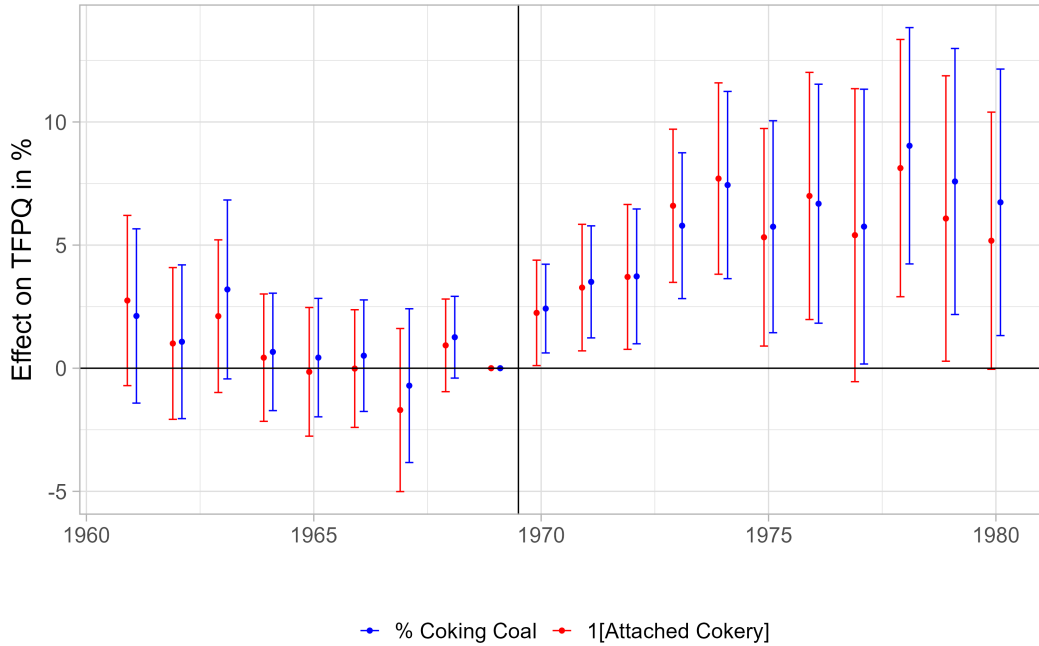


Figure 4: Effect of the Policy on TFPQ

Notes: This figure gives the regression results of Equation (2) with TFPQ as an outcome. Standard errors are clustered at the mine level and 90% confidence bands are reported. TFPQ is estimated based on a Leontief production function and estimation procedures, as shown in Section 3.2.

Productivity Dispersion. The average effect on TFPQ documented above does not reveal how the policy affected the distribution of productivity across mines. This distributional response is particularly relevant in our setting because the policy was intended to stabilize a declining industry and thereby prolong its operation.

Mines at greater risk of closure, typically those with lower productivity, are therefore of particular interest.¹² More generally, the effects of changes in competition may vary across mines depending on their characteristics and initial productivity (e.g., [Melitz, 2003](#); [Shu and Steinwender, 2019](#)).

To characterize the policy’s effects throughout the productivity distribution, we follow the distribution-regression approach of [Chernozhukov et al. \(2013\)](#) and estimate the counterfactual TFPQ distribution that would have prevailed absent the policy. This approach has been widely used to estimate the distributional effects of quasi-experimental shocks in other settings, e.g., [Dube \(2019\)](#) and [Fischer et al. \(2026\)](#). For each threshold c , we replace the outcome in the baseline difference-in-differences specification in Equation (1) with an indicator for whether TFPQ is at or below c and estimate

$$\mathbb{1}[TFPQ_{it} \leq c] = \beta_c Exposure_i \times Post_t + \alpha_i + \lambda_t + \varepsilon_{it}, \quad (8)$$

where the dependent variable equals one if $TFPQ_{it} \leq c$ and zero otherwise. Under the identifying assumptions discussed above, β_c captures the differential post-policy change in the probability that TFPQ does not exceed c associated with a one-unit increase in policy exposure. Repeating this regression over a grid of thresholds traces the estimated policy effect across the TFPQ distribution. We obtain the estimated counterfactual cumulative distribution function (CDF) by multiplying the estimated policy-induced change β_c at each threshold c with the mean exposure of strictly positively treated mines and subtracting the product from the empirical post-policy CDF for exposed mines.

Figure 5 compares the empirical post-policy CDF of estimated TFPQ with the corresponding estimated counterfactual CDF under the two exposure measures. The blue curve plots the empirical CDF for mines with positive exposure, while the red curve plots the estimated counterfactual CDF together with its 90% confidence intervals. In both panels, the divergence is concentrated in the lower tail: at low TFPQ thresholds, the counterfactual CDF lies above the empirical CDF, indicating that, absent the policy, a larger share of observations would have fallen below these low-productivity thresholds. This pattern is consistent with productivity gains being concentrated among less productive mines and with a narrowing of productivity dispersion. At higher TFPQ thresholds, the estimated differences are not statistically distinguishable from zero. Taken together, the

¹²[Jopp \(2023\)](#) finds that labor productivity was not a driver of exit among German coal mines between 1948 and 1978. [Fischer \(2026a\)](#) shows that mines with lower TFPQ were more likely to exit the German coal mining industry during the 1960s.

results suggest that the productivity response was concentrated in the lower tail, where mines historically faced a greater risk of exit. Because the specification includes mine fixed effects, the estimates are identified from within-mine changes, by which the effect at the left tail is not driven by selection and exit.

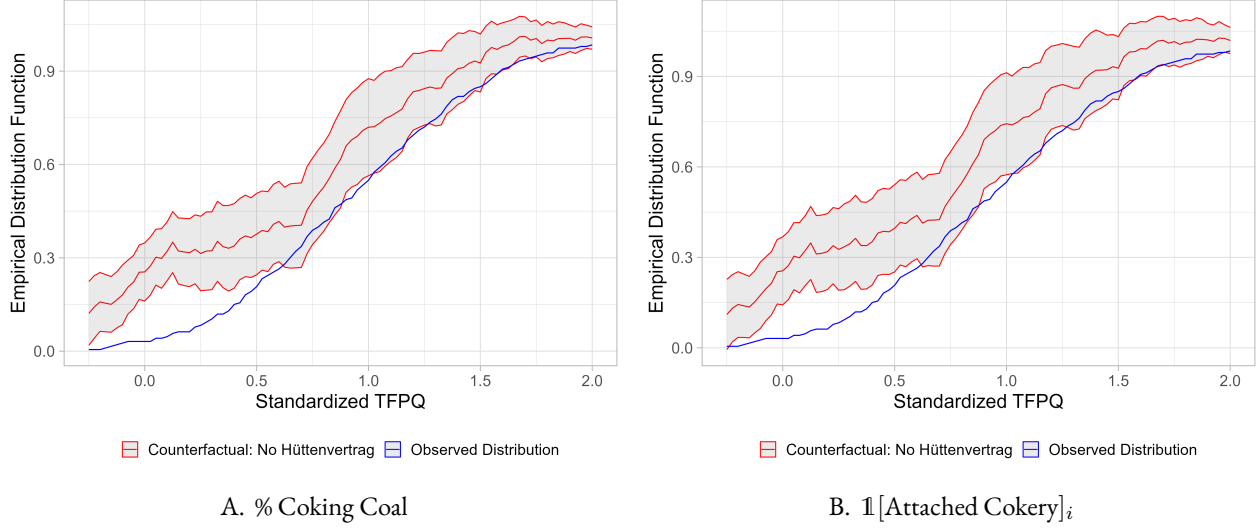


Figure 5: Distributional Effect of the *Hüttenvertrag* Policy on Productivity

Notes: This figure reports the distribution-regression estimates from Equation (8), following Chernozhukov et al. (2013). The blue line gives the observed distribution of TFPQ for post-treatment observations of treated units. The red lines give the point estimate of the counterfactual CDF and its 90% confidence interval. Standard errors are clustered at the mine level.

Exit and Survival. We next examine whether the *Hüttenvertrag* affected mine survival. Survival is a particularly important extensive-margin outcome in a declining industry, where changes in the number of operating mines occur almost entirely through exit. Only one mine entered during our sample period, consistent with substantial barriers to entry: establishing a new coal mine required a 7–10-year preparation period and DM 1 billion in fixed costs (Keyser, 1974). Because the production panel contains observations only while mines operate, it is necessarily unbalanced. We therefore construct a balanced mine-year panel containing one observation for each mine in every sample year. The survival indicator equals one in every year a mine operates and remains zero from the year of exit onward. We exclude mines with fewer than three observed years of operation.

Panel A of Figure 6 reports the dynamic effects of the policy on survival. The pre-policy estimates are statistically indistinguishable from zero and show no discernible differential trend. Following the policy’s introduction, the survival probability of more-exposed mines rises progressively relative to that of less-exposed mines, with the estimates becoming statistically significant at longer horizons. By 1980, both exposure specifications imply an

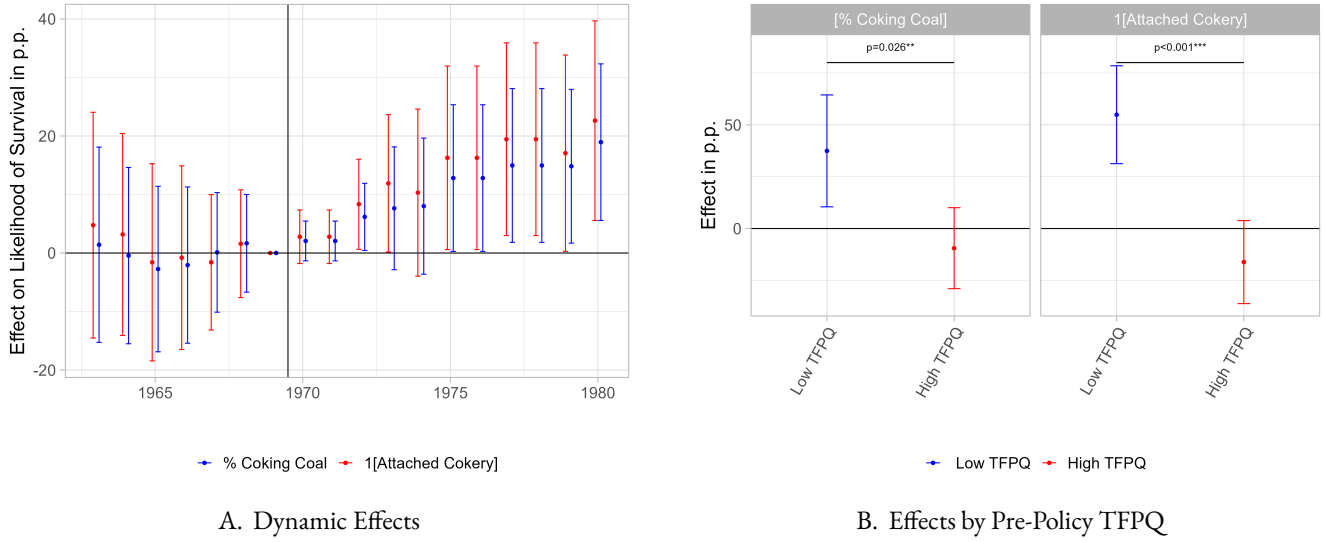


Figure 6: Effects of the *Hüttenvertrag* Policy on Mine Survival

Notes: Panel A reports dynamic estimates from Equation (2) using survival as the outcome in a balanced mine-year panel. The survival indicator equals one if a mine operates in year t and zero if it exits in year t or earlier. The blue and red estimates correspond to $[\% \text{Coking Coal}]_i$ and $\mathbb{1}[\text{Attached Cokery}]_i$, respectively; the vertical line marks the policy's introduction. Panel B reports pooled post-policy estimates from Equation (1), allowing the policy effect to differ between mines with TFPQ in 1968 at or below the sample mean and those with TFPQ above the mean. The left and right subpanels use $[\% \text{Coking Coal}]_i$ and $\mathbb{1}[\text{Attached Cokery}]_i$, respectively; within each subpanel, the blue and red estimates correspond to the low- and high-TFPQ groups, respectively. The reported p -values test the equality of the policy effects across the two productivity groups. For $[\% \text{Coking Coal}]_i$, coefficients and standard errors are scaled by the mean exposure among mines with strictly positive exposure. Standard errors are clustered at the mine level, and 90% confidence intervals are reported.

approximately 20-percentage-point increase in the probability of continued operation.

We also study heterogeneity across different groups of mine productivity. Panel B of Figure 6 examines heterogeneity in these effects by pre-policy productivity. We define low-TFPQ mines as those with TFPQ in 1968 at or below the sample mean and high-TFPQ mines as those above the mean, and allow the policy effect to vary across the two groups. Across the two exposure measures, the estimated policy-induced increase in the survival probability of initially low-TFPQ mines ranges from 35 to 55 percentage points. By contrast, the corresponding estimates for initially high-TFPQ mines are statistically indistinguishable from zero.

These findings document reduced exit as an important extensive-margin component of the policy response. The survival gains are concentrated among the same group of initially low-productivity mines that experienced the largest productivity improvements (as shown above). Together, these patterns suggest that the policy helped initially vulnerable mines become more productive and remain in operation.

4.2 Markups and Marginal Costs

The post-policy efficiency gains documented above contrast with the positive association between competition and productivity found by many in the literature (e.g., [Pavcnik, 2002](#)).¹³ To examine whether these gains were passed through to downstream buyers, we estimate mine-level markups and marginal costs as described in Section 3.2. We recover markups from the estimated production function and the expenditure shares of flexible inputs. We compute marginal costs by dividing the weighted mine-year output price described in Section 2.2 by estimated markups.

Table 2 reports pooled difference-in-differences estimates for productivity, output prices, marginal costs, wages, and markups. Column (1) reproduces the pooled TFPQ estimate corresponding to the dynamic estimates in Figure 4. Column (2) shows that output prices increased by 10% in response to the policy-induced reduction in import competition, consistent with the standard prediction that weaker competitive pressure raises prices. In the spirit of the trade liberalization literature ([De Loecker et al., 2016](#); [Brandt et al., 2017](#)), this price effect can be decomposed into changes in markups and changes in efficiency within the mine. We therefore use the estimated markups and marginal cost measures to examine both channels directly.

Table 2: Effects of the Policy on Productivity, Prices, Marginal Costs, Wages, and Markups

	ln(TFPQ) (1)	ln(Price) (2)	ln(MC) (3)	ln(Wage) (4)	ln(Markup) (5)
Panel A: [%Coking Coal] _i					
<i>Exposure_i × Post_t</i>	0.044*** (0.013)	0.100*** (0.011)	−0.035** (0.017)	0.008 (0.007)	0.133*** (0.018)
Panel B: 1[Attached Cokery] _i					
<i>Exposure_i × Post_t</i>	0.044*** (0.016)	0.098*** (0.012)	−0.040** (0.017)	0.011 (0.007)	0.136*** (0.019)
Mine FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Observations	885	974	885	974	885

Notes: *p<0.1; **p<0.05; ***p<0.01. Coefficients and standard errors for the continuous exposure measure are scaled by the mean exposure among mines with strictly positive exposure. Standard errors are clustered at the mine level.

¹³Appendix Figure A3 shows that the positive TFPQ effects of the policy are robust to alternative production-function specifications, including Cobb–Douglas and translog specifications that allow pitwood to be substituted with other inputs.

We first consider marginal costs in column (3), which decline by 3–4% in response to the policy. Marginal costs may change because of either improvements in productive efficiency or changes in input prices. We therefore use mine-level average wages in column (4) to assess the contribution of input-price changes. The absence of a statistically significant wage response, together with the TFPQ gains in column (1), suggests that the decline in marginal costs primarily reflects the relative efficiency improvements at more-exposed mines. Based on the estimated 4% reduction and the DM 33.09 billion in post-policy marginal costs aggregated across all cokery mines, the implied cost savings relative to non-cokery mines amount to approximately DM 1.38 billion through 1980.¹⁴ This estimate is close to the cumulative state compensation for the gap between German and world-market prices over the same period.

If markups had remained constant, the decline in marginal costs would have translated into lower output prices. Instead, column (5) shows that the estimated increase in markups exceeded the increase in prices, indicating that mines retained the cost savings rather than passing them on to downstream buyers. Thus, at least in the short run, the *Hüttenvertrag* benefited upstream coal mines more than downstream steel producers or other consumers. These gains, however, came at a growing fiscal cost as the gap between domestic and world market prices widened.

Anti-Competitive Effects. The overall effect of the *Hüttenvertrag* on markups combines anti-competitive and efficiency effects. It is important to note that drawing conclusions about anti-competitive effects directly from markup regressions would be misleading. Since marginal costs are not controlled for, the policy may affect costs through efficiency improvements, which in turn induce firms to adjust markups, creating endogeneity concerns when identifying anti-competitive effects. We therefore explicitly control for marginal costs to isolate the anti-competitive component. The results are reported in Table B4. The coefficient in column (2), where we control for log marginal costs, is positive but slightly smaller than in the baseline specification in column (1). In column (3), we instrument marginal costs with lagged log marginal costs, following De Loecker et al. (2016), to address potential measurement error, and the results remain qualitatively unchanged. This suggests that the increase in markups is mainly driven by the anti-competitive effect of the *Hüttenvertrag*.

¹⁴ $-\frac{\text{DM } 33.09 \text{ billion}}{1-4\%} \times 4\% = -\text{DM } 1.38 \text{ billion.}$

4.3 Organization of Production

This subsection investigates the production-side origins of the policy-induced productivity gains. We examine whether they reflect an expansion in mine scale or improvements in within-mine production organization.

Mine Scope. To distinguish expansion in operating scale and mining conditions from production improvements within mines, we estimate the policy effects on output, employment, physical mine scope, and coal-seam quality. Figure 7 reports the results. We find no statistically detectable effects on total employment or either of our two output measures: gross extracted material, which includes both coal and non-coal content, and coal output alone. The policy likewise had no statistically significant effects on measures of physical mine scope, including coalfield size, the total length of the coalface worked, maximum mine depth, and total undermined volume.

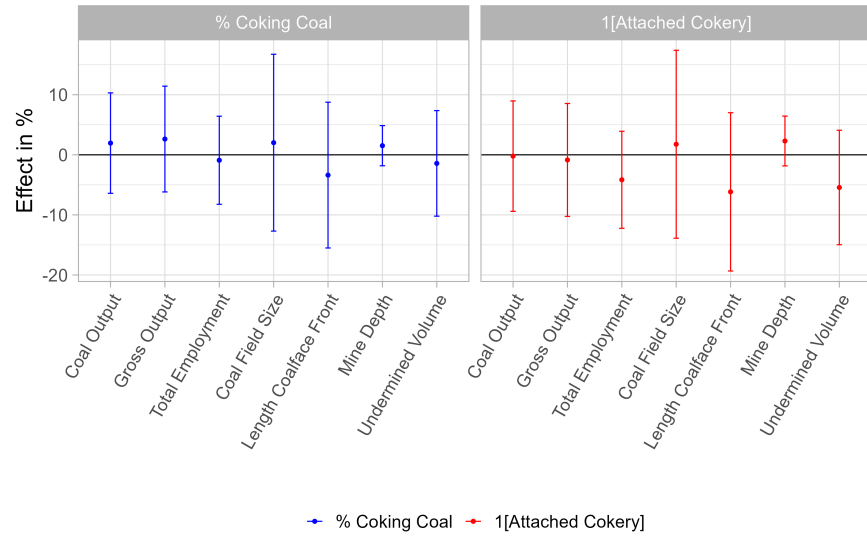


Figure 7: Effects of the *Hüttenvertrag* Policy on Output, Employment, Mine Scope, and Coal-Seam Quality

Notes: This figure reports pooled estimates from Equation (1) for operating days, output, employment, and measures of mine scope and coal-seam quality. Standard errors are clustered at the mine level, and 90% confidence intervals are reported.

In sum, these results provide little evidence that the productivity gains arose from an expansion in operating scale or mine scope. Instead, the gains appear to have been achieved within existing mine boundaries and at broadly unchanged scale, making economies of scale an unlikely explanation. This pattern points to improvements in the within-mine organization of production as a more likely source of productivity growth. We next examine measures of within-mine organization.

Workforce Composition, Turnover, and Shift Allocation. Having found no statistically detectable changes in total employment or mine scale, we next examine whether more-exposed mines adjusted the composition and deployment of their workforce. Figure 8 presents the estimated policy effects on worker separations, which we use as a proxy for labor turnover, as well as on workforce composition and the allocation of underground shifts across tasks.

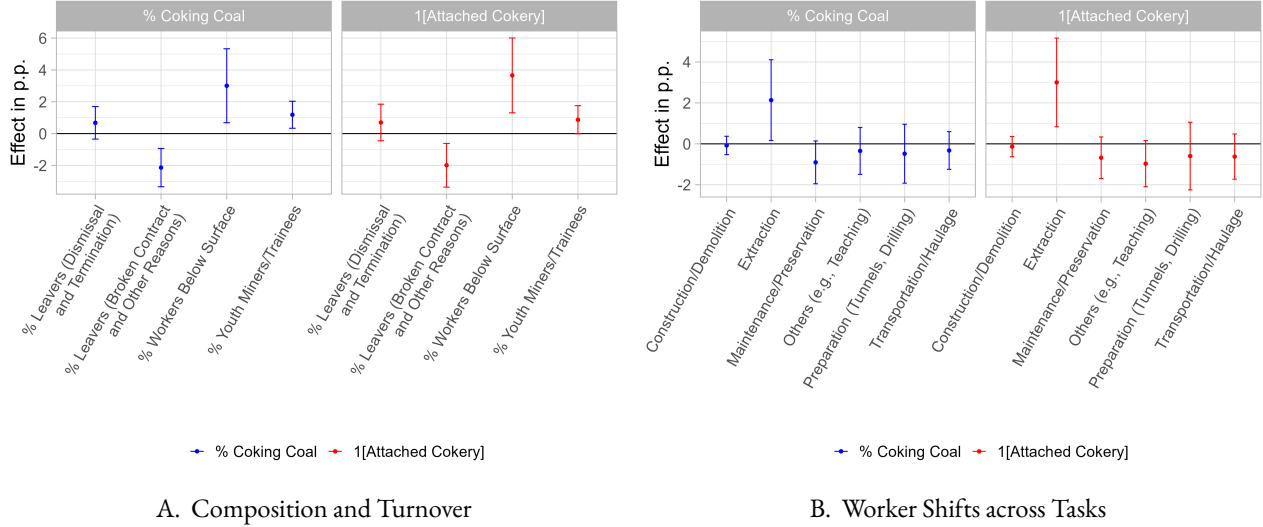


Figure 8: Effects of the *Hüttenvertrag* Policy on Workforce Composition, Turnover, and Shift Allocation

Notes: This figure reports pooled estimates of Equation (1) using $[\%Coking\ Coal]_i$ and $1[Attached\ Cokery]_i$ as measures of policy exposure. Panel A reports estimates for two annual separation rates, the share of workers employed underground, and the share of young miners and trainees. Each separation rate is defined as the number of underground workers leaving a mine during year t for the indicated set of reasons, divided by underground employment in year $t - 1$. Panel B reports estimates for the shares of underground worker shifts allocated across tasks. Coefficients and standard errors for the continuous exposure measure are scaled by the mean exposure among mines with strictly positive exposure. All estimates are reported in percentage points. Standard errors are clustered at the mine level, and 90% confidence intervals are reported.

Panel A first considers annual separation rates among underground employees.¹⁵ We distinguish dismissals and terminations from a residual category comprising all other recorded separations, including contract breaches. The policy had no statistically detectable effect on the dismissal-and-termination rate, but reduced the residual separation rate by approximately 2 percentage points, or 19%. The latter category pools reasons for separation such as voluntary quits or worker conduct. Hence, it provides indication for lower underground-worker turnover along these margins at more-exposed mines. This response may be relevant for productivity, as lower worker turnover has been shown to improve mine-level productivity in this industry (Fischer, 2026b).

Panel A also shows that the share of the workforce employed underground increased by approximately 3 per-

¹⁵Separation data are available only for underground employees, who account for two-thirds of total employment.

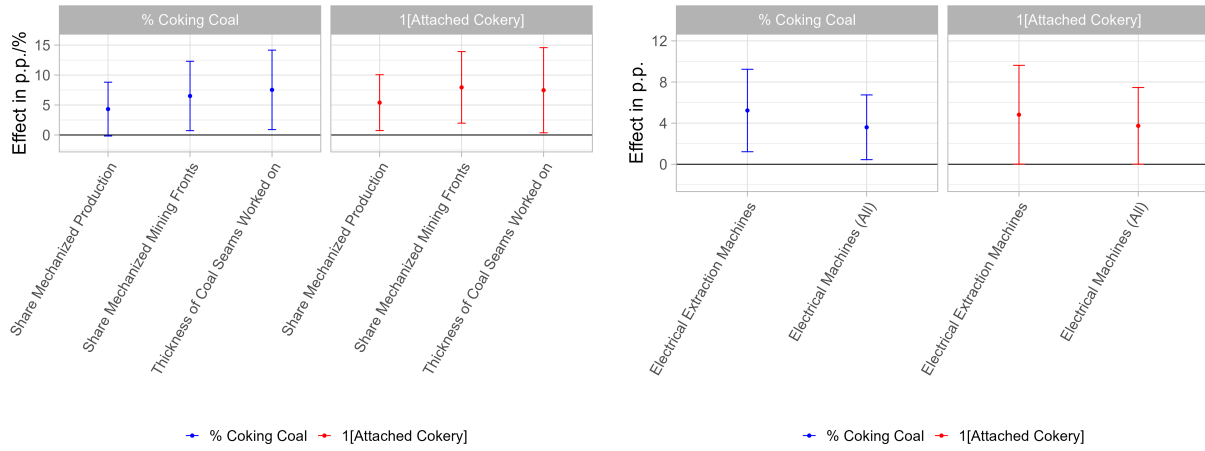
centage points, or 5%, despite the absence of a detectable change in total employment. Underground workers participate directly in coal extraction, whereas surface workers are primarily engaged in coal cleaning, processing, and administrative activities. The workforce therefore became more concentrated in extraction-related employment. The share of young miners and trainees also rose by approximately 1 percentage point, or 20%. One possible interpretation is that the reduction in import competition improved the perceived viability of more-exposed mines, lengthening their expected operating horizons and raising the returns to recruiting and training younger workers. At the same time, Table 2 shows no statistically detectable change in average mine wages, indicating that the workforce changes were not driven by broad-based wage growth.

Panel B examines the distribution of underground worker shifts across tasks. The share allocated to coal extraction increased by 2–3 percentage points, or 4–5%, while none of the other task categories exhibited a statistically detectable increase. Because these outcomes are shares, the estimates document a relative reallocation of underground shifts rather than an increase in their total number. Together with the unchanged level of total employment, the results point to an internal reorganization of labor toward core extraction activities. The reshaping of the workforce and the internal reorganization of production provide a potential explanation for the observed productivity gains.

Technology Adoption. Technology adoption constitutes a second margin of within-mine adjustment. The *Hüttenvertrag* was introduced during a period of rapid technological change in the German coal industry (Ingenierf, 2021).¹⁶ We examine whether more-exposed mines intensified their use of mechanized and electrically powered technologies following the policy.

Panel A of Figure 9 reports pooled estimates for the share of production extracted using mechanized methods, the share of mechanized mining fronts, and the average thickness of the coal seams worked. The estimates imply that the policy increased the share of mechanized production by approximately 5 percentage points, or 8% relative to its pre-policy mean, and the share of mechanized mining fronts by approximately 7 percentage points, or 11%. The production-weighted average thickness of the seams worked also increased by 7–8%. Because a mine’s geological endowment is predetermined, the latter estimate is naturally interpreted as a reallocation of extraction toward thicker seams rather than a change in geological conditions. Since large-scale machinery is particularly productive in thicker seams, the joint pattern is consistent with complementary adjustments in

¹⁶For example, the industry-wide share of fully mechanized tunnel development rose from 20% to 80% between 1965 and 1975.



A. Mechanization and Thickness of Seams Worked

B. Electrification

Figure 9: Effects of the *Hüttenvertrag* Policy on Mechanization, Electrification, and the Thickness of Seams Worked

Notes: This figure reports pooled estimates of Equation (1) using $[\% \text{Coking Coal}]_i$ and $1[\text{Attached Cokery}]_i$ as measures of policy exposure. Panel A reports estimates for the share of production extracted using mechanized methods, the share of mechanized mining fronts, and the log of the production-weighted average thickness of the coal seams worked. Panel B reports estimates for the share of installed machinery power accounted for by electrical equipment, separately for extraction machinery and for the broader category of all machinery, which also includes non-extraction equipment such as water pumps. All share outcomes are reported in percentage points. The seam-thickness coefficient is multiplied by 100 and can be interpreted as an approximate percentage effect. Coefficients and standard errors for the continuous exposure measure are scaled by the mean exposure among mines with strictly positive exposure. Standard errors are clustered at the mine level, and 90% confidence intervals are reported.

technology use and the allocation of extraction across seams (Bleidick, 2013; Döhl et al., 1982).

Panel B examines electrification, defined as the share of installed machinery power accounted for by electrical equipment. The estimates imply an increase of 4–5 percentage points, or 6–8%, relative to the corresponding pre-policy mean. The results are similar for machinery used directly in extraction and for the broader category of all mine machinery, which also includes equipment such as water pumps. Together, the two panels indicate that more-exposed mines shifted toward mechanized and electrically powered production technologies following the policy. These changes constitute a second margin of within-mine adjustment that may have contributed to the observed productivity gains.

Overall, these findings indicate that productivity improved through within-mine adjustments rather than scale expansion. More-exposed mines reallocated labor toward core extraction activities and accelerated mechanization and electrification. However, these results identify only the proximate production changes associated with the productivity gains, not the underlying decision-making mechanism. This raises a deeper question: what drove these adjustments and ultimately contributed to productivity growth? Because managers oversee

both workforce allocation and technology adoption ([Adhvaryu et al., 2022](#); [Rubens, 2023](#)), changes in management naturally emerge as a plausible explanation for these patterns. We will next examine whether management constitutes an important mechanism linking the policy to the observed within-mine productivity gains.

5 Mechanism: The Role of Management

This section examines management as a potential channel linking the *Hüttenvertrag* to the within-mine productivity gains documented above. For management to constitute such a channel, two relationships must hold: management must affect mine productivity, and the policy-induced reduction in import competition must affect management-team composition, incumbent managers' performance, or both. We discuss each relationship in turn and derive corresponding testable predictions.

5.1 Conceptual Framework

Management and Productivity. Standard production models often leave management implicit, but a substantial literature identifies management as an important determinant of firm productivity ([Adhvaryu et al., 2022](#); [Bloom and Van Reenen, 2007, 2010](#); [Bloom et al., 2016b, 2020](#); [Becker and Hvide, 2022](#); [Fenizia, 2022](#); [Demirer and Karaduman, 2026](#)). Two channels are particularly relevant in our setting. First, managerial quality may act as a Hicks-neutral productivity shifter ([Lucas, 1978](#); [Melitz, 2003](#); [Bloom and Van Reenen, 2007](#); [Bloom et al., 2013](#); [Bao et al., 2026](#)), raising the efficiency with which a given bundle of inputs is transformed into output. Second, managers may affect productivity indirectly through choices over input allocation, organizational design, and technology adoption, including machinery upgrades and new production methods ([Burhop and Lübbers, 2009](#); [Gibbons and Roberts, 2013](#); [Rubens, 2023](#)). These choices shape, among other things, the effective capital stock and the composition and deployment of the workforce.

Both channels may operate in our setting. The direct channel would be reflected in higher mine-level TFPQ for a given bundle of measured inputs. The indirect channel could affect TFPQ through changes in production organization and technology, particularly mechanization. Whether managerial upgrading is associated with these outcomes in the German coal mining industry remains an empirical question. This yields our first testable prediction:

Prediction 1: If management contributes to the policy-induced productivity gains, managerial upgrading should be associated with subsequent increases in mine-level TFPQ.

Competition and Management. The managerial response to the policy-induced reduction in import competition is theoretically ambiguous. We distinguish between two margins: changes in the behavior of incumbent managers and changes in management-team composition.

At the first margin, stronger competitive pressure may discipline managers and encourage the adoption of better practices (Schmidt, 1997; Van Reenen, 2011; Bloom et al., 2016b), but it may also reduce profits and managerial compensation (Schmidt, 1997; Bao et al., 2026), thereby weakening incentives for managerial effort. Its net effect on incumbent managerial performance, and hence on productivity, is therefore ambiguous.

At the second margin, the policy may reshape manager–mine matching, and hence management-team composition, by altering mines’ relative attractiveness in the managerial labor market (Terviö, 2008). In a declining industry, protection from foreign competition may lengthen the expected operating horizons of more-exposed mines and improve perceived job security, making managerial positions at these mines more attractive along nonpecuniary dimensions (Brown and Matsa, 2016; Baghai et al., 2021).

Such changes may reshape manager–mine matching through two channels: (i) inflows of higher-quality managers from outside the existing pool of mine managers (e.g., Graham et al., 2020) and (ii) the reallocation of managers across mines (e.g., Bloom et al., 2026). Such inflows of managers can expand the stock of managerial talent available to the industry and thereby raise industry-level productivity. Reallocation leaves this stock unchanged but can nevertheless raise aggregate productivity when it improves manager–mine match quality (Alder, 2016). The incidence and strength of both responses may depend on mines’ initial productivity and attractiveness. Appendix Section C provides complementary descriptive evidence on these margins, showing that managerial hiring in our setting involved both cross-mine mobility and inflows from outside the existing pool of mine managers.

We find that the reduction in import competition induced by the *Hüttenvertrag* raised TFPQ at more-exposed mines relative to less-exposed mines, in contrast to the positive relationship between competition and productivity widely documented in the literature (Olley and Pakes, 1996; Pavcnik, 2002; Amiti and Konings, 2007; De Loecker et al., 2016; Brandt et al., 2017; Backus, 2020). Much of this literature, however, gives limited attention to management as a margin of adjustment to changes in competitive conditions. Applying the

two-margin distinction above to our setting yields our second testable prediction:

Prediction 2: If management contributes to the policy-induced productivity gains, following the policy-induced reduction in import competition, more-exposed mines should exhibit improvements in incumbent managerial performance, management-team composition, or both, relative to less-exposed mines.

5.2 Measures of Managerial Ability

Measuring managerial ability, which we also refer to as managerial quality, is challenging (Weidmann et al., 2026), but central to our analysis. We therefore construct three complementary measures based on (i) managers' observed educational backgrounds, (ii) the productivity of prior employers for managers who move across mines, and (iii) manager-team contributions obtained from a structural decomposition of mine-level productivity. We consider these measures jointly when assessing the role of management.

First, we construct a direct proxy from observed manager characteristics. Prior work shows that such characteristics predict managerial practices (e.g., Bertrand and Schoar, 2003; Goldfarb and Xiao, 2011). In particular, we use managers' educational backgrounds and an indicator for whether they hold a STEM degree. Technical training may be especially valuable during a period of rapid technological change in coal mining, where mining-engineering qualifications were common. The share of managers holding a STEM degree rose from 52% in 1969 to 91% in 1980, reflecting the increasing prevalence of technical expertise over this period.

Second, for managers who move between mines, we can proxy the quality of recruited managers using the productivity of their prior mines. Specifically, we track the number of mine-year hirings in which a mine hires managers whose previous mine had above-median productivity before the move. This measure captures the recruitment of managers with experience in relatively productive operating environments, who may bring valuable knowledge and managerial practices to their new mines.

Third, we construct a productivity-based proxy for managerial ability by decomposing mine-year TFPQ into mine, year, and manager-combination components. Following the regression-based approaches in Bertrand and Schoar (2003), Fenizia (2022), Giardili et al. (2023), and Metcalfe et al. (2023), we estimate the following specification:¹⁷

$$\ln(TFPQ_{it}) = \alpha_i + \lambda_t + \phi_m + \epsilon_{it}, \quad (9)$$

¹⁷Although our approach is related to Abowd et al. (1999), we use firm-level rather than worker-level productivity measures because manager-specific wages are unavailable.

where α_i , λ_t , and ϕ_m denote mine, year, and manager-combination fixed effects, respectively. Because mines often employ multiple managers in a given year, we estimate fixed effects for manager combinations (or teams) rather than for individual managers. These fixed effects need not equal the sum of individual manager effects and therefore accommodate complementarities and other interactions among managers (Peeters et al., 2020).¹⁸

We interpret the estimated manager-combination fixed effect, $\hat{\phi}_m$, as a productivity-based proxy for managerial ability. It captures the systematic association between a management team and mine-level TFPQ after accounting for time-invariant mine heterogeneity and common year shocks.¹⁹ To prevent policy-induced productivity gains from being mechanically attributed to managerial ability, we also estimate Equation (9) including the policy interaction term on the right-hand side. Overall, the three complementary measures provide a multidimensional assessment of managerial ability.

5.3 Manager Changes in Response to the Protective Policy

We first evaluate Prediction 2. To do so, we exploit the matched manager–mine data to examine whether the policy affected management through changes in management-team composition, changes in the performance of incumbent management teams, or both. We start by studying the policy’s effects on managerial turnover and hiring composition and then assess whether managerial performance changed within unchanged teams. We define a mine’s management team as the managers heading the mine; the average team comprises two managers per mine-year.

Short-Run Hiring Response. We begin by examining the short-run response of managerial hiring to the policy. Appendix Figure A4 presents the dynamic estimates for the number of newly hired managers. Managerial hiring at more-exposed mines increased immediately following the policy. The estimated increase is approximately 0.5 manager, relative to an average management-team size of 1.95.²⁰ The rapid response may have been

¹⁸Separately identifying mine and manager-combination effects requires managers to move across mines. Moreover, manager-combination fixed effects are comparable only within connected sets of mines that are connected through manager moves. In our sample, 81% of mines experience a change in management, and 27% of managers work at more than one mine, providing sufficient identifying variation.

¹⁹The AKM-style specification assumes that manager mobility is conditionally random. Managers may therefore sort across mines based on time-invariant mine and manager characteristics absorbed by the fixed effects, but not on unobserved time-varying determinants of productivity. Appendix Table B5 assesses this assumption by regressing the fixed effects of newly hired manager combinations on region fixed effects, a cokery indicator, baseline mine characteristics measured in 1960, and mine-year changes in these characteristics. We find no robust significant relationship between manager-combination fixed effects and changes in mine characteristics. We will further assess this concern by examining pre-trends around manager-hiring events.

²⁰Demirer and Karaduman (2026) document a similarly immediate increase in managerial turnover following acquisitions of

facilitated by relatively fluid internal managerial mobility within the quasi-monopolist *Ruhrkohle AG*.

To characterize the composition of this initial hiring response, we estimate the following modified difference-in-differences specification:

$$Y_{it} = \beta_1 Exposure_i \times \mathbb{1}[Year_t = 1970] + \beta_2 Exposure_i \times \mathbb{1}[Year_t > 1970] + \alpha_i + \lambda_t + \varepsilon_{it}. \quad (10)$$

The outcome Y_{it} captures alternative dimensions of managerial turnover and hiring composition, including the numbers of entering and departing managers and the numbers of new hires with the ability-related characteristics defined above.

Figure 10 reports β_1 , which captures the differential response of managerial hiring to policy exposure in the first post-policy year. Several patterns emerge. First, policy exposure increases the total number of newly hired managers, as indicated by the blue estimates. The magnitude is sizable: more-exposed mines hire 0.6–0.7 additional managers relative to less-exposed mines immediately following the policy. However, this inflow is offset by a similarly sized increase in managerial departures, indicating replacement of managers rather than an expansion of management-team size.

Second, the increase in managerial hiring is concentrated among managers with STEM training, as shown by the red estimates. Such managers are likely to possess greater technical expertise, particularly in mining engineering and related technologies (Rubens, 2023), which may have been especially valuable during the industry’s transition from manual to mechanized production.

Third, the black estimates distinguish hiring events that improve or reduce the manager-combination fixed effect relative to the preceding management team. The positive estimate for ability-improving hires indicates that more-exposed mines disproportionately recruited management teams associated with higher estimated managerial ability.

Fourth, we examine whether more-exposed mines recruited managers with experience at high-productivity mines. Approximately one-third of new hires previously held managerial positions at other mines.²¹ The green estimates show that policy exposure significantly increased the number of manager hirings from an above-median-productivity mine, while having no statistically significant effect on managers hired from a below-median

power plants.

²¹ Among others, concerns about potential mine closures could induce highly qualified workers, including managers, to leave for other mines in the German coal mining industry (Vollmer, 2013).

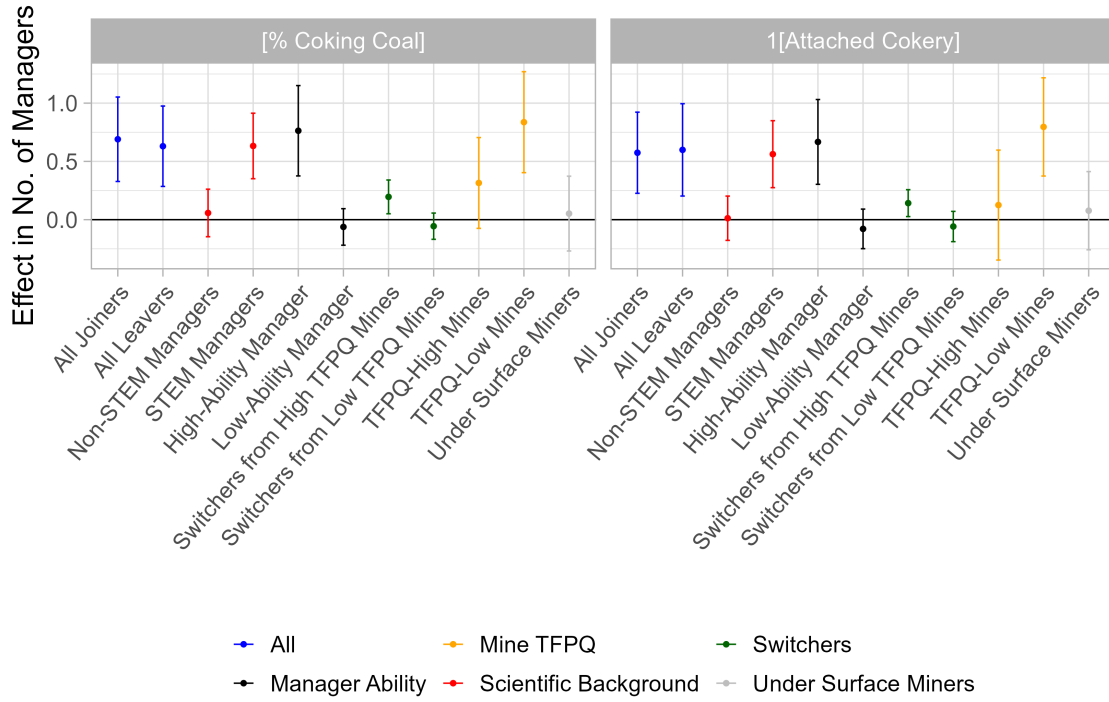


Figure 10: Short-Run Effects of the *Hüttenvertrag* Policy on Managerial Turnover and Hiring Composition

Notes: This figure reports β_1 from Equation (10), which captures the differential effect of policy exposure in the first post-policy year, 1970. The left panel measures exposure using the mine's 1961 share of coking-coal output, while the right panel uses an indicator for whether the mine had an attached cokery before 1963. Separate regressions use the following outcomes: the numbers of all managers entering and departing; the numbers of entering managers with non-STEM and STEM backgrounds; hiring events associated with an improvement or deterioration in the manager-combination fixed effect relative to the preceding combination (high- and low-ability hires); the number of managers hired, estimated separately for destination mines with above- and below-median TFPQ immediately before the policy; and the number of underground miners hired. All managerial outcomes are measured as counts. The underground-miner estimate is reported as a semi-elasticity. For the continuous exposure measure, coefficients and standard errors are scaled by the mean exposure among mines with strictly positive exposure. The dots denote point estimates, and the capped vertical lines report 90% confidence intervals based on standard errors clustered at the mine level.

productivity mine. These results are consistent with more-exposed mines recruiting managers who may bring knowledge of more advanced technologies and superior managerial practices. As only one-third of managers switched across mines, the point effects do not add up to the aggregate number of hirings (in blue). The aggregate hiring response, therefore, reflects both within-industry mobility and entry into mine management through promotions or hiring from outside the observed sample.

Fifth, we examine heterogeneity in managerial hiring by destination mines' pre-policy productivity. The yellow estimates show that the hiring response is concentrated among mines with below-median TFPQ immediately before the policy. This pattern mirrors the concentration of the policy-induced productivity gains among initially less-productive mines.

Finally, we examine whether hiring also increased at lower levels of the organizational hierarchy. Using

underground-miner hiring as the outcome, we find no statistically significant response immediately following the policy. This result suggests that management was the primary margin of short-run organizational adjustment.

Persistent Shifts in Management Composition. We next examine whether the initial hiring response resulted in persistent changes in the composition of mine management teams. Table 3 presents the results. In column (1), we use the mine-year measure of managerial ability, $\hat{\phi}_{m(i,t)}$, from Equation (9) as the outcome in our baseline difference-in-differences specification. The estimates indicate that the estimated ability of management teams rose throughout the post-policy period; the pooled estimate is 0.6 standard deviations.²² Column (2) uses manager-combination fixed effects estimated from an AKM specification that explicitly controls for the policy treatment by adding the interactions $Exposure_i \times Post_t$ for both exposure variable variants to the right-hand side of Equation (9). The estimate remains positive and statistically significant. This result indicates that the estimated increase in managerial ability is not an artifact of the AKM fixed effects absorbing the policy's direct effect on productivity and is therefore consistent with Prediction 2.

Table 3: Persistent Changes in Management Composition

	$\hat{\phi}_{m(i,t)}$ (1)	$\hat{\phi}_{m(i,t)}$ (Policy in AKM) (2)	$\hat{\phi}_{m,p(i,t)}$ (Dynamic) (3)	$\hat{\phi}_{m,p(i,t)}$ (Dynamic) (4)	STEM Managers (5)	1[Manager Change] (6)	1[Same Managers as 3 Years Ago] (7)	1[Same Managers as 5 Years Ago] (8)
Panel A: % Coking Coal								
$Exposure_i \times Post_t$	0.621*** (0.181)	0.613*** (0.184)	0.619*** (0.185)	0.145 (0.351)	0.381* (0.197)	0.063 (0.057)	−0.051 (0.100)	−0.026 (0.099)
Panel B: 1[Attached Cokery]_i								
$Exposure_i \times Post_t$	0.621*** (0.217)	0.557** (0.223)	0.630*** (0.223)	0.175 (0.305)	0.353* (0.198)	0.012 (0.073)	−0.013 (0.121)	0.020 (0.119)
Mine FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Team FEs	No	No	No	Yes	No	No	No	No
Observations	956	956	974	974	974	974	974	974

Notes: *p<0.1; **p<0.05; ***p<0.01. Coefficients and standard errors for the continuous exposure measure are scaled by the mean exposure among mines with strictly positive exposure. The outcome variables in columns (1)–(4) are standardized. Column (5) controls for the total number of managers. See Figure A5 in the Appendix for the dynamic difference-in-differences estimates corresponding to columns (1) and (5).

We next distinguish changes in management-team composition from improvements within a given team. To do so, we allow the contribution of each manager combination to vary between the pre- and post-policy periods. Specifically, rather than including time-invariant manager-combination fixed effects, ϕ_m , in the AKM-

²²The measure $\hat{\phi}_{m(i,t)}$ is identified only within connected sets of observations linked through manager moves. It can nevertheless be used as an outcome in regressions spanning multiple connected sets because mine fixed effects imply that identification in these regressions comes from within-mine variation. By construction, all observations for a given mine belong to the same connected set.

style specification (9), we include manager-combination-by-period fixed effects, $\phi_{m,p(i,t)}$, where $p(i,t)$ indexes the pre-policy period ($t \leq 1969$) or the post-policy period ($t \geq 1970$). Column (3) of Table 3 yields an estimated increase of around 0.61–0.63 standard deviations, closely matching the estimate in column (1). In column (4), we additionally absorb manager-combination fixed effects, so that the coefficient is identified from changes within a given management team. The estimated effect becomes substantially smaller and statistically indistinguishable from zero. This pattern suggests that the policy-related increase in estimated managerial ability was driven primarily by changes in team composition rather than improvements within unchanged management teams.

Column (5) provides complementary evidence of a persistent change in management composition. The number of STEM-trained managers increases even after controlling for the total number of managers. The estimated increase of approximately 0.35–0.38 STEM-trained managers is economically sizable relative to the sample means of 1.95 managers in total and 1.05 STEM-trained managers per mine-year. Although managerial hiring at cokery mines rose sharply in 1970, column (6) shows that the estimated effect on the annual probability of a management change is statistically indistinguishable from zero over the full post-policy period. We define a management change as the entry or exit of at least one manager between consecutive years. Consistent with this result, columns (7) and (8) show no statistically detectable change in the probability that a mine retains the same management team as three or five years earlier. Taken together, these findings are consistent with an immediate reconfiguration of management teams following the policy, after which the new composition remained relatively persistent.

5.4 Managerial Hiring and Mine Performance at Cokery Mines

Consistent with Prediction 2, we find that the policy induced an immediate hiring response and persistent upgrading of management teams at cokery mines, with no statistically detectable improvement within unchanged teams. We now turn to Prediction 1. Specifically, we examine whether post-policy managerial hiring is associated with subsequent increases in mine productivity and markups.

Explanatory Power of Managers. Before analyzing managerial hiring events, we assess whether managers have economically meaningful explanatory power for mine performance. Following [Bertrand and Schoar \(2003\)](#) and [Fenizia \(2022\)](#), we compare adjusted R^2 values from productivity and markup regressions with mine and year

fixed effects, with and without manager fixed effects. Table B6 reports the results. In Panel A, adding manager fixed effects increases the adjusted R^2 from 0.915 to 0.945 for TFPQ and from 0.830 to 0.905 for markups. Relative to the variation unexplained by mine and year fixed effects, these gains represent 35% ($= \frac{0.945-0.915}{1-0.915}$) and 44% ($= \frac{0.905-0.830}{1-0.830}$), respectively. The results are similar when controlling for the policy-treatment interaction. Because 45% of mine-year observations include multiple managers, we also estimate specifications with manager-combination fixed effects, consistent with our AKM approach. These allow for potential synergies and complementarities within management teams and yield nearly identical adjusted R^2 values.

The proportional gains are sizable, although the corresponding increases in overall adjusted R^2 are more modest because mine and year fixed effects already absorb substantial variation. In particular, year fixed effects capture strong secular productivity growth associated with technological change: the least productive mine in 1980 was approximately as productive as the most productive mine in 1961. To isolate variation net of common year effects, Panel B residualizes TFPQ and markups with respect to year fixed effects and repeats the analysis. Relative to specifications with mine fixed effects alone, adding manager fixed effects increases adjusted R^2 by 13.5 percentage points for TFPQ and 15.2 percentage points for markups.

Mine Performance Around Managerial Hiring Events. Building on the evidence that the *Hüttenvertrag* increased managerial hiring at cokery mines and shifted hiring toward higher-ability managers, we assess Prediction 1 by examining how mine performance evolves around these hiring events. Specifically, we augment the baseline difference-in-differences specification in Equation (1) with event-time indicators for post-policy managerial hiring at cokery mines:

$$Y_{it} = \sum_{\substack{k=-7 \\ k \neq -1}}^6 \beta_k \times Manager_{it-k} + \sum_{j=1}^2 \gamma_j Exposure_i^{(j)} \times Post_t + \alpha_i + \lambda_t + \varepsilon_{it}. \quad (11)$$

We define an event as the hiring of at least one manager by a cokery mine during the post-policy period. Accordingly, $Manager_{it-k}$ equals one if mine i experiences such an event in year $t - k$, and zero otherwise. The coefficients β_k trace the dynamics of mine outcomes around these events relative to the omitted reference event time.²³

²³Unlike Fenizia (2022), we do not weight managerial changes by the difference in ability between departing and incoming managers. Because these differences are constructed from estimated manager-combination fixed effects, they may be measured imprecisely and introduce additional noise. Moreover, our sample size does not permit us to omit multiple event-time periods as in Fenizia (2022). We therefore use all post-policy hiring events at cokery mines as events of interest, noting that these events are associated, on average, with improvements in managerial quality (see Figure 10).

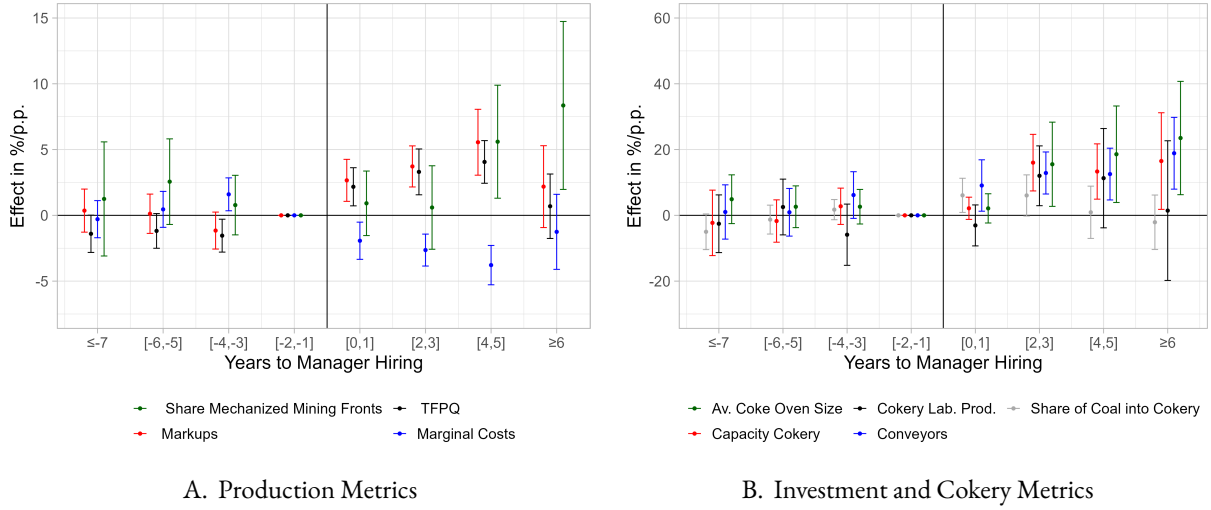


Figure 11: Managerial Hiring and Mine-Level Outcomes

Notes: This figure reports estimates from the event-study specification in Equation (11) for post-policy managerial hiring events at cokery mines. We bin leads and lags in two-year bins. Event time $k = [-2, -1]$ is the omitted reference period. The endpoints of the event-time window are binned following Schmidheiny and Sieglöch (2023). Standard errors are clustered at the mine level, and 90% confidence intervals are shown.

The specification also includes both baseline policy-treatment interactions, $Exposure_i^{(j)} \times Post_t$ for $j = 1, 2$. Here, $j = 1$ corresponds to the pre-policy share of coal output used for coke production, and $j = 2$ corresponds to the indicator for an attached cokery before the policy. We therefore interpret β_k as conditional associations between managerial hiring and mine outcomes beyond other effects of the policy.

Figure 11 presents the estimates from Equation (11). Panel A traces the dynamics of TFPQ, markups, marginal costs, and the mechanization rate around post-policy managerial hiring events at cokery mines. The mechanization rate captures a dimension of production technology not fully reflected in the machinery input used to estimate the production function. Because the specification conditions on both baseline policy-treatment interactions, the event-time coefficients do not mechanically reproduce the average policy effect; rather, they capture the additional association between managerial hiring and mine performance.

Panel A shows that managerial hiring events are followed by an increase of more than 5 percentage points in the mechanization rate. TFPQ rises by 2–3% and remains elevated for several years, while markups increase by approximately 3% and marginal costs decline. These patterns emerge shortly after hiring and are consistent with both the rapid productivity gains observed following the policy and Prediction 1. The magnitude of the TFPQ estimates is economically meaningful relative to the overall policy effect of approximately 5%. Given an average of 2.3 post-policy hiring events per cokery mine, the estimates suggest that managerial turnover may account for

a substantial share of the policy-induced productivity gains. Section 5.5 assesses this possibility using coefficient comparisons and mediation analysis.

Panel B shows that managerial hiring is also followed by increased investment in mines and cokeries. Conveyor-tunnel infrastructure expands by up to 15%, while cokery capacity and average oven size increase by as much as 25%. These patterns point to substantial capital adjustment in both mining operations and downstream cokeries. Cokery labor productivity also increases temporarily, consistent with improvements in downstream performance accompanying this capital adjustment. At the same time, the share of coal output allocated to cokeries rises temporarily by 5 percentage points, or approximately 10%. Thus, managerial hiring is followed not only by changes in investment and productivity but also by a temporary reallocation of production toward downstream cokery operations.

5.5 Quantifying the Management Channel

We next examine the extent to which the managerial changes documented above can account for the estimated effects of the *Hüttenvertrag* on mine performance. We focus on TFPQ and markups. We first conduct a descriptive coefficient-comparison exercise and then complement this analysis with a causal mediation analysis following Schenk (2025).

Coefficient-Comparison Exercise. Motivated by the causal-steps literature (Baron and Kenny, 1986; Alesina and Zhuravskaya, 2011; Jiang and Zhang, 2026), we begin by reestimating our baseline difference-in-differences specification with and without controls for managerial hiring.²⁴ The augmented specifications include leads and lags of post-policy managerial hiring events at cokery mines. We consider three variants: (i) pooled leads and lags for all hiring events, (ii) separate leads and lags for managers with and without STEM training, and (iii) separate leads and lags for low- and high- $\hat{\phi}_m$ hiring events.

Figure A6 reports the estimated policy coefficient from each specification, with TFPQ as the outcome. The blue estimate reproduces the baseline policy effect of 4–5% reported in Table 2. In the first set of specifications, controlling for leads and lags of all post-policy managerial hiring events at cokery mines reduces the estimated policy coefficient to a small, slightly negative value that is statistically indistinguishable from zero. The second

²⁴This exercise is not a formal causal mediation analysis. Managerial hiring is a nonrandom, post-treatment outcome, so conditioning on it may introduce post-treatment bias. We therefore interpret changes in the estimated policy coefficient as descriptive evidence consistent with a potential management channel, rather than as a causal decomposition of the policy effect.

and third sets of specifications show that this attenuation is concentrated in specifications that control for the hiring of STEM-trained and high-ability managers. By contrast, controlling for non-STEM, low-ability, and other hiring events leaves the estimated policy coefficient largely unchanged. The near-complete attenuation of the TFPQ coefficient when controlling for STEM-trained and high-ability hires suggests that the observed upgrading of management teams may account for an economically important share of the estimated policy effect on productivity. Together with the preceding evidence for Predictions 1 and 2, these results are consistent with changes in management-team composition constituting an important channel through which the policy affected productivity.²⁵

Appendix Figure A7 presents the analogous coefficient-comparison exercise for markups. Controlling for managerial hiring attenuates, but does not eliminate, the estimated policy effect on markups. This pattern is consistent with managerial changes contributing to the markup response, while also leaving room for direct pricing effects associated with reduced competition.

Causal Mediation Analysis. To complement the descriptive evidence, we next estimate causal mediation effects following Schenk (2025). This framework accommodates nonrandom assignment of both treatment and the mediator, allowing us to examine policy exposure and managerial hiring without requiring either to be randomly assigned (Deuchert et al., 2019; Kwon and Roth, 2026). It also relaxes the parallel-trends restrictions imposed by other panel-data mediation approaches (Huber and Oberhänsli, 2026) and does not require an instrumental variable (Frölich and Huber, 2017; Dippel et al., 2022). The approach combines three regressions: (i) a regression of the outcome Y on the policy indicator T , corresponding to Equation (1); (ii) a regression of the mediator M on T ; and (iii) a regression of Y on both T and M . The indirect effect operating through the mediator is the product of the coefficient on T in the second regression and the coefficient on M in the third. The coefficient on T in the third regression identifies the direct effect, defined as the component of the policy effect not operating through the mediator. A causal interpretation relies on additional identifying assumptions, including a monotonic effect of the policy on the mediator, limited heterogeneity in the mediator’s effect on the outcome across treatment states, and an extension of the parallel-trends assumption to the mediator.

²⁵Because the hiring controls are introduced sequentially, we also apply the decomposition proposed by Gelbach (2016) to the change between the baseline policy coefficient and the coefficient obtained after including all hiring controls. For the ability-based specification using the continuous exposure measure [% Coking Coal], controls for low- and high- $\hat{\phi}_m$ hiring events account for 19% and 83%, respectively, of the total attenuation, while controls for other hiring events account for -3% . Thus, high-ability hiring accounts for the largest share of the attenuation in the estimated policy coefficient.

Table 4 reports the results. Because the approach requires both treatment and the mediator to be binary, we use our binary exposure measure and define the mediator as an indicator for whether a cokery mine hired a manager during the five years following the policy’s introduction. Column (1) reports the pooled policy effect, corresponding to our baseline estimate. Column (2) reports the policy effect on managerial hiring, while column (3) includes both treatment and the mediator in the outcome regression. The estimated indirect effect is $0.043 \times 0.634 \approx 0.028$, accounting for approximately 62% of the total effect of 0.044. The 95% bootstrap confidence interval for the mediated share is [41.5%, 88.9%].

Table 4: Managerial Hiring as a Mediator of Policy Effects

	TFPQ			Markups		
	ln(TFPQ) (1)	Manager hiring (2)	ln(TFPQ) (3)	ln(Markup) (4)	Manager hiring (5)	ln(Markup) (6)
$1[\text{Attached Cokery}]_i \times \text{Post}_t$	0.044*** (0.016)	0.634*** (0.063)	0.017 (0.018)	0.136*** (0.019)	0.634*** (0.063)	0.118*** (0.021)
$1[\text{Post Manager Hiring}]_{it}$			0.043*** (0.014)			0.029** (0.013)
<i>Indirect effect</i>			0.028***, ^x (0.009)			0.018***, ^x (0.009)
<i>Mediated share</i>			62.0%			13.4%
95% bootstrap CI			[41.5%, 88.9%]			[6.8%, 20.1%]
Mine FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	885	885	885	885	885	885

Notes: Columns (1)–(3) analyze TFPQ; columns (4)–(6) analyze markups. Columns (1) and (4) report the total policy effect; columns (2) and (5) use the binary managerial-hiring mediator as the outcome; columns (3) and (6) additionally control for this mediator. Manager hiring refers to the indicator defined in Section 5.5 for hiring a manager during the first five years after the policy. Attached cokery is a pre-policy indicator, and Post denotes years after 1969. Standard errors are in parentheses. The mediated share is expressed as a percentage; its 95% confidence interval, in brackets, is based on 299 bootstrap repetitions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. ^x indicates rejection at the 5% level in the VGVG test for the indirect effect (van Garderen and van Giersbergen, 2024).

For markups, the estimates in columns (4) to (6) imply a smaller mediated share, consistent with the descriptive evidence. The estimated indirect effect of 0.018 accounts for 13% of the total effect of 0.136, with a 95% bootstrap confidence interval for the mediated share of [6.8%, 20.1%].

6 Robustness Checks

We next assess whether our findings are robust to alternative explanations and threats to identification.

Aggregate Productivity Growth: Within-Mine Improvements and Reallocation. Our baseline identification strategy estimates relative policy effects by comparing German coal mines with heterogeneous exposure. By itself, this comparison does not reveal much about industry-level productivity trends and reallocation across mines. We therefore provide two pieces of complementary evidence.

First, following [Olley and Pakes \(1996\)](#), we decompose the change in aggregate TFPQ in the German coal industry into within-mine productivity growth and reallocation across mines. Aggregate TFPQ is 23% higher in the post-policy period than in the pre-policy period, with within-mine productivity growth accounting for 16 percentage points of this increase. Thus, most of the aggregate productivity growth reflects improvements within mines rather than the reallocation of economic activity across mines.

Second, Appendix Figure [A8](#) compares the evolution of the German and Belgian coal industries around the policy's introduction. Mines in the two countries operated in the same cross-border coalfield and thus faced broadly comparable geological conditions and natural endowments. Consistent with this comparability, labor productivity, output, and employment followed similar trends in the two countries before the policy.²⁶ Following the policy, industry-level labor productivity rose more strongly in Germany than in Belgium (Panel A), consistent with an aggregate productivity gain in the German coal industry. Moreover, output and employment declined more slowly in Germany (Panels B and C), making it unlikely that selective exit of low-productivity German mines fully accounts for the productivity divergence.

Strategic Responses by Non-Cokery Mines. Although mines without integrated cokeries could not produce coke themselves, those with access to fat-coal deposits could nevertheless respond to the policy by expanding their production of coking coal for sale to external cokeries. Over our sample period, 41% of non-cokery mines produced at least some fat coal. Appendix Figure [A9](#) documents fat-coal production by mine type along two margins. The extensive margin indicates whether a mine produced any fat coal, while the intensive margin measures the share of fat coal in its total output. Nearly all cokery mines produced fat coal, whereas approximately

²⁶Both countries were members of the European Coal and Steel Community, the predecessor of today's European Union, which provided a common tariff-free market for coal ([Nonn, 2001](#); [Fischer, 2026a](#)). Direct cross-country spillovers through bilateral coal trade were nevertheless limited: Germany exported approximately 3% of its coal output to Belgium, while Belgium exported approximately 2% of its output to Germany.

40% of non-cokery mines did so before the policy. Following the policy, the share of fat coal in total output also increased among non-cokery mines.

This pattern indicates that some less-exposed mines also adjusted production in response to the policy, although the response appears smaller than at mines with integrated cokeres. If this indirect response affected outcomes at comparison mines in the same direction as at more-exposed mines, it would attenuate the estimated differences between the groups. Under this condition, our difference-in-differences estimates provide a conservative measure of the policy effects at more-exposed mines. To address this spillover, we reestimate the baseline specifications using only non-cokery mines with no fat-coal deposits as controls. This restriction relies on predetermined geological endowments rather than post-policy production choices. The results remain qualitatively unchanged (Table B7).

Demand Shocks and Input Underutilization. A potential concern is that unanticipated demand shocks could correlate with the policy exposure. If demand is realized only after input choices have been made, a negative demand shock can leave previously committed inputs underutilized and thereby mechanically reduce measured TFPQ. Conversely, if the policy disproportionately reduced demand shortfalls at more-exposed mines, their measured TFPQ could increase without an improvement in underlying productive efficiency. To examine this possibility, we exploit the data on shift cancellations due to insufficient demand. The left panel of Appendix Figure A10 shows no statistically detectable policy effect on either the extensive margin, measured by whether a mine cancels any shifts because of insufficient demand, or the intensive margin, measured by the share of shifts canceled. These results provide no evidence that differential demand shortfalls or changes in input utilization explain our estimated productivity effects.

The left-panel results also provide little support for increased demand certainty as an alternative explanation for the estimated policy effects. To examine broader changes in demand volatility, we calculate within-mine standard deviations of annual output and employment separately for the pre- and post-policy periods and use these measures as outcomes in our difference-in-differences specification. The right panel of Appendix Figure A10 shows no statistically detectable reduction in either measure at more-exposed mines relative to less-exposed mines. Overall, these findings provide no evidence that the policy materially reduced demand uncertainty.

Coal-Type-Specific Trends. A separate identification concern is that policy exposure may be correlated with mines' pre-policy product mix and that different coal types may follow distinct secular trends. We address this

concern by augmenting our baseline TFPQ specification with interactions between a linear time trend and each mine's pre-policy production shares across seven coal types. As shown in Appendix Figure A11, the estimates remain virtually unchanged, indicating that differential trends associated with mines' pre-policy product mix do not account for our findings.

Balanced Sample. Because the policy affected mine survival, differential exit could alter the composition of the estimation sample and potentially contribute to the estimated productivity effects. As a sensitivity check, we restrict the sample to mines that remain in operation in 1980 and reestimate our baseline specifications. Appendix Figure A11 shows that the estimates remain stable, indicating that the productivity findings also hold among long-lived mines and are not solely attributable to selective attrition.

Local Economic Shocks. A potential concern is that policy exposure may be correlated with time-varying local economic conditions, causing the baseline estimates to capture regional shocks rather than policy effects. We address this concern by augmenting our baseline specifications with mining-region-by-year fixed effects for five mining regions. Identification therefore comes from comparisons among mines with different levels of policy exposure within the same mining region and year, absorbing time-varying shocks common to each region. These regions were delineated by *Ruhrkohle AG* and correspond to distinct subsidiary management structures. As shown in Appendix Figure A11, the productivity estimates remain stable, indicating that differential regional shocks do not account for our findings.

Mine Mergers. Several mines underwent consolidation during the sample period. Because merger decisions may reflect expectations about future profitability or productive potential and may independently affect mine performance, merger activity could confound the estimated policy effects. We therefore augment our baseline specifications with controls for mine-level mergers. As shown in Appendix Figure A11, the estimates remain virtually unchanged, indicating that mine consolidation does not account for our findings.

Spatial and Finite-Sample Inference. We assess the sensitivity of our statistical inference to spatial dependence and alternative finite-sample procedures. Because the mines are concentrated within an area of approximately 50×100 km, local spillovers may induce spatial correlation in the regression residuals. We therefore conduct inference using the spatial standard errors developed by Conley (1999), with a 10-km distance cutoff. As a complementary check, we implement a wild-cluster bootstrap at the mine level using 999 replications. Appendix

Figure [A11](#) shows that our conclusions remain unchanged under both procedures.

Weighting by Mine Size. Our baseline specifications assign equal weight to each mine-year observation despite substantial differences in mine size: output in 1969 ranged from 0.3 to 3.5 million tonnes. Because larger mines contribute more to industry-level aggregates, we reestimate the baseline specifications weighting each observation by the mine’s pre-policy output in 1969. Appendix Figure [A11](#) shows that the weighted estimates are similar to the baseline estimates, indicating that our findings are not sensitive to the equal-weighting scheme.

Alternative Pre-Policy Exposure Window. Our baseline exposure measures draw on the full pre-policy period to capture mines’ longer-run exposure. Because exposure in the years immediately preceding the policy may more closely reflect treatment intensity at the time of implementation, we reconstruct both measures using only the final four pre-policy years and reestimate the baseline specifications. Appendix Figure [A11](#) shows that the resulting estimates are similar to their baseline counterparts, indicating that the findings are not sensitive to the pre-policy window used to construct the exposure measures.

Synthetic Difference-in-Differences. As an alternative estimation strategy, we apply the synthetic difference-in-differences estimator of [Arkhangelsky et al. \(2021\)](#). The estimator reweights units and pre-treatment periods to improve balance in pre-policy outcome dynamics, providing a complementary robustness check on the baseline difference-in-differences estimates. Its implementation in our setting imposes two restrictions. First, it requires a balanced panel, restricting the analysis to mines observed throughout the sample period. Second, it requires a binary treatment, so we implement it using only the binary exposure indicator. Appendix Figure [A11](#) shows that the resulting estimate is similar to the corresponding baseline estimate. Statistical inference is based on a mine-level cluster bootstrap.

Staggered Difference-in-Differences. The staggered difference-in-differences estimates presented above show that managerial hiring is associated with increases in productivity and markups. However, recent work shows that conventional event-study estimates can be biased when treatment effects vary across cohorts treated at different times (e.g., [De Chaisemartin and d’Haultfoeuille, 2020](#); [Callaway and Sant’Anna, 2021](#); [Sun and Abraham, 2021](#); [Borusyak et al., 2024](#)). To address this concern, we reestimate the effects on our main outcomes using the estimator proposed by [De Chaisemartin and d’Haultfoeuille \(2026\)](#). The results, reported in Figure [A12](#), remain qualitatively unchanged.

7 Conclusion

This paper examines the mine-level productivity effects of the *Hüttenvertrag*, a protective policy that sharply curtailed German steel producers' foreign sourcing of coking coal and coke and thereby reduced the import competition faced by domestic coal suppliers. We employ a difference-in-differences design that exploits variation in mines' pre-policy exposure to coke production, which is closely tied to the geological suitability of their coal deposits for coking. Our estimates indicate that the policy increased TFPQ by 4–5% and reduced marginal costs by 3–4% at more exposed mines relative to less exposed mines. The productivity gains were concentrated among mines with low pre-policy TFPQ. More-exposed mines also increased mechanization, reorganized production, and experienced higher survival probabilities over the subsequent decade. At the same time, output prices and markups increased, indicating that the efficiency gains were not fully passed through to downstream buyers. In this setting, the policy thus generated within-mine efficiency gains alongside the standard price and markup effects of weaker competition.

Matched manager–mine data point to changes in management-team composition as an important adjustment channel. More-exposed mines recruited more STEM-trained managers and shifted toward management teams with higher estimated ability. These compositional changes persisted beyond the initial hiring response. Post-policy managerial hiring events at cokery mines were associated with subsequent increases in mechanization, investment, and TFPQ and with declines in marginal costs. These patterns are consistent with the policy reshaping manager–mine matching and point to managerial upgrading as an economically important margin of adjustment.

These findings qualify, rather than overturn, the conventional view that weaker competition can reduce productive efficiency. They do not imply that protection generally raises productivity, nor do they establish that the *Hüttenvertrag* improved welfare. Although the policy generated within-mine efficiency gains, it also increased output prices and markups. Rather, our results indicate that the productivity consequences of protection may depend partly on how the associated change in competitive conditions reshapes the allocation of managerial talent and firms' capacity to reorganize production. More broadly, our findings speak to renewed concerns over the return of protectionism ([Amiti et al., 2019](#); [Fajgelbaum et al., 2020](#); [Antràs and Chor, 2022](#); [Fajgelbaum et al., 2024](#)) by highlighting managerial composition and manager–firm matching as important margins to consider alongside conventional product-market effects when evaluating industrial policy.

Appendix

A Figures

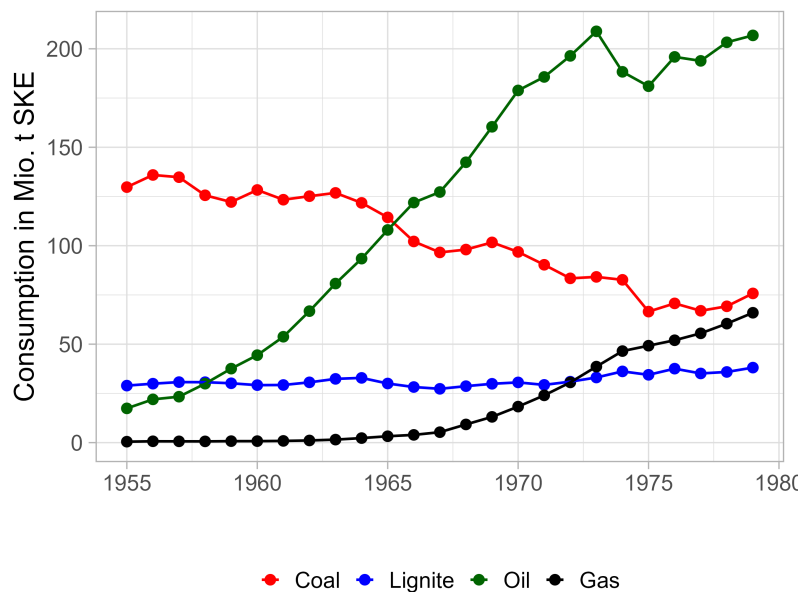


Figure A1: German Energy Consumption by Source

Notes: This figure reports the composition of the German energy mix over time. The data are drawn from the statistical sections of the *Yearbooks of the German Mining Industry*.

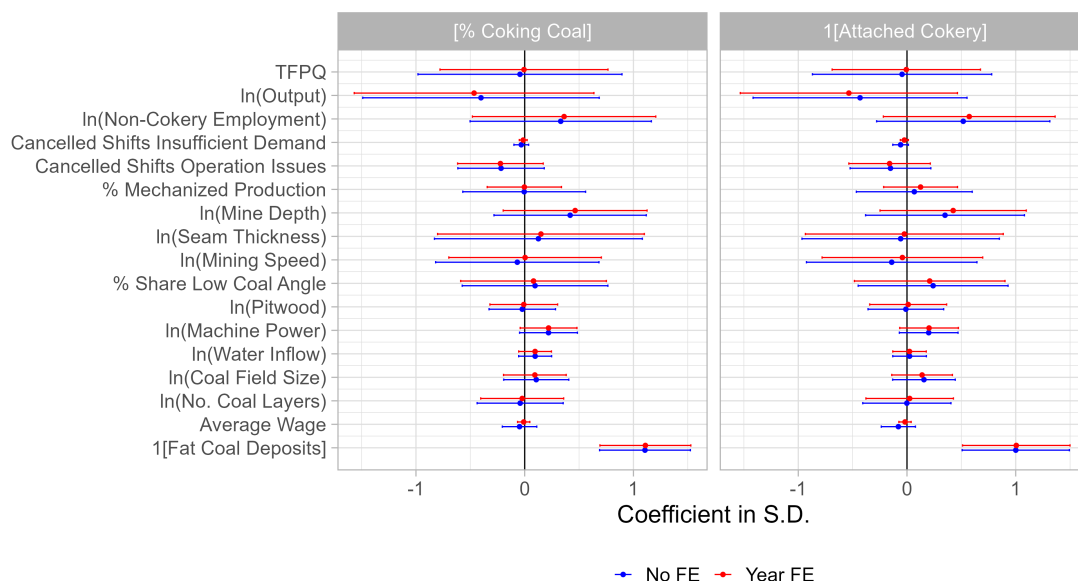


Figure A2: Pre-Policy Correlates of Policy Exposure

Notes: The figure reports coefficient estimates from multivariate regressions of each standardized policy-exposure measure on the listed pre-policy mine-year characteristics. The dependent exposure measure is indicated in each panel heading. Specifications with and without year fixed effects are shown. Standard errors are clustered at the mine level; horizontal bars denote 90% confidence intervals.

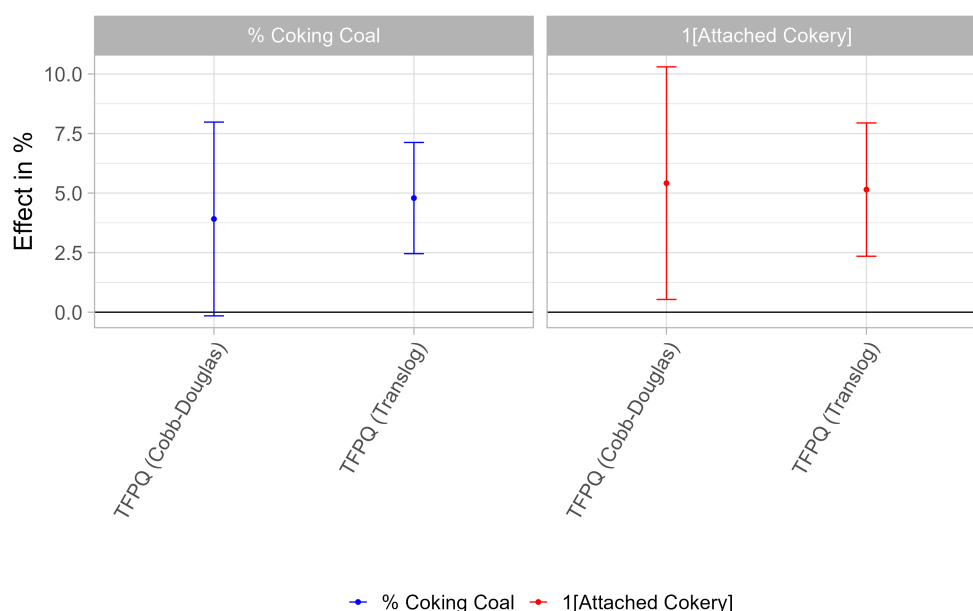


Figure A3: Policy Effects on TFPQ under Alternative Production-Function Specifications

Notes: This figure gives the regression results of Equation (2) with TFPQ based on different production functions as outcomes. Standard errors are clustered at the mine level and 90% confidence bands are reported.

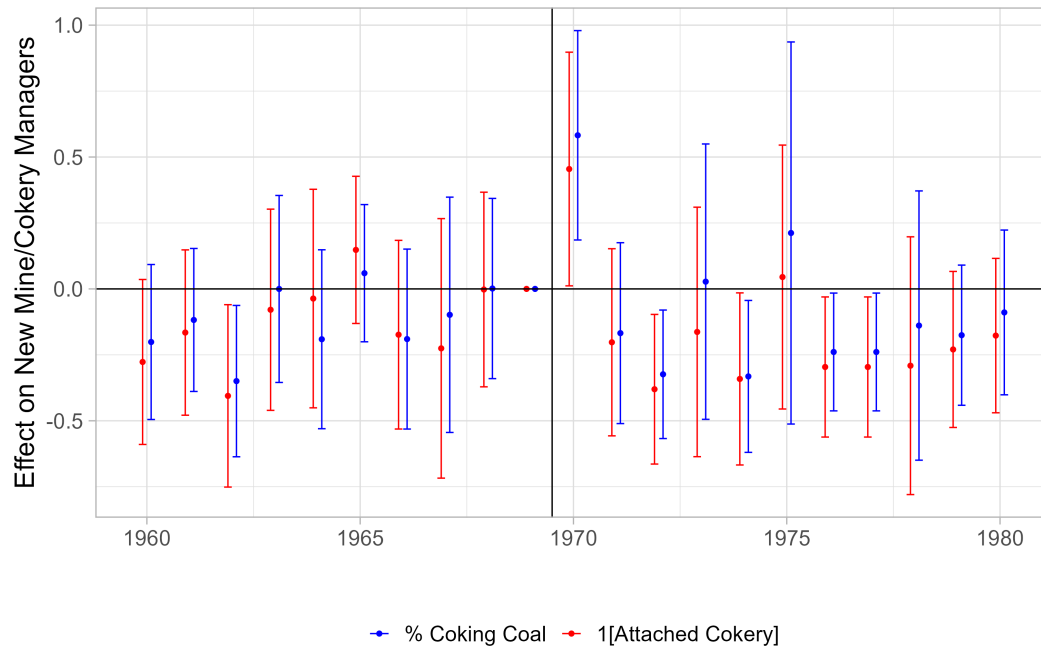


Figure A4: Effect of the *Hüttenvertrag* Policy on the Number of Newly Hired Mine/Cokery Managers

Notes: This figure gives the regression results of Equation (2) with the number of newly hired managers as an outcome. Standard errors are clustered at the mine level and 90% confidence bands are reported.

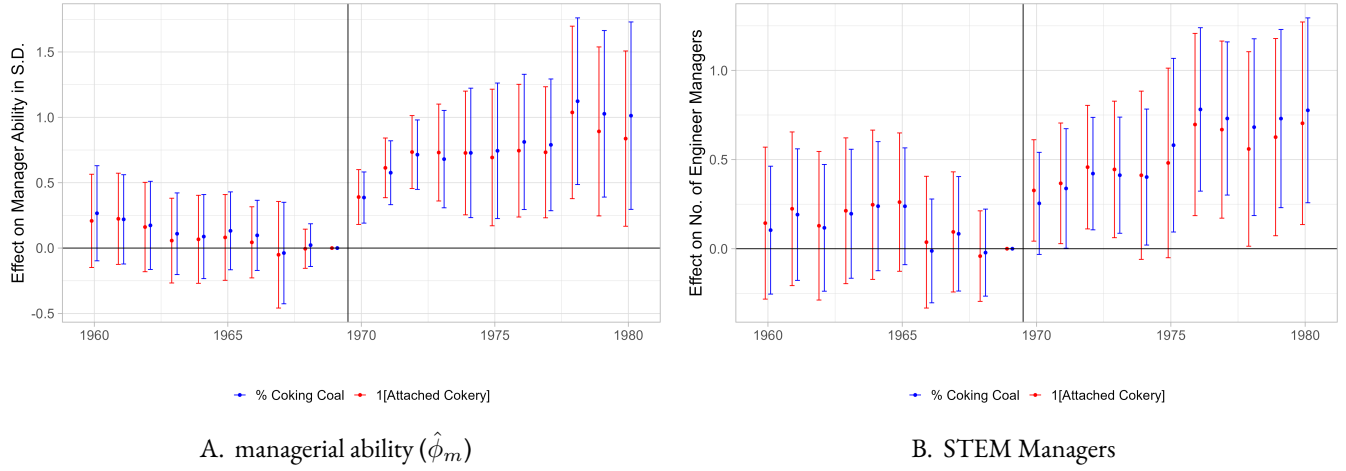


Figure A5: Dynamic Effects of the *Hüttenvertrag* Policy on Managerial Ability and STEM-Trained Managers

Notes: This figure gives the effect on managerial ability and the number of STEM managers. In the regressions on STEM managers, we control for the overall number of managers.

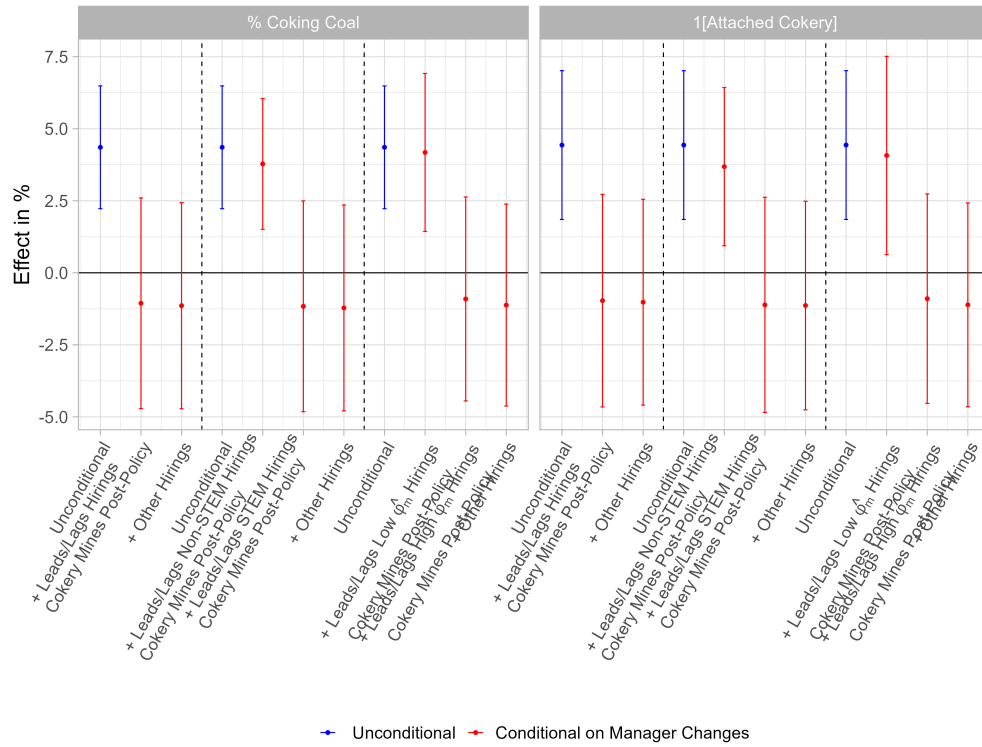


Figure A6: Policy Effect on TFPQ with and without Controls for Managerial Hiring

Notes: This figure reports the estimated coefficient on the policy interaction from the baseline difference-in-differences specification and from augmented specifications that include leads and lags around managerial hiring events. The outcome is TFPQ. Standard errors are clustered at the mine level, and 90% confidence intervals are shown.

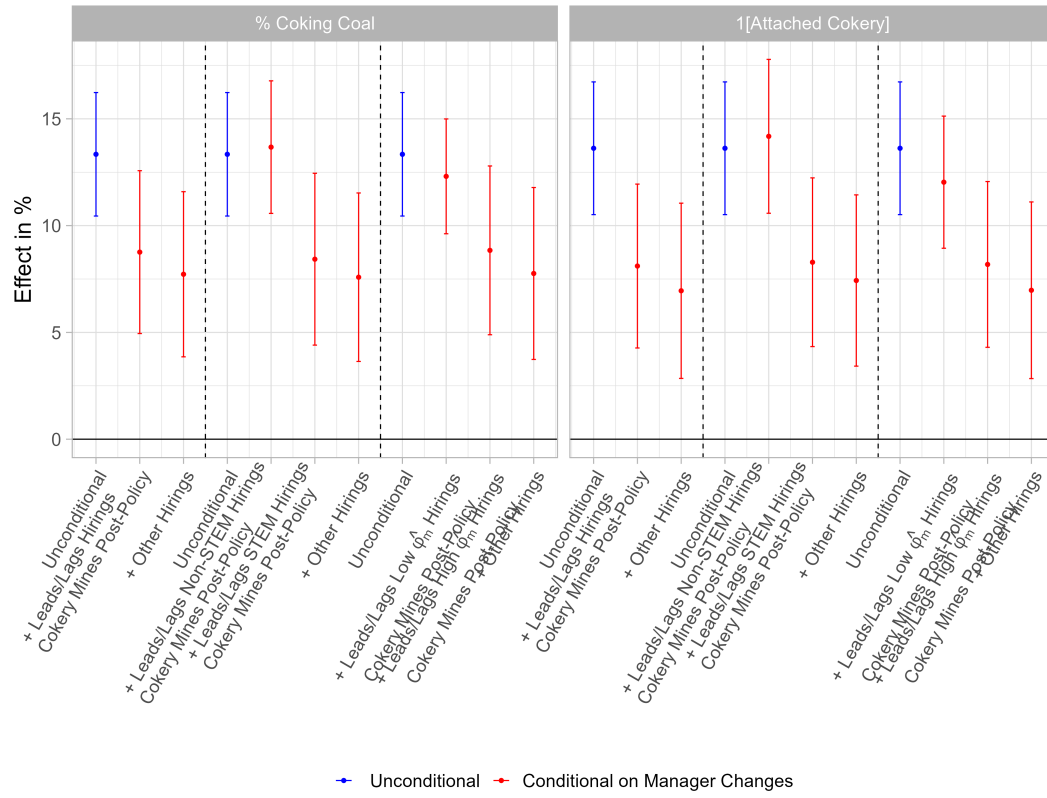


Figure A7: Policy Effects on Markups with and without Managerial-Hiring Controls

Notes: This figure gives the effect of the policy (un)conditional on manager hirings. Standard errors are clustered at the mine level and 90% confidence bands are reported.

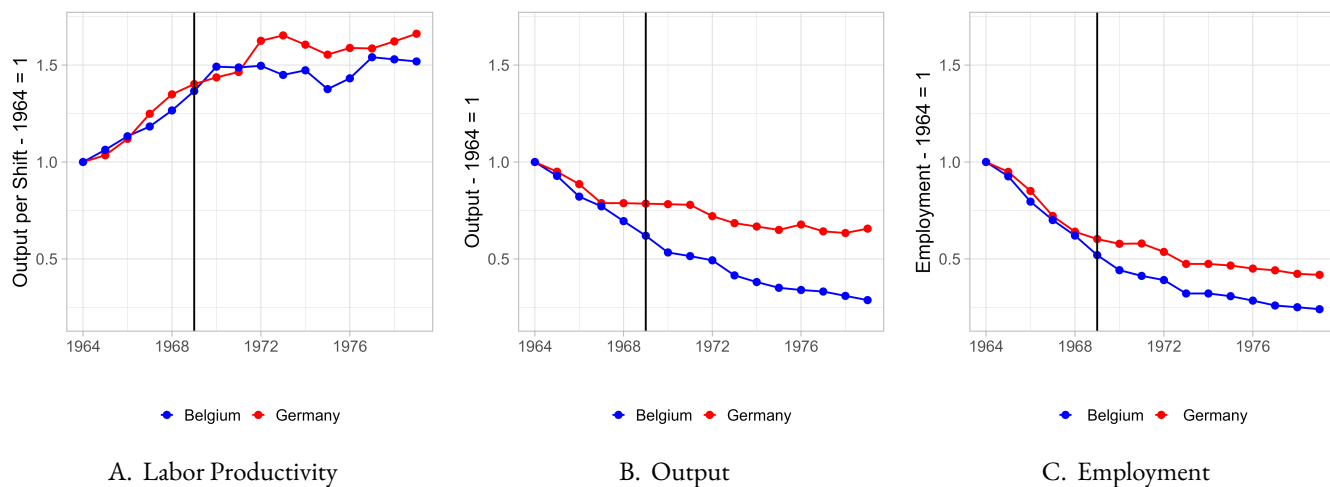


Figure A8: German and Belgian Coal Mining Industry: Development over Time

Notes: This figure gives the size and productivity of the German and Belgian coal mining industry over time. Variables standardized to 1964 values to make pre-trends comparable.

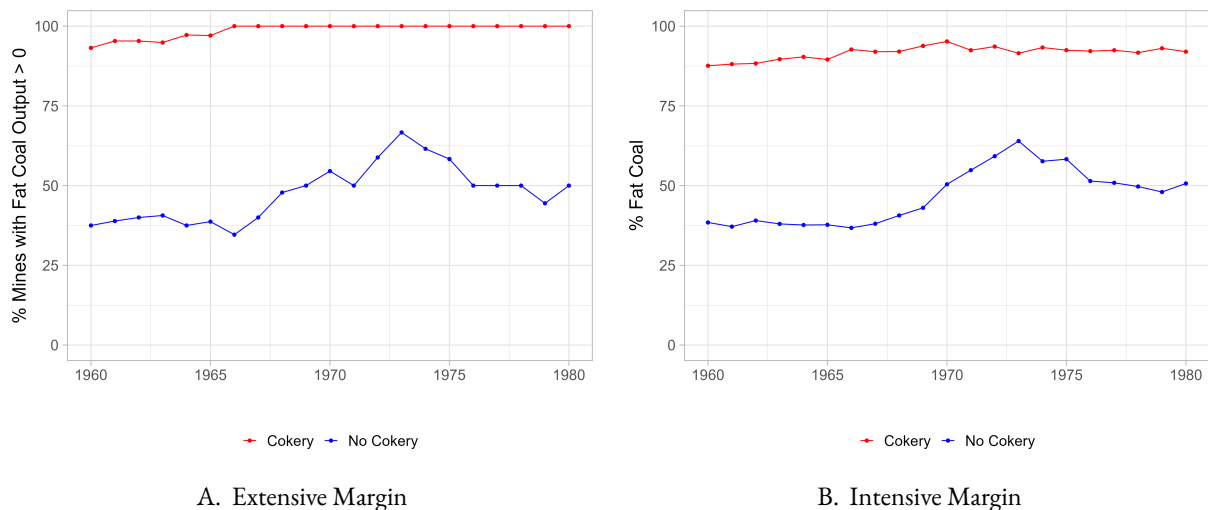
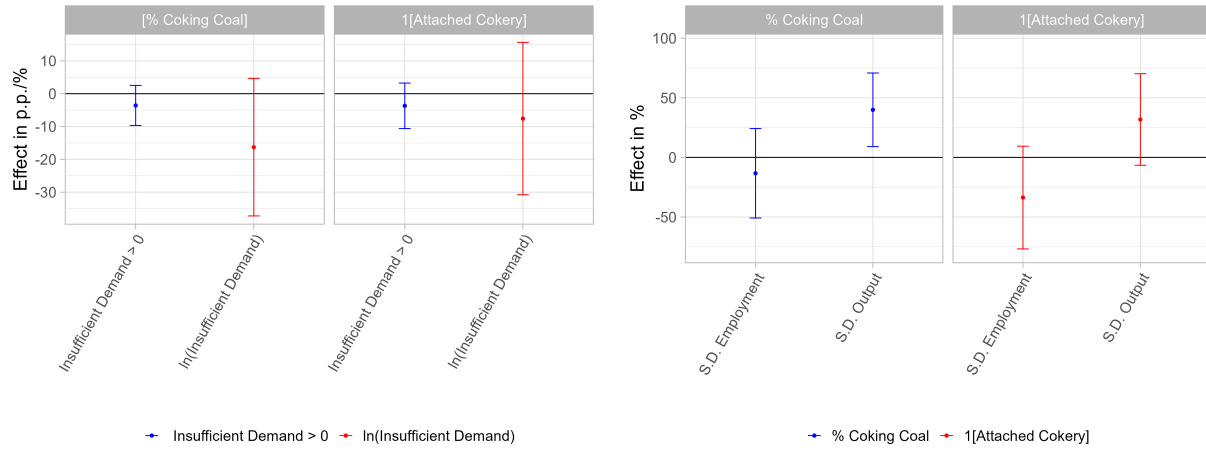


Figure A9: Trends in Fat-Coal Production by Cokery Status

Notes: Panel A reports the share of mines with positive fat-coal output (the extensive margin), and Panel B reports fat coal as a share of total mine output (the intensive margin), separately by cokery status over time.



A. Insufficient Demand

B. Output and Employment Fluctuations

Figure A10: Effect of the *Hüttenvertrag* Policy on Insufficient Demand and Output/Employment Fluctuations

Notes: The left panel reports pooled estimates from Equation (1). The outcomes are the number of shifts cancelled because of insufficient demand per 100 shifts and an indicator for whether this measure is positive. The right panel reports mine-level difference-in-differences estimates for the within-mine standard deviations of output and employment. Each standard deviation is calculated separately for the pre- and post-policy periods, and the regressions include mine fixed effects, a post-policy indicator, the policy-exposure interaction, and the logarithm of the number of observed years in the relevant mine-period. Standard errors are clustered at the mine level, and 90% confidence intervals are reported.

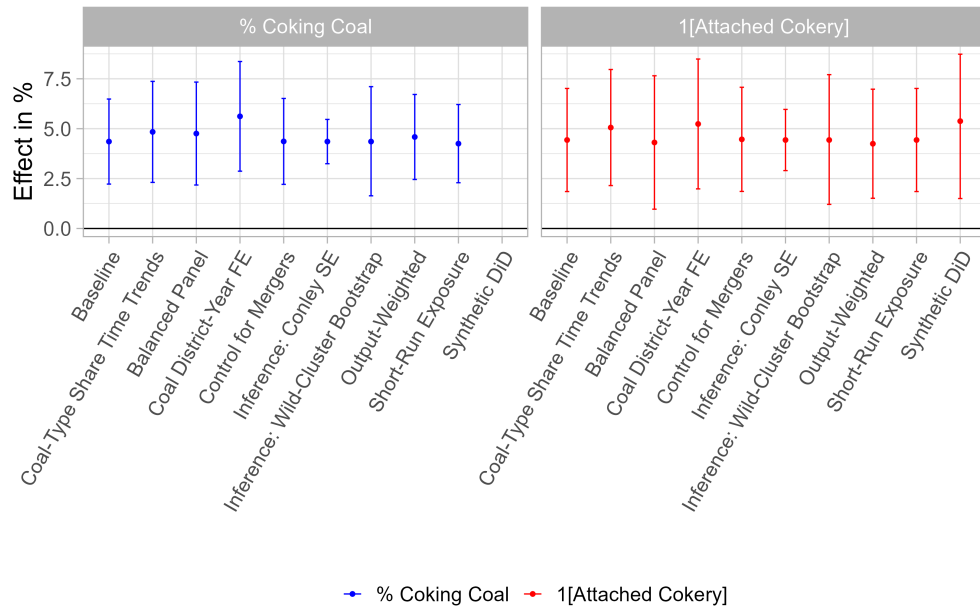


Figure A11: Effect of the *Hüttenvertrag* Policy on TFPQ: Various Robustness Checks

Notes: This figure reports the estimated policy coefficient for TFPQ from the baseline pooled specification in Equation (1) and from the alternative pooled specifications indicated on the horizontal axis. These alternatives vary the fixed effects, sample restrictions, and control variables relative to the baseline. For the Conley, wild-cluster-bootstrap, and synthetic DiD specifications, confidence intervals are constructed using the corresponding inference procedure; all other specifications report standard errors clustered at the mine level. 90% confidence intervals are reported.

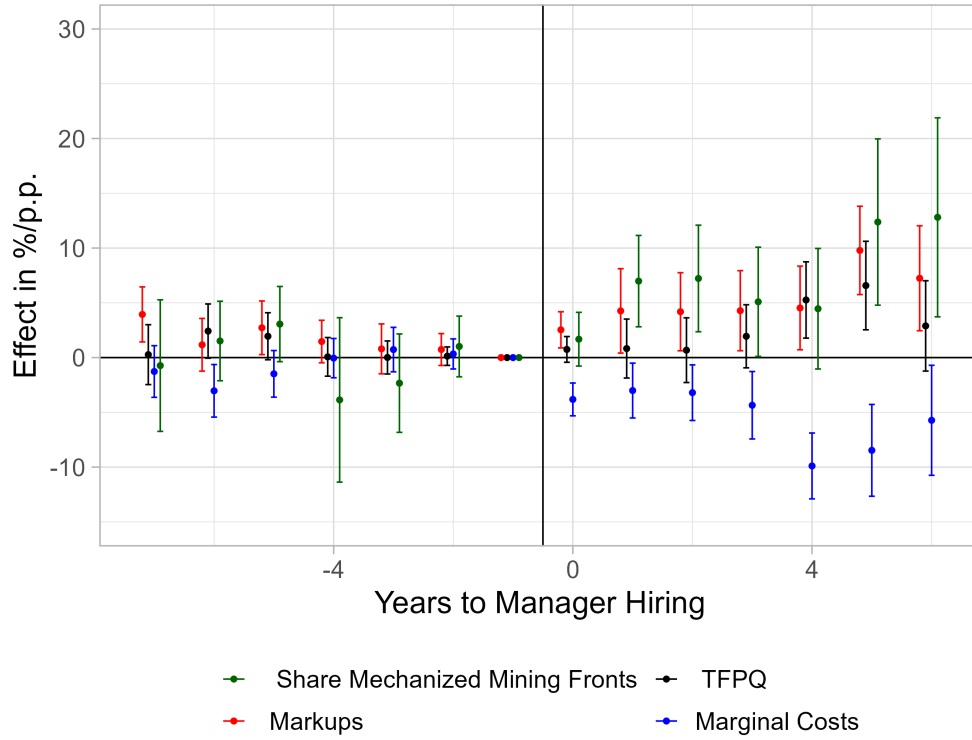


Figure A12: Managerial Hiring and Mine-Level Outcomes

Notes: This figure reports estimates from the event-study specification in Equation (11) for post-policy managerial hiring events at cokery mines. We estimate the specification using the method of [De Chaisemartin and d'Haultfoeuille \(2026\)](#). Event time $k = -1$ is the omitted reference period. Estimates are not binned and endpoints are not binned. Standard errors are clustered at the mine level, and 90% confidence intervals are shown.

B Tables

Table B1: Descriptive Statistics for Mines

Variable	N	Mean	SD	Minimum	Maximum
Panel A: Output and Inputs					
Raw Output (in Mio. t)	974	2.57	1.4	0.19	7.53
Pure Coal Output (in Mio. t)	974	1.66	0.82	0.15	4.21
Total Employment	974	3736.03	1709.23	249	12001
Below Surface Employment	974	2382.03	1115.44	159	8022
Machinery Power (in kW)	974	9447.1	7791.9	295	45774
Pit Wood Consumption (in fm)	974	22589.48	14420.79	678.36	83170.71
Leaving Staff	974	414.39	260.24	16	2223
Joining Staff	974	244.62	225.39	0	2378
Panel B: Mechanization and Electrification					
Mining Points	974	10.37	6.48	1	46
Mechanized Mining Points	974	6.07	3.69	0	28
Fully Mechanized Production (in %)	594	88.45	18.92	0	100
Partly Mechanized Production (in %)	594	1.21	6.64	0	80.2
Electrified Machinery Power (in kW)	974	7547.17	7072.1	0	41723
Panel C: Cokery Information					
1[Cokery]	974	0.48	0.5	0	1
Cokery Employment	462	316.51	148.9	4	790
Cokery Output (in Mio t.)	463	0.76	0.45	0	2.6
Cokery Coal Input (in t per Day)	463	2737.19	1619.97	5	9240
Cokery Ovens	465	155.99	66.6	45	360
Panel D: Mine Infrastructure					
Non-Winding Shafts	974	4.64	3.24	0	15
Winding Shafts	974	1.71	0.8	1	5
Depth of Coal Mine (in m)	974	977.93	172.42	284	1324
Number Coal Seams Worked On	974	9.91	6.26	1	33

Table B2: Manager Characteristics

	All Managers	Managers with ≥ 2 Mines
Male	1.00	1.00
PhD	0.26	0.25
STEM Education	0.51	0.56
Mining Education	0.33	0.40
First Year Observed	1961	1962
Last Year Observed	1969	1972
Years in Sample	9.00	13.51
Number of Mines Worked For	1.43	2.39
N	313	96

Table B3: Production Function Estimates

	OLS (1)	GMM (2) (3)	
β_L	0.809 (0.050)	0.769 (0.083)	0.760 (0.089)
β_K	0.186 (0.032)	0.126 (0.046)	0.138 (0.063)
$\beta_L \times \mathbb{1}[Cokery_{it}]$			0.040 (0.106)
$\beta_K \times \mathbb{1}[Cokery_{it}]$			-0.037 (0.097)
Scale Economies	0.995 (0.030)	0.895 (0.068)	
Observations	885	885	885

Notes: Block-bootstrapped standard errors for mine- and cokery-level clusters with 100 bootstrap repetitions. OLS controlling for treatment variables, GMM through law of motion.

Table B4: Policy Effects on Markups with(out) Marginal-Cost Controls

	ln(Markup) (1)	ln(Markup) (2)	ln(Markup) (3)
Panel A: % Coking Coal			
$Exposure_i \times Post_t$	0.133*** (0.018)	0.100*** (0.011)	0.104*** (0.011)
First-stage F -test			76.08
Panel B: $1[Attached\ Cokery]_i$			
$Exposure_i \times Post_t$	0.136*** (0.019)	0.098*** (0.012)	0.103*** (0.012)
First-stage F -test			75.47
Mine FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Control: Logged MC	No	Yes	Yes
Instruments			$\ln(MC_{it-1})$
Observations	885	885	801

Notes: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Coefficients for the continuous exposure multiplied by the mean exposure for cokery mines to scale coefficients and standard errors. Standard errors clustered at the mine level. F-statistic of first stage is reported for the IV.

Table B5: Correlates of Newly Hired Manager-Combination Fixed Effects

	Newly Hired Manager-Combination FE
$\% \Delta \text{Output}_{t,t-1}$	-0.261 (0.177)
$\% \Delta \text{Employment}_{t,t-1}$	0.256* (0.150)
$\% \Delta \text{Miners}_{t,t-1}$	0.092 (0.144)
$\% \Delta \text{Coal Field Size}_{t,t-1}$	0.0003 (0.017)
$\% \Delta \text{Labor Productivity}_{t,t-1}$	0.208 (0.153)
Region FE	Yes
Positive Coking-Coal Exposure	Yes
Baseline Values	Yes
Connected Set FE	Yes
Observations	278

Notes: The dependent variable is the estimated fixed effect of the manager combination observed after a change in a mine's management team, $\hat{\phi}_{m(i,t)}$, obtained from Equation (9). Standard errors clustered at the mine level are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table B6: Explanatory Power of Manager and Manager-Combination Fixed Effects

	ln(TFPQ) (1)	ln(TFPQ) (2)	ln(TFPQ) (3)	ln(Markup) (4)	ln(Markup) (5)	ln(Markup) (6)
Panel A: Baseline Specifications						
Without Policy Controls:						
<i>Adj. R²</i>	0.915	0.945	0.945	0.830	0.905	0.907
With Policy Controls:						
<i>Adj. R²</i>	0.918	0.945	0.945	0.873	0.907	0.907
Mine FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Manager FEs	No	Yes	No	No	Yes	No
Man. Combination FEs	No	No	Yes	No	No	Yes
Panel B: Outcomes Residualized by Year FEs						
Without Policy Controls:						
<i>Adj. R²</i>	0.669	0.804	0.805	0.631	0.783	0.782
With Policy Controls:						
<i>Adj. R²</i>	0.678	0.805	0.805	0.656	0.782	0.782
Mine FEs	Yes	Yes	Yes	Yes	Yes	Yes
Manager FEs	No	Yes	No	No	Yes	No
Man. Combination FEs	No	No	Yes	No	No	Yes
Observations	885	885	885	885	885	885
Managers		313			313	
Manager Combinations			315			315

Notes: The table reports adjusted R^2 values from mine-year regressions. Columns (1)–(3) use $\ln(\text{TFPQ})$ as the dependent variable, and columns (4)–(6) use $\ln(\text{Markup})$. All specifications in Panel A include mine and year fixed effects. In Panel B, each dependent variable is first residualized with respect to year fixed effects, and all specifications then include mine fixed effects. Columns (2) and (5) additionally include indicators for the individual managers serving at a mine in a given year, whereas columns (3) and (6) instead include fixed effects for the complete manager combination observed in that mine-year. Specifications labeled “With Policy Controls” additionally control for $[\% \text{ Coking Coal}]_i \times \text{Post}_t$ and $\mathbb{1}[\text{Attached Cokery}]_i \times \text{Post}_t$. All columns use the same 885 mine-year observations, covering 313 managers and 315 manager combinations.

Table B7: Effects of the Policy on Productivity and Markups: With and Without Non-Cokery Coal Mines with Positive Fat Coal Output

	ln(TFPQ)		ln(Markup)	
	(1)	(2)	(3)	(4)
Panel A: % Coking Coal				
$Exposure_i \times Post_t$	0.044*** (0.013)	0.039*** (0.014)	0.133*** (0.018)	0.146*** (0.019)
Panel B: $1[Attached\ Cokery]_i$				
$Exposure_i \times Post_t$	0.044*** (0.016)	0.038** (0.017)	0.136*** (0.019)	0.153*** (0.020)
Mine FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Non-Cokery Mines With Fat Coal Output Included	Yes	No	Yes	No
Observations	885	674	885	674

Notes: *p<0.1; **p<0.05; ***p<0.01. Non-cokery mines with fat coal output are defined as those with fat coal output in at least one year in the sample period. Coefficients for the continuous exposure multiplied by the mean exposure for cokery mines to scale coefficients and standard errors.

C Manager Turnover in German Coal Mining

To characterize managerial mobility in the German coal mining industry, we trace the origins of entering managers and the destinations of departing managers. Our annual managerial rosters do not capture complete employment histories outside mine management. We, however, supplement them with biennial information on second-tier positions, which allows us to classify additional transitions within the industry. Table C1 summarizes managerial mobility.

Table C1: Managerial Mobility: Origins and Destinations

	Share
Panel A: Origins of Entering Managers	
Previously a manager at another mine	32%
Promoted from a second-tier position at the same mine	13%
Previously in a second-tier position at another mine	13%
Other origins	42%
Panel B: Destinations of Departing Managers	
Subsequently a manager at another mine	24%
Other destinations	76%

Notes: Panel A reports shares of observed managerial entries by prior position; Panel B reports shares of observed managerial departures by subsequent destination. Each panel sums to 100%. Transitions are traced using annual managerial rosters supplemented by biennial records of second-tier positions.

Panel A shows that, among newly hired managers, 32% previously managed another mine. A further 13% previously held a second-tier position at another mine, while another 13% were promoted from a second-tier position within the same mine. The remaining hires are not observed in either managerial or second-tier positions in our sample before their appointment. They may have entered from other industries or from positions outside the operational mine rosters, such as corporate board positions at mine-owning firms. Thus, managerial hiring involved not only cross-mine reallocation but also promotions and entry from outside the observed pool of mine managers.

The destinations of departing managers reveal a similar pattern. Panel B shows that 24% of departing managers are subsequently observed as managers at another mine in the sample; the remaining 76% are not observed in such positions. Moreover, 12% of all departures coincide with the closure of the mine previously managed by the departing individual. These patterns further indicate that managerial turnover was not limited to the reallocation of managers across observed mines.

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