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The Cost of Job Loss for Coal Miners in Turkey

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Abstract

This paper estimates the labor-market costs of job displacement among coal miners in Turkey, a middle-income country where coal phase-out has not yet begun but may become increasingly relevant under domestic and international decarbonization pressures. To get close to the true costs of a coal shutdown, our main focus is on those displaced workers in the coal sector who find re-employment in other sectors. Using linked employer-employee administrative data, we identify displaced workers through mass layoffs in coal mining and estimate event-study models combined with propensity score matching. We document large and persistent earnings losses after displacement. Among workers who return to registered employment, real monthly earnings fall by about 25 percent immediately after job loss, and daily wages decline by roughly 11–14 percent. Employment also drops sharply and remains below its pre-displacement level several years later, implying that earnings losses reflect both weaker employment attachment and lower-paying re-employment. The costs are highly heterogeneous across post-displacement trajectories. Workers re-employed within coal mining experience little or no persistent wage penalty, whereas those moving to non-coal sectors face lasting daily-wage losses of about 18 percent. Losses are particularly severe for core underground miners moving out of coal, whose daily wages fall by roughly 26–30 percent, as well as for longer-tenured workers and workers who spend longer periods outside registered employment before re-employment. Over five years, the present discounted value of earnings losses amounts to roughly one year of pre-displacement earnings, but the aggregate burden is geographically concentrated in coal-dependent provinces. The results suggest that the direct labor-market cost of coal exit is manageable at the national level, but its spatial concentration calls for targeted just-transition policies in the most exposed local labor markets.

JEL classification

J63, J65, Q54, Q58, R23

Keywords

coal phase-out, job displacement, just transition, wage losses, employment

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I. Introduction

Coal phase-out is indispensable for achieving climate targets consistent with keeping global warming well below 2°C above pre-industrial levels, but it also imposes adjustment costs that are concentrated among workers and local labor markets in coal-dependent regions (IPCC, 2022). Since 2015, many coal-producing countries have announced target dates for coal exit. In addition to the 14 OECD countries already free of coal, 13 more countries —including many EU member states as well as Canada, Chile, Israel, and New Zealand— have committed to phasing out coal by 2030[1] (EMBER, 2024). Globally, this transition could lead to job losses for nearly five million coal miners, with potentially substantial welfare losses for affected workers and communities (Ruppert Bulmer et al., 2021). These expected employment and earnings losses represent a major obstacle to the political acceptability and practical feasibility of the transition away from coal.

Securing the interests of workers and communities adversely affected by the coal transition has become increasingly prominent on the global policy agenda (European Commission, 2020; Messetchkova, 2021). This policy focus has also generated growing academic interest. Yet quantitative evidence on the employment and welfare effects of coal exit remains limited, relying largely on macro-level simulation models and focusing predominantly on industrialized countries.[2] To the best of our knowledge, Rud et al. (2024) provide the only direct estimate of the long-term earnings losses of displaced coal miners, studying the collapse of the coal industry in the United Kingdom. Since most coal production and coal-fired power generation today take place in developing countries, many of which have not yet committed to a coal phase-out (Clark and Zhang, 2022), it is important to establish the cost of job loss for miners in these settings. This is precisely the contribution of our study: we provide the first direct evidence on the medium-term employment and earnings losses of displaced coal miners in a developing-country context. These costs often feature in political debates over decarbonization as an argument for delaying the phase-out of coal-fired energy production, yet until now there has been little direct micro-level evidence on their magnitude. A key reason is the lack of appropriate administrative panel data or the non-accessibility of such data. Using linked employer–employee data from Turkey, a middle-income developing country, we estimate the employment and wage losses associated with displacement among coal miners.

[1] Meanwhile, Germany and Poland, the EU's largest coal producers, have announced their coal phase-out dates as 2038 and 2049, respectively. The EMBER 2024 report also highlights five OECD countries that have yet to make any commitments regarding a coal phase-out: Turkey, Australia, Japan, Colombia, and Mexico.

[2] One of the very few macro-level simulation studies focusing on a developing country is Clark and Zhang (2022), who examine China and simulate the welfare costs associated with job losses in the coal power industry under alternative coal phase-out scenarios.

We study the potential welfare costs of coal phase-out in Turkey, a major coal producer ranked among the world's top 20 and currently the third-largest builder of new coal-fired power plants after China and India (Ruppert Bulmer et al., 2021; Shearer et al., 2020). Until recently, Turkey had been slow to advance decarbonization efforts (Şahin, 2018). However, following the announcement of the European Green Deal (EGD) and the accompanying Carbon Border Adjustment Mechanism (CBAM), Turkey has taken more tangible steps in climate policy since 2021. These include adopting an EGD action plan, ratifying the Paris Agreement, and pledging to achieve net-zero emissions by 2053 (Acar & Aşıcı, 2021; Şahin et al., 2021). Although fossil-fuel phase-out remains absent from Turkey's action plan, its international climate commitments and concerns over the competitiveness of carbon-intensive exports to the EU under CBAM are likely to increase pressure on policymakers to accelerate decarbonization, and may eventually bring coal phase-out onto the policy agenda. At the same time, Turkey has not yet committed to phasing out coal. The transition away from coal is therefore not currently underway; on the contrary, the government continues to pursue a development strategy in which coal-fired energy remains an important component (Çelik, 2021).

Consequently, in this study we cannot look at employment and earnings losses during coal phase-out, as Rud et al. (2024) do for the United Kingdom following the collapse of its coal industry. What we can do, though, is to estimate employment and wage losses associated with job displacement in coal mining by tracking worker movements out of jobs held in the sector. The literature that deals with potential coal phase-out in developing countries provides simulations of general welfare costs that depend on distinctive policy scenarios (see, e.g., Clark and Zhang, 2022, on China). To our knowledge there are, however, no studies in a development context that look directly at employment and wage losses of coal miners who have been displaced. By estimating these losses in Turkey, our study provides the first micro-level building block for assessing the welfare costs of a future coal phase-out in developing-country settings.

Displaced Turkish coal miners can subsequently either find employment in mining again or in an alternative sector. When looking at employment and wage losses of miners who find employment outside mining, we get close to the true costs of job loss during an actual coal phase-out since miners are forced to take up employment in an alternative sector when the bulk of mines is closing or has been closed and no new mines are opened. Since miners who switch sectors are confronted with the nearly complete depreciation of their firm-specific human capital that might be of little value outside mining, we would expect losses for these miners to be substantially larger and more lasting than the losses of miners who find reemployment in the mining sector.

We also explore potential heterogeneity of outcomes along several dimensions. To account for potential differences in labor-market behavior between longer- and shorter-tenured workers, we first divide both displaced workers and controls into two groups based on pre-displacement tenure in coal employment: those with tenure of three years or less and those with tenure exceeding three years. Whether coal miners with long tenure experience larger or smaller costs of job loss than their counterparts with relatively short tenure is a priori not clear[3] and, therefore, an interesting question worth exploring. We also make a distinction between technical or administrative staff in the mining sector and actual underground miners, whom we refer to as miners proper hereafter, since the human capital of the latter might be more negatively affected by displacement than that of the former.[4] Furthermore, we also condition on initial duration of non-employment following displacement and compare the outcomes of those displaced workers who find re-employment after six months of non-employment with the outcomes of those who re-enter employment only after one year of non-employment.[5]

Building on the seminal studies of Jacobson et al. (1993) and Couch and Placzek (2010) regarding the costs of job displacement in the U.S., we employ an event study model that compares wage changes between displaced (the treated) and non-displaced workers (the controls), allowing the effects of displacement to vary by the number of quarters relative to separation. For this analysis, we utilize linked employer-employee data from quarterly administrative records, enabling us to track worker transitions both in and out of employment as well as across different employers and sectors. To ensure that the treated and the controls are similar in their observables we first perform propensity score matching. Given that we have the universe of non-financial firms and of their formal employees in our matched data set we can produce statistically meaningful results for the mining sector despite its overall small employment share in Turkish industry.

A few papers exist that analyze the costs of displacement in coal or high-emission sectors using micro-level data, as we do. The study methodologically closest to ours is Rud et

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[3] On one hand, miners with long tenure might be more productive than their short-tenure counterparts and might be able to rapidly find new jobs of particularly high quality; on the other hand, long-tenure miners might have acquired a substantial amount of firm-specific human capital that they loose when they are laid off.

[4] Clearly a secretary or an accountant working in the mining sector might be able to use his or her human capital better outside mining than a miner proper even if this human capital could to some degree be firm specific.

[5] Like in most developing countries distinguishing clearly between the labor market states *unemployment* and *out of the labor force* is virtually impossible for the majority of workers in Turkey and our administrative data only provide the information whether a worker in a given quarter works formally or does not work formally.

al. (2024), which employs an event study design to estimate the long-term earnings losses of displaced miners following the essential closure of the UK coal industry. Their findings indicate an approximately 40% wage decline one year after a job loss, which remains persistent and significantly depressed even fifteen years later. Another recent study by Haywood et al. (2024) estimates the welfare costs of job losses resulting from mine closures in Germany through a stochastic job search model and identifies policy instruments that effectively mitigate these costs. Looking at high-carbon-intensity (HCI) sectors, Barreto et al. (2023) estimate earnings losses in German HCI sectors relative to low-carbon-intensity (LCI) sectors. They find that workers displaced from HCI sectors earn 43% less than their pre-displacement earnings trend in the first year, with losses remaining persistent at 6.3 percentage points after five years. In comparison, workers displaced from LCI sectors experience an earnings decline of 38% in the first year. Expanding this analysis to fourteen OECD countries, Barreto et al. (2024) reach similar conclusions, showing that displaced workers from energy-intensive industries face greater earnings losses than those in non-energy-intensive and transport sectors.

Alongside the evolving literature on the welfare impacts of decarbonization, our study contributes to the broader literature on the costs of job loss, pioneered by the seminal work of Jacobson et al. (1993) and Couch and Placzek (2010). This literature broadly agrees that job displacement leads to sustained earnings losses; however, the magnitude of these losses varies depending on the data sources and empirical methods used.[6] Building on this body of research, our study provides evidence on the costs of job loss in the Turkish coal mining industry, hence focusing on a context from outside industrialized countries, that thus far has been virtually unexplored.

We find large and persistent displacement costs for coal miners. Among re-employed workers, real monthly earnings fall by about 25 percent immediately after job loss, while unconditional earnings losses are much larger as many displaced workers initially leave registered employment. Employment drops by roughly 85 percentage points after displacement and remains 10–12 percentage points below its pre-displacement level even five years later. Daily wages among re-employed workers decline by 12–15 log points, or approximately 11–14 percent, showing that displacement costs reflect not only non-employment but also lower-paying post-displacement jobs. These penalties are concentrated among workers who leave coal

[6] The work by Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010) have been followed by other studies on the U.S., e.g., Lachowska et al. (2020), and Huckfeldt (2022), which introduce various methodological and/or scope improvements. A selection of studies from other countries includes Lehmann et al. (2005, 2006) for Estonia and Ukraine, Hijzen et al. (2010) for the UK, Raposo et al. (2021) for Portugal, Schmieder et al. (2024) for Germany, and Bertheau et al. (2023) for a cross-country comparative analysis within the EU.

mining: those re-employed within coal experience little or no persistent wage loss, whereas those moving to non-coal sectors face lasting daily-wage penalties of around 20 log points. Losses are especially severe for core underground miners and for workers with longer pre-displacement tenure, consistent with the loss of sector-, occupation-, and match-specific wage premia. Over five years, the present discounted value of earnings losses amounts to roughly one year of pre-displacement earnings, with the burden concentrated in a few coal-dependent provinces. Robustness exercises provide no strong evidence of selection along pre-layoff wage-based firm quality, although they cannot rule out selection on unobserved firm characteristics. Taken together, the results suggest that the aggregate labor-market cost of coal exit is economically manageable, but its uneven spatial distribution calls for targeted just-transition policies in the most exposed provinces.

The next section describes the administrative data that we have at our disposal and presents some descriptive statistics, while section III discusses selection and identification issues. This is followed by the main section, IV, where we present our results, first for the overall sample and then heterogeneous outcomes along the above discussed dimensions. Section V discusses a potential robustness check and explores an important endogeneity issue. Since we define worker displacement by mass layoffs, with this issue we explore whether firms who engage in mass layoffs have the same pre-displacement labor productivity as firms who do not experience layoffs. The penultimate section then discusses our findings, focusing on the overall welfare costs of a complete coal shutdown and on the appropriate policy responses to these costs, while section VII gives some overall conclusions.

II. Data and descriptive statistics

Our analysis uses a linked employer-employee dataset covering the universe of nonfinancial private firms and their employees registered in the administrative records system of the Turkish economy. The *Entrepreneur Information System (EIS)* is constructed by the Ministry of Science, Industry, and Technology (MSIT), which compiles administrative datasets from various public institutions (MSIT, 2025).[7] Employer data, spanning the years 2006–2021, is sourced from the Business Registers, where the enterprise serves as the unit of observation. This enterprise-level dataset includes annual information on the economic sector, classified using 4-digit ISIC and NACE Rev. 2 codes, which is essential for identifying coal sector workers. It also provides quarterly information on the number of employees, which is used to

[7] The annual count of enterprises in 2021 is about 2.1 million firms (excluding one-person enterprises/self-employed) employing about 14.5 million employees. For further details on the dataset, see Ayhan et al. (2025).

define firm size. We use the dynamics of firm size to pinpoint mass layoffs by firms, whose importance for identifying displacement is discussed in the next section.

The main employee data source comes from the Social Security Institution (SSI) records, which provide information on worker characteristics on a quarterly basis from 2012 to 2021.[8] Both firms and employees are assigned unique identifiers, enabling the construction of a linked employer-employee dataset for the period 2012–2021. This employer-employee-linked data set based on administrative data is one of the very few outstanding data sets originating in a developing country. It allows tracking of worker movements out of and into employment and between firms, as long as workers remain formally employed across firms, i.e., as long as they have been registered with the social security authority in the firm from which they are displaced and are registered in the new firm which hires them.[9]

The worker data includes detailed employee characteristics, such as age, gender, number of contribution days, monthly gross wages[10], and occupation. Summary statistics for the workers who are employed in the mining sector are presented in Table 1. It shows that 97 percent of all employees in this sector are men, that we deal with a relatively young workforce and that average tenure is slightly above four years. The upper panel of the table also reports average real monthly wages over the period 2012–2021, amounting to 2150 Turkish Lira, substantially higher than the average real monthly wage in other industrial sectors in Turkey (see the discussion of figure 2 below). Real wages are obtained by deflating nominal wages using the Consumer Price Index (CPI), with 2010 as the base year (2010 = 1). In the lower panel we display the occupational structure of the mining sector which allows us to discriminate between ‘proper’ miners and the rest of the employees in the sector. According to the 1-digit occupational (ISCO-08) classification, the occupational group “*Plant and machine operators, and assemblers*” accounts for more than half of employment in the coal mining sector. Refining this 1-digit occupational structure to the 4-digit level, we further identify the actual underground

[8] Strictly speaking, employee data are available on a quarterly basis from 2012 to 2019 and on a monthly basis for 2020 and 2021. By aggregating the monthly data into quarterly intervals, a quarterly panel covering the period 2012–2021 is constructed.

[9] Given that wages of miners are subsidized by the government, it strikes us as highly unlikely that a large part of workers in the mining sector is informal. By comparing coal-mining employment recorded in Social Security administrative data with unregistered employment reported in the Household Labor Force Survey, Ozen and Asik (2023) show that informal employment in coal mining is very limited.

[10] The monthly wages reflect gross/social-security-contribution-based earnings reported by firms to the Turkish Social Security Institution. Accordingly, the earnings variable should be understood as a gross earnings measure based on social security records, rather than workers’ net wages. The number of contribution days measures the number of social-security premium days reported for the registered employees in a given month. It is therefore an administrative measure of insured employment duration, not a direct measure of hours worked or actual calendar days worked. Under Turkish social security reporting rules, a full month of employment is generally recorded as 30 contribution days, irrespective of the actual number of days in the month.

miners —for the heterogeneity analysis in Section IV— as those belonging to core mining occupations (8111, 9311) and mining- and quarrying-related machine operators (8112, 8113, 8114, 8183, 8211)[11].

Table 1: Summary statistics for workers in the coal mining sector 2012-2021

	Obs	Mean	Std. dev.	Min	Max
Real monthly wages	1.373.962	2.150,39	1.255,66	0	7.508,86
Real daily wages	1.336.398	80,43	41,15	0	3.199,99
No of contribution days	1.373.962	26,40	7,27	0	30
Male	1.373.962	0,97	0,15	0	1
Age	1.373.962	36,00	7,94	16	64
Tenure in quarters	1.373.962	16,72	11,19	1	40
Occupation groups:		(%)			
Managers	4.889	0,66			
Professionals	38.314	5,21			
Technicians and associate professionals	38.108	5,18			
Clerical support workers	25.227	3,43			
Service and sales workers	21.235	2,89			
Skilled agricultural, forestry and fishery	870	0,12			
Craft and related trades workers	84.383	11,47			
Plant-machine operators, assemblers	403.438	54,85			
Elementary occupations	119.048	16,19			

Source: Authors' calculations based on employee–employer linked EIS data. The table reports the characteristics of miners actively employed in the coal sector, averaged over 2012–2021, using the pre-selection sample that includes women.

Besides the summary statistics on the characteristics of employees in the mining sector, we also want to highlight the overall trend of employment in coal mining, its regional distribution and as already stated the relatively high wages earned in this sector. Figure 1 shows the employment levels in mining for the years 2006 to 2022. In 2010 employment was at its height in this period with roughly 40,000 workers in coal mining, while in 2022 the employment level fell to roughly 33,000 workers.[12] In parallel with the decline in the number of coal miners, the number of firms also exhibits a secular decline, especially after 2013, as shown in Table A1 in the Appendix. Given that mining firms in Turkey receive state support through various subsidy schemes (Acar & Yeldan, 2016), we interpret the decline in the number of mines as evidence of a genuine weakening of economic opportunities in the sector, driven by high

[11] The indicated ISCO-08 occupational codes correspond to the following categories: *8111* — Miners and quarriers; *8112* — Mineral and stone processing plant operators; *8113* — Well drillers and borers and related workers; *8114* — Cement, stone, and other mineral products machine operators; *8183* — Packing, bottling, and labelling machine operators; *8211* — Mechanical machinery assemblers; and *9311* — Mining and quarrying laborers.

[12] As of 2022, there are around 33,000 registered coal miners, with one-third employed in hard coal and two-thirds in lignite mining.

production costs, labor intensity, and limited competitiveness (Demir & Acar, 2025).[13] This contraction in coal production and employment is further associated with large-scale privatizations and the closure of numerous mining pits since the early 2000s (Çelik, 2021; Şengül et al., 2012).

The current number of coal miners represents roughly 0.17% of total industrial employment in Turkey. Such a small percentage of industrial jobs might one lead to believe that even a complete shut-down of coal mining will not create any disturbance for Turkish policy makers. However, if one inspects the distribution of coal mining employment across provinces, shown in Figure A1 in the appendix, it becomes clear that in some regions in the west and northwest of Turkey coal mining is an important branch of industrial employment. A total elimination of this industrial activity can, at least in the short run, create major unemployment problems in local economies.

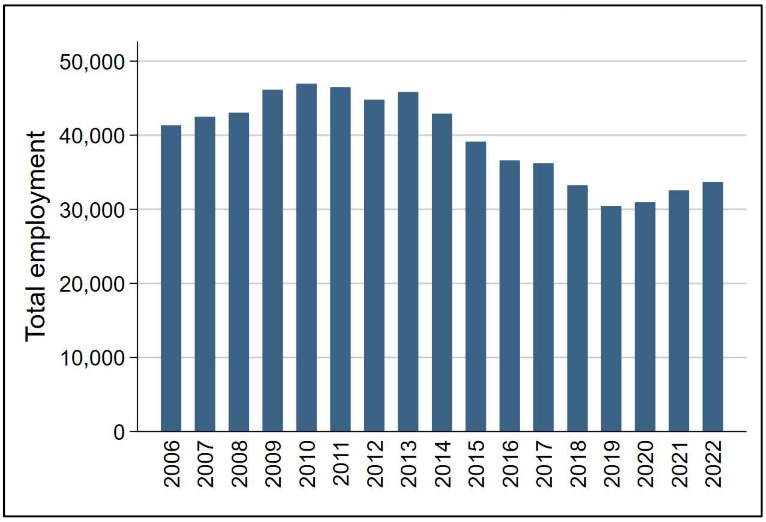

Figure 1. Total employment in coal mining over years

Source: Authors' calculations based on the Business Registers of EIS, 2006-2022

In addition, since average wages for miners are substantially higher than average wages in other industrial sectors (see Figure 2)[14], a complete shutdown can produce a recessionary

[13] The power sector in Turkey is highly reliant on imported coal, largely due to the low calorific value of domestic lignite reserves. Imported coal accounts for approximately 9% of total installed capacity, yet it generates about 62% of electricity production (Kat et al., 2024). According to official projections, Turkey is expected to remain dependent on imported coal in the coming years (Şahin et al., 2016). This dependence is partly driven by the fact that imported coal has become relatively cheaper, which is widely believed to exert downward pressure on domestic coal production. As a result, this trend will contribute to employment losses in domestic mining and has already generated growing discontent among coal miners (Cumhuriyet, 2024).

[14] We restrict the sample to workers who ever worked in coal mining during the observation window. Accordingly, "non-coal sector wages" are the average wages these same workers earned in jobs outside coal mining.

trajectory in these regions in the medium run, even if displaced miners find employment in other sectors relatively soon. Hence, we argue that, while a phase-out of coal mining in Turkey is indispensable for achieving climate objectives, it is also likely to create substantial economic and social challenges in regions where coal extraction is concentrated. The challenge for policymakers is thus not whether to phase out coal mining, but how to do so in a way that minimizes the economic and social costs borne by coal-dependent regions through appropriately designed, place-based transition policies.

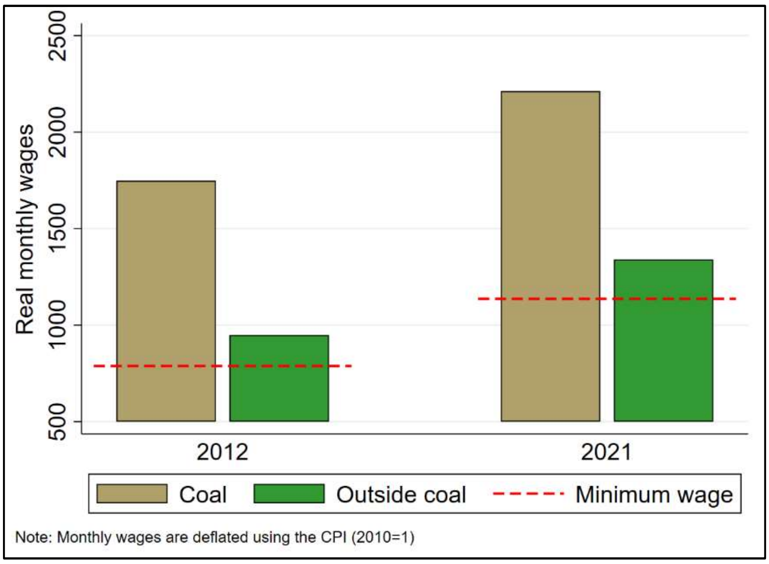

Figure 2: Wages in Coal Mining vs. the Non-Coal Sector

Source: Authors' calculations based on the linked employer-employee EIS data, 2012 & 2021.

Note: To compute real wages, nominal wages are deflated using the Consumer Price Index (CPI), with 2010 as the base year (2010 = 1). The analysis sample includes all workers who, at any point in the observation window, were employed in coal mining. "Non-coal sector wages" therefore refer to the average earnings these workers received when employed outside coal mining.

III. Selection and identification issues

Our administrative data show separations from employment relationships, they do not allow us to distinguish between a displacement, i.e. the involuntary separation from a job, and a voluntary quit. We follow the literature by assuming that separations occurring during a mass layoff are involuntary. In the spirit of Jacobson et al. (1993) and all relevant subsequent studies we define a *mass layoff* as an employment reduction exceeding 30% of the workforce within the previous four quarters. Hence, workers are classified as *displaced* if they leave their current employer during a mass layoff within the last four quarters. To be able to observe the evolution of wages for at least two years before and after displacement, we restrict the baseline quarter, i.e. the quarter of actual displacement, to the period between the first quarter of 2014 and the fourth quarter of 2019. Since our interest lies in the wage losses of workers in coal mining, we restrict our focus to displaced workers whose initial employment was in this sector.[15] We exclude multiple displacements from the treatment group; hence those who have been displaced from coal mining jobs for the first time in case of multiple displacements or those who only experienced a displacement just once constitute the *treated* group. In the literature, various restrictions are applied to define the control group. Following Bertheau et al. (2023), we define potential *control* workers as individuals who do not jointly satisfy the two conditions used to identify treated workers: (*i*) separation from their main employer in the coal sector, and (*ii*) a decline of at least 30 percent in employment at that firm within the previous four quarters. The control group therefore includes workers who remain employed in coal mining, even if their firms experience a mass layoff, provided that the workers themselves are not displaced. This is consistent with common practice in the job displacement literature, where non-displaced workers at mass-layoff firms may serve as controls (e.g., Bertheau et al., 2023; Schmieder et al., 2024). The control group may also include workers who separate from firms that do not undergo a mass layoff; such separations are plausibly voluntary rather than displacement-induced.[16] Our analysis sample is further restricted to men aged between 16 and 64 years, excluding approximately 3% of female employees in the coal mining sector.

We also generate two additional samples. To account for potential differences in labor market behavior between longer-tenured and shorter-tenured workers —often linked to productivity and wage differences but also to differences in acquired firm-specific human capital— we split the treatment group into workers with up to 12 quarters of tenure prior to displacement and into their counterparts with more than 12 quarters of tenure[17]. Whether displaced workers with relatively short tenure have larger or smaller overall wage losses relative to their counterparts with relatively long tenure and whether these losses are less or more persistent is an interesting question that we will investigate. As we mentioned in the introduction, displaced workers with relatively short tenure might be less productive than displaced workers with relatively long tenure and might therefore have particularly low job search effectiveness. In contrast, if duration of tenure is linked to the acquisition of firm-specific

[15] Approximately 6% of firms experience a mass layoff in any given quarter, and about 1% of workers in the analysis sample are displaced from coal mining jobs.

[16] Defining control workers as those who remain continuously employed with the same employer, as done by Jacobson et al. (1993) and Lachowska et al. (2020), is too restrictive for our case, given that our analysis focuses on coal miners, who constitute a relatively small sample.

[17] We define long-tenured workers as those with more than three years of pre-displacement tenure. This cutoff divides the sample almost evenly, with 48.4 percent of workers classified as long-tenured and 51.6 percent as short-tenured, indicating that the median pre-displacement tenure is close to three years.

human capital, which might be lost when reemployed, displaced workers with relatively long tenure might have particularly large overall wage losses. To ensure comparability between the treated and control groups, we apply the same tenure condition to the control groups.

Based on these three samples of treated and controls, we identify the employment and wage losses of displaced workers by first estimating the following event study model, which is very much standard in the literature:

$$y_{itc} = \sum_{\substack{k=-20 \\ k\neq -1}}^{k=2} \delta_k \ [\mathbf{1}(t = c + k)\ x\ Disp_i] + \sum_{\substack{k=-20 \\ k\neq -1}}^{k=20} \gamma_k\ \mathbf{1}(t = c + k) + \alpha_{ic} + \phi_t + X_{it}'\beta + u_{itc} \quad (1)$$

The variable Y_{itc} represents our outcome variables for individual i, including normalized real monthly earnings, log real daily wages[18], or an employment indicator, depending on the specification. The subscript c refers to the displacement cohort of treated workers and their matched controls, that is, workers assigned to the same baseline quarter. The subscript k denotes the quarter relative to the baseline quarter of displacement, ranging from -20 to 20, which corresponds to up to five years before and after displacement. The quarter k=*-1* is omitted as the reference period. The term $Disp_i$ is a treatment dummy variable that takes the value of 1 if the worker is displaced and 0 otherwise. Thus, our primary coefficient of interest, δ_k, compares measures the change in the outcome gap between displaced workers and their matched controls at event time k, relative to the quarter immediately preceding displacement (k=*-1*).

The model further includes worker-by-stack fixed effects[19], α_{ic}, which allow the same worker to appear in multiple matched displacement cohorts and absorb time-invariant worker characteristics separately within each stack. The relative-time fixed effects, γ_k, capture changes in the outcome at event time k that are common to treated and control workers across displacement cohorts.[20] Calendar-quarter effects, ϕ_t, absorb shocks common to all workers in a given calendar quarter, including macroeconomic conditions, seasonality, and nationwide policy changes. Additionally, time-varying characteristics, such as age squared and, in

[18] To compute real wages, nominal wages are deflated using the Consumer Price Index (CPI), with 2010 as the base year (2010 = 1).

[19] For each displacement cohort, defined by the quarter of displacement, we construct a separate matched panel by matching displaced workers to observationally similar non-displaced workers using pre-displacement characteristics. These cohort-specific matched panels are then appended into a stacked dataset, where relative time is defined with respect to the displacement quarter of each cohort. Since the same control worker may appear in more than one matched cohort, the relevant fixed effect is a worker-by-stack fixed effect, rather than a standard worker fixed effect. This choice does not affect the identification strategy but provides a more flexible way of absorbing worker-specific heterogeneity within each matched cohort.

[20] For a discussion of the role of relative-time fixed effects in stacked event-study designs, see the online appendix of Schmieder et al. (2024).

robustness specifications, the province of the pre-displacement workplace, are included in X_{it}.[21] Standard errors are clustered at the worker level. We look at both monthly and daily wage losses with this model since if the former are larger than daily wage losses this tells us also something about smaller employment stability for the treated compared to the control group.

To ensure closeness in observables of the treatment and control groups regarding our two samples, we first implement propensity score matching using the nearest-neighbor matching algorithm based on pre-determined characteristics. These include real wages (measured three years before the displacement), age, tenure, and firm size (all measured one year before the displacement). Table 2 reports covariate balance before and after propensity score matching. For each covariate, it presents mean values for treated and control groups, standardized differences, t-tests, and variance ratios.[22] The results are reported for the overall sample, but the same matching quality is also achieved for the two samples that are based on duration of tenure.[23]

The results show that matching substantially improves covariate balance. Before matching, there are large differences between treated and control groups, particularly in lagged wages and firm size, with standardized biases exceeding 60–80 percent. After matching, these differences are largely eliminated. For lagged wages, the standardized bias falls sharply and becomes small and statistically insignificant. Age and tenure are also well balanced after matching, with small standardized differences and insignificant t-tests. Firm size, which exhibits substantial imbalance prior to matching, is effectively balanced after matching, with negligible differences in means.

[21] Given the limited demographic information in the administrative data, one of its main drawbacks, the only additional covariates we can include beyond those used for matching are age squared and province dummies. Hence, the vector of control variables is restricted to these variables.

[22] The standardized bias measures the difference in means relative to a pooled standard deviation; values close to zero indicate better balance, and values below 10 percent are generally considered acceptable. The percentage bias reduction captures the extent to which matching improves comparability relative to the unmatched sample. The t-tests provide a statistical comparison of means, where insignificant differences after matching indicate successful balancing. The variance ratio compares the dispersion of covariates across groups; values close to one suggest similar distributions, while values outside conventional ranges indicate residual imbalance. Furthermore, see Appendix Table A2. for the summary of standard diagnostics including mean and median bias, Rubin's B & R statistics, and likelihood ratio test results.

[23] We do not show these results here, but they are available upon request.

Table 2: Mean differences between treatment and control groups for the matched and unmatched sample

		Treated Mean	Control Mean	Std. Bias (%)	% Bias Reduction	t-stat	p-value	Var. Ratio
Real wages	Unmatched	941.5	1572.6	-66.8	—	-5.23	0.000	0.53*
	Matched	948.4	904.1	4.7	93.0	0.40	0.689	1.83*
Age	Unmatched	37.7	34.3	40.6	—	4.06	0.000	1.51
	Matched	37.4	37.0	4.7	88.3	0.29	0.771	1.15
Tenure	Unmatched	4.6	4.6	-7.5	—	-0.68	0.498	1.04
	Matched	4.6	4.6	-1.4	81.9	-0.08	0.933	0.95
Firm size	Unmatched	128.0	843.3	-81.4	—	-5.24	0.000	0.04*
	Matched	129.5	147.4	-2.0	97.5	-0.44	0.658	0.70

Note: The table reports covariate balance before and after propensity score matching for a representative displacement cohort (2014q1). Matching is performed separately for each displacement cohort, as is standard in staggered difference-in-differences event study designs. The results shown are representative of the balancing performance across all cohorts. Propensity score matching and balance diagnostics are implemented using the *psmatch2* and *pstest* commands in Stata. 'Real wages' refers to real monthly wages measured three years before displacement. All other covariates —age in years, tenure in quarters, and firm size in number of workers— are measured one year before displacement. The large average firm size should not be misleading. The median firm size is 60 (62) for the treatment group and 72 (208) for the control group in the matched (unmatched) sample.

We apply our event study model to various heterogeneous outcomes using the overall sample initially. We first examine monthly earning losses both for displaced workers who become re-employed and for those who experience one or more spells of non-employment after displacement, assigning zero earnings during periods of non-employment. These wage effects are calculated as percentage changes relative to pre-displacement monthly wages. However, monthly wage losses combine several margins of adjustment. They may reflect a lower probability of being employed, fewer days worked conditional on employment, lower daily wages, or some combination of these mechanisms. To better understand what drives the monthly wage effects, we therefore examine two additional outcomes. First, we estimate employment losses using an indicator equal to one if the worker has at least one day of registered employment in the corresponding period. Second, among displaced workers who are re-employed, we estimate changes in daily wages, measured in log points. This decomposition allows us to distinguish whether monthly earnings losses are mainly driven by reduced employment attachment or by lower wages in post-displacement jobs.

Our first heterogeneity exercise looks at daily and monthly wage effects for displaced workers who find re-employment in the mining sector versus for those displaced who find new jobs in other sectors. With the outcomes for the latter group, we might approach miners' losses caused by a coal phase-out when a return to employment in the coal mining sector is no longer possible. Furthermore, since average wages are larger in the coal sector than in other sectors of

Turkish industry, we would predict larger wage penalties for those displaced workers who find re-employment outside the coal sector. We then repeat the analysis for the two samples that are based on the duration of tenure.

Using the overall sample, we proceed with two additional interesting heterogeneity exercises. First, we compare the wage losses of miners proper, that is, core underground mining occupations, with those of non-core occupations, namely technical and administrative staff in coal mining. Our prior here is that proper miners have skills that can be less used in non-coal sectors than the skills of other workers in the coal mining sector. We finally investigate how wage losses differ by duration of non-employment to test the hypothesis that longer non-employment spells result in greater income losses, consistent with evidence from the United States (Couch and Placzek, 2010) and the United Kingdom (Layard et al. 2005). [24]

IV. Results

IV.1 Central results using the overall and the tenure-based samples

We start off with the results for the encompassing sample, where we do not impose restrictions on pre-displacement tenure. Panel A of Figure 3 plots real monthly wage losses from Eq. (1), measured as percentage changes relative to average pre-displacement wages. The estimates are shown both not conditioning on employment, with zero earnings assigned during non-employment spells, and conditioning on employment, restricting the sample to displaced coal miners with positive earnings and a positive number of recorded workdays after job loss. For the re-employment sample the estimates indicate a substantial drop in wages of initially 25% (blue solid line), while unconditional earning losses amount to nearly 90% right after displacement (black solid line). Earnings improve over time but do not fully recover even after five years: wage losses remain between 10 and 15 percent for the conditional-on-employment sample and roughly 15 percent for the unconditional sample. Hence, when we look at the entire sample of displaced workers, we find persistent wage losses over the reported period, even for those who become re-employed immediately after job loss.

[24] Another potentially important source of heterogeneity is workers' age at the time of displacement. In particular, one may expect different adjustment patterns for miners younger than 40 and those aged 40 or above. This distinction is motivated by qualitative evidence from interviews with coal miners and sector representatives in Soma (Manisa), reported in Ayhan and Çelik (2024). Because underground miners in Turkey become eligible for statutory retirement at age 40, many retired miners reportedly continue working in the sector to supplement their pension income with earnings from mining employment. We find some suggestive evidence consistent with this mechanism in our data (see Figure A10 in the online appendix). However, the differences in post-displacement outcomes between younger and older workers are generally not statistically significant. Given the lack of robust evidence, we do not explore this heterogeneity further.

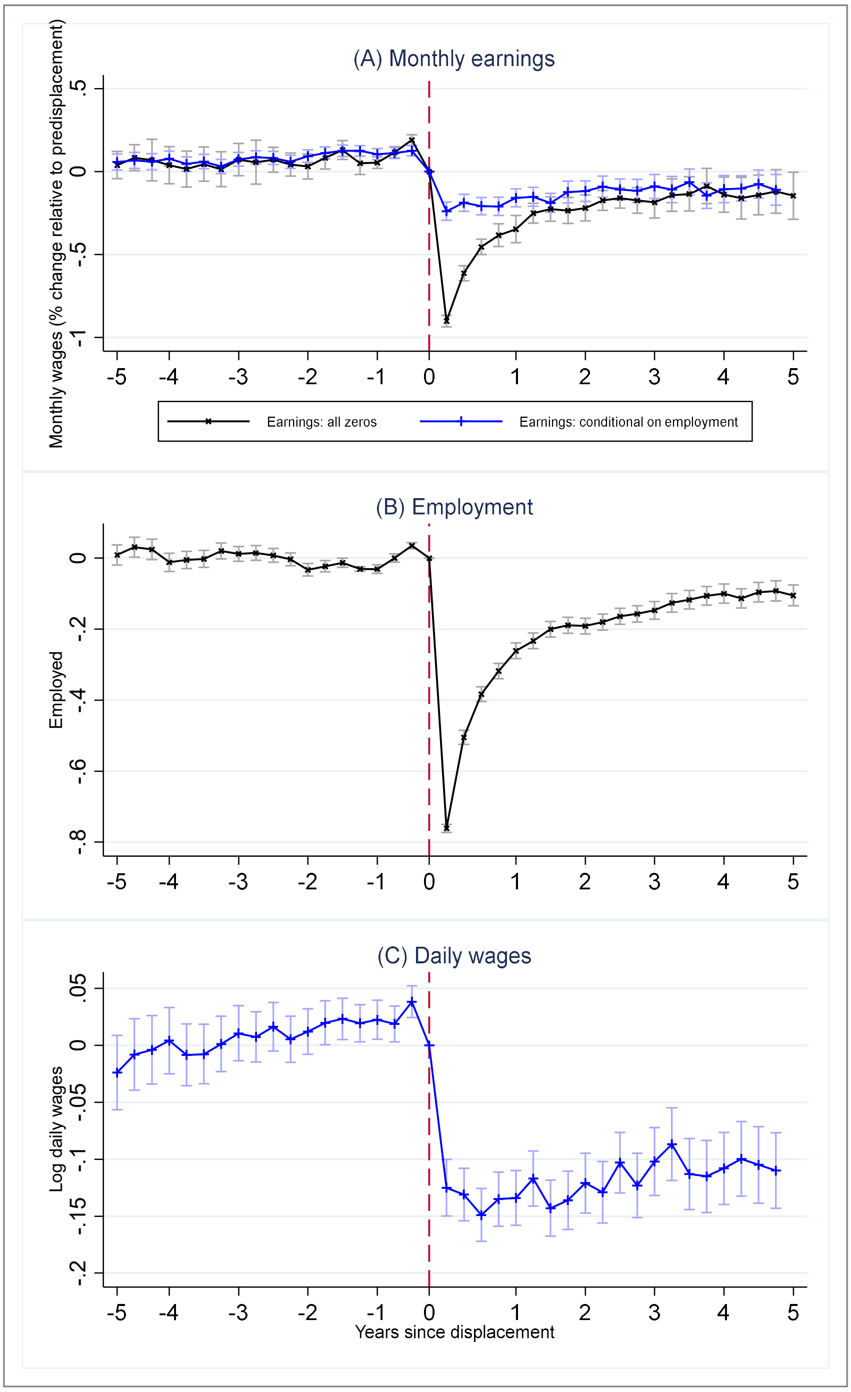

Figure 3: The effects of job loss on earnings and employment

Notes: In **Panel A**, the y-axis shows monthly real wages normalized by the individual's average pre-displacement wage. The "all zeros" sample, shown by the black line, assigns zero earnings for those periods when miners are not observed in the employment records after displacement. The "conditional on employment" sample, shown by the blue line, includes only observations with positive post-displacement earnings and positive days worked within five years after job loss. In **Panel B**, the y-axis refers to "employed," a binary indicator equal to one if the worker has at least one day of work in the corresponding year. In **Panel C**, the y-axis denotes real daily wages in natural logarithms, and the estimates are based on the subsample of employed workers. Whiskers indicate 95% confidence intervals.

The striking gap between the unconditional and conditional-on-employment estimates points to the severe impact of displacement on post-displacement employment. Panel B of

Figure 3 confirms this pattern, showing an immediate employment decline of 85 percentage points. Employment recovers rapidly thereafter but remains 10–12 percentage points below its pre-displacement level even after five years. This sharp employment loss helps explain the large divergence in monthly wage losses between the "all zeros" sample[25] and the re-employment sample during the first two years after displacement.

Panel C of Figure 3 presents losses in daily wages for re-employed displaced miners. Daily wages drop by between 12 and 15 log points, corresponding to approximately 11–14 percent right after displacement and remain depressed for the entire observed period of post-displacement. These results clearly indicate that even displaced miners with newly found jobs pay a persistent wage penalty. Furthermore, the fact that daily wage losses are somewhat smaller than monthly wage losses for the re-employed seems to imply that this group of displaced miners have fewer working hours or working days even when employed than their non-displaced counterparts.

One central fact that we wish to better understand is the persistent daily wage penalty experienced by re-employed displaced workers from the mining sector. Figure 4 helps to shed light on the underlying mechanism. We split the sample of re-employed displaced workers into those who find re-employment within the coal sector and those who find jobs outside the sector. Persistent daily wage penalties are observed only among workers who switch to another sector, amounting to roughly 20 log points, whereas workers who return to coal experience virtually no long-run wage penalty. This result is intuitive given that wages in coal mining are consistently higher than in most other industries (see Figure 2). More importantly, the absence of persistent wage losses among workers returning to coal suggests that the observed earnings penalties are unlikely to be driven primarily by the destruction of firm-specific human capital or a permanent loss of worker productivity. If displacement itself generated lasting productivity losses, one would expect wage penalties even among workers who are re-employed within the coal sector. Instead, the evidence points to the loss of a sector-specific wage premium as the dominant mechanism behind the persistent daily wage losses. From a policy perspective, this distinction is important because it suggests that the medium-term costs of coal phase-out arise less from the displacement event itself than from workers' transition into lower-paying sectors.

Figure A2 in the appendix shows that monthly earnings losses closely mirror this pattern. Panel A indicates that displaced workers returning to the coal sector experience a sharp but short-lived decline in monthly earnings immediately after displacement. Their earnings

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[25] Where we assign zero earnings for those periods when miners are not observed in the employment records after displacement.

recover rapidly after the first post-displacement quarter, and from the second quarter onward are generally indistinguishable from pre-displacement levels. By contrast, workers who transition to non-coal sectors experience persistent and statistically significant monthly earnings losses that stabilize at roughly 20 percent of pre-displacement average monthly earnings. Panel B shows that this divergence is partly explained by differences in employment recovery. Although both groups experience a substantial employment decline immediately after displacement, workers returning to coal regain employment much more quickly and almost completely, whereas those moving to non-coal sectors continue to exhibit modest but persistent employment deficits. Taken together, Figures 4 and A2 suggest that the persistent monthly earnings losses among workers leaving coal reflect two distinct mechanisms: lower employment probabilities after displacement and, conditional on re-employment, the loss of the sector-specific wage premium enjoyed in coal mining. Consequently, the persistent earnings costs of coal displacement appear to arise primarily from transitions into lower-paying sectors rather than from a permanent deterioration in workers' productive capacity.

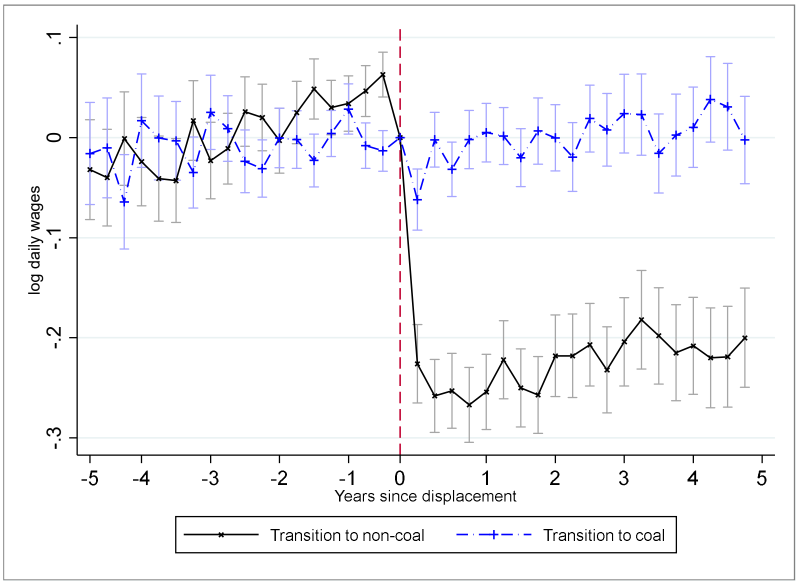

Figure 4: Daily wage losses: Re-employment in coal vs. non-coal

Notes: "*Transition to non-coal*" refers to daily wage losses among workers re-employed outside coal mining, while "*transition to coal*" refers to daily wage losses among workers re-employed in coal mining; in both cases, re-employment occurs after at least one quarter of non-employment. The estimates are based on employed workers only (positive earnings), as in Figure 3, Panel C. The y-axis shows real daily wages in natural logarithms. Whiskers indicate 95% confidence intervals.

Next, we split the treatment group by pre-displacement tenure. Workers are classified as short- or long-tenured depending on whether their pre-displacement tenure is below or above three years. Figure 5 reports the corresponding estimates for monthly wages, employment, and daily wages. Panel A shows that both short- and long-tenured workers experience sharp monthly wage losses immediately after displacement, but the losses are more persistent among long-tenured workers. The employment estimates, shown in Panel B, indicate that this difference is not primarily driven by differential employment recovery: employment falls sharply for both groups and recovers along very similar paths. The daily-wage estimates help explain the divergence in monthly earnings (Panel C). While daily wages initially decline for both groups, short-tenured workers recover substantially after two to three years, whereas long-tenured workers continue to experience persistent daily-wage losses. Taken together, Figure 5 suggests that long-tenured workers do not necessarily suffer weaker employment recovery than short-tenured workers. Rather, their larger earnings losses appear to reflect the loss of accumulated wage premia associated with their pre-displacement jobs. These premia may be firm-specific, sector-specific, occupation-specific, or related to seniority within coal employment. In this sense, longer tenure may indicate stronger attachment to a relatively high-paying coal-sector job, but also a larger loss when this match is destroyed. This interpretation aligns with the job displacement literature initiated by Jacobson et al. (1993) and extended by studies such as Lachowska et al. (2020), which shows that displaced workers with stronger pre-displacement attachment to high-wage jobs often experience large and persistent earnings losses, consistent with the destruction of firm-, sector-, or match-specific wage premia.

The tenure-specific destination results, shown in Appendix Figure A3, further support this interpretation. Among workers who return to coal, neither short- nor long-tenured workers experience persistent daily-wage losses; if anything, short-tenured workers show positive daily-wage effects in later years, though the confidence intervals are relatively wide. In contrast, among workers who move to non-coal sectors, both tenure groups suffer substantial and persistent daily-wage losses. These losses are somewhat larger for long-tenured workers, especially after the first post-displacement year, suggesting that longer-tenured workers lose more of the wage premia accumulated in coal employment when they leave the sector. Thus, the tenure gap in daily wages is concentrated among workers transitioning out of coal, while re-employment within coal largely preserves daily wages for both groups.

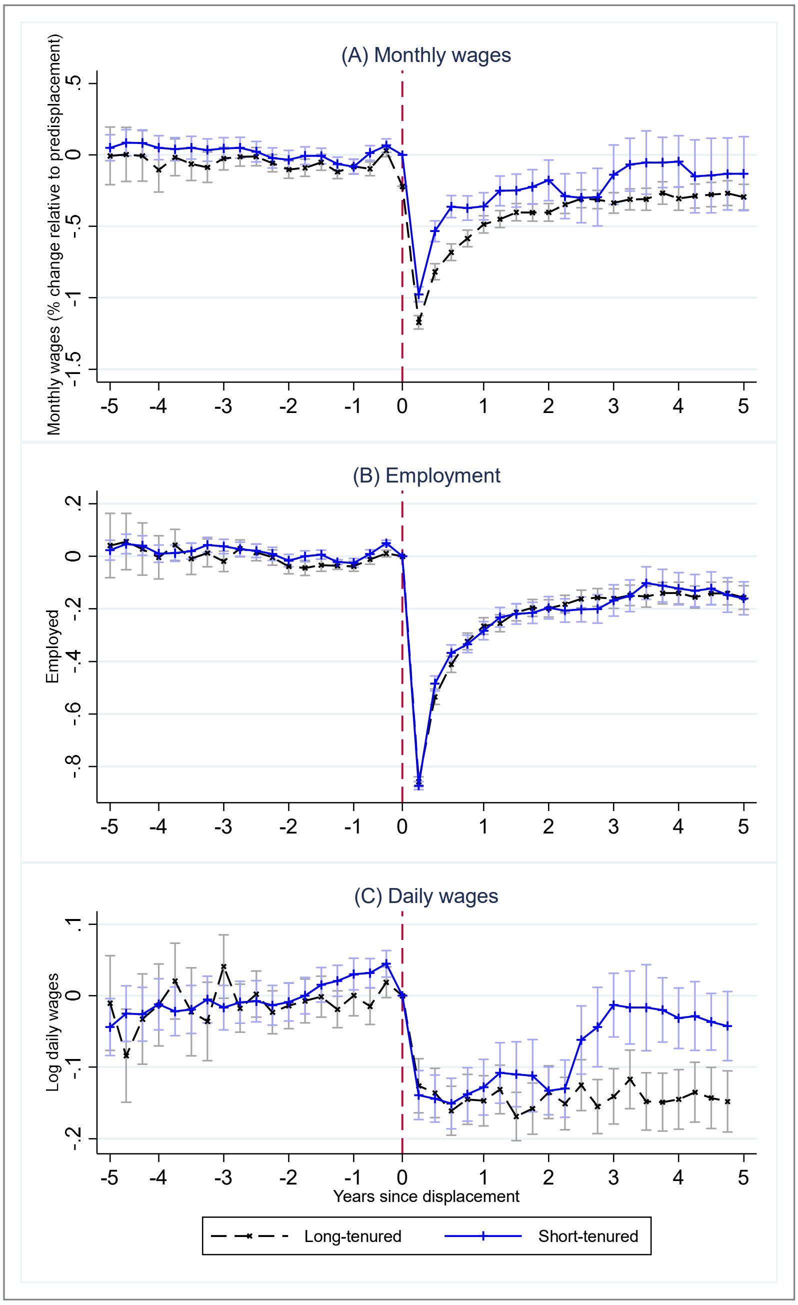

Figure 5. The effects of job loss by pre-displacement tenure

Notes: The figure reports estimates separately for displaced workers with short and long pre-displacement tenure, using 12 quarters as the cutoff for the tenure split. Outcomes and sample definitions follow Figure 3: *Panel A* reports monthly real wages normalized by average pre-displacement wages using the "all zeros" sample; *Panel B* reports the employment indicator; and *Panel C* reports log daily wages among employed workers. Whiskers indicate 95% confidence intervals.

IV.2 Additional heterogeneity analyses

The coal mining sector comprises not only underground miners whom we call *actual or proper miners* but also technical and administrative staff. The latter group is expected to experience smaller wage losses when its workers find re-employment in non-coal sectors, as their human capital is less sector-specific and their more general skills should be more easily transferable to jobs in other sectors. To investigate this potential divergence in post-displacement experiences, we first refine our identification of actual coal miners by using four-digit occupation codes (NACE2) to isolate "core" occupations of underground miners —going beyond the sector-based classification used in the previous specification. We then divide the overall sample of displaced workers in the coal sector into three groups: actual miners who find re-employment in the coal sector, actual miners who get jobs in non-coal sectors, and as a third group administrative staff, technicians, and engineers who are also re-employed outside coal.

Plotting daily wage outcomes for these three groups in the pre- and post-displacement periods in Figure 6, we find patterns that coincide with our priors. Displaced actual miners who find another job in coal mining experience no losses (dash-dotted green line). In contrast, members of the same group who are re-employed outside coal experience particularly heavy wage penalties, amounting to between 30% and 35% throughout the entire observed post-displacement period (solid black line). Conditional on re-employment outside coal, workers who are not directly involved in underground coal extraction experience smaller wage penalties than miners proper, with losses hovering around 20 percent (dashed blue line). However, also these wage losses are persistent throughout the five years of post-displacement.

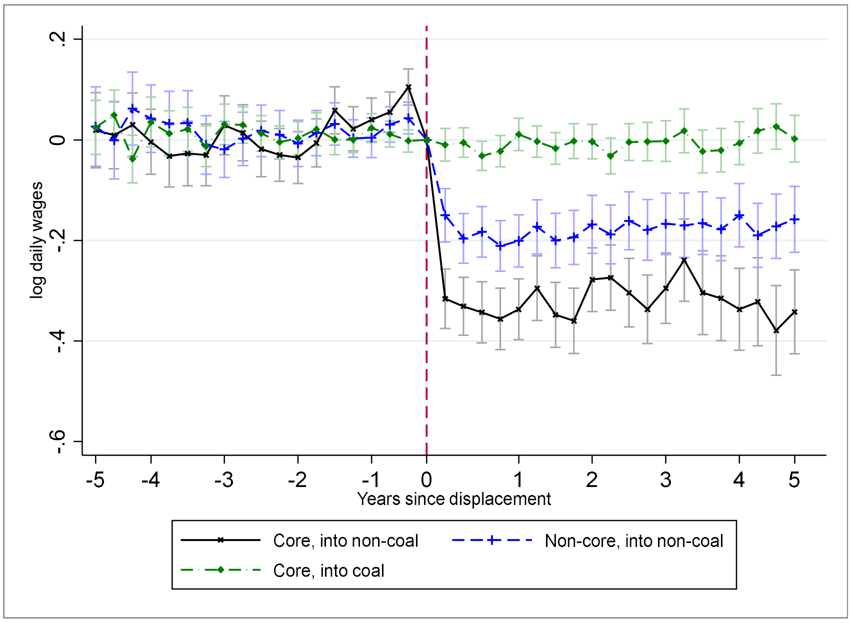

Figure 6: Daily wage losses by occupation and re-employment sector

Notes: "*Core, into non-coal*" refers to core occupations —mining and quarrying workers (ISCO-08 8111) and mining labourers (ISCO-08 9311)— among displaced coal workers who switch to non-coal jobs. "*Non-core, into non-coal*" includes all other occupations among those switching to non-coal. "*Core, into coal*" is defined analogously. The estimates are based on employed workers only (positive earnings), as in Figure 3, Panel C. The y-axis shows real daily wages in natural logarithms. Whiskers indicate 95% confidence intervals.

The diverging patterns of daily wage penalties across the three groups are mirrored in the monthly earning losses that are reported in Panel A of Figure A4 in the appendix. Core mining workers who return to coal experience only a short-lived monthly earnings loss after displacement and recover quickly thereafter, with estimates close to zero or positive in later periods. In contrast, core mining workers who move to non-coal sectors experience large and persistent monthly earnings losses, generally around 35–45 percent of pre-displacement earnings. Non-core workers transitioning to non-coal sectors also face persistent losses, but these are smaller than those of core miners moving out of coal.[26] Panel B indicates that all groups experience a sharp initial employment drop, followed by substantial recovery, but employment remains somewhat below pre-displacement levels for workers moving to non-coal jobs. Thus, the particularly large monthly earnings losses among core miners transitioning out of coal reflect both persistent daily-wage penalties and weaker employment attachment after displacement.

We now turn to the question of whether the duration of non-employment matters for wage losses. Our data cannot identify unemployment per se; individuals outside registered employment are coded as non-employed[27], a category that may include informal work. Accordingly, our estimates should be viewed as upper bounds of true wage losses.

Before we discuss the results of these losses, we present Kaplan-Meier estimates of survival in non-employment for displaced workers. The estimated Kaplan-Meier survival function in Appendix Figure A6 implies a large hazard rate from non-employment in the first quarter, followed by rapidly declining hazard rates in the subsequent quarters: while in the first post-displacement quarter the implied hazard rate is 0.416, it declines sharply to 0.232, 0.155, 0.120 in quarters 2, 3, and 4 respectively. This sharp decline in the implied hazard rate most likely points to adverse selection of the group of displaced workers who remain in non-employment.[28] Hence, we would predict larger wage losses, the longer it takes displaced workers to flow back into employment.

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[26] We find a similar pattern when comparing short- and long-tenured workers: among displaced coal workers who switch to the non-coal sector, those in core mining occupations experience larger daily wage losses than those in non-core occupations, with the losses being particularly pronounced among longer-tenured workers. See Figure A5 in the appendix.

[27] As discussed in footnote 7, like in most developing countries distinguishing clearly between the labor market states *unemployment* and *out of the labor force* is virtually impossible for the majority of workers in Turkey and our administrative data only provide the information whether a worker in a given quarter works formally or does not work formally.

[28] In their seminal book Layard et al. (2005) relate declining duration-dependent outflows from unemployment to a deterioration of the "quality" of the workers remaining in unemployment (adverse selection) or to state dependence. State dependence becomes relevant when a large shock causes a particularly massive inflow into unemployment. While we cannot test this distinction with our data, we would moot that adverse selection can

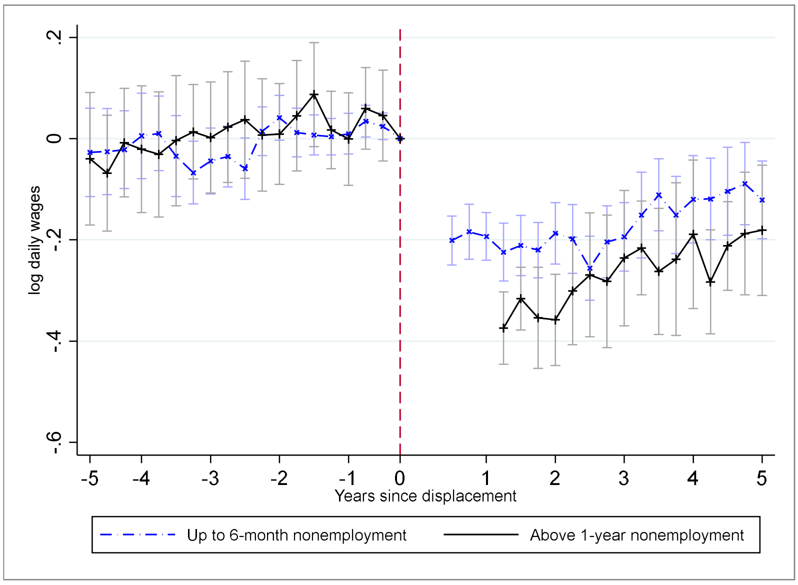

Figure 7: Daily wage losses by duration of non-employment spells

Notes: The black line shows wage losses for workers who experience *up to six months* of nonemployment after displacement, while the blue line shows wage losses for workers who experience *more than one year* of nonemployment after displacement. The estimates are based on employed workers only (positive earnings), as in Figure 3, Panel C. The y-axis shows real daily wages in natural logarithms. Whiskers indicate 95% confidence intervals.

Figure 7 reports daily wage losses, with the dashed blue line denoting workers who return to registered employment after two quarters of absence and the solid black line denoting returnees after one year. The results confirm our prediction that longer nonemployment spells are associated with larger wage losses upon reemployment. Although the wage trajectories of both groups improve gradually with time in re-employment, substantial differences persist during the first two to three years. Only after roughly three years do the two profiles converge —particularly for monthly wages (Figure A7 in the appendix)— suggesting that the initial penalties associated with longer non-employment spells fade slowly but never fully disappear. Overall, these results indicate that prolonged detachment from registered employment is associated with significantly larger and more persistent wage losses, a pattern consistent with evidence on displacement costs in other settings, including the United States (Couch & Placzek, 2010) and the United Kingdom (Layard et al. 2005).

explain the declining hazard rate from non-employment since the displacement events occur relatively evenly over time.

V. Discussion of a potential robustness check and exploration of an endogeneity issue

In the specification thus far, the control group consists of non-displaced workers who are matched to displaced workers based on pre-displacement characteristics. This gives us a large and observationally comparable control group. A potential concern is that never-displaced workers may differ systematically from displaced workers in unobserved ways, especially if displacement risk is correlated with worker or firm characteristics. We mitigate this concern by matching on pre-displacement characteristics and by documenting the absence of differential pre-trends.[29]

A not-yet-treated design in the spirit of Callaway–Sant'Anna could be an alternative approach to address this concern by comparing earlier- and later-displaced workers within the ever-displaced population. Its appeal is that both treatment and control workers eventually experience displacement and may therefore be more comparable in terms of underlying displacement risk. However, later-treated workers are not automatically clean controls in displacement settings. Because they are themselves selected into eventual displacement, they may already exhibit different outcome dynamics before their own layoff, so using them as controls could attenuate estimated losses by comparing early-displaced workers to workers whose earnings are already beginning to decline. Just as importantly, in our setting displaced workers account for only about 1 percent of the analysis sample. Restricting the control group to later-treated workers would therefore sharply reduce the effective sample size, especially in an event-study specification with multiple leads and lags, and would likely lead to a substantial loss of precision.

For these reasons, we follow the classic displacement literature, which typically compares displaced workers with continuously employed or otherwise non-displaced workers. We therefore rely on a matched non-displaced control group, which allows us to maintain both covariate balance and statistical power. Together with the absence of differential pre-trends, this supports the validity of our matched event-study design.

A related but distinct concern is selection into mass layoffs at the firm level. If firms that experience mass layoffs are already systematically weaker prior to the layoff, then the post-displacement losses of their workers may partly reflect pre-existing firm differences rather than the causal effect of job loss alone. To assess this possibility, we compare mass-layoff and non-mass-layoff firms in terms of pre-layoff labor productivity, proxied by average worker wages

[29] In all figures where we document post-displacement wage losses, differences in pre-displacement wage trends between treated and controls are clearly absent (see figures 3 through 7 and figures A2 through A7).

prior to the event. Table A3 shows that average pre-layoff monthly and daily nominal wages are lower among mass-layoff firms, but the differences are not statistically significant. The estimated gap is 9,801 TL for monthly wages (p=0.423) and 288 TL for daily wages (p=0.507). These results therefore do not provide strong evidence that mass-layoff firms were systematically lower quality before the layoff, at least when firm quality is measured by average pre-layoff wage levels.

As an additional diagnostic, we examine the position of mass-layoff firms in the pre-layoff wage-based productivity distribution. We divide firms into bins based on average pre-layoff wages and compute the share of mass-layoff firms within each bin. If mass layoffs were concentrated in the lower tail, this would suggest that displacement is disproportionately associated with low-wage or low-productivity firms. Figure A8 does not support this pattern. Although the mass-layoff share varies across bins, mass-layoff firms are not disproportionately concentrated in the lower tail of the distribution; if anything, the highest pre-layoff wage bins display higher mass-layoff shares. This evidence is consistent with the mean-comparison results and provides further descriptive support against the concern that our estimated displacement effects are driven primarily by workers being selected from systematically low-productivity firms. While these exercises cannot rule out selection on unobserved firm characteristics, they provide no indication that mass layoffs are concentrated among firms with low pre-layoff wage-based productivity.

VI. Direct welfare costs of a potential coal shutdown and policy implications

To assess the total economic burden of displacement over time, we estimate the present discounted value (PDV) of average earnings losses in terms of years of pre-displacement annual earnings, based on our main specification using real monthly earnings, shown in panel A of Figure 3. We apply a quarterly discount rate r_q of 1.227 percent, corresponding to an annual rate of 5 percent, to remain comparable to Davis and von Wachter (2011) and subsequent studies (e.g., Lachowska et al., 2020; Rud et al., 2024). The PDV of earnings losses over the first 20 post-displacement quarters is calculated as: $PDV = \sum_{k=1}^{20} \frac{3\delta_k}{(1+r_q)^k}$, where δ_k denotes the estimated real monthly wage loss in quarter *k*. We normalize the PDV by average pre-displacement earnings to express losses relative to workers' prior earnings.

Our estimates imply a PDV loss of 16,447 TL in real terms (in 2010 prices) over the first five years following displacement for workers who exit coal employment, based on the Figure 3 coefficients. This corresponds to about 1.02 years of pre-displacement annual earnings[30]. Losses are of similar magnitude for workers who transit to non-coal employment, whereas they are much smaller for workers who return to coal, amounting to roughly one quarter of a year of prior earnings.

Our five-year PDV loss of about one year of pre-displacement annual earnings is comparable in magnitude to the 0.8 years after six years reported by Couch and Placzek (2010) and the approximately one year after five years reported by Lachowska et al. (2022) for UI claimants in the United States. However, our estimate is substantially smaller than that of Rud et al. (2024), who report that displacement costs among coal miners in the United Kingdom amount to between four and six years of pre-displacement earnings over a 15-year horizon. While differences in time horizons partly account for this gap, the context also differs markedly: Rud et al. (2024) study a period of advanced coal transition and large-scale restructuring during the 1990s in the UK, whereas Turkey has not yet experienced a comparable coal phase-out. In the absence of an industrial collapse, local labor markets in coal regions may remain relatively resilient, limiting persistent scarring and prolonged non-employment exposure compared to the UK case. Moreover, informal employment may act as an adjustment margin (Ponczek and Ulyssea, 2021), reducing measured losses in registered earnings and therefore mechanically yielding smaller PDV estimates in our setting.

The PDV estimates also allow us to quantify the aggregate burden of coal-worker displacement. We calculate this burden by multiplying the estimated worker-level PDV loss by the total number of coal miners. This yields an aggregate burden of approximately 616.2 million TL in real terms (2010 prices), or about 410 million USD at the 2010 exchange rate.[31] Relative to the size of the national economy, this corresponds to roughly 0.05 percent of Turkey's 2010 GDP. While modest from a macroeconomic perspective, this aggregate figure conceals substantial regional disparities and local vulnerabilities.

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[30] For scale, the estimated worker-level PDV loss of 16,447 TL is expressed in 2010 TL. The gross monthly minimum wage in Turkey was 729 TL in the first half of 2010 and 760.5 TL in the second half of 2010. Thus, the estimated displacement loss corresponds to approximately 22 months of gross minimum-wage earnings, or about 1.8 years of minimum-wage income.

[31] The aggregate burden is calculated by multiplying the estimated worker-level PDV of earnings losses (16,447 TL per displaced coal worker) by the average number of coal workers in 2014–2015 (37,468), yielding a total burden of approximately 616 million TL in real terms. We use the 2014–2015 average because 2014 is the first displacement-cohort year in our event-study analysis. Averaging over the first two years of the treatment period provides a stable baseline measure of the coal workforce while avoiding the use of later employment levels that already reflect the sector's subsequent contraction, which would mechanically understate the implied burden of a complete coal shutdown. Because nominal wages are deflated using the CPI (2010 = 1), all monetary values are expressed in constant 2010 prices. Accordingly, the USD conversion uses the average 2010 exchange rate (approximately 1.503 TL per USD), corresponding to roughly USD 410 million. The USD figure is intended solely to facilitate international comparisons and should not be interpreted as the equivalent amount in current prices or at current exchange rates.

These vulnerabilities arise because coal employment in Turkey is highly geographically concentrated, as shown in Figure A1 in the appendix. To capture the spatial distribution of transition costs, we allocate the estimated aggregate burden across regions and provinces using the worker-level five-year PDV estimate and the geographic distribution of coal employment. Panel A of figure A9 in the appendix shows that the burden is heavily concentrated in the Aegean and West Black Sea regions, which together account for about 77 percent of total losses. Panel B further highlights considerable within-region concentration, with provinces such as Zonguldak and Manisa bearing a disproportionate share of the total burden. This concentration implies that, even if the aggregate burden remains modest at the national level, the local economic consequences of a coal phase-out could be substantial in a small number of coal-dependent areas.

The West Black Sea region, and especially Zonguldak, combines a high aggregate burden with strong dependence on coal employment. Zonguldak has historically been Turkey's main hard-coal province and remains the only province where hard-coal extraction takes place (Eroğlu, 2025). This legacy has shaped a highly coal-dependent urban identity and local economy, characterized by a mono-centric economic structure and limited diversification (Demir & Acar, 2025). In our analysis sample for 2012–2021, underground coal mining accounted, on average, for 7.8% of total employment in Zonguldak.[32] Although this city remains the highest coal-employment share among Turkish provinces, the share declined steadily over the period, reflecting the longer-run restructuring of the province's coal-based economy and the gradual contraction of mining activity since the late twentieth century (Demir & Acar, 2025). Under a complete coal shutdown, the implied PDV burden for Zonguldak is equivalent to roughly 4.5% of the province's annual labor earnings bill[33].[34] This makes

[32] Our calculations are based on registered employee records. Therefore, the denominator for total employment is likely smaller than true provincial employment, which may overstate the coal-employment share in our sample. Nevertheless, the magnitude for Zonguldak is broadly comparable to population-level statistics: Özen and Aşık (2023) report that coal mining accounted for about 6% of provincial employment in Zonguldak in 2020. They also report that, in other provinces with notable coal-mining activity, coal's share in total employment was below 1%. In our sample-based calculations, averaged over the full analysis period, Zonguldak has the largest coal-employment share, at 7.8%, followed by Bartın (3.2%), Manisa (2.5%), Kütahya (2.1%), and Karaman (1.2%). In all other provinces, the share remains below 1%. See the distributional map in Figure A1 in the appendix.

[33] Burden share of the annual provincial wage bill is calculated as: $\frac{N_p^{coal} x\ PDV}{12\ x\ w_p^{avg}\ x\ E_p}$, where N_p^{coal} is the number of coal workers in province p, PDV is the estimated worker-level present discounted value of earnings losses, equal to 16,447 TL in real terms in our case, w_p^{avg} is the average monthly wage in province p, and E_p is total employment in province p.

[34] Because the PDV burden is allocated using the provincial share of coal employment in total employment, the resulting province-level burden may appear small in major coal-producing provinces with large overall labor markets, although coal extraction and coal-fired generation remain highly important in specific localities. This is the case in larger provinces such as Kahramanmaraş and Muğla, where the province-wide burden appears modest relative to smaller provinces such as Karaman, despite larger-scale lignite extraction activity in the former.

Zonguldak a critical transition region, where both the scale of losses and local reliance on coal are substantial. By contrast, the Aegean region, particularly Manisa, also accounts for a high absolute burden, but its lower dependence on coal at province level suggests a more diversified local economy, even though the number of affected workers is large.[35]

This spatial concentration is important for policy design. The strong spatial heterogeneity suggests that the adjustment costs of coal exit are real but far from uniformly distributed across the country. This pattern is consistent with other coal-producing settings in Europe. In Poland, for example, hard-coal mining employed about 74,400 workers in November 2024, substantially more than in Turkey, and mining activity is highly concentrated in two regions: the Upper Silesian Coal Basin and the Lublin Coal Basin (Instrat Foundation 2025, WUG 2024). Upper Silesia and Western Małopolska are also identified as among the regions most severely affected by the coal transition. This local vulnerability underscores the need for place-based transition policies rather than uniform national compensation schemes.

In highly coal-dependent provinces with a narrow industrial base centered on coal, such as Zonguldak, transition policy must go beyond worker compensation and retraining to include a broader regional development strategy. Drawing on Demir and Acar's (2025) discussion of Zonguldak's transition challenges, such a strategy could include redirecting coal subsidies toward local economic diversification, supporting alternative sectors, preserving and repurposing mining heritage for tourism and cultural uses, and building formal just-transition institutions that bring together local governments, firms, unions, and community actors. In provinces with a more diversified economic base, however, the policy focus should be more spatially targeted. In Manisa, for example, province-level averages mask the much stronger dependence of districts such as Soma on coal. In such cases, transition support should be concentrated at the district or coal-basin level, with measures centered on displaced workers and affected local suppliers, including temporary income support, retraining and job-placement assistance, incentives for new investment in the affected district, and redevelopment of former mining sites. A similar logic applies to other coal-producing provinces such as Muğla and

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Furthermore, in Muğla, coal activity is concentrated mainly in Milas and Yatağan, while in Kahramanmaraş it is centered in the Afşin–Elbistan basin. This highlights substantial within-province heterogeneity: province-level averages may understate the burden in coal-dependent districts even when the province as a whole appears relatively diversified. See Figure A9 in the Appendix.

[35] Province-level averages may mask substantial within-province heterogeneity. For example, while Manisa province as a whole has a relatively diversified economic structure, Soma, a town within Manisa province, is much more strongly organized around coal mining. Similar patterns may apply to other coal-producing provinces, such as Muğla and Kahramanmaraş, where coal dependence is concentrated in one or two districts rather than spread evenly across the province.

Kahramanmaraş, where coal dependence is concentrated in one or two localities rather than spread evenly across the province.

A final consideration concerns the net fiscal cost of a potential coal-exit scenario. The estimated displacement burden should be viewed alongside the public resources already devoted to supporting coal. Existing coal-related subsidies and tax incentives suggest that part of the transition could be financed by gradually redirecting these resources toward income support, retraining, and local economic diversification. Demir and Acar (2025) make a similar point for Zonguldak, emphasizing the reallocation of coal-sector support toward clean sectors and broader regional development. Yet up-to-date empirical evidence on coal-related subsidies in Turkey is scarce. To our knowledge, the latest comprehensive quantitative assessment is Acar and Yeldan (2016), partly reflecting the lack of transparent and publicly available data on coal support. This data limitation also prevents us from providing a fuller accounting of the fiscal resources that could be reallocated toward transition policy. Nevertheless, available estimates indicate that coal continues to receive substantial direct and indirect support. Based on SHURA(2019)'s estimates, support to coal suppliers averaged USD 447 million per year over 2015–2017, covering only quantifiable items such as public investments, state-owned enterprise losses, tax exemptions, and investor incentives. Major non-quantified forms of support, such as concessions, market guarantees, and priority access to scarce resources such as land, are not included and should therefore be viewed as additional. Moreover, following the 2014 Soma mine disaster, in which 301 miners died, amendments to Turkey's mining and labor legislation introduced more protective working conditions for underground mine workers. In particular, the Mining Law mandated that underground workers employed in lignite and hard-coal mines receive wages no lower than twice the statutory minimum wage (MLSS, 2015), a wage premium that is also reflected in our data (see Figure 2). Subsequent support mechanisms provided government compensation to employers for part of the additional labor costs arising from this wage floor, reduced working hours, and expanded leave entitlements. Taken together, these existing support mechanisms suggest that a coal phase-out need not be financed entirely through new public spending, as part of the transition cost could be offset by gradually reducing and reallocating coal-related subsidies, including employer compensation for higher labor costs in underground lignite and hard-coal mining.

Finally, it is important to emphasize that our estimates capture only the direct cost side of a coal phase-out, measured through wage and earnings losses among displaced coal miners. They do not include potential benefits from coal exit, such as improvements in health, environmental quality, or fiscal savings from reduced coal-related support. Nor do they capture the broader indirect effects on local economies tied to coal-related activities, including coal-fired power plants, downstream industries, and small businesses serving coal communities. Thus, the estimated PDV should be interpreted as a measure of direct labor-market adjustment costs for miners, not as a full welfare evaluation of coal phase-out.

VII. Conclusions

Since Turkey has not yet begun a coal phase-out, we cannot directly observe the employment and earnings losses associated with a large-scale shutdown of the sector. Instead, using linked employer–employee administrative data, we estimate the costs of job loss in coal mining by studying workers displaced from coal jobs through mass layoffs and comparing them to observationally similar non-displaced workers in a matched event-study design. To get close to the true labor-market costs of a future coal transition, our main focus is on displaced coal workers who subsequently find re-employment outside the sector, since under a large-scale coal exit a return to coal employment would no longer be feasible for most workers.

We document large and persistent displacement costs for coal miners in Turkey. Among workers who return to registered employment, real monthly earnings fall by about 25 percent immediately after job loss, while employment drops sharply and remains below its pre-displacement level several years later. Daily wages among re-employed displaced workers decline by about 12–15 log points, corresponding to roughly 11–14 percent, and remain depressed throughout the post-displacement period. Taken together, these results show that displacement costs reflect both weaker employment attachment and lower pay in post-displacement jobs.

A central finding of the paper is that these losses are highly heterogeneous across post-displacement trajectories. Workers who return to coal mining experience little or no persistent wage penalty, whereas those moving to non-coal sectors face substantial and lasting losses. The persistent wage penalty among re-employed displaced workers is therefore driven primarily by transitions out of coal rather than by displacement per se. This pattern is consistent with the loss of coal-sector wage premia and with the limited transferability of sector- and occupation-specific skills to jobs outside mining. It also implies that observed displacement losses in the current setting likely understate the costs of an actual coal phase-out, under which re-employment within coal would largely cease to be an option.

The heterogeneity analysis further shows that the burden of displacement is not evenly distributed across workers. Wage losses are particularly severe for core underground miners who move to non-coal sectors, suggesting that the destruction of occupation-specific human capital is especially costly for workers in core mining occupations. Longer non-employment spells after displacement are likewise associated with substantially larger and more persistent earnings losses, indicating that delayed re-entry into registered employment is an important margin through which welfare losses accumulate. We also find that long-tenured workers experience more persistent wage losses than short-tenured workers, especially when they leave coal, consistent with the loss of wage premia accumulated through sector-, occupation-, or match-specific attachment.

Our results also speak to the aggregate cost of a future coal transition. Back-of-the-envelope calculations suggest that the aggregate wage-bill cost of a complete coal shutdown, measured by the present discounted value of miners' earnings losses, is modest relative to national output and therefore appears manageable in macroeconomic terms. At the same time, these aggregate figures mask pronounced spatial concentration. A small number of coal-dependent provinces account for a disproportionate share of the total burden, implying that even a transition with a limited aggregate cost may generate substantial local labor-market distress and strong political resistance if left unmanaged.

The policy implication is therefore not that coal transition should be delayed because it creates losers, but rather that the existence of concentrated and persistent losses strengthens the case for a credible just-transition strategy. In the Turkish context, such a strategy should combine targeted income support with measures that improve re-employment prospects outside coal, especially for core miners and for workers in highly coal-dependent provinces. Because our estimates capture only the direct labor-market cost side of coal exit, they should not be interpreted as a full welfare evaluation of decarbonization. They do not account for potential benefits of coal phase-out, such as gains in health, environmental quality, or fiscal savings from reduced support to the coal sector, nor do they capture broader indirect effects on local economies linked to coal-related activity. What the paper does show, however, is that the main challenge of coal transition in Turkey is unlikely to be aggregate affordability, but rather the design of policies that address concentrated worker- and place-specific adjustment costs.

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APPENDIX FIGURES

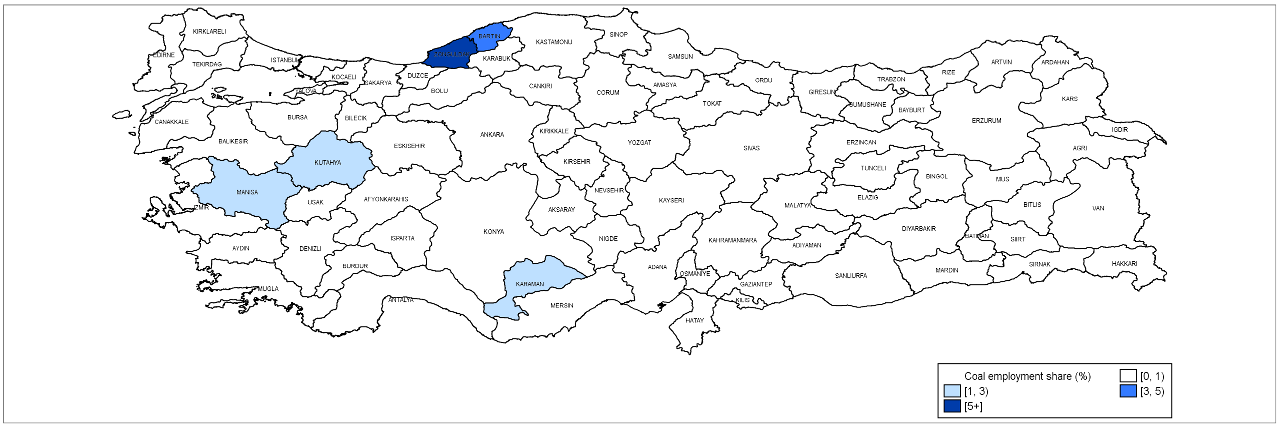

Figure A1. Coal employment share (%) by province

Notes: The map shows coal employment in each province as a percentage of total provincial employment. Zonguldak has the largest coal employment share, at 7.8%, followed by Bartın (3.2%), Manisa (2.5%), Kütahya (2.1%), and Karaman (1.2%). These shares are computed as averages over the full analysis period. Although the map reports period averages, the underlying annual shares display a steady downward trend over time. Since the calculations are based on registered employee data, they may overestimate the true coal employment share in total provincial employment.

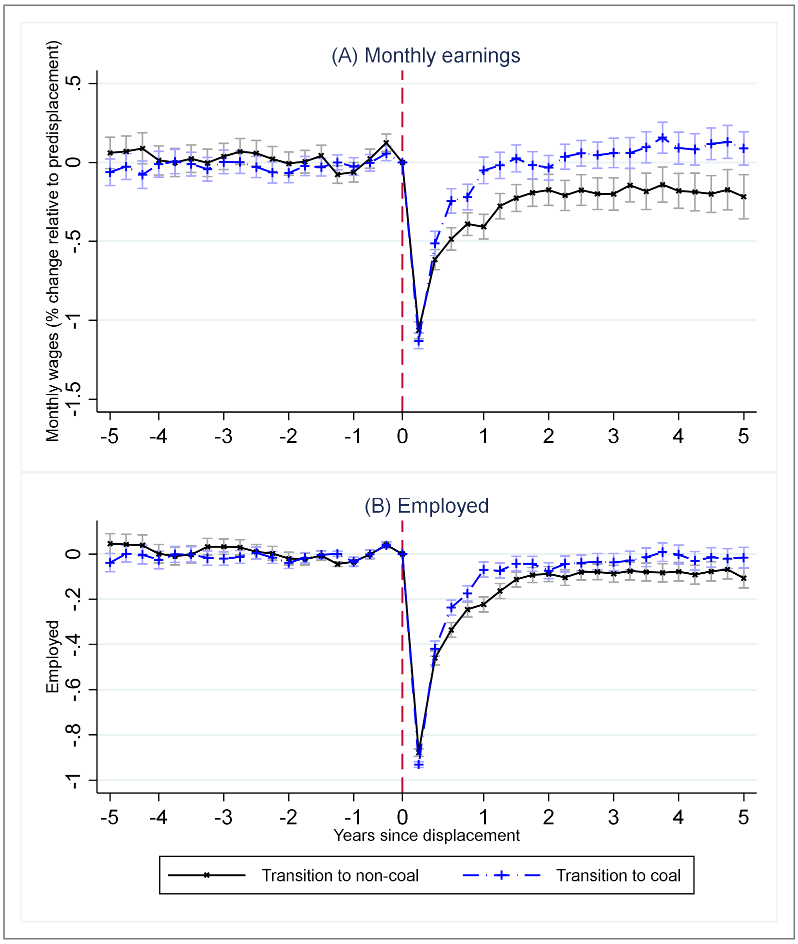

Figure A2. Monthly earning and employment losses: Re-employment in coal vs. non-coal

Notes: "**Transition to non-coal**" refers to earnings and employment losses among workers re-employed outside coal mining, while "**transition to coal**" refers to losses among workers re-employed in coal mining. In both cases, re-employment occurs after at least one quarter of non-employment. The estimates are based on the "all zeros" sample, as in Panels A and B of Figure 3. In **Panel A**, the y-axis shows monthly real wages normalized by the individual's average pre-displacement wage. In **Panel B**, the y-axis refers to "employed," a binary indicator equal to one if the worker has at least one day of work in the corresponding year. Whiskers indicate 95% confidence intervals.

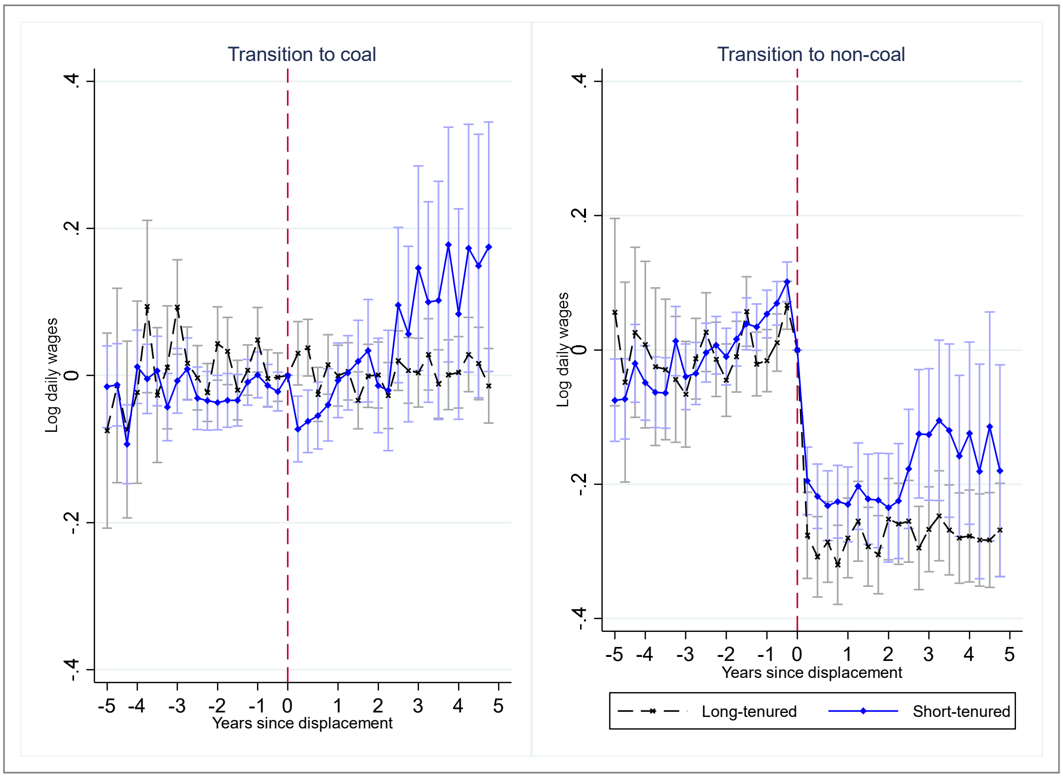

Figure A3. Daily wage losses by pre-displacement tenure: Re-employment in coal vs. non-coal

Notes: The figure reports estimates of daily wage losses separately for displaced workers with short and long pre-displacement tenure, using 12 quarters as the cutoff for the tenure split. "Transition to non-coal" refers to daily wage losses among workers re-employed outside coal mining, while "transition to coal" refers to daily wage losses among workers re-employed in coal mining; in both cases, re-employment occurs after at least one quarter of non-employment. The estimates are based on employed workers only (positive earnings), as in Figure 3, Panel C. The y-axis shows real daily wages in natural logarithms. Whiskers indicate 95% confidence intervals.

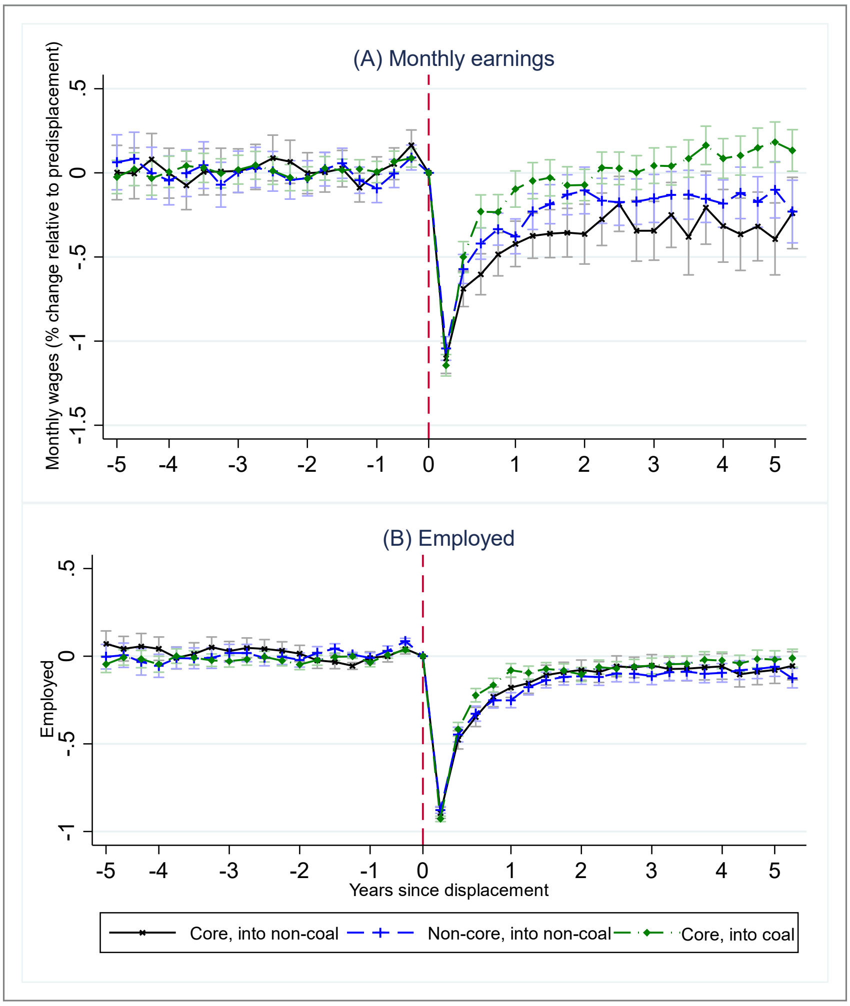

Figure A4. Monthly earnings and employment losses by pre-displacement occupation and re-employment sector

Notes: "*Core, into non-coal*" refers to displaced coal workers previously employed in core mining occupations — mining and quarrying workers (ISCO-08 8111) and mining laborers (ISCO-08 9311) — who are re-employed outside coal mining. "*Non-core, into non-coal*" refers to all other pre-displacement occupations among workers re-employed outside coal mining. "*Core, into coal*" is defined analogously for core mining workers re-employed in coal mining. The estimates are based on the "all zeros" sample, as in Panels A and B of Figure 3. *Panel A* reports real monthly wages normalized by the individual's average pre-displacement wage. *Panel B* reports an employment indicator equal to one if the worker has at least one day of registered employment in the corresponding year. Whiskers indicate 95% confidence intervals.

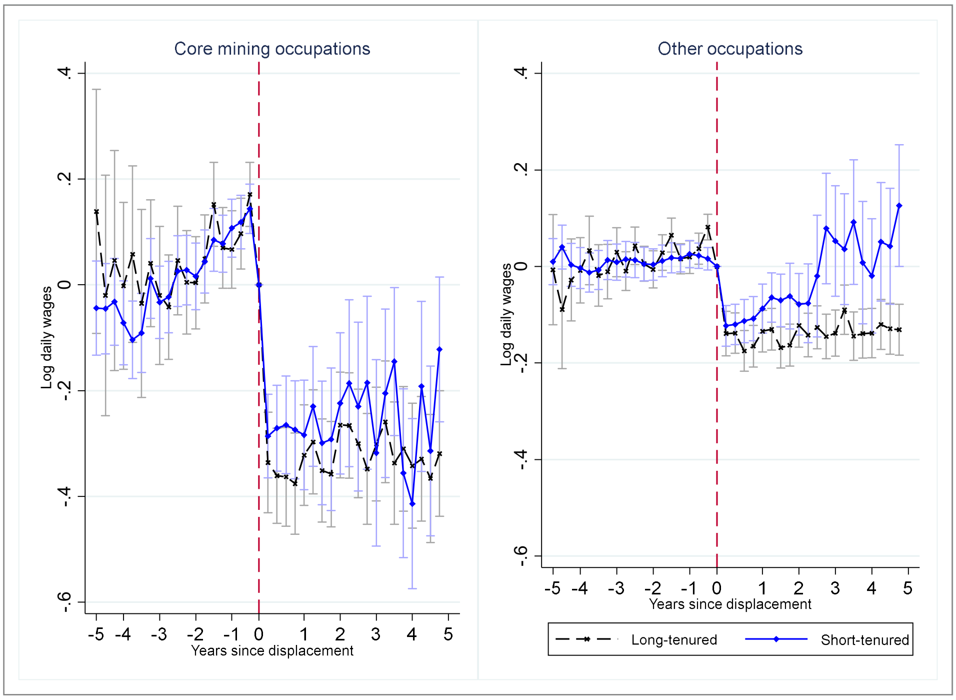

Figure A5. Daily wage losses by pre-displacement tenure and occupation: core underground mining versus other occupations

Notes: The figure reports estimates separately by pre-displacement tenure and occupation group. Short- and long-tenure workers are defined using 12 quarters of pre-displacement tenure as the cutoff. "Core occupations" include mining and quarrying workers (ISCO-08 8111) and mining labourers (ISCO-08 9311) among displaced coal workers who switch to non-coal jobs; "non-core occupations" include all other occupations among non-coal switchers. Estimates are based on employed workers only, as described in Figure 3, Panel C. The y-axis shows real daily wages in natural logarithms. Whiskers indicate 95% confidence intervals.

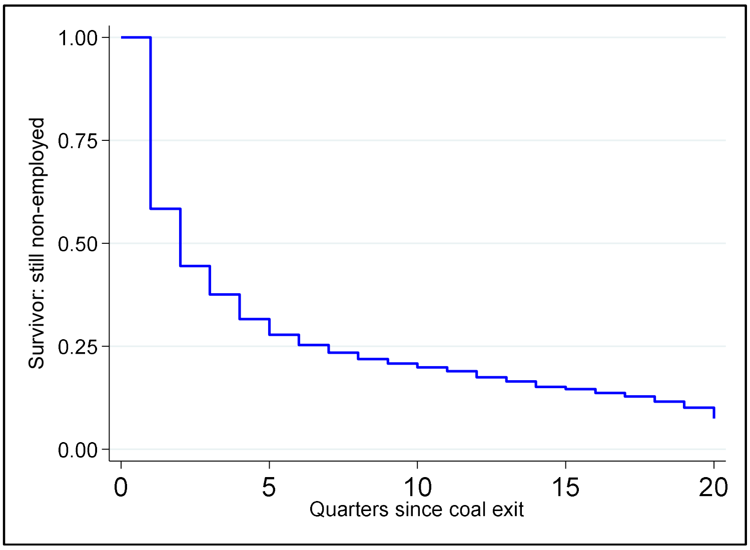

Figure A6. Kaplan–Meier survivor function

Notes: The figure shows the survivor function for non-employment following coal exit. The y-axis reports the share of displaced coal workers who remain non-employed at each quarter since displacement. The x-axis reports quarters since the displacement.

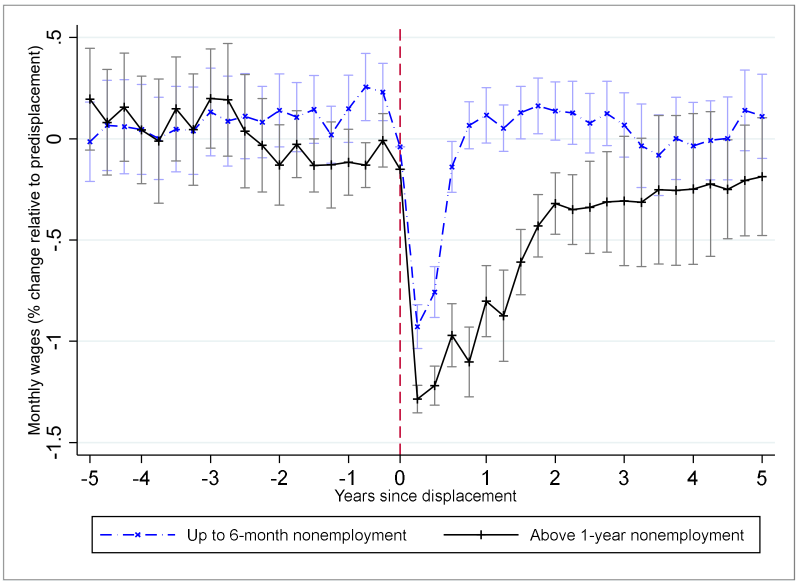

Figure A7. Monthly wage losses by duration of nonemployment spells

Notes: The black line shows wage losses for workers who experience up to six months of nonemployment after displacement, while the blue line shows wage losses for workers who experience more than one year of nonemployment after displacement. The estimates are based on the "all zeros" sample, as in Figure 3, Panel A (black line). The y-axis shows monthly real wages normalized by the individual's average pre-displacement wage.

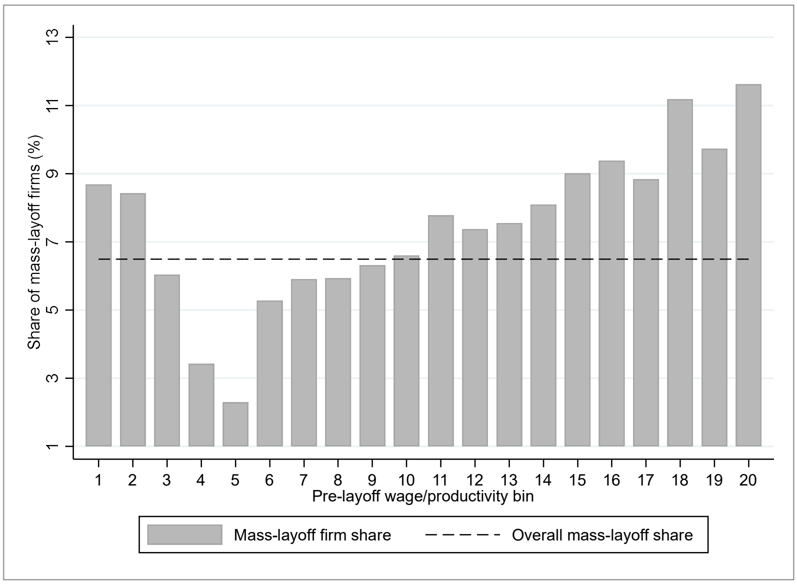

Figure A8. Mass-layoff firm share across the pre-layoff wage/productivity distribution

Notes: The figure reports the share of firms experiencing mass layoffs within each bin of the pre-layoff wage/productivity distribution. Pre-layoff firm quality is proxied by average pre-layoff monthly wages. The dashed horizontal line shows the overall mass-layoff share across all bins.

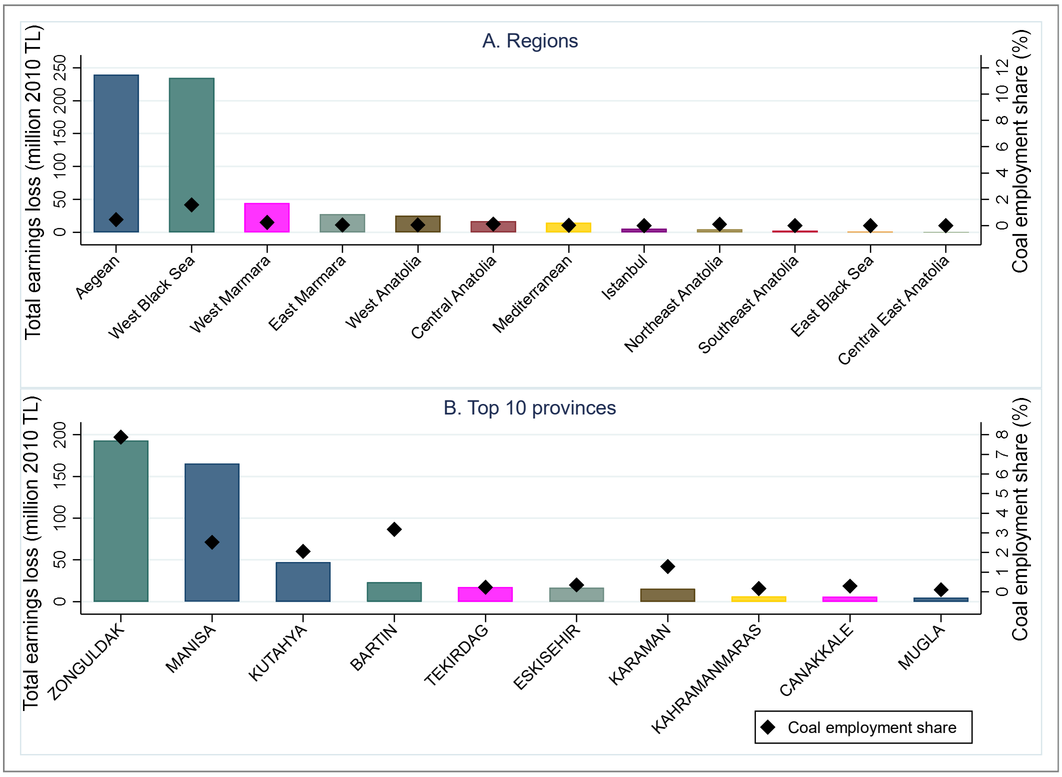

Figure A9. Regional PDV burden and coal dependence

Notes: Bars allocate the aggregate PDV burden across regions (Panel A) and provinces (Panel B) using coal employment counts from the 2014-2015 average employment data. The total burden equals 16,447 TL per coal worker multiplied by 37,468 coal workers, yielding about 616 million TL in real terms (in 2010 prices). Regions and provinces are ordered by their aggregate PDV burden, while the black dots indicate coal employment shares in regional and provincial employment (right scale). Bar colors are assigned by region. Accordingly, each province in Panel B is shown in the same color as the region to which it belongs in Panel A, facilitating comparisons between regional and provincial burdens.

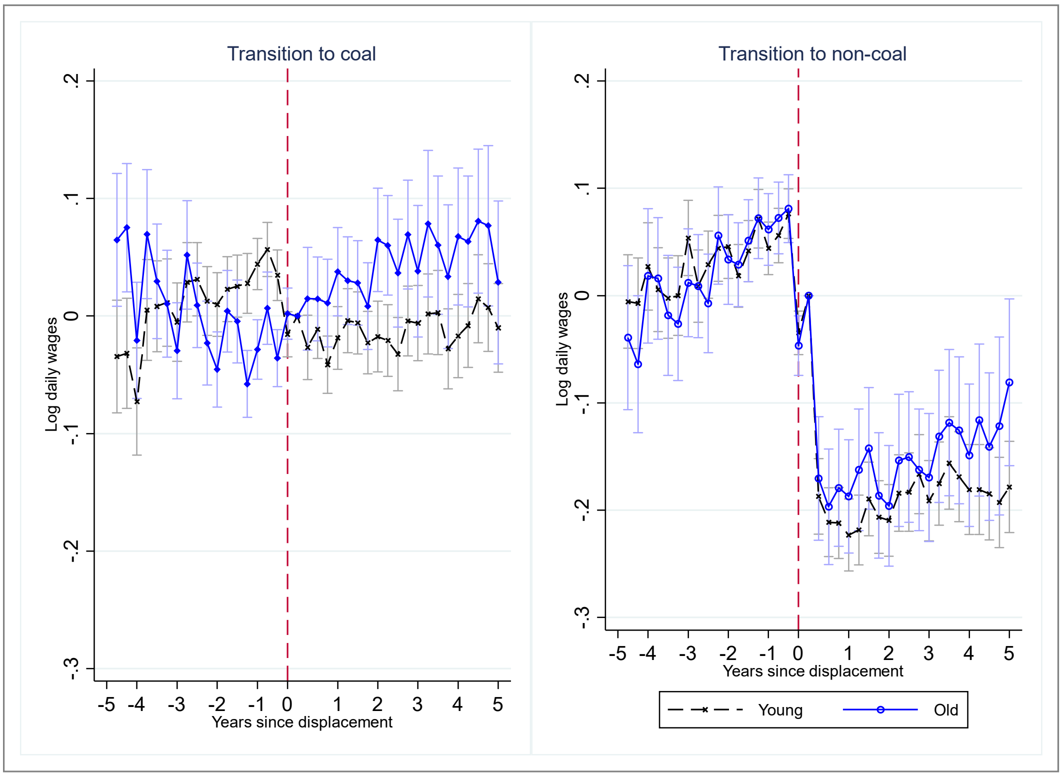

Figure A10. Daily wage losses by workers' age group

Notes: The figure reports estimates separately for workers younger than 40 (dashed black line) and workers aged 40 and above (solid blue line), where age 40 is the statutory retirement eligibility age for underground miners in Turkey. Estimates are based on employed workers only, as described in Figure 3, Panel C. The y-axis shows real daily wages in natural logarithms. Whiskers indicate 95% confidence intervals.

APPENDIX TABLES

Table A1 presents the annual counts of firms, including the total number of firms, the number of new entries and exits, along with their total and average employment figures for each quarter. The results presented in this table exclude one-person enterprises that do not expand over the entire panel period (i.e., the self-employed). With an approximately one-million increase from 2006, the number of firms in our sample reaches 2.1 million in 2021. We do, however, observe a decline between 2019 and 2020. This decline can be attributed to a significant increase in the number of exiting firms during the same period and a noticeable decrease in the number of new entrants from 2017 onwards. We also observe a similarly large increase in total employment across years over the period analyzed. As of 2021, around 14.5 million employees were reported in the SSI records, which was more than double the number in 2006.

In the right panel of Table A1 (in columns 5-8) we show similar statistics for Turkish firms in the coal mining sector. In contrast to the full EIS sample, we see a steady decline in the mining sector regarding the number of firms and employment after 2014: the number of firms falls from slightly above 600 in 2014 to 370 in 2021, while employment is reduced from roughly 43.000 in 2014 to about 32.500 in 2021. However, this decrease in the number of firms and in employment should not be taken as a sign for efforts to reduce the reliance on coal for energy production, since we see many new firms entering the coal sector.

In the left panel of table A1 we observe a strong secular rise of the number of firms between 2006 and 2021, which we interpret as a growing willingness of firm owners or firm managers to register their firms. The right panel of the table tells a different story: since mining has been continuously subsidized throughout the reported period, mine owners had strong incentives to register their activities with the authorities.

Table A1. Firm counts and total employment: Full EIS sample and coal sector sample, 2006–2021

	ALL SECTORS				COAL MINING SECTOR			
	Firms	Entrants	Exiters	Employment	Firms	Entrants	Exiters	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2006	1,110,500			6,798,237	466			41,325
2007	1,302,877	243	51	7,448,459	512	34	38	42,477
2008	1,391,626	172	83	8,041,562	538	42	43	43,035
2009	1,456,606	168	103	7,873,384	562	41	41	46,123
2010	1,534,768	191	113	8,696,141	559	39	44	46,934
2011	1,615,988	200	119	9,798,132	552	50	51	46,477
2012	1,694,233	204	126	10,881,693	563	55	58	44,776
2013	1,807,388	242	129	11,542,911	616	59	66	45,812
2014	1,884,832	210	133	12,213,442	613	71	80	42,891
2015	1,909,758	213	188	13,268,132	497	43	50	39,118
2016	1,915,929	186	180	13,065,739	551	62	65	36,581
2017	2,055,633	297	157	14,259,460	438	48	41	36,202
2018	2,144,549	255	166	14,444,079	392	43	60	33,246
2019	2,251,535	208	101	12,816,825	394	66	60	30,449
2020	2,112,301	207	346	13,856,029	380	46	50	30,947
2021	2,094,724	120	137	15,130,502	370	35	32	32,540

Source: Authors' calculations based on the *Business Registers* data of the EIS.

Note: Columns 1–4 report the annual counts of firms, including the total number of firms, the number of new entries and exits, as well as total registered employment for the entire EIS sample, excluding one-person enterprises (i.e., the self-employed). Columns 5–8 present the same information for the coal sector sample in the same order. Entrants and exits are identified based on the firm's net employment growth rate within the interval [−2,2], where −2 denotes exiting firms and +2 denotes entrants, following Ayhan et al. (2025). The left panel of Table 1 is adapted from the discussion paper version of the same study (see Table 1, Ayhan et al. (2023)).

Table A2. Overall balance diagnostics for matched and unmatched samples

	Mean Bias	Median Bias	Rubin's B	Rubin's R	%Var Outside Range	p-value (LR test)
Unmatched	49.1	53.7	106.7*	0.45*	50	0.000
Matched	3.7	3.3	10.2	0.72	25	0.981

Table A2. summarizes overall balance using standard diagnostics. The mean standardized bias declines from 49.1 in the unmatched sample to 3.7 in the matched sample, while the median bias falls from 53.7 to 3.3, indicating a substantial improvement in balance across covariates. Rubin's B is the absolute standardized difference in the means of the linear index of the propensity score between the treated and matched non-treated groups. Rubin's R is the ratio of the variances of the propensity score index in the treated and matched non-treated groups. Rubin (2001) recommends that B should be less than 25 and that R should lie between 0.5 and 2 for the samples to be considered sufficiently balanced. An asterisk is displayed next to B and R values that fall outside these limits. As shown in the table, Rubin's B statistic decreases from 106.7 to 10.2, well below the commonly used threshold of 25, and Rubin's R lies within the recommended range. The share of covariates with variance ratios outside the acceptable range declines from 50 percent to 25 percent. Finally, the likelihood ratio test shows that covariates strongly predict treatment status before matching but not after matching, as reflected in the high post-matching p-value.

Overall, these results indicate that propensity score matching achieves a high degree of balance between treated and control groups. Although a minor variance imbalance remains for lagged wages, the combination of low standardized biases, insignificant mean differences, and favorable summary diagnostics suggests that the matched sample provides a credible basis for subsequent analysis.

Table A3. Pre-layoff firm wage levels by mass-layoff status

	Non-mass-layoff	Mass-layoff	Difference	p-value
Average monthly wage	39,181	29,381	9,801	0.423
Average daily wage	1,423	1,134	288	0.507
Observations	26,748	2,399		

Notes: The table compares average pre-layoff wage levels between mass-layoff and non-mass-layoff firms. Average monthly and daily nominal wages are used as wage-based proxies for pre-layoff firm productivity or firm quality. Differences are based on two-sample t-tests. Monthly and daily wages are measured in nominal terms.

Appendix - References

Rubin, D.B. (2001). Using Propensity Scores to Help Design Observational Studies: Application to the Tobacco Litigation. Health Services & Outcomes Research Methodology 2, 169–188. https://doi.org/10.1023/A:1020363010465