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Short communication

Job displacement costs of phasing out coal☆

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ABSTRACT

The reduction of carbon emissions will require a rapid phasing out of coal and the displacement of millions of coal miners. How much could this energy transition cost mining workers? We use the dramatic collapse of the UK coal industry to estimate the long-term impact on displaced miners. We find evidence of substantial losses: hourly wages fell by 40% and earnings fell by 80% to 90% one year after job loss. These losses are persistent and remain significantly depressed fifteen years later, amounting to present discounted value earnings losses of between four and six times the miners pre-displacement earnings.

> We intend to support communities and regions that are particularly vulnerable to the economic, employment and social effects of a global transition away from carbon-intensive activity
>
> [Just Transition Declaration, 04.11.2021, U.N. climate change conference COP26]

A well-established literature finds that displaced workers suffer substantive earning losses that persist several years after losing their job (Jacobson et al., 1993; Stevens, 1997; Couch and Placzek, 2010; Davis and von Wachter, 2012; Bertheau et al., 2023).[1] These findings raise concerns regarding the future of millions of coal miners who will likely be displaced and forced to switch sectors if plans to reduce carbon emissions by phasing out coal are implemented due to climate change concerns (United Nations, 2022; Ruppert Bulmer et al., 2021).[2] How much will this energy transition cost mining workers? Answering this question is important to account for the economic losses of phasing out coal and to inform policies aimed at honoring the Just Transition Declaration quoted in the epigraph.

The existing evidence on job displacement costs is, however, unsuitable to quantitatively answer this question. The majority of the existing studies rely on mass layoff events using samples representative of the

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[1] Displacements also have been shown to impact health (Sullivan and von Wachter, 2009; Lindo, 2011; Schaller and Stevens, 2015), schooling (Rege et al., 2011), fertility (Huttenen and Kellokumpu, 2016), divorce (Charles and Stephens, 2012; Eliason, 2012) and retirement decisions (Chan and Stevens, 1999, 2001; Merkurieva, 2019). Losses also vary with the business cycle (Davis and von Wachter, 2012; Gulyas and Pytka, 2020; Schmieder et al., 2023). A related literature also studies the underlying causes of the losses in earnings (Krolikowski, 2017b; Jung and Kuhn, 2018; Burdett et al., 2020; Lachowska et al., 2020; Gulyas and Pytka, 2020; Raposo et al., 2021; Jarosch, 2023; Simmons, 2024; Huckfeldt, 2022; Braxton and Taska, 2022; Leenders, 2022; Leenders and Wallenius, 2024).

[2] See Appendix C for our back-of-the-envelope calculation of the aggregate number of active coal miners adding up to around 5.4 million.

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whole workforce to estimate the costs of job displacement. These estimates of average earnings losses are likely to be well below the costs of job loss for a coal miner who faces the complete phase-out of an industry. The collapse of an entire sector requires sectoral reallocations, whereas sectoral transitions are not compulsory following typically studied layoffs. Since coal mining is a highly specialized occupation (Samuel, 2016), this specificity of human capital may reduce miners' ability to transfer their skills, in particular as they are forced to switch occupations.[3] Moreover, most coal miners are employed in remote and rural areas where mining is often the main employer. This feature reduces local economies' capacity to absorb workers after a mine closure and, due to the need to emigrate, may increase workers' job search costs and lengthen periods of unemployment.

The contribution of this paper is to provide estimates of job displacement costs during a well known episode of sector collapse specific to coal miners. We study the dissolution of the UK coal industry that accelerated in the mid-1980s under Margaret Thatcher. This dissolution was comprehensive and fast: in just over a decade, the majority of coal mines in the UK closed and more than 200,000 miners (almost 90 percent of the industry's workforce) lost their jobs as shown in Fig. 1 (Glyn and Machin, 1997; Aragon et al., 2018). This setting is one of the few documented experiences of a large and systematic displacement of miners, and thus offers an important opportunity to learn about the possible long-term impact of sectoral collapse and in particular coal phase-out.[4]

Our empirical analysis uses data from the UK New Earnings Panel Survey, which collects earnings information from a representative sample of individuals from 1975 to the present. The richness of the data allows us to construct a longitudinal dataset tracking more than 2,000 displaced coal miners many years before and after job separation. We estimate the impact of the average mine worker's final displacement from a mine on wages and earnings using a panel data model with time and individual fixed effects that is commonly used in the literature. This estimator compares the evolution of earnings of displaced workers relative to a group of observationally similar workers. The baseline control group is constructed by matching coal miners to blue-collar manufacturing workers with similar pre-displacement characteristics. We provide a battery of further findings, in particular, several variations to the definition of the treatment and control groups and show that our results remain robust and quantitatively very similar.

We find evidence of large and persistent earnings losses. Wages for those who found a new job after displacement drop by around 40% during the first years after job loss, and remain around 20% below the wages of the control group fifteen years later. Overall earnings fall by 80% to 90% in the year after displacement and remain depressed by 20% to 30% fifteen years later. Over the fifteen year period, present discounted earnings losses amount to between 4 and 6 times the miners pre-displacement earnings. In the heterogeneity analysis we show that older males, craftsmen, those in economically remote regions, such as the South of Wales, and those displaced during the early period of the phase out were particularly strongly affected, albeit, these differences are often not statistically significantly different.

Our findings are qualitatively similar to other studies on job displacement. However, the magnitudes are substantially larger. For instance, Couch and Placzek (2010) document more moderate earnings losses of 32%–33% in the first year and 13%–15% after six years, while Davis and von Wachter (2012) estimate cumulative losses over a twenty year period of around 1.7 times pre-displacement earnings.

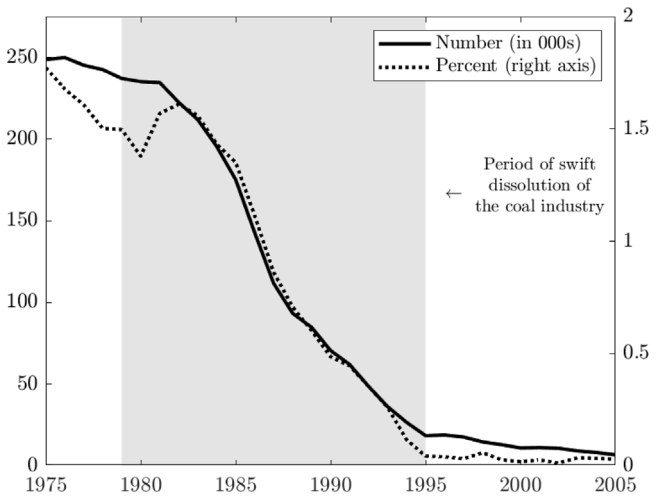


Fig. 1. Coal mining employment in the UK 1975–2005.
Note: The grey area denotes the period where we will entertain separations from in the empirical analysis.
Source: The number employed is collected from National Coal Board (1970-1993) and used in Aragon et al. (2018). The percent shown on the right axis was calculated from the New Earnings Survey.

The large and persistent earning losses in our findings are consistent with coal miners being particularly vulnerable to job displacement, perhaps due to the low transferability of occupation-specific skills or lower labour mobility. Assuming that these conditions apply to coal miners in other settings today, our results suggest that the phasing out of coal could bring substantial disruptions to miners, their families and mining communities.

The remainder of the paper proceeds as follows. In Section 1 we briefly discuss the dissolution of the UK coal industry. In Section 2 we discuss our empirical strategy and Section 3 presents the results. We conclude in Section 4.

1. Historical context

Coal played a key role fuelling UK's industrialization process and was an important source of well-paid, manual jobs. By the early 1980s, however, the industry had experienced a long, albeit gradual, decline and was reliant on government subsidies (NUM, 2021; Glyn, 1988). In 1985, after a year-long strike, from which the National Union of Coal miners would never recover, the government started withdrawing its support and the mine closures accelerated.

The dissolution of the industry was comprehensive and fast (Glyn, 1988; Glyn and Machin, 1997; Aragon et al., 2018). In just two years, 1985 to 1986, one-third of coal mines closed. By 1994, when the industry was privatized, only 26 mines were operational out of more than 200 at the beginning of 1980s. By the early 2000s, only a handful of mines remained. The closure of mines was mirrored by a massive displacement of coal miners. Between 1980 and 1994, more than 200,000 miners lost their jobs (see Fig. 1). This amount represented a reduction of approximately 90 percent of the industry's workforce.

The socioeconomic impact of coal mine closures at the community level has been widely studied. These studies document severe and persistent negative impacts on employment and labour force participation rates, demographic and living conditions in the affected mining communities (Beatty and Fothergill, 1996; Bennett et al., 2000; Beatty et al., 2007; Aragon et al., 2018). We zoom in on the displaced miners, a group of predominately male manual workers who presumably carried most of the costs to the communities created by the phase-out of coal.

[3] Neal (1995), Huckfeldt (2022) and Braxton and Taska (2022) find that those who switch sectors and occupations following job loss fare worse than those who do not. See Carrillo-Tudela and Visschers (2021) for more work on occupational transitions.

[4] See other recent work by Haywood et al. (2021) looking at the decline in coal mining in Germany.

2. Methods

2.1. Data

Data: We use data from the 2017 New Earnings Panel Survey (Office for National Statistics, 2017). This is a longitudinal survey that tracks cohorts of individuals from 1975 to the present and provides annual information on weekly earnings and hours worked during April each year. Throughout, we deflate earnings to 2000 prices. Information is also provided on occupation, industry, gender, age, geographical administrative unit, and whether the wage was determined through a collective agreement. Individuals are tracked using their National Insurance number, a unique tax identifier which does not change over the lifetime of a worker. This feature allows us to track individuals even after unemployment spells, and across different employers and locations.

Treatment: Studies focusing on the identification of job displacements, typically rely on mass layoffs in situations in which a firm's employment permanently declines by thirty or more percent over a short period of time such as for example in Davis and von Wachter (2012), Flaaen et al. (2019) and Bertheau et al. (2023). As discussed in the background section, the dissolution of the coal industry proceeded fairly quickly and was unexpected[5] and plausibly exogenous to the individual coal miner, implying selection is less of a concern. This allows us to study all separations during this period, which is distinguishing us from the previous literature which focuses only on displacements during mass layoffs. Moreover, sectoral dissolution allows us to be confident that reemployment as a coal miner was not an option and so sectoral reallocation was a requirement for future employment.[6] The dissolution of the coal sector was determined on the national level and brought to a conclusion 30 years later, even though 80% of the task was accomplished within a period of 10 years, by 1995.

Displaced miners: We follow the literature and choose for each year t all workers that work full time at a coal mine. From this group of miners we keep those who are between 25 and 55 years old, and those who were employed by the same firm for at least 2 consecutive years, in t and $t-1$, before being laid-off between t and $t+1$. We focus on the periods during which the coal industry collapses, and analyse displacements in the years between, and inclusive of, 1979 and 1995. Since we are interested in displacements associated with being subsequently unable to work in coal mining, we focus on a worker's final separation from a mine. This leaves us with 17 year-specific cohorts and a total of 2152 displaced miners. Each cohort contains 20 years, 4 prior to displacement and 15 after. Let $k = \{-4, 15\}$ represent these years where a displacement occurs between $k = 0$ and $k = 1$. We discuss alternative adjustments to the sample of displaced miners in Section 3.

The New Earnings Survey is based on a 1% random sample of workers enrolled in the pay-as-you-earn (PAYE) scheme. The PAYE is a payroll-deduction system in which employers collect taxes and insurance payments from their employees' wages. The data is collected through a questionnaire that firms are required by law to complete with reference to payrolls. This system does not include the self-employed, a data limitation which is shared by many other studies in the literature such as for example Jacobson et al. (1993), Schmieder et al. (2023) and Bertheau et al. (2023).[7] These papers deal with this limitation in two distinct ways. On the one hand, periods without any observed labour earnings in the data are interpreted as zero individual earnings, such as in Schmieder et al. (2023) and Bertheau et al. (2023). Using the British Household Panel Survey and studying the costs of job loss in the UK, Upward and Wright (2017) find that reassigning earnings during periods of self-employment as zero makes very little difference to the estimated earnings losses. Bertheau et al. (2023) also find that a similar exercise using Swedish data results in only minor differences. In our specific setting, using the UK Labour Force Survey for the period 1984 to 1991, we find that only 4% of individuals who previously have been employed as miner reported to be self-employed (see column 2 in Table A2), suggesting that this is unlikely to have a large impact on our results. Nevertheless, we complement our results with a similar approach to Jacobson et al. (1993), and only keep those individuals who report positive earnings within four years of displacement. This latter approach provides a more conservative estimate of displacement costs by focusing on individuals who eventually return to work within a certain time frame.

Baseline control group: In order to estimate the causal effect of being displaced as a coal miner, in an ideal experiment we would randomly displace some miners and force them to switch sectors while allowing other miners, the control group, to continue working at a mine and enjoy similar earnings and employment dynamics as they did prior to displacement. Such an experiment, however, is impossible if the complete sector is being phased-out as was the case for the UK coal miners. Instead, we proceed as follows. Using the pool of non-coal miners in our data, we create several observationally comparable groups of workers and show that the earnings path of the about to be displaced coal miners and the earnings path of the carefully designed controls is close to identical before the treatment.

In the choice of our baseline control group, we keep the restrictions of our donor pool to a minimum and match miners to blue collar manufacturing workers with the same two-digit occupation code using propensity score matching. To match individuals from the treated to individuals in the control group we estimate a probit model in which the dependent variable, taking the value of 1 for the treated and 0 otherwise, depends on an array of observable characteristics. In particular, we model the treatment dummy variable as a function of pre-displacement individual characteristics such as age, gender,[8] hours worked, full-time status, geographical administrative unit (county), whether the wage was determined by a collective agreement, as well as the worker's pre-layoff weekly earnings,[9] overpay dummy and hours worked between $k = -4$ and $k = -1$ and employment levels between

[5] One could argue that some miners may have been able to anticipate displacement since most must have been aware of the labour disputes. This concern, however, is not specific to this particular episode. Indeed, recent work has shown that workers appear to correctly anticipate job loss in typical displacements (see Hendren (2017), Simmons (2024), Grindaker et al. (2023), Grindaker and Simmons (2024).). Moreover anticipation should reduce the earnings losses of the miners as they have time to reallocate to new industries, implying that we are estimating a lower bound.

[6] Note that the complete dissolution of the sector also allows us not to worry about mergers, takeovers, or changes in the identification number of firms.

[7] Similarly, a very small amount of low paying jobs are also not included in PAYE. This may result in understating the wage losses and overstating the employment losses after displacement.

[8] Female miners make up about 6% of the mining workers in our treatment group. The results do not change by removing all female miners.

[9] Note that in the presence of inter-industry wage differentials and, in particular, large wage premia in the extractive sector, the matching on earnings could result in less productive miners being matched to observationally similar but more productive non-miners. This would be problematic if the earnings dynamics of the matched individuals would greatly deviate post treatment from the hypothetical earnings dynamics of the miners in the absence of displacement. While we cannot exclude this possibility, we do think that it has the potential to strongly bias our results for three reasons. First, there is quite a bit of evidence that inter industry wage differentials are persistent across space and time (Goux and Maurin, 1999; Du Caju et al., 2010). A stylized fact, which has also been documented for the post-war period in the UK (Lawson, 1982). Second, exploiting the longitudinal nature of our data set, we estimate the wage premium of miners conditional on some observable characteristics and individual fixed effects and find the premium to be between 5%–10% for individuals who switch into the mining sector before 1984, which is in line with the estimates documented by, for example, Goux and Maurin (1999).

$k = -4$ and $k = -2$. We then proceed with the nearest neighbour matching according to which the displaced miners are matched to those individuals from the control group with the closest propensity score (conditional probability of receiving the treatment) without replacement.[10] Thus, we end up with $2152 \times 2 = 4304$ individuals in our panel. Note that we do not require the comparison group to be employed during any year after displacement including the displacement year, i.e. for $k > 0$. Such a requirement could lead to systematic differences between displaced and control groups, and possible overestimation of the earnings losses (Krolikowski, 2017a). We discuss alternative adjustments to the sample of control workers in Section 3.

In Table 1 we provide basic descriptive statistics for the main observable characteristics on which we match before displacement for $k = -4$. In the different columns we show mean and standard deviations for three distinct groups: the treated, the individuals from our baseline matching as well as the individuals from the non-matched control group (blue collar manufacturing workers). In comparison to the non-matched sample, we see that the displaced coal miners are special since they are more likely to be male, a bit older and have higher earnings. They also are more likely to be located in the coal rich regions and be involved in blue-collar activities such as being a craftsman. As expected, the matching reduces these differences in some dimensions.

2.2. Empirical model

Using the 17 stacked treatment and control cohorts, we run a similar regression model as other studies in the job displacement literature (Jacobson et al., 1993; Couch and Placzek, 2010; Schmieder et al., 2023). In particular, we estimate the following panel data model:

$$y_{itc} = \sum_{k=-3}^{15} \delta_k \times \mathbb{1}\{t = c+k\} \times D_i + \sum_{k=-4}^{15} \omega_k \times \mathbb{1}\{t = c+k\} + a_i + \gamma_t + \mathbf{x}'_{it}\beta + u_{itk}, \quad (1)$$

where y_{itk} is the placeholder for the outcome we are interested in, such as earnings, wages, or employment status for individual i at time t and displacement cohort c. The sum of ω_k's controls for the year relative to the calendar year in which the individual has been displaced. a_i and γ_t are person and year fixed effects, respectively. $\mathbf{x}_{it}$ is a vector of observables, in particular the age of the worker. u_{itc} is the error term which we cluster the standard error on the matched pair level as recommended by Abadie and Spiess (2022).[11]

D_i indicates displaced miners with a 1 and the coefficients of interest are the estimates of δ_k. We omit δ_{-4} as well as one of the year dummies such that δ_k captures how the different the outcomes of the displaced and the matched non-treated individuals develop k periods after displacement relative to $t = c - 4$. We normalize the continuous outcome variables (such as earnings and wages) by dividing them by the individual's pre-displacement average, i.e. for $k < 0$. Thus, we can interpret δ_k as the percentage change relative to pre-displacement values. Similar to previous work, the identification assumption is that, conditional on the control variables, the evolution of the displaced miners' outcomes would have followed a similar path as the comparison group's (captured by γ_t), have they not been displaced.

Thus, even in the presence of some changes in the wage premium, the bias on our cumulative estimates is unlikely to be large. Third, in one of our robustness exercises, we assemble the control group from individuals employed in primary metal manufacturing, an industry which is also known for wage premia which are comparable in magnitude to the wage premia in mining (Lawson, 1982; Goux and Maurin, 1999).

[10] We use the psmatch2 function within STATA to perform the propensity score matching (Leuven, 2003).

[11] Alternatively, we cluster the standard errors on the regional level or bootstrap the standard errors and document the largely unchanged results in Figure A1.

Table 1
Observables of the treated group, matched control group and non-matched control group 4 periods before displacement.

	(1)	(2)	(3)
	Treated	Main control	No matching
Weekly earnings (£)	321.86	317.12	252.50
	(160.12)	(207.92)	(190.60)
Employment	.921	.907	.806
	(.270)	(.291)	(.395)
Age	39.35	39.26	38.11
	(8.07)	(7.91)	(7.67)
Full-Time Emp.	0.98	0.97	0.95
	(0.10)	(0.15)	(0.23)
Weekly hours	37.21	37.88	37.63
	(2.20)	(3.82)	(4.66)
Male	.947	.913	.764
	(.225)	(.288)	(.425)
Collective Agreement	0.99	0.98	0.95
	(0.03)	(0.11)	(0.22)
Overtime pay	25.10	24.89	14.82
	(43.45)	(41.48)	(31.87)
Occupation in %			
Managers	8	11	13
Professionals	5	9	7
Technicians	2	7	5
Clerks	19	28	10
Crafts	48	25	3
Other Jointly	18	20	62
Region in England and Wales in %			
North England	18	18	13
Yorkshire	25	25	7
East Midlands	25	25	5
West Midlands	10	10	10
Wales	10	10	3
South East	2	2	2
Other Jointly	10	10	60
N	2,152	2,152	379,904

Note: Standard deviations are shown in parentheses. Weekly earnings are in 2000 prices.

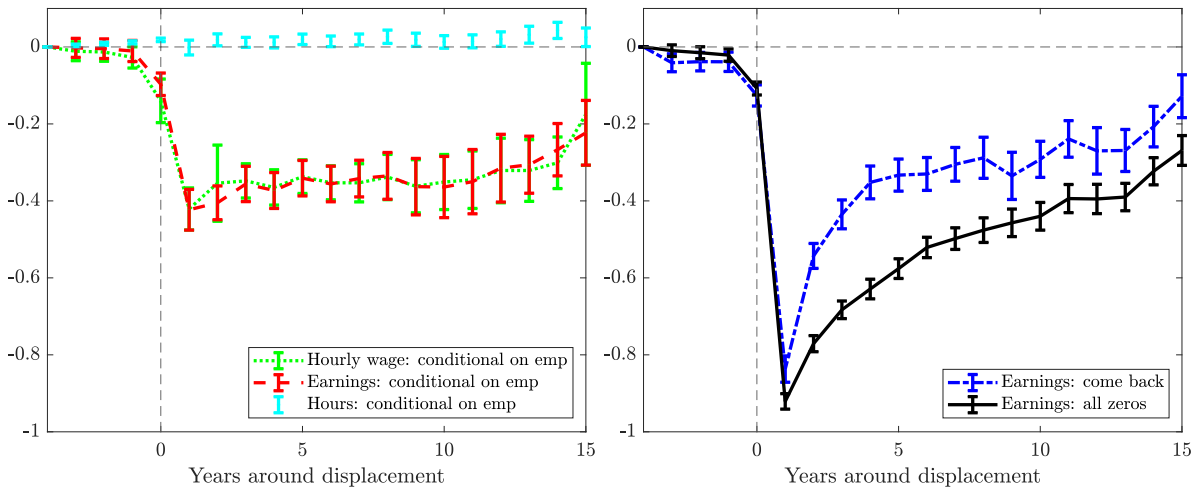
3. Results

Fig. 2 shows our main results. Using the baseline control group we document regression results for specifications in which the dependent variable in (1) takes on hourly wages; weekly earnings conditional on employment; weekly earnings unconditional on employment (including zeros), with only those miners who have positive earnings within four years after job loss, as in Jacobson et al. (1993); and weekly earnings unconditional on employment (including all zeros), as in Davis and von Wachter (2012), Schmieder et al. (2023), Bertheau et al. (2023). Here, we only focus on earnings conditional and not conditional on employment, while our estimates on employment are represented graphically in Fig. 4(c), confirming the negative and devastating impact on the probability of being unemployed after being laid-off.

Our estimates show devastating losses for the miners. In the year following job loss, the miners earnings conditional on employment fall by 40% and remain depressed by 20% fifteen years later. These losses are not driven by a reduction in hours worked since the losses in hourly wages (dotted line) and weekly earnings conditional on employment (dashed line) are close to identical.

Unconditional earning losses (solid line) are 90% in the first year, and remain 30% below the earnings of the control group fifteen years later. Even if we focus on the more conservative earnings losses, where we only include individuals who return to work within four years (dashed-dotted line), the losses are still substantial, at 80% in the first year and 10%–20% fifteen years later.

In the top panel of Fig. 3 (and also reported in Table A1) we use our baseline specification to estimate the associated losses in present



(a) Wages and earnings conditional on employment (b) Earnings including zeros

Fig. 2. The estimated proportional changes in wages and earnings following job loss.
Note: "Earnings: come back" refers to the treatment group where we only include those who have positive earnings at some point four years after job loss, while replacing their earnings with a zero if the miner is not observed for any $k > 0$. "Earnings: all zeros" refers to the treatment in which we replace the earning of any miners with a zero if the miner is not observed for any $k > 0$, without any restrictions. Standard errors are clustered at the matched pair level following Abadie and Spiess (2022).

discounted value (PDV).[12] We use a 5% discount rate to remain comparable to Davis and von Wachter (2012). We estimate present discounted losses in: earnings conditional on employment, earnings conditional on having positive earnings within four years of displacement, and earnings losses including all zeros. Across these specifications our estimates range from 3.8 to 6 years of pre-displacement earnings. 6 years of pre-displacement earnings represent our upper bound estimate and is equivalent to around $140,000 or £100,000 in 2000 prices. Alternatively, the losses can be represented as the present discounted earnings of the displaced miners relative to the control.[13] For our upper bound, including all miners who have been displaced, and assuming that they have zero earnings if they do not reappear in our data set, the miners present discounted earnings were 40% of the counterfactual. By construction, losses in wages were particularly important for those who return to work. However, even if we include all individuals, 60% of the losses are still explained by the drop in wages while 40% of the losses are explained by a drop in employment.[14]

In the bottom panel of Fig. 3 we explore some of the heterogeneity in the data by focusing on gender, age, geography, type of occupation and period of displacement. Our results indicate that males and miners who were above 40 when displaced are slightly worse off, albeit these differences are not statistically significant. Similarly, we find that miners laid off in Yorkshire, the North of England and the South of Wales did a bit worse than miners who were laid off in the Midlands and in the South-East — the economically more developed regions closer to London. We also find that Managers and Technicians employed in the coal mines outperformed the craftsmen — presumably because the skills of the craftsmen are more difficult to reemploy in the other occupations and industries as discussed above. Finally, we provide evidence on the heterogeneous effects of being displaced at different points in time. The results suggest that miners which have been displaced during the early period of the phase out ended up having the highest economic losses.

3.1. Robustness

We complement our baseline results by choosing alternative control groups. This is important since it may be the case that there exist unobserved and time varying differences between the miners and our baseline control group which may affect our estimates of the earnings losses. To gauge the importance of unobserved heterogeneity, we restrict the pool of workers from which we match to workers who are involved in the creation of primary metal products consisting of iron, aluminium and copper as well as their alloys.[15] Primary metal manufacturing plants traditionally employ male manual workers with sector-specific skills which are difficult to reemploy anywhere else. Workers in this industry are also known to benefit from wage premia (Goux and Maurin, 1999) and due to the nature of their activity are often operating in spatially remote locations. To produce primary metal products, heat from a variety of fuels, most commonly coal, is used for smelting such that metal production plants are often located next to coal deposits to keep transportation costs of coal at a minimum (Michielsen, 2013). Since this control group is chosen from a subset of blue collar manufacturing workers, we are leaning less heavily on the propensity score matching routine relative to the baseline. The advantage to the approach is that this control group may be better suited to capture the dynamics of unobserved characteristics of the displaced miners, such as long-run trends in technological progress and structural transformation as well as changing wage premia dynamics. Alternatively, we aim for a more parsimonious choice of the control group by leaving the pool of all manufacturing workers unrestricted and only match individuals on age and pre-displacement earnings.

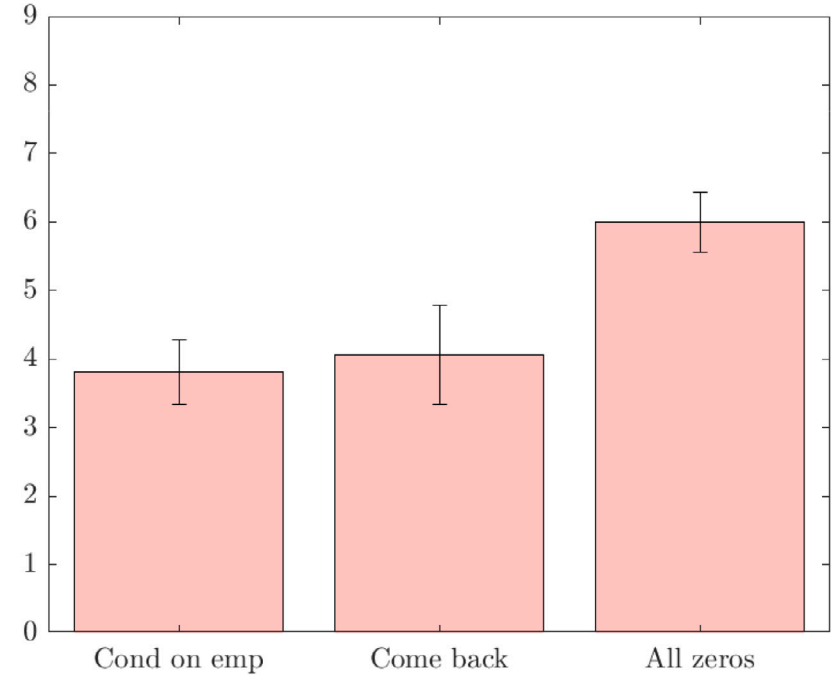
Instead of changing our control group, we also alter the definitions of our treated group in two ways. First, we only consider those displacements as treated which coincide with mine closure at the county level. This approach is more comparable to strategies employed in the literature on mass-layoffs. Alternatively, we restrict the treated group to miners working underground in the year prior to displacement. The variety of results emerging from changes in the control as well as the treated group are presented in the top panel of Fig. 4 on top of our baseline. Overall, the results clearly indicate that our baseline

[12] The present discounted value of the displacement costs is equal to $\sum_{k=1}^{15} \frac{\hat{\delta}_k}{(1.05)^{k-1}}$.

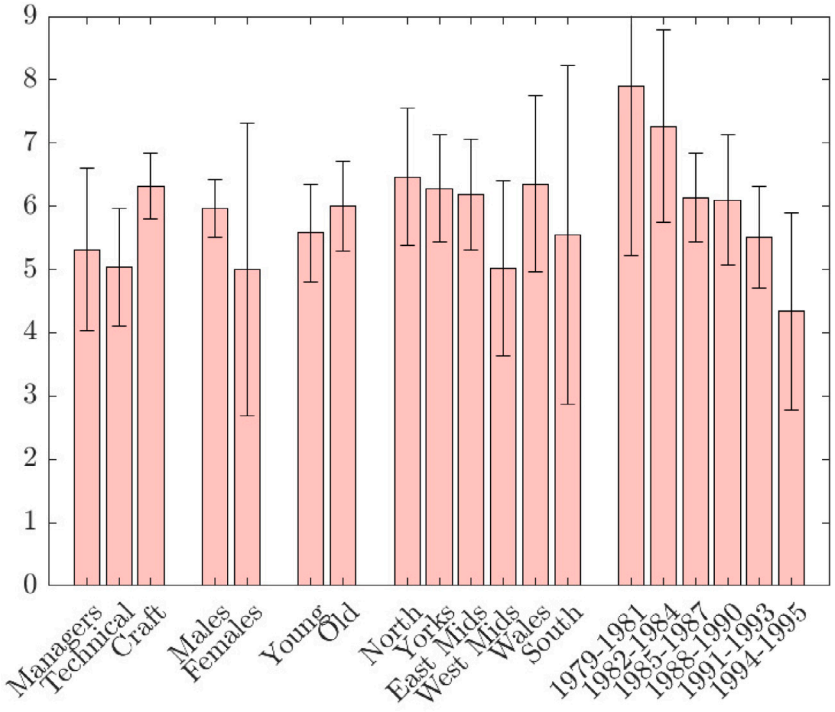
[13] The present discounted earnings for the miners relative to the control is equal to $\sum_{k=1}^{15} \frac{\hat{y}_k^T}{1.05^{k-1}} / \sum_{k=1}^{15} \frac{\hat{y}_k^C}{1.05^{k-1}}$, where $\hat{y}^T$ and $\hat{y}^C$ indicate the estimated earnings from (1) for the treated and control groups, respectively.

[14] See Table A1 for the decomposition results and Appendix B for the decomposition exercise.

[15] The associated standard industrial classification codes that we chose are 311, 312, 313, 321, 322, 333 (before 1984), 2210, 2220, 2234, 2235, 2245, 2246, 2247 (after 1984).



(a) Average



(b) Heterogeneity

Fig. 3. The present discounted value earnings losses as a multiple of pre-displacement earnings on average and across heterogeneous groups.
Note: Present discounted earnings losses relative to our baseline counterfactual, with 95% confidence bounds, and the earnings being standardized by the average individual pre-displacement earnings from period $t-1$ to $t-4$ and displacement taking place between t and $t+1$: $\sum_{k=1}^{15} \frac{\delta_k}{1.05^{k-1}}$.

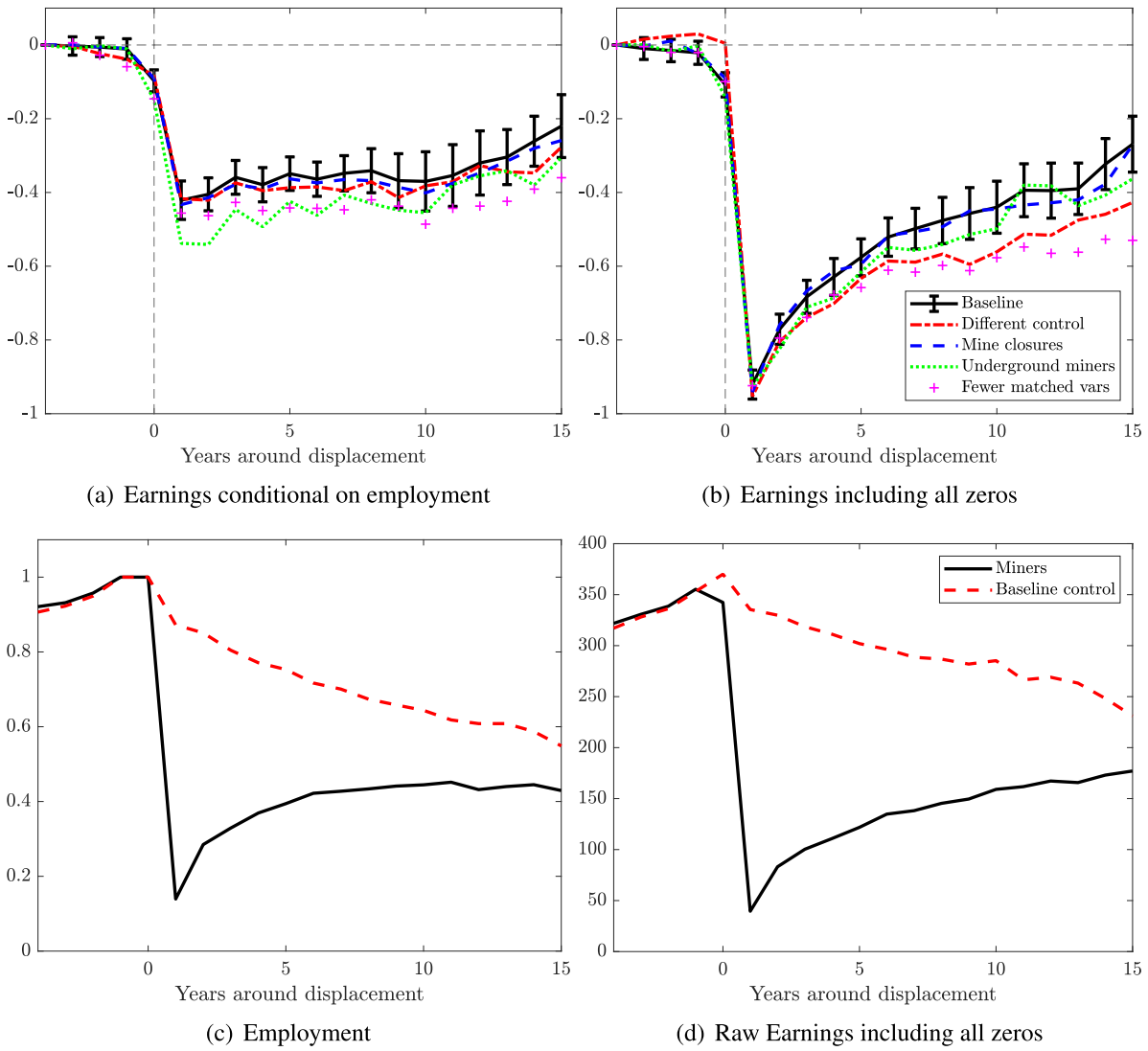


Fig. 4. The estimated proportional changes in earnings following job loss including all zeros over different treatments and controls.
Note for top panel: "Baseline" shows the estimated costs as well as the 95% confidence interval of job loss using the baseline control group described in Section 2. "Different control" shows the estimated costs of job loss in comparison to a more restricted control group, focusing blue collar manufacturing workers who are involved in the creation of primary metal products consisting of iron, aluminium and copper as well as their alloys. "Mine closure" reports the costs of job loss using the baseline control group where we only include those miners who experienced job loss when a mine also closed in the county. "Underground miners" reports the costs of job loss using the baseline control group, where we only include those miners who were working underground. Finally, "Fewer matched vars" reports the costs of job loss where we only match the miners to a control group using the variables age and pre-displacement earnings.
Note for the bottom panel: Raw earnings and employment for the miners and baseline control group. Figure in the bottom left shows weekly earnings in GBP in 2000 prices.

result represents a lower bound of the economic costs imposed on the displaced coal miners during the phasing out of coal. All the changes to the control as well as the treated group suggest a larger, at times statistically significant, economic costs carried by the displaced miners. In the bottom panel of Fig. 4 we present the average earning of the miners in the periods before and after being laid-off using the black solid line. We also plot our baseline counterfactual using the red dashed line. Note that the dramatic collapse in earning and employment just after being laid off and the slow and limited recovery mirrors our dramatic estimate and makes it graphically apparent why our results are not very sensitive to different controls.

3.2. Comparison to the literature

How do our estimates compare to the previous literature on mass layoffs? (Couch and Placzek, 2010) reconsider the earnings losses estimated in the seminal work of Jacobson et al. (1993), and also review the literature on mass layoffs. The initial loss tends to be in the range of 25 to 50%, with the largest fall in earnings that they report in the first year after displacement being 66%. In a more recent study, Huckfeldt (2022) finds that workers laid-off during a recession and who switched occupation experienced a first year reduction of 42% in earnings and that the relative losses remain around 10%–15% a decade later. This is in line with Lachowska et al. (2020) who document first year losses of 45%–49% that decline to 15%–25% after 5 years. Upward and Wright (2017) use data from the British Household Panel Survey and also find the losses to be below 50% in the year after separation. We, on the other hand, find the initial earning losses to be between 80 and 90%, with the initial losses being around 40% only for the sample of those miners who manage to find another job, abstracting from those miners who fail to find alternative employment. Davis and von Wachter (2012) find for the US that the cumulative losses over a period of 20 years are close to 1.7 times the displaced workers' pre-displacement earnings, while we find that the miners' losses add up to between 4 and 6 times the pre-displacement earning for a shorter period of 15 years.

3.3. Policy environment and other displacement costs

Our estimates show higher impacts on earnings and wages for displaced miners than previously documented. However, these estimates do not cover all associated costs. Those finding new jobs face transition challenges and relocation costs. Others leaving the labour force rely on severance packages, social transfers, or early retirement pensions. Drawing from the NES, the UK Labour Force Survey (see Table A2), and relevant literature, we discuss various costs and benefits of displaced miners.

Recall that miners facing layoff had weekly earnings of £350 (see Fig. 4(d)). After the great strike, a new 25% income tax rate was introduced, affecting nearly 95% of taxpayers (Adam and Frayne, 2000). Consequently, miners' average net weekly income was £260. This figure also applies to our chosen counterfactual, as we match on pre-displacement earnings, indicating a 40% decrease in net earnings post-displacement for those reemployed, compared to the counterfactual.

On top of the drop in net income, reemployed miners faced costly transitions to new industries, with 32% entering light and heavy manufacturing, and 15% staying in the primary sector. Other destinations included Construction (12%), Distribution, Restaurants, and Repair (10%), while Public administration, Transportation, Communication, Finance, and Real Estate each absorbed 6%–7% of reemployed miners. These shifts likely incurred increased transportation or relocation expenses (Beatty and Fothergill, 1996). Our data indicates that 38%–47% of reemployed miners moved to a different region, implying the 40% earnings drop post-displacement may underestimate the total displacement costs (Duan et al., 2022).

Displaced miners actively seeking employment were eligible for unemployment benefits, averaging 20%–30% of pre-displacement earnings for up to 12 months in 1980s UK (Glyn, 1992; Casey, 1992; Rutherford, 2013). With severance packages ranging from £20,000 to £30,000 (Glyn, 1992; Turnbull and Wass, 1997), they could additionally sustain a weekly income of approximately £160 (60% of pre-displacement earnings) for about three years. Within this period, roughly one-third found new jobs (Fig. 4(c)), consistent with Fieldhouse and Hollywood (1999) reporting a 30% employment rate among identified miners in the 1991 Census versus 50% for non-miners, and the UK Labour Force Survey indicating that 24% of previously employed miners were employees one year after displacement (see column 1 in Table A2).

Over 50% of displaced miners remained unemployed, opting for early retirement or registering as permanently sick, termed "hidden unemployment" (Beatty and Fothergill, 1996; Fieldhouse and Hollywood, 1999; Hollywood, 2002; Aragon et al., 2018). Those registered as permanently sick received around 30% of pre-displacement earnings (Rutherford, 2013), while average non-mining retirees received 55%–65% of pre-displacement earnings (Smith, 2006), with early retirement eligibility at 55 for men and 50 for women. Thus, taking together, miners, on average, received 60% of their pre-displacement earnings through unemployment benefits, early retirement transfers, and severance payments (Glyn, 1988).

Policies aimed to reduce costs of displacement and facilitate industry transitions through reeducation and local job creation near closed mines (Beatty et al., 2007; Aragon et al., 2018). Labour market analysis by Beatty and Fothergill (1996) estimated that between 1981 and 1991 in England and Wales coalfields, the increase in male employment outside coal offset about a quarter of lost coal industry jobs, which is consistent with our findings. Roughly one-fourth of displaced miners (60% of reemployed) found new jobs within the same county, while others relocated (40% of reemployed).

Despite policymakers advocating self-employment, only around 4% pursued it (see column 2 in Table A2). Policies also fell short in job creation, consistent with qualitative studies reflecting miners' pessimism (Warwick and Littlejohn, 1992). Transferring skills to new industries proved challenging, despite a focus on reeducation. However, minimal enrollment in full-time education or training schemes was reported (Fieldhouse and Hollywood, 1999; Guy, 1994), consistent with our findings indicating low interest in reeducation (bottom row, Table A2). While some success stories exist, the fact that only 30%–40% of displaced miners found jobs, mostly within the first two years, suggests limited success in attracting miners to reeducation programs and matching them to new jobs.

4. Final remarks

We examine the impact of job displacement on coal miners' earnings. Our analysis exploits individual panel data from the UK and the dramatic collapse of the coal industry that accelerated in mid-1980s. We find evidence of a substantial reduction in earnings of displaced miners that persists in the long-term. As far as we know, our estimates are the largest in the literature.

While specific to the UK context, these findings suggest that the phase out of the coal industry, a policy which has been repeatedly proposed as one way to reduce carbon emissions, could impose large costs on coal miners, their families and mining communities that may persist in the long term. The external validity of our results may hinge on location, job and worker characteristics at existing coal mines. Overall, the phasing out of coal around the world will displace more than 5 million workers, most of them in China (around 3 million). Just as in the case of the UK in the 1980s, coal mining remains an important employer of male workers with low educational attainment in many remote locations of China, the United States, Poland, India, Indonesia, Russia and South Africa (Ruppert Bulmer et al., 2021). In these remote locations the capacity of the local labour markets to absorb the displaced miners is limited. All this suggests that the UK experience, given the characteristics of workers and communities in coal mining areas and the speed with which the dissolution of the coal industry took place, makes it similar to the one that coal miners around the world may experience during the upcoming energy transition.

While our analysis can identify the size of the earning losses, we are unable to pin down a possible cause. Previous work suggests that earning loses reflect the loss of occupation of specific human capital. While this could certainly be the case for coal miners, we cannot rule out other possible factors specific to coal mining in the UK such as compensating differentials (due to the higher risk of mining jobs), union wage premiums, or lower labour mobility.

Declaration of competing interest

We formally declare no conflict of interest.

Data availability

The authors do not have permission to share data.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2024.105167.

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