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E-mail: florian.egli@gess.ethz.ch**Keywords:** economic voting, political economy, climate change, fossil fuels, coal, phase-out, transitionSupplementary material for this article is available [online](#)Original content from
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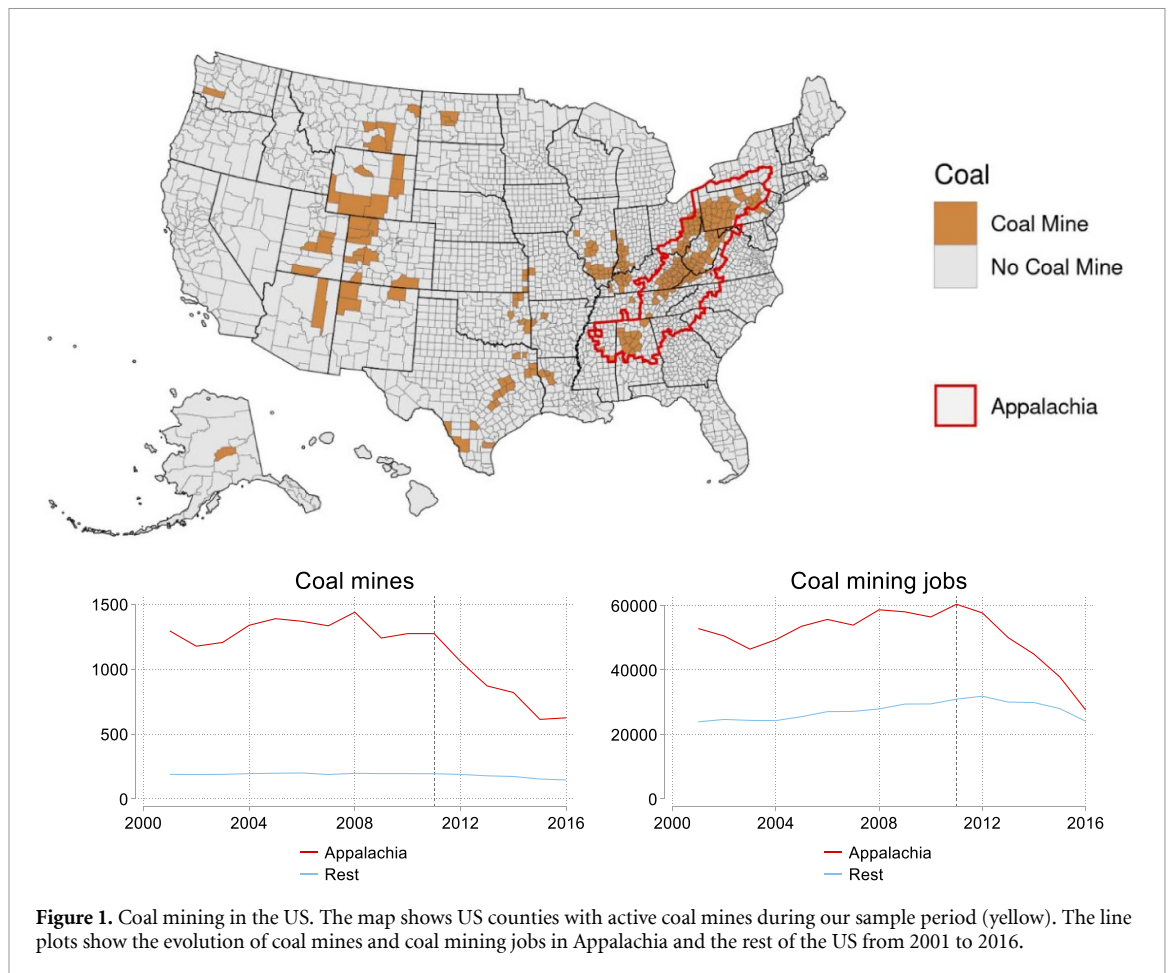
Phasing out coal is a crucial lever in reaching international climate targets. However, the resulting jobs losses might trigger voter backlash, making phase-outs politically costly. Here, we present an analysis of the electoral response to coal mining job losses in US presidential elections using matched and bordering difference-in-difference estimators. Our findings confirm that fossil fuel phase-outs can result in voter backlash. In our main specification, we find a four percentage-point (pp) increase in the Republican vote share in 2012 (range across specs. = 3.6 pp–4.5 pp), declining to 3.2 pp in 2016 (range across specs. = 3.2 pp–4.2 pp), in counties suffering from coal mining job loss. The estimated electoral response is around three times as large as the number of jobs lost. We observe this response only in places where there was significant job loss, where these jobs accounted for a large share of locally available jobs and where income levels were low. Relative party strengths do not influence the results.

1. Introduction

Phasing out coal plays a central role in virtually all scenarios that are compatible with the Paris Agreement to mitigate climate change (Edenhofer *et al* 2018, Tong *et al* 2019, Fofrich *et al* 2020). At the international climate summit 2021 in Glasgow more than 40 countries have committed to shift away from coal. Policymakers can phase out coal by imposing restrictions on its use or by increasing the cost competitiveness of alternative energy sources. In many countries, such as the United States, the United Kingdom and Germany, coal production is on a rapid decline due to a combination of these policy levers. Such transitions from old to new energy sources affect value chains and jobs (Carley *et al* 2018a, Burke *et al* 2019, Mayfield *et al* 2019) and, in turn, can provoke political backlash (Acemoglu and Robinson 2006, Dasgupta 2018). In democracies, there is evidence that voters respond at the ballot box to policies that impact their socioeconomic situation, for example in the case of job losses due to automation (Frey *et al* 2018) or trade (Margalit

2011, Autor *et al* 2020). In energy transitions, studies have shown that phasing-in new technologies affects voting behaviour (Stokes 2016). So far, however, very few qualitative studies have investigated the political effects of *phasing-out* energy technologies (Carley *et al* 2018b, Vona 2019), leaving researchers and analysts in the dark regarding the electoral risks and rewards of phase-outs.

To tackle this question, we focus on the United States, the third-largest coal producer worldwide in 2016, and analyse the effect of coal job losses on electoral outcomes in the presidential elections from 2000 to 2016. Coal mining was a particularly salient and partisan issue during the presidential campaigns of 2012 and 2016, with Republican and Democratic candidates holding completely opposing views on the issue (Brown and Sovacool 2017). Data on campaign contributions from the coal mining industry shows donations sharply increased from USD 1.3 million in the years prior to the 2004 and the 2008 elections (2001–2004 and 2005–2008) to USD 2.2 million from 2009 to 2012 and USD 3.7 million from 2013



to 2016 despite a simultaneous wave of bankruptcies as described in supplementary note S.1. These contributions helped raising the attention on the topic to the national level and they were highly partisan: 83% (USD 8.7 million) of campaign contributions from the coal mining industry between 2001 and 2016 went to the Republican Party (see figure S.1 in the supplementary material). Republicans supported coal mining and consistently framed coal as an abundant, clean, affordable and reliable energy carrier (Thurber 2019) describing the Obama administration's energy and climate policies (e.g. the Clean Power Plan of 2015) as a 'war on coal' from the 2012 election campaign onwards (Eilperin 2013). In 2016, presidential candidate Donald Trump made coal a centrepiece of his campaign (Weber 2020), promising to 'bring back coal' by cutting Obama-era regulations (Blondeel and Van de Graaf 2018). Democrats on the other hand promised to accelerate the transition away from coal towards renewable energy sources. In 2016, the party platform opposed mountaintop removal mining operations, and Democratic candidate Hillary Clinton's campaign promised a large investment plan in favour of clean energy technologies and was perceived as anti-coal in affected regions (Goode 2015). At a town hall event, she said, 'We are going to put a lot of coal miners and coal companies out of business'

(Pai and Zerriffi 2018), which she would later call her biggest regret of the campaign (Clinton 2017).

In this study, we use the distinct party positions and exploit the rapid and locally concentrated decline in US coal mining to analyse the effect of coal mining decline on electoral outcomes in presidential elections. Between 2011 and 2016, the US coal mining industry lost 43% of all coal mining jobs, amounting to almost 40 000 jobs, and 697 coal mines closed due to falling domestic coal-fired power generation, which was mainly pushed out of the market by alternative energy sources (see figure 1 and note S.1 in the supplementary material). Geographically, the US coal mining industry is divided into two regions: the Montana mountain range in the west and the Appalachian range in the east (see figure 1). Empirically, we focus our analysis on Appalachia, where 83% of the total coal mining job loss happened. Between 2011 and 2016, the region lost 54% of its coal mining jobs (32 000 of 60 000) mainly in counties located in Alabama, Kentucky, Ohio, Tennessee, Virginia and West Virginia.

We employed three analytical steps. First, we used a matched difference-in-difference (DiD) estimator to identify the effect of coal mining job loss on voting in presidential elections. We confirm the obtained result in a specification where we compared counties

with coal mining job loss to neighbouring counties instead of matching. Second, we investigated heterogeneous treatment effects by splitting the sample along treatment intensity and a set of covariates, such as income and pre-treatment vote shares. Finally, we ran a series of robustness checks, ruling out interference from turnout effects or shale gas development, among other factors. Conditioning on socio-economic controls and comparing bordering affected and unaffected counties, we find that the loss of coal mining jobs led to an increase of approximately four percentage points (pp) in the Republican vote share in 2012. This effect persists, albeit to a somewhat smaller extent, in 2016 and translates into a vote shift about three times as large as the number of jobs lost. We find that these average effects are driven by counties where a disproportionate amount of coal mining jobs were lost, where these jobs constituted a higher share of total available jobs and where income levels were low.

2. Data and empirical methods

We used existing, partly proprietary data to prepare an original panel of presidential voting, coal and socio-economic data on the county level. The panel covers all Appalachian counties ($N = 420$) and spans five presidential elections from 2000 to 2016 (see table S.1 for details on all variables). Control variables were selected based on recent political science literature on voting behaviour (e.g. Stokes 2016, see also note S.2 in the supplementary material). Our dependent variable is the Republican vote share at county level⁴ and our treatment variable is a binary indicator of job loss between 2011 and 2016. We chose the simple binary difference specification from the peak (2011) to the most recent year (2016) for two reasons. First, it is the most straightforward to interpret. If a county experienced coal mining job loss with some up and downs in between, we would still expect a political reaction. Second, it considers anticipation effects. For example, a county where the onset of the job loss is after 2011 is still in the treated group for 2012 and 2016. Realistically, voters know of the existence and state of the local coal mining industry and they anticipate job losses even if the trend started in other counties nearby. By contrast, it is reasonable to assume that voters do not anticipate a decline in coal mining before 2011, that is during a period where jobs in the industry were growing moderately in Appalachia and the rest of the US too (see figure 1). In the robustness section, we demonstrate that a continuous formulation with coal mining job loss in the year prior to the election or

the years between the previous election and the current one as the treatment indicator yields very similar results.

Our basic setup estimates the effect of coal mining job loss on presidential voting outcomes by comparing affected (i.e. treated) counties to the most similar control counties (1:1 matching) or unaffected bordering counties (i.e. control) according to equation (1)⁵:

$$\text{GOP}_{it} = \alpha T_i + \lambda E_t + \delta(T_i \times E_t) + X_{it}\phi + \gamma_i + \varepsilon_{it}. \quad (1)$$

GOP is the Republican vote share in county i and election year t , T is the binary treatment, E is the election year (λ represents election year fixed effects, which absorb time-varying factors common to all counties, such as price changes or voting trends), the vector X represents the socio-economic controls (see below), γ_i represents county fixed effects, which control for pre-existing and time-invariant differences between countries beyond what is captured by the controls (e.g. structural differences in the local economy or topography) and ε_{it} is the error term. The coefficient of interest is δ estimated for each presidential election, hence, we deliberately allow for the treatment effect to vary by election. To analyse the heterogeneous treatment effect, we split the sample along the median of the variable of interest (φ) and estimate two equations (equation (2)) separately

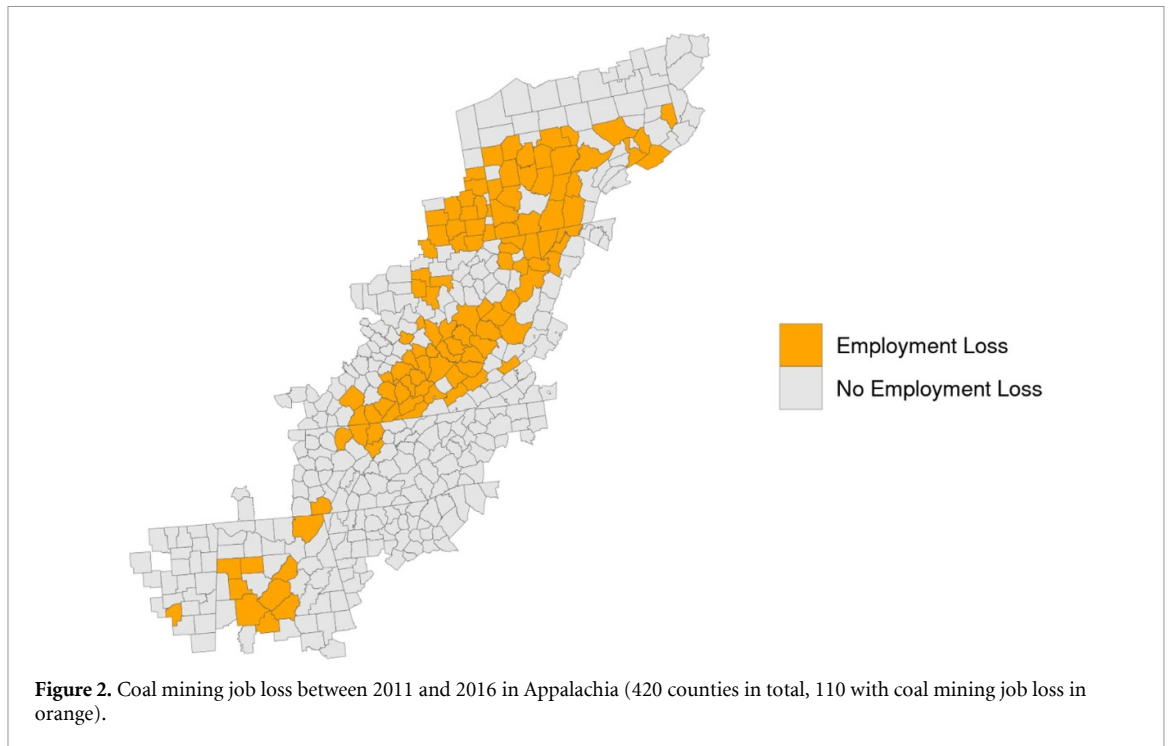
$$\begin{aligned} \text{GOP}_{it} &= \alpha T_i + \lambda E_t + \delta_{\text{low}}(T_i \times E_t) + X_{it}\phi + \gamma_i \\ &\quad + \varepsilon_{it} \text{ if } \varphi \leq p50 \\ \text{GOP}_{it} &= \alpha T_i + \lambda E_t + \delta_{\text{high}}(T_i \times E_t) + X_{it}\phi + \gamma_i \\ &\quad + \varepsilon_{it} \text{ if } \varphi > p50. \end{aligned} \quad (2)$$

We restricted the analysis to Appalachia for two reasons. First, as shown in figure 1, most coal mining jobs were lost in Appalachia. Second, the effect of unobserved confounding factors is reduced within Appalachia because it is a culturally, socially and economically homogenous region (Scott 2010, Carley *et al* 2018b). The map in figure 2 shows Appalachia and the distribution of counties with coal mining job loss between 2011 and 2016; 142 of the 420 counties reported an active coal mine at any given time between 2001 and 2016, and 110 of those lost coal mining jobs between 2011 and 2016 and, therefore, constitute our set of treated units.

In the first specification (matching), we implement a 1:1 propensity score matching without replacement on the socio-economic control variables

⁴ A concern may be that third-party vote shares are unevenly spread between coal and non-coal counties, introducing a bias into our outcome measure. Third-party vote shares moved over time but did not differ between the compared groups in our study (see supplementary figure S.2).

⁵ Note that the local incidence of job loss was driven by each mine's marginal production cost, which is primarily a function of locally varying geological factors, such as thickness of coal seam, type and nature of mineralization, and surface topography (Culver and Hong 2016, Coglianesse *et al* 2020, Watson *et al* 2022). Local county-level policies are, therefore, unlikely to confound our estimators; nonetheless, we checked for differences in effect sizes dependent on prevailing party strength (i.e. Republican vote shares).



and pre-treatment outcomes in 2000 and 2008 to select the 110 most similar control counties within Appalachia⁶. This means that each treated unit is matched to one control unit and that each control unit can only be used once. For the second specification (bordering), the condition for control units is sharing a direct border with a treated unit. This condition results in 120 control units, which we compared to 96 treated units, excluding treated units that share their borders exclusively with other treated units. Control variables, identical for the matching and in the regression, are selected based on political science literature as discussed in supplementary note S.2.

The key identification assumption of our approach is that we would have observed the same average change in vote shares among all units, conditional on controls, if there had been no coal mining job loss. We report parallel pre-treatment trends between treated and control units in figures S.3 and S.4 in the supplementary material⁷. We also ran two placebo tests, from which we expect zero effects to confirm the robustness of the results. Finally, we analysed whether changes in voter turnout or local shale gas development affect our results. For the former, we

replaced our dependent variable with voter turnout to make sure that turnout does not differ because of the treatment. If it differed between treated and control units, the measured effect of treatment on GOP shares could in fact be via turnout, something we could not rule out by including turnout as a control. For the latter, we used data from Mayfield *et al* (2019) and first interacted the presence of a shale field in the county (10% of the area is in the Utica and/or Marcellus shale formations) and the presence of an active well (producing or spud) with our treatment in two specifications⁸. As such, we measured whether the effect of coal mining job loss differed depending on the local prevalence of shale. Second, we used the employment estimates from Mayfield *et al* (2019), i.e. 4 job-years per producing well and 16 job-years per spud well, to estimate the local prevalence of shale gas jobs. Importantly, these estimates include indirect jobs and therefore likely overestimate an effect—if there is any—particularly in comparison to the coal mining job specification where the data allows us to look at direct jobs only. We applied robust standard errors (Huber–White Sandwich estimator) throughout the analysis to relax the assumption of independent observations and allow the variance of residuals to differ between counties⁹.

⁶ We used `psmatch2` in Stata/SE 16.0 (StataCorp LLC, College Station, TX, USA). Education is only included in the 2000 matching due to data availability constraints (see supplementary table S.1).

⁷ Parallel trends continue backwards even further until at least 1992 covering the elections of Democratic (President Bill Clinton in 1992 and 1996) as well as Republican (President George W Bush in 2000 and 2004) presidents. The structural break takes place during the Presidency of Barack Obama between 2008 and 2012.

⁸ We introduce a binary variable taking the value of 1 if there was at least one active producing or spud well in the election year and 0 otherwise. We consider both producing and spud because both create employment.

⁹ I.e. relaxing the assumption of county-year observation independence.

Table 1. Main results.

Variables	(1) GOP	(2) GOP	(3) GOP	(4) GOP	(5) GOP	(6) GOP
2004 effect	0.615 (0.475)	0.472 (0.512)	0.408 (0.529)	0.535 (0.445)	0.400 (0.489)	0.373 (0.503)
2008 effect	1.329 (0.976)	1.444 (0.968)	1.388 (0.974)	1.450 (0.918)	1.593* (0.925)	1.527 (0.936)
2012 effect	4.289*** (1.219)	4.512*** (1.210)	4.380*** (1.214)	3.855*** (1.101)	3.799*** (1.088)	3.635*** (1.105)
2016 effect	4.228*** (1.351)	3.679*** (1.250)	3.226*** (1.219)	4.185*** (1.278)	3.745*** (1.203)	3.260*** (1.161)
Coal mining job loss	-2.894** (1.336)	-2.861** (1.360)		-3.743*** (1.274)	-3.592*** (1.326)	
Income		-0.000 379*** (9.8 × 10 ⁻⁵)	-0.000 476*** (0.000 118)		-0.000 192** (8.65 × 10 ⁻⁵)	-0.000 293*** (0.000 111)
Population density		-0.0144*** (0.005 23)	-0.0842** (0.0382)		-0.0169*** (0.00530)	-0.102*** (0.0304)
Unemployment		72.21*** (14.57)	92.81*** (15.41)		53.31*** (14.82)	80.71*** (14.75)
Share of white people		6.539 (5.813)	5.562 (6.338)		14.61** (6.302)	6.270 (5.817)
Share of manuf. jobs		13.22*** (5.029)	6.793 (6.622)		7.526* (3.979)	2.405 (4.447)
Constant	55.85*** (0.907)	56.39*** (6.565)	66.72*** (7.773)	57.41*** (0.794)	47.23*** (6.878)	65.86*** (6.912)
Specification	Matching	Matching	Matching	Bordering	Bordering	Bordering
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
County fixed effects	No	No	Yes	No	No	Yes
Observations	1100	1100	1100	1080	1080	1080
R-squared	0.653	0.685	0.691	0.643	0.664	0.676
Number of FIPS	220	220	220	216	216	216

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3. Results

Table 1 shows the results from our two main empirical strategies, matched DiD and bordering DiD. The matching successfully eliminated differences between treated and control units along covariates, with the exception of the 2000 Republican vote share, where the matching only reduced the difference¹⁰. Prior to the matching, income, educational attainment, manufacturing industry shares and Republican vote shares were significantly lower in treated counties as compared to control counties, whereas the share of whites in the population was significantly higher (see supplementary table S.2). In both the matched and the bordering sample setup, the treated and control counties followed similar pre-treatment voting

trends from 2000 (George W Bush's first election) to 2008 (Barack Obama's first election). These trends changed abruptly in 2012, and the change persisted in the subsequent election (see supplementary figures S.3 and S.4). After the matching, a slight difference in GOP vote shares between treated and control counties remains¹¹. We therefore report results along a set of specifications, including the bordering setup and discuss results across specifications. Specifications (1), (2) and (3) in table 1 show the results for the sample matched on controls and GOP levels in 2000 and 2008; specifications (4), (5) and (6) show the results for the setting where counties with coal mining job loss were compared to bordering counties without job loss. For both strategies,

¹⁰ Note that we still included the covariates as controls in the DiD regressions.

¹¹ Difference in GOP vote shares between treated and matched control counties is 3% in 2000 and 2% in 2008 (see supplementary table S.2).

we report results without controls, with controls and with controls and county fixed effects (all specifications include year fixed effects). Controls show the expected signs with higher income and population density (i.e. more urban) counties showing lower GOP vote shares and counties with higher unemployment, higher white shares and higher manufacturing showing higher GOP shares (although the latter two being insignificant in most specifications). In the supplementary table S.9, we further show that including gender ratio, age structure, job losses in coal-fired power plants (see supplementary note S.1) and voter turnout increases the estimated effect sizes slightly, strengthening the reported main results.

Across all specifications, the 2012 and 2016 effects are positive and highly significant ($p < 0.01$), whereas the effects before the onset of coal mining job losses in 2011 (2004, 2008) are close to zero and statistically insignificant. This confirms the expectation that voters in counties with coal mining job losses from 2011 to 2016 did not anticipate the industry decline in 2008 where the financial crisis was the main topic of the election. Effect sizes are relatively stable across specifications, with a tendency towards a smaller 2016 effect compared to that of 2012 when including controls and county fixed effects in specifications (3) and (6). In our main specification with full controls and fixed effects, we estimate the effect of coal mining job loss on Republican vote shares to be approximately 4 pp in 2012 and 3.2 pp in 2016 (average between matched and bordering specifications). These effect sizes are substantial when expressed in average coal mining job losses in the treated counties (see supplementary figure S.5 for the distribution of coal job losses). Job losses ranged from 1 to 3218 per county, with an average of 310 across treated counties; using the effect sizes quoted above, the loss of 100 coal mining jobs led to a 1.3 pp increase in the Republican vote share in 2012 and a 1 pp increase in 2016. We can also express the effect in terms of mine closures; after 2011, on average, 5.9 coal mines closed in each treated county. Hence, for each coal mine closure between 2011 and 2016, we find a 0.7 pp increase in the Republican vote share in 2012 and a 0.5 pp increase in 2016. These effect sizes suggest that not only affected coal miners voted differently. In 2016, voters in treated counties cast 3567 851 votes; hence, using the effect size of 3.2 pp on average across all cast votes yields a vote shift of around 114 000 votes, which is more than three times the total number of coal mining jobs lost in treated counties (34 071). In other words, the electoral response goes far beyond the direct job losses.

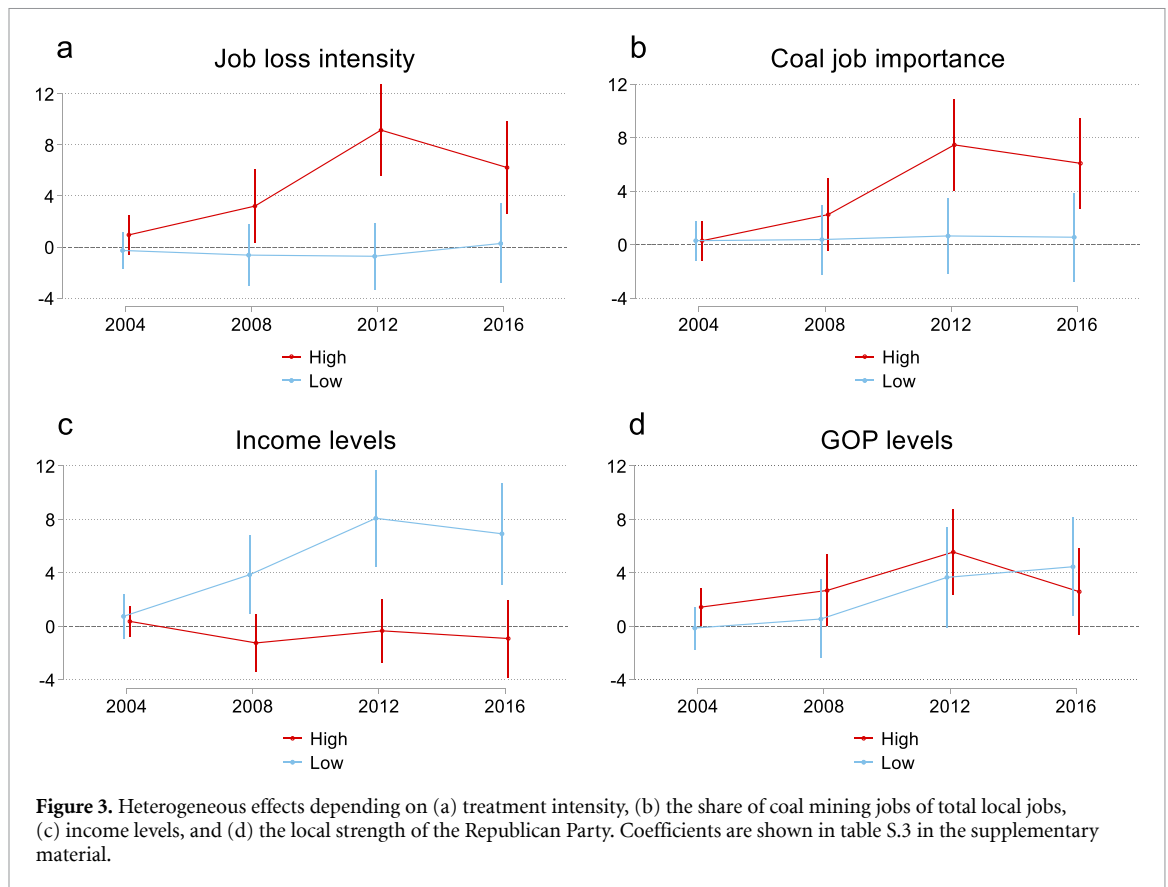
4. Heterogeneity

Next, we turn to heterogeneous treatment effects. First, we separated the sample into low and high

treatment intensity to account for the uneven distribution of coal mining job loss. Counties with low job loss intensity lost from 1 to 95 coal jobs, while high job loss intensity indicates more than 95 coal jobs lost (maximum = 3218). Panel A of figure 3 shows that only counties with high job loss intensities increased their vote shares for the Republican candidate in 2012 and 2016. We observe an increase of roughly 9 pp in Republican vote shares in 2012, decreasing to roughly 6 pp in 2016. Residents of counties with low job loss, however, did not vote statistically differently from residents of the control counties.

Second, we investigated three moderating factors: the availability of employment alternatives, income levels and local party strength. The political science literature suggests that lower possibility of re-employment can explain higher electoral responsiveness to job losses (Margalit 2011). Hence, the effects of job decline on voting may be more pronounced in less-diversified labour markets that historically specialised (Autor *et al* 2020), or labour markets with higher shares of low-skilled (and low-income) manufacturing workers (Jensen *et al* 2017). This suggests that voting effects may be moderated by income levels. Finally, the effects of job loss on vote choice may be moderated by the local strength of political parties, for example due to differing capacity to mobilize voters (Huckfeldt and Sprague 1992). We report results along the three moderating factors, each split at the median (see section 2). First, we see that the reaction to coal job losses only played out where these jobs constituted an important share of the total jobs available in that county. Panel B of figure 3 shows that counties where alternatives existed (i.e. the coal job share compared to total jobs was below the median) did not change their voting behaviour in response to a local loss of coal mining jobs. Counties where these jobs constituted an important share of total jobs and alternatives were scarcer reacted very strongly, with an increase of roughly 7.5 pp in Republican vote shares in 2012, declining to 6 pp in 2016¹². Similarly, we see different reactions depending on income level, as shown in panel C of figure 3. While high-income counties did not vote statistically differently after a local decline in coal mining jobs, low-income counties responded strongly, with an 8.5 pp increase in Republican vote shares in 2012 and a 7.5 pp increase in 2016. Finally, we do not observe different reactions depending on the prevailing party strengths, as shown in panel D of figure 3. While other studies have found such effects, for example, in the case of wildfires in

¹² Evidently, high coal mining job loss and large coal mining job shares in the local economy correlate (corr. = 0.73); 44 of the 55 counties with coal mining job loss above the median also feature coal mining job shares above the median.



California (Hazlett and Mildenerger 2020), we analysed a sample that is skewed towards Republican majorities.

5. Robustness

We proceed by investigating the robustness of these results. First, we demonstrate that the results are not driven by differences in turnout between treated and control counties. Turnout matters because a candidate promising to ‘bring back coal’ could motivate voters in counties affected by coal job losses to increasingly go to the ballot box¹³. The political science literature has shown that several factors influence turnout, including socio-economic conditions and demographic characteristics, as well as local electoral rules and mobilization efforts (Rolfe 2012, Dassonneville and Kostelka 2021). Table S.4 in the supplementary material shows that coal mining job loss had no effect on voting behaviour in the matching (1) and the bordering (2) setup, including all controls and county and year fixed effects (see also unchanged results when including voter turnout as a control in supplementary table S.9 to account for

potential omitted variables, which may influence voting via turnout).

Second, shale gas development in Appalachia during our study period is arguably another important potential confounder (see supplementary note S.3 for details). Local well development may have provided jobs that compensated for the loss of coal mining jobs. In the Appalachian basin, the number of jobs in shale gas has increased strongly since 2006, with most jobs concentrated in rural and mixed rural-urban areas (Mayfield *et al* 2019). A growing local shale gas industry may not only have compensated workers financially but also reinstated part of the identity attached to the coal industry. In specification (3) in supplementary table S.4, we test for a moderating effect of being located in the shale formation on our estimated coefficient for counties in Ohio, Pennsylvania and West Virginia where data is available (139 counties). The table shows that the interaction term between shale formation and coal mining job loss is small and statistically insignificant in all elections. Given that the shale boom may have been unevenly spread and most counties are located in the shale formation (130 of 139), we show an additional robustness check in specification (4) of supplementary table S.4 and interact the presence of at least one active producing or spud well with our treatment variable (94 of 139 counties). Both the well variable and the interaction are statistically insignificant in all elections ($p > 0.05$), however the presence of a shale

¹³ This would be unproblematic if all voters in those counties were equally more likely to vote, because we measured Republican Party vote shares by county and election; it would be problematic if only affected voters changed their behaviour and if they voted differently compared to unaffected voters in the same county.

gas well seems to have a slightly positive effect on GOP vote shares as shown in specification (5). We suggest abstaining from interpreting the estimated effect sizes of coal job loss in these specifications because the reduced sample together with the high number of interaction effects risks overfitting the regression. However, results point to the possibility that the 2012 effects were driven by shale development together with the anticipation effect. Finally, we show that neither the number of shale gas jobs per county per year, nor the year-to-year change in shale gas jobs (specifications (1) and (2) in supplementary table S.9) change the estimated effect sizes, which are more comparable to our main estimates here because there are no interaction effects.

A final issue concerns the time-invariant specification of our treatment variable (see section 2). In supplementary table S.5 we show two placebo tests, from which we expect a zero effect. First, for counties that experienced an *increase* in coal mining jobs after 2011 and second, for counties that have at least one coal mine open in at least one year of our sample period, but experienced *no decline* in coal mining jobs after 2011. In both specifications, we find zero effects in all elections, providing additional evidence that the estimated effect size is not driven by the presence of the coal industry but rather by the decline in jobs. Finally, we report two continuous specifications in supplementary tables S.6 and S.7. In supplementary table S.6 we estimate the effect of losing coal mining jobs in the year prior to an election across all elections. We use the entire sample of Appalachian counties ($N = 420$) for these specifications to capture maximum variance because job losses in singular years also occur in counties that neither experienced a net job loss from 2011 to 2016 nor were assigned to the control group¹⁴. Specifications (1) and (2) show that the estimated coefficient of coal mining job loss is around 3 pp and therefore very similar to our main DiD setup. Specifications (3) and (4) estimate a continuous effect of 100 coal mining jobs lost. For each 100 jobs lost, we estimate an increase in the Republican vote share between 1 pp and 1.4 pp, which corresponds precisely to the estimated coefficients for 2012 and 2016 in the DiD setup (see section 3). Related research has found a time-decaying effect of voting responses to natural disasters and researchers therefore typically used a 12–24 month time window before an election to define the treatment (Bechtel and Hainmueller 2011, Hazlett and Mildenberger 2020, Baccini and Leemann

2021). However, there may be reasons to believe that voter reactions to coal mining job loss last longer than reactions to disasters because job losses are perceived as permanent changes. We therefore show a replication of supplementary table S.6 in S.7 with the only change being that we specify the four years prior to an election (time between the previous and the current election) as the relevant time window for treatment, that is coal mining job loss. The results confirm our initial estimates with a slightly lower effect size for each 100 jobs lost.

6. Conclusion

Our findings show that there is a local political price for a coal phase-out: when party positions are clear, coal decline triggered by external factors, such as market prices in conjunction with national policy, can lead to vote gains for the party supporting the industry. Such localized electoral effects can be problematic because they incentivize political candidates to slow down the phase-out in fear of voter backlash. A slow-down can take place either by dismantling phase-out policies or by promoting coal support policies keeping coal in the energy mix despite its deteriorating cost competitiveness. Both actions put climate action at risk because rapidly phasing out coal is required in virtually all scenarios in line with climate targets.

While these findings have implications for the transition to cleaner energy systems worldwide, it remains an open question under what conditions voter backlash may arise. Transition plans in Australia, Germany and South Africa, for example, face fierce opposition from relatively small but politically well-organized communities of coal workers and supporters (Diluiso *et al* 2021), but an effect on voting behaviour has not yet been quantified in these countries. Further, our study cannot describe the exact mechanisms linking coal phase-out to voting behaviour. More qualitative research could investigate more thoroughly to what extent economic or cultural reasons play a role and whether a response can be triggered in a strategic political campaign (Inglehart and Norris 2016). Such research could inform policymaking aimed at avoiding negative backlash: Solutions would look different if voters were solely interested in their personal economic circumstances, compared to a scenario in which voter backlash was rooted in their identification with a ‘coal culture’. While in the former scenario, lump-sum transfers to affected workers and communities might be the best option (Carattini *et al* 2017, Jagers *et al* 2019). The latter case is more complicated and might require the built-up of an alternative industry around new technologies, such as renewables, through industrial policy (Jakob *et al* 2020, Muttitt and Kartha 2020, Bang *et al* 2022, Lu and Nemet 2022). Similarly, future

¹⁴ In supplementary table S.8 we also show results for continuous specifications on the matched sample. Effects remain highly significant and effect sizes are reduced because we are estimating the average effect across all elections but excluding counties that have incurred job losses in prior to previous elections. We show this specification for comparability with the matched results.

research should investigate in more detail how other industry developments, such as shale gas, interact with the phase-outs of high-carbon industries and influence voter reactions. Our findings suggest that the shale gas industry development did not moderate the effect of coal job loss, but it could have influenced voter reactions in the early stage of coal decline in 2012. Exploring these dynamics requires more research, in part also because the decline of coal mining is a structural trend whereas the development of shale gas and associated jobs follow a boom-and-bust cycle. Finally, our study did not examine the electoral effects of indirect job loss in other industries. Yet, examining these aspects would be worthwhile for future research. For instance, in important coal-importing countries such as China or Japan, backlash to domestic phase-out may arise from employees of power plants rather than coal miners.

In sum, our results imply that the coal phase-outs required to meet the targets of the Paris Agreement may be politically costly and may require active transition strategies, for example in the context of the proposed Green New Deal in the US or the Green Deal in the European Union.

Data availability statement

No new data were created or analysed in this study.

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Conflict of interest

The authors declare no competing interest.

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