The Long-Term Effects of Air Pollution on Health and Labor Market Outcomes: Evidence from Socialist East Germany

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What are the long-run effects of sustained exposure to air pollution? A unique natural experiment allows us to examine this question. In 1982, a sudden cut in Soviet oil forced Socialist East Germany to switch to highly polluting lignite coal. While the shock sharply increased air pollution near mining regions, authoritarian restrictions on mobility, housing, and jobs prevented sorting responses. We document persistent labor market impacts over three decades. Exposed individuals work less, earn lower wages, and retire earlier. Health is a key mechanism: infant mortality rises by 9% and the long-run incidence of asthma and cardiopathy increases significantly.

Keywords: Air pollution, labor supply, migration, place effects
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Six and a half million premature deaths each year are attributed to air pollution exposure (World Health Organization 2024). A large literature documents its adverse health effects, a central component of human capital.[1] Yet, much less is known about its consequences for labor market outcomes, particularly the long-run effects of sustained exposure. Evidence is notoriously scarce because tracking individual outcomes over a long period of time is challenging, and because endogenous responses to pollution like selective migration complicate causal inference in most settings (Deryugina and Reif 2023).

In this paper, we use a natural experiment in Socialist East Germany to study the long-run health and labor market effects of sustained air pollution exposure over more than three decades. After World War II, East Germany's economy relied heavily on subsidized imports of oil from the Soviet Union. In 1982, the Soviet Union suddenly and unexpectedly ended East Germany's preferential access to these imports (Schürer 1999; Pfaff 2006). This trade shock forced East German authorities to switch to lignite (brown coal), the only locally available energy source. Lignite, however, is highly polluting, a low quality energy carrier, and costly to transport. It was therefore primarily used to generate energy and heating near mining sites. The sudden increase in lignite usage sharply increased air pollution in counties close to mines. Higher pollution persisted until German reunification in 1990.

Socialist East Germany provides a unique setting to study the effects of sustained air pollution exposure. Specifically, the setting addresses identification concerns related to endogenous responses present in other contexts: Authoritarian restrictions on citizens' freedom of movement prevented migration away from pollution even though higher pollution persisted over a long period of time. The command economy strictly regulated market exchanges and housing prices did not respond to changes in air quality. Finally, rich administrative data allow us to track labor market outcomes more than three decades after first exposure.

Our paper consists of two parts: First, using a difference-in-differences design, we show that the 1982 shock increased local air pollution in areas close to lignite mines compared to areas further away. We also find adverse short-term effects on childrens' health in treated areas. Second, we use an inverse movers design and exploit the collapse of Socialism in East Germany as an additional natural experiment to document negative long-term labor market effects of pollution. Treated individuals earn less and retire earlier. Individuals of almost all ages are affected, with larger effects on the young and the elderly. We also show that pollution affects long-term health, both as a relevant mechanism and an important outcome in its own right.

To begin the analysis, we show that the 1982 shock increased local air pollution in areas close to lignite mines. We use previously classified data on East German air quality monitors and historical maps of lignite mines to establish that increased lignite usage caused an immediate, large, and persistent increase in sulfur dioxide pollution. Difference-in-differences estimates

[1] Among others Currie and Neidell (2005); Currie and Walker (2011); Knittel, Miller, and Sanders (2016); Deryugina, Heutel, Miller, Molitor, and Reif (2019); Deryugina and Molitor (2021); Alexander and Schwandt (2022); Deryugina and Reif (2023).

show that pollution in counties close to lignite mines increased by 19% after 1982 (+28.808 $\mu g/m^3$). The increase corresponds to moving from the 10th to the 90th percentile of sulfur dioxide pollution in the 2017 United States.

Next, we show that the air pollution shock affected health outcomes in the local population. To implement this analysis, we use administrative data on infant mortality at the county level and birth weights for more than 2 million infants from the East German birth register. Difference-in-differences estimates indicate that East German families from counties close to lignite mines suffer one additional infant death per thousand live births per year between 1983 and 1989. Infant mortality rises by 9.0%. Using proximity to lignite mines after 1982 as an instrument for air quality, we estimate infant mortality elasticities between 0.26 and 0.82 – considerably higher than estimates from comparable settings in which endogenous mitigation is possible. Infants born close to lignite mines are also 2.7% more likely to be born with low birth weight after 1982.

Our main results document long-term effects on labor market outcomes for treated individuals based on social security data. We built a dataset with detailed information on 6.2 million individuals, the universe of workers from East Germany after German reunification. The data track individual labor market outcomes until 2020 and include rich individual-level baseline characteristics and detailed information on occupation and industry.

Results from OLS regressions indicate that individuals who lived in treated counties under socialism have significantly worse labor market outcomes compared with individuals from control counties. These estimates, however, may be biased due to omitted variable concerns, for example if the post-reunification labor market prospects of treated individuals are systematically different from those of other individuals. For instance, individuals might be differentially exposed to post-reunification changes in the industrial structure or to place-based policies (Akcigit, Alp, Diegmann, and Serrano-Velarde 2024; Mergele, Hennicke, and Lubczyk 2025).

To address these identification challenges, we isolate long-run effects of pollution in an inverse movers design (Card and Krueger 1992; Deryugina, Molitor et al. 2020; Finkelstein, Gentzkow, and Williams 2021; Deryugina and Molitor 2021). Specifically, we leverage the collapse of authoritarian rule and the lifting of restrictions on mobility, which resulted in a sudden increase in cross-county migration after German reunification. As an intuitive example, we compare two individuals, one resided in treated county A under socialism, one resided in control county B. After the collapse of mobility restrictions, both immediately move to the same destination county C and stay there for the remainder of the time. Migration destination county fixed effects allow us to absorb all post-reunification differences. This allows us to account for pull factor determinants of migration and place-specific post-reunification effects like differences in local labor markets, place-based subsidies, and later-life pollution exposure. The movers design isolates variation in pre-migration pollution exposure to identify long-term effects.

Up to four decades after the initial shock, East German movers from treated counties have significantly worse labor market outcomes than movers from control counties. On average, exposed individuals spend 0.372 fewer years (4.5 months) in employment, retire earlier (1.96

months), and suffer a wage penalty over their working life, amounting to 3% of average daily wages. A back-of-the-envelope calculation shows that the post-reunification employment effects of air pollution alone account for social costs of about 1% of German GDP in 1989.

We provide new evidence on the labor market effects of air pollution across age groups. Existing studies commonly exploit policy changes to estimate the effect of air quality on cohorts born right after compared with just before the introduction of the policy. Our setting allows us to estimate air pollution exposure effects on the entire population of workers from East Germany. This has two advantages: First, it allows us to estimate effects on individuals in age groups that are often excluded from the analysis. Second, we can examine heterogeneity and estimate how pollution effects differ across the age distribution. Our results document adverse labor market effects on a wide range of age groups: those up to 12 years or younger and those as young as 27 years or older. The estimates show an inverse U-shaped relationship between age and pollution effects: The effects are largest for the youngest and oldest individuals in our sample.

We use this insight to estimate a model with destination-by-origin fixed effects that explicitly accounts for both differences between destination countries and differences between origin counties.[2] In this model, we compare individuals who move between the same pair of origin and destination counties but differ in their vulnerability to air pollution due to age. We find significant negative wage effects on movers from treated areas that were of vulnerable age when exposed compared to other individuals from the same treated county.

To confirm that our results capture the effects of exposure to increased pollution from lignite usage, we estimate placebo tests. We define pseudo-treated counties in East Germany based on distance to potash, rather than lignite mines. Potash, like lignite, is an exploitative mining industry but does not result in additional air pollution and was the only other natural resource available in East Germany.[3] When we estimate our difference-in-differences and movers design specifications based on distance to potash, we find precise zero effects of the placebo on air pollution, birth weights, infant mortality, and labor market outcomes.

Finally, we show that declining individual health is a main mechanism behind labor market effects – and an important outcome in its own right. Using survey data on East Germans from the German Socio-Economic Panel, we find that individuals who lived in treated counties before 1990 have worse health outcomes and higher healthcare utilization than other East Germans. Three decades after the initial shock, treated individuals have significantly higher incidence rates for pollution-related health issues like asthma (+229.1%) or cardiopathy (+83.1%), but *not* for pollution-*un*related health issues like chronic back pain.

Our paper contributes to the literature on the effects of air pollution, especially to the literature on the long-term effects of air pollution on labor market outcomes. While short-term effects of air pollution on health and labor market outcomes are well-documented, evidence on long-term

[2]The inverse movers design identifies the long-term effect of sustained pollution exposure under the assumption that unobserved differences between origin counties do not correlate with later-life labor market outcomes.

[3]Potash is primarily used in fertilizer production for agriculture, whereas lignite is burned in power plants for energy generation or used in domestic heating which creates air pollution.

effects of air pollution on labor market outcomes is scarce. Studies show that air pollution exposure *in utero* or early in life has negative effects on college attendance (Voorheis et al. 2017; Colmer, Voorheis, and Williams 2022), income, and employment (Isen, Rossin-Slater, and Walker 2017). These studies use decreases in air pollution induced by changes in environmental regulations and compare early-career outcomes for infants born around a cut-off. By construction, these studies are focused on the effects of acute pollution exposure in children. We find large wage effects when exposure is longer than in other settings, consistent with pollution effects cumulating over time. We further document large effects across the entire age distribution, specifically for children up to 12 years of age and for adults as young as 27 years. Our results show that air pollution affects labor market outcomes for a large part of the population and indicate that the full labor market costs of air pollution are larger than previously thought.

A small number of papers provides evidence on the health effects of long-term exposure to air pollution, especially Chen, Ebenstein, Greenstone, and Li (2013), Ebenstein, Fan, Greenstone, He, and Zhou (2017), Beach and Hanlon (2018), Anderson (2020), and Barreca, Neidell, and Sanders (2021). We contribute to this literature by documenting the effects of air pollution in a unique setting where – due political constraints – spatial sorting (Banzhaf and Walsh 2008; Currie 2011; Chen, Oliva, and Zhang 2022) was minimized and latent sorting, e.g., through pollution pricing in the housing market, was impossible. The setting allows us to identify long-term effects of pollution exposure on both short- and long-term health outcomes.

Most literature on the effects of air pollution focuses on short-term effects on health outcomes, especially in children (Chay and Greenstone 2003; Currie and Neidell 2005; Currie and Walker 2011; Luechinger 2014; Knittel et al. 2016; Deryugina et al. 2019; Hanlon 2020; Alexander and Schwandt 2022). We show that, in a setting with minimal scope for endogenous mitigation, estimated infant mortality elasticities are notably larger than results from other developed countries. Our findings suggest that endogenous mitigation and sorting may downward bias effect sizes in other settings.

Our paper also speaks to the literature on the long-term effects of health on labor market outcomes such as wages (Almond 2006; Black, Devereux, and Salvanes 2007; Bleakley 2010; Hoddinott, Maluccio, Behrman, Flores, and Martorell 2008; Maccini and Yang 2009) and labor force participation (Oreopoulos, Stabile, Walld, and Roos 2008; Hamory, Miguel, Walker, Kremer, and Baird 2021); see Currie (2009) for a comprehensive review. While most of this literature uses health shocks at young ages we show that a health shock at almost any age affects long-term labor market outcomes.

Finally, our analyses inform questions about the long-term effects of life under authoritarianism. Authoritarian regimes restrict their citizens' freedoms of expression, assembly, and movement which limits their ability to offset the adverse effects of authoritarian policies. We show these policies affected individuals in East Germany immediately, severely, and for decades – long after these policies had been replaced. We extend existing literature that documents lasting effects of life under socialism on socioeconomic outcomes like trust (Lichter, Löffler, and Siegloch 2021), preferences (Alesina and Fuchs-Schündeln 2007), or savings decisions (Fuchs-Schündeln and Schündeln 2005). In related work, Boelmann, Raute, and Schönberg (2025) use post-reunification migration behavior to document that exposure to more egalitarian East German gender norms affected labor market participation among West German women. Burchardi and Hassan (2013) use migration out of East Germany in the aftermath of World War II to identify the effect of social ties on regional economic growth.

1 Setting

The Crude Oil Trade Shock of 1982

Before World War II, industrial activity in East Germany relied heavily on natural resources from the west of the country. After the division of Germany in 1949, the only abundant resources within the new East German state's borders were lignite and potash. Quickly, the socialist economy became dependent on importing natural resources, especially fossil fuels, mostly from other countries in the Eastern Bloc (Pfaff 2006): crude oil and natural gas from the Soviet Union, bituminous coal from Czechoslovakia, Poland, the Soviet Union and from West Germany. Trading within the Bloc was preferable because political alignment facilitated trade negotiations. The Eastern Bloc also relied on a lagged, moving average price mechanism that allowed the GDR to buy resources below world market prices. This price mechanism was meant to lower the impact of world market price volatility on buyers and proved to be an important economic advantage for importers within the Eastern Bloc during the oil crises of the 1970s. Finally, imports from the Eastern Bloc could be off-set with exports of goods produced by the GDR rather than costly foreign exchange (Steiner 2004).

Subsidized crude oil imports from the Soviet Union formed the core of the GDR's economic strategy (Pfaff 2006, p.36). As a result, the country was unprepared when, in 1982, the Soviet Union suddenly and unexpectedly reduced oil deliveries to the GDR (Schürer 1999). In fact, the GDR's central planners initially feared "the existence of the country endangered."[4] While imports increased at a steady rate between 1960 and 1981, they remained capped at 17 million tons per year after 1982 and for the remainder of Socialism in East Germany (Figure 1). After 1982, the Soviet Union also demanded that oil imports be compensated with exports of higher-valued goods.[5] This requirement both raised the effective price of oil and forced the GDR to forego the foreign exchange they could have earned by selling those goods on the world market.

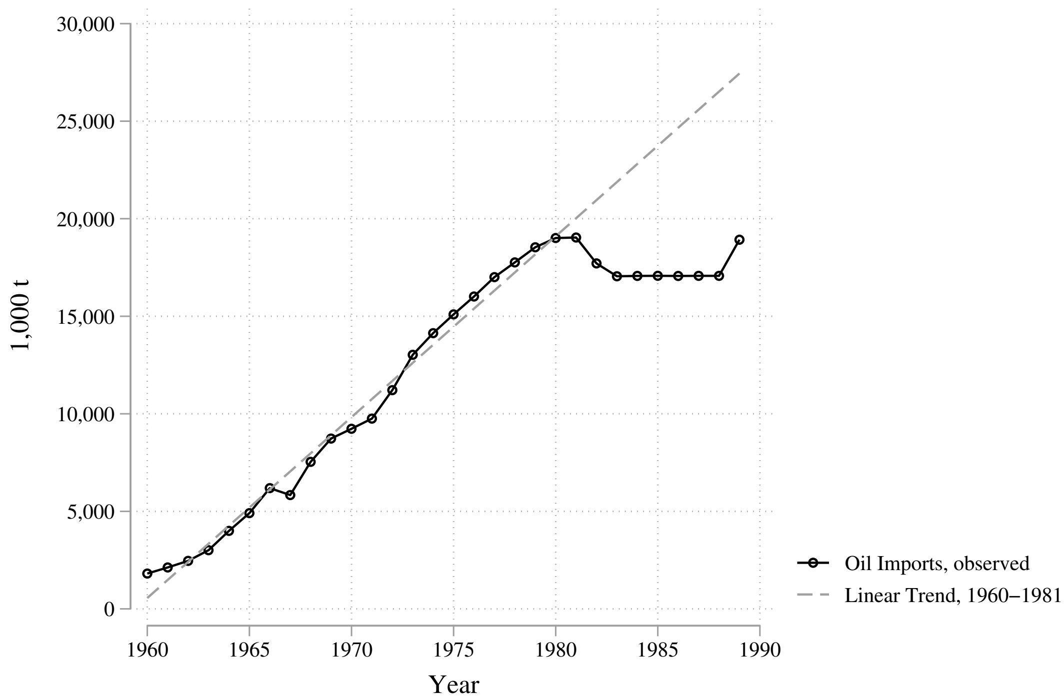
The Soviet Union's motivation for the 1982 oil import cuts was threefold. First, the Soviet economy was experiencing a downturn after a string of poor harvests and negative productivity shocks (Steiner 2004). Second, the intensified geopolitical situation after the Soviet invasion of Afghanistan in 1979 and resulting military investments put additional strain on the Soviet budget. Third, the GDR used a portion of subsidized oil imports

[4] Own translation. For original quote see Schürer (1999), p.77.

[5] Defined as goods that would be "exportable" to world markets and the global West (Schürer 1999).

to refine crude oil into higher quality petrochemicals to sell for foreign exchange in the West (Stokes 2013). The fact that East Germany effectively conducted arbitrage on Soviet subsidies outweighed political goodwill, even though the foreign exchange gained in return was increasingly indispensable for East German fiscal stability (Steiner 2004). As it was impossible to offset oil cuts with other imports, East Germany's increasingly energy-intensive economy left the socialist regime with only one option: swiftly increasing domestic lignite mining.

Figure 1: East German Imports of Crude Oil, 1960-89



East German imports of crude oil from the Soviet Union per year between 1960 and 1989. Dashed line indicates linear fit between 1960 and 1981, extrapolated to 1982-89.

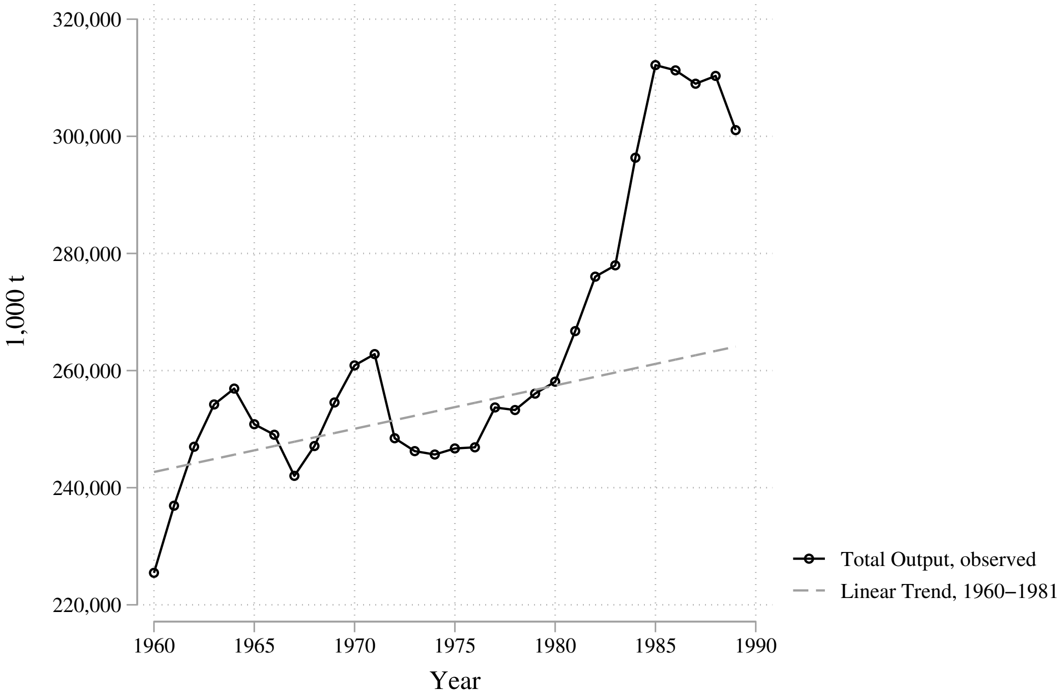
Lignite Production and Usage

After 1982, East Germany rapidly increased lignite production for consumption and industrial use (Figure 2).[6] East Germany retooled industrial and power plants for lignite use. Lignite briquettes partially replaced oil in domestic heating. This rapid adjustment was possible because substitution strategies had been crafted - but not implemented - in the late 1970s during the oil crisis (Steiner 2004).[7] The main sources of lignite were open pit strip mines in the southern regions of East Germany.

[6]In an alternative comparison, we account for differences in fuel energy density between oil and lignite coal. We find that the increase in lignite output matched the decrease in oil imports (Figure A.13). Specifically, we find that the increase in lignite output, relative to the linear pre-1982 trend, accounts for 118.4% of the energy density lost due to the oil import shock.

[7]To maintain access to foreign exchange, the GDR continued to export refined petrochemicals. To enable these exports, the substitution of oil went beyond the 2 million ton decrease in imports (Stokes 2013).

Figure 2: East German Production of Lignite, 1960-89



Total lignite production in East Germany between 1960 and 1989. Dashed line indicates linear fit between 1960 and 1981, extrapolated to 1982-89.

East Germany increased lignite usage to successfully offset the Soviet oil import cuts. Lignite, however, is known as "the lowest rank of coal."[8] It is highly polluting and has a high ratio of pollutant emissions to heat generation relative to other energy carriers. Originating from compressed natural peat, lignite has high moisture content of up to 45%, compared with less than 20% for bituminous coal and less than 15% for anthracite (U.S. Energy Information Administration). It also has a crumbly texture, disintegrates easily, and deteriorates rapidly when exposed to air. As a result, lignite is expensive and impractical to transport, and ill-suited for long-distance trade.

Yet, lacking alternatives, East Germany became the world's largest lignite producer by 1989 and covered close to 70% of its energy demand with domestic lignite. Lignite only lost prominence after German reunification, when alternative fuels became available and tighter environmental regulations reduced the profitability of lignite-based plants (Luechinger 2014).

Lignite, Air Pollution, Health, and Productivity

Lignite releases large amounts of sulfur dioxide and carbon dioxide when burned. These pollutants carry in the air over dozens of kilometers (Almeida, Manousakas, Diapouli, Kertesz, Samek, Hristova, Šega, Alvarez, Belis, Eleftheriadis et al. 2020). In addition, both sulfur dioxide and carbon dioxide are precursor pollutants that, once in the air, decay to smaller particulate

[8] U.S. Energy Information Administration , https://www.eia.gov/tools/glossary/?id=coal, last accessed 13 July 2025.

matter, such as $PM_{2.5}$. $PM_{2.5}$ micro particles penetrate indoors even when they are released outdoors (Hoek, Kos, Harrison, de Hartog, Meliefste, ten Brink, Katsouyanni, Karakatsani, Lianou, Kotronarou et al. 2008).

Air pollution impacts individuals when they breathe in particulate matter. Reviews of the medical literature highlight three channels (Aguilar-Gomez, Dwyer, Graff Zivin, and Neidell 2022). First, air pollution harms the respiratory and cardiovascular systems where it causes acute and potentially chronic inflammation. These injuries, particularly if they occur repeatedly, can reduce an individual's physical ability and increase fatigue. They can also negatively impact cognitive performance, such as concentration, focus or memory. Second, air pollution harms the brain and the nervous system. Air pollution causes neuro-inflammations and oxidative stress, which in turn can impair brain functionality and even impact the development of cognitive capabilities in the long-term. These effects are especially harmful for infants whose nervous system is still developing. Third, air pollution can cause mutations in the expressions of gene sequences. These epigenetic effects can result in cognitive impairments or latent impacts on individual development and traits. All of these factors impact individual health in the short run under acute exposure and in the long run when health impacts can turn into chronic conditions.

Studies have shown these effects in individuals under short-term and acute air pollution exposure. The health and cognition effects of air pollution lead to measurable reductions in labor productivity. This has been documented in the aggregate (Dechezleprêtre, Rivers, and Stadler 2019; Borgschulte, Molitor, and Zou 2022; Leroutier and Ollivier 2025), for low-skilled workers in agriculture (Graff Zivin and Neidell 2012), textile production (He, Liu, and Salvo 2019) and fruit picking (Chang, Graff Zivin, Gross, and Neidell 2016), for high-skilled workers in software development (Holub and Thies 2023), for cognitively demanding work environments like call centers (Chang, Graff Zivin, Gross, and Neidell 2019), as well as in sports (Lichter, Pestel, and Sommer 2017; Archsmith, Heyes, and Saberian 2018) and politics (Heyes, Rivers, and Schaufele 2019). While these studies confirm the existence of labor productivity channels under acute exposure, our paper provides evidence on how these channels manifest in longer-term labor market outcomes.

2 Data

Lignite Mining Locations

To measure the causal effects of increased air pollution following the 1982 shock, we compare individuals and counties located near lignite deposits with those farther away. To construct this distance-to-lignite-based research design, we digitize a map of natural resource extraction sites from the GDR's statistical yearbook. From this map, we extract the geographical coordinates of all 21 lignite mines and 8 potash mines that were active at the time of the 1982 shock. We use historical shapefiles of East German administrative regions from Bundesamt für Kartographie und Geodäsie (2020) to calculate the straight-line distance between each county's and municipality's geographical centroid and the lignite and potash mines.

Air Quality Data

To measure changes in air pollution, we use new data on airborne sulfur dioxide concentration under socialism. In East Germany, these data were classified as confidential. The German Federal Environmental Agency (*Umweltbundesamt*) decrypted, digitized, and quality-tested them (Schmidtke, Schmidt, and Schmidt 2025). We observe annual mean SO_2 concentration readings for 131 geo-located air quality monitors (Figure A.1). While a few stations have initial readings dating back to 1969, most begin to report data in the mid-1970s. Between 1978 and 1988, we observe sulfur dioxide levels for an average of 37 monitors per year. We use the geo-locations of these stations to assign sulfur dioxide concentration levels to East German counties. Specifically, we apply an inverse-distance weighted imputation algorithm. We calculate straight-line distances between each county centroid and each monitor location and assign to each centroid the inverse-distance weighted average of readings within a 100-kilometer radius. We use a linear weight-decay function to give lower weight to monitors that are further away from a county's centroid.

Output, Imports, Investments, and Net Migration

We use the East German government's official statistical yearbooks to measure output, imports, investment, employment, and migration.[9] To measure lignite production, we use data on aggregate production digitized by Glitz and Meyersson (2020). We digitize new data on imports and exports, as well as public investment and employment in mining and utilities' sectors from the yearbooks. Finally, we use county-level population and mortality data to calculate net migration rates. Specifically, we compute year-on-year population change net of births and deaths to test for residential sorting effects of increased air pollution levels. Recent research raises concerns about the trustworthiness of data reported by authoritarian regimes (Martínez 2022). To mitigate these issues, we use disaggregated micro data whenever possible. We mainly use socialist data in difference-in-differences specifications where the presence of fixed effects should account for the types of bias we would expect to result from authoritarian misreporting (for instance, inflation or deflation of figures in a specific county or year).

Infant Health

We use data on infant mortality and infant birth weights to measure the short-term impacts of increased air pollution. We observe annual county-level infant mortality rates between 1970 and 1989.[10] To measure birth weights, we use an anonymized copy of the administrative birth register of East Germany, provided by the German Federal Archives (*Bundesarchiv*). The birth

[9]The statistical yearbooks can be accessed under https://digi.bib.uni-mannheim.de/en/statistische-jahrbuecher/uebersicht/, last accessed on 07 August 2024.

[10]The data are reported in the statistical yearbooks and were digitized by Class and von den Driesch (2017).

register provides information on 2,210,149 individual births between 1979 to 1989. For each birth, we observe the municipality of birth, the date of birth, the weight, height and gender of the newborn, as well as information on the parents' age, occupation, and education.

Social Security Data

To investigate the long-term effects of air pollution on the labor market, we use administrative data from the social security records of the German Federal Employment Agency (*IAB*). After the official reunification of Germany in October 1990, social security registration requirements extended to East Germany. All East German workers had to be registered in the unified social security records starting in January 1991. We observe the universe of these social security records and construct a baseline dataset of 6,222,849 East German workers. For each worker, we use the first available record to capture their occupation, education, and location at the point when they emerged from socialism. We pool the 1991 and 1992 data to account for potential delays in recording. For each individual, we consider the first post-socialism observation to construct our baseline measures. We remove all individuals who ever had a social security record in West German administrative data between 1975 and 1991 to avoid capturing effects of early migration waves between the different regions of reunified Germany. The data do generally not include any information on individuals working in the military, secret services and most civil service occupations. We follow Findeisen, Lee, Porzio, and Dauth (2021), Heise and Porzio (2023), and Boelmann et al. (2025) and proxy individuals' location under socialism with the location at which they first appear in the data.[11] We show that our main results are robust when we use survey data from the G-SOEP which include explicit information on individuals' location in 1989 and 1990. Next, we create individual labor market trajectories that summarize each workers' career between 1992 and 2020. For each worker, we use detailed data on social security contributions and benefits to calculate the share of time they spent in employment, unemployment and retirement.[12] We calculate average daily wages for employment spells and convert wages into constant 2015-Euros based on consumer price indices from the German Federal Statistical Office (*Statistisches Bundesamt*). To capture retirement, we measure individuals' exit from the labor force. The data explicitly record retirement for individuals who are unemployed at the time of retirement or who retire early. For all other individuals, we follow Haywood, Janser, and Koch (2021) and assign a retirement date at individuals' last observation in the social security data, provided that they are at least 50 years old at this point in time.

We use variation from movers to identify the long-term effects of air pollution exposure. To implement this analysis, we measure each individual's location each year. We define movers as individuals registered in a different county than the previous year. We disregard moves

[11]The data do not include information on individuals' place of birth, location is measured at the county and municipality level. Migration flows in 1990 were small (Hunt 2006; Burchardi and Hassan 2013).

[12]The data also provide records of worker death, as long as death occurred before retirement. This implies that both death and retirement are absorbing states. As individuals who die prematurely might also be more likely to retire earlier (e.g. if both are functions of latent health), we do not study death as a separate outcome.

between different municipalities within the same county. We assign each origin location to the treatment or control group based on the calculated straight-line distance between the respective municipality's centroid and the nearest lignite mine. To test that treated and control locations are statistically balanced, we use information on GDR counties' local characteristics from Lichter et al. (2021). We restrict the movers' sample to individuals who move exactly once during their post-reunification life and whose single move took place at exactly the first time we can observe such a move after reunification: between 1992 and 1993. We record each individual's destination and origin county and municipality and restrict the analysis to all destinations that experience at least two migration inflows: one each from a treatment and from a control origin. Our final movers sample covers $144,338$ individuals who move from 71 origin counties in East Germany to 391 destination counties in all of reunified Germany by 1993.[13] The destination counties in our sample receive between 2 and 6,800 movers (median: 43 movers, mean: 375 movers) who continue to reside in these destinations for the rest of their career.

Survey Data

We use survey data from the German Socio-Economic Panel (G-SOEP, v38.1) to investigate individual-level health outcomes. The G-SOEP is an annual panel of German households and individuals first established in 1984. The survey extended to East Germany right after reunification in the summer of 1990. Our analyses focus on survey participants who state that they lived in East Germany in 1989 (Q: *Where did you live in 1989?*). We select all observations on individuals who participated in the summer 1990 survey and declared a county of residence in East Germany in 1990. We assume that individuals' county of residence in the summer of 1990 corresponds to their location under socialism and use these counties to assign individuals into treated and control based on distance to the nearest lignite mine.

To measure health effects, we focus on two sets of survey items. First, to capture health effects in the medium term, we use survey questions that ask about individuals' healthcare utilization. These items were introduced to the G-SOEP in 1992 – ten years after the air pollution shock in East Germany. Specifically, we focus on two survey questions: an indicator for doctor visits in the survey year (Q: *Have you seen a GP in the last three months?*) and an indicator for missed time at work due to illness (Q: *How many days did you not work due to illness in 1992?*). We observe these outcomes for 3,635 East Germans in the 1992 survey.

Second, to measure long-term health effects, we use survey items that inquire about medical diagnoses (Q: *Have you ever been diagnosed with one or more of the following diseases by a doctor ? (...)*). Specifically, we focus on whether individuals have been diagnosed with four conditions, two of which (asthma and cardiopathy) are medically related to pollution exposure (Guarnieri and Balmes 2014; Rajagopalan, Al-Kindi, and Brook 2018) and two of which are

[13]The movers' analysis uses contemporary German counties, while the short-term analysis relies on historical East German counties. Administrative reforms after reunification drastically reduced the count of local government units in East Germany: the number of counties fell from 217 to 76, and municipalities dropped from more than 7,500 in 1990 to fewer than 2,300.

placebo outcomes that are *un*related to pollution exposure (diabetes and chronic back pain). The full set of diagnoses items was first surveyed in the G-SOEP in 2011 – 29 years after the air pollution shock. We observe diagnoses for 1,329 East Germans in the 2011 survey. Diagnoses were re-surveyed in 2013, 2015, and 2017 and we pool the 2011-17 cross sections in a robustness test with more statistical power (the pooled cross sections cover 4,472 observations).

Lastly, we use G-SOEP to test whether the labor market effects we observe in social security data can also be measured in survey data. We record individuals gross income and binary indicators for employment and retirement for the pooled 2011, 2013, 2015, and 2017 cross sections. We use the provided G-SOEP survey weights throughout.

3 Research Design

Treatment Definition

To identify the causal effects of air pollution exposure in East Germany, we compare counties and individuals located close to lignite mines with those that are not. This empirical design leverages variation in the cost of substituting oil with lignite: It was easier to substitute oil with lignite in locations closer to mines because lignite is ill-suited for long-distance trade and transport. Importantly, lignite mines do not need to be pollutant emission sources themselves. Rather, distance to the nearest mine defines the area in which any economic activity that uses fossil fuel as a production input could switch from using oil to using more polluting lignite coal. In East Germany, these economic activities encompassed three broad categories: electricity generation, heat generation in industrial applications, and domestic heating with lignite briquettes. In Section 4.1, we confirm that our treatment definition directly maps to large and persistent differences in local levels of air pollution. To implement this analysis, we use air quality monitor readings as a catch-all measure of pollution exposure that is independent of the definition of emission sources.

Some studies on short-term air pollution effects study day-to-day variations in wind direction for causal identification. These identification strategies cannot be used for long-term exposure: Wind directions vary in the short term due to small-scale swirls of motion (called eddies) that cause turbulence. In the long-term, wind directions are highly persistent because they are caused by differences in atmospheric pressure. These arise due to temperature differentials that stem from the difference between heating at the equator and the poles, and by the Earth's rotation.

Short-Term Effects on Health: Difference-in-Differences

We identify the short-term effects of the lignite shock in a difference-in-differences analysis. Specifically, we compare the evolution of outcomes in counties close to lignite mines, where substituting oil with lignite was easy, with outcomes in other counties. Difference-in-differences estimation allows us to control for additional characteristics of the treated counties that might potentially correlate with air pollution, including unobserved time-invariant covariates. For instance, pollution may be higher in counties with higher levels of economic activity – but these counties may also have better medical infrastructure or in other ways differ in their average health outcomes. We estimate two-way fixed effects regressions with OLS.

$$Y_{ct} = \alpha_c + \phi_t + \big(1[t > 1982] \times D_c\big)\beta + \epsilon_{ct} \tag{1}$$

The outcome variable Y_{ct} is either county c's average sulfur dioxide concentration, infant mortality rate per 1,000 live births, low-birth weight probability or average infant birth weight. County fixed effects α_c control for variation that is county-specific and constant over time, e.g. as a result of persistent differences in industrial development. Year fixed effects ϕ_t control for variation over time, e.g. due to decreases in infant mortality that are shared across counties. The treatment variable D_c indicates counties within 60 kilometers of a lignite mine (Figure 3), the excluded period is 1982, the year in which import cuts were announced. We set the treatment distance to 60km based on estimates that use non-parametric distance bins. We show that our results are robust to various alternative definitions of treatment.

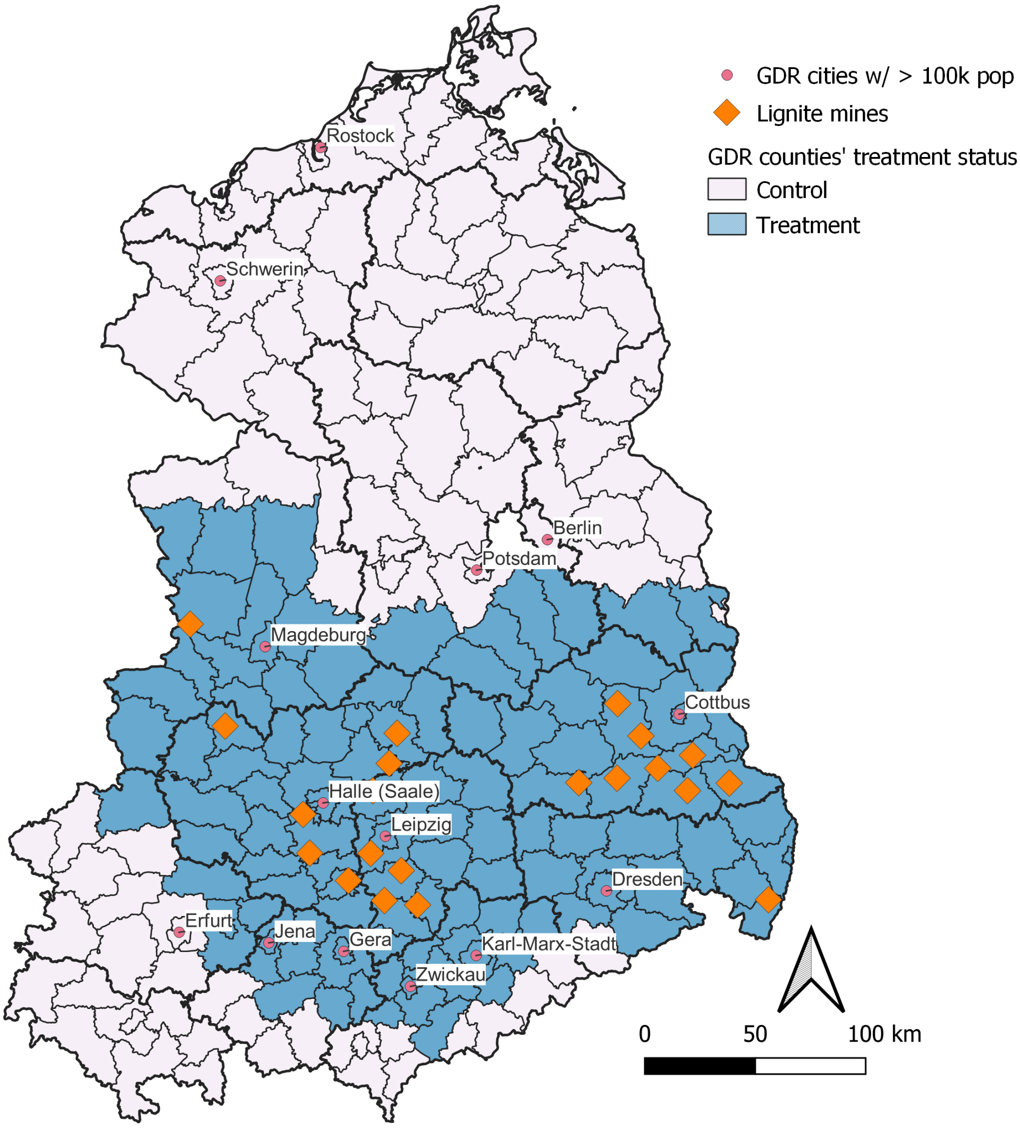
Our setting provides a single uniform policy change and all treatment counties are treated at the same time. In this context, the β-coefficient from OLS estimates of Equation 1 on the balanced panel of counties yields a straight-forward estimate of the average treatment effect on the treated counties (Roth, Sant'Anna, Bilinski, and Poe 2022). In Chapter 4.2, we augment our difference-in-differences approach to estimate the infant mortality elasticity of air pollution. To implement this analysis, we use the treatment variable D_i as an instrument for local levels of air pollution. Taking infant mortality rates and air pollution readings in logs, we then estimate the elasticity based on this DiD-IV specification.[14]

The key identification challenge in difference-in-differences designs is that the true counterfactual outcome for treated observations can never be observed. Difference-in-differences overcomes this challenge with the assumption that, in absence of treatment, the treated counties would have on average evolved in parallel to the change observed in the untreated counties (Roth et al. 2022). We estimate event-study regressions to test the plausibility of this parallel trends assumption. We saturate event studies with lags and leads of the interaction term between post-treatment indicator $1[t > 1982]$ and the treatment variable D_i. We plot these coefficients relative to the omitted period.

To assess the robustness of our findings, we conduct two falsification exercises as placebo tests. First, we conduct placebo tests in space that use distance to potash mines, rather than lignite mines, as a pseudo-treatment. This placebo lets us separate the effects of exposure to exploitative industries (i.e., lignite or potash) from the specific effects of air pollution (i.e., lignite but not potash). Second, we conduct placebo tests in time that assign treatment to the same set of counties but three, six, or nine years earlier. This placebo uses only pre-treatment data and shifts the treatment to earlier years. It lets us test the impact of underlying differences between the treated and control counties. We cluster standard errors at the county-level throughout.

[14]Our main DiD specification effectively is the reduced form of this instrumental variables strategy.

Figure 3: County-Level Treatment Assignment



Counties are assigned treatment or control status depending on whether their geographical centroid is within 60 kilometers of a lignite mine.

Long-Term Effects on Labor Market Outcomes: Linear Regression

To estimate the effect of sustained pollution exposure on long-term labor market outcomes, we use two approaches that compare individuals exogenously exposed to the air pollution shock with others. In the first approach, we estimate OLS regressions on the 6,222,849 East German workers who appear in the social security data after reunification. Under socialism, restrictions on the freedom of movement, insufficient housing supply, and the absence of a price-based housing market meant that East Germans could not respond to increased pollution by endogenously sorting across space. Individuals' location, predetermined and exogenous to the pollution shock, therefore dictated their level of exposure.

$$Y_i = \alpha_i + \beta D_c + \mathbf{X_i'b} + \epsilon_i \tag{2}$$

The outcome variable Y_i measures cumulative labor market outcomes for individual i between 1992 and 2020. We study four outcomes: time in employment, time in unemployment, retirement age, and average daily wages in constant 2015-euros.[15] D_c is an indicator for counties exposed to the air pollution shock. OLS estimates of β measure differences in long-term labor market outcomes for individuals exposed to air pollution compared with others. $\mathbf{X}$ is a vector of controls. We control for birth year fixed effects that capture age and cohort effects, state fixed effects that capture geographical variation and differences in local policies, fixed effects for each individuals' highest level of education that capture differences in labor market outcomes across skill groups, fixed effects for five-digit occupational codes that capture differences in labor market outcomes across job types, and three-digit industry fixed effects that capture sectoral differences in labor market outcomes. We measure all controls when individuals first appear in our data at the end of socialism. We measure controls at this baseline because some characteristics, for instance educational attainment or occupations, may become endogenous to perceived labor market opportunities after socialism. Under socialism, however, access to (higher) education and occupational choice was determined by political considerations rather than merit.[16] As a robustness test, we report all specifications excluding control variables that may become endogenous after German reunification. We cluster standard errors at the county-level, measured as each individuals' location under socialism.

Long-Term Effects on Labor Market Outcomes: Inverse Movers Design

OLS estimates of Equation 2 compare the long-term labour market outcomes of individuals exposed to the air pollution shock under socialism with those of others. These estimates are valid if treatment and control groups are comparable except for their exposure to pollution. However, the groups might differ in other ways. We group potential differences in two categories: (1) post-reunification differences and (2) pre-reunification differences. We first address concerns related to (1) post-reunification differences and turn to the second category thereafter.

Following reunification, the German government extensively used place-based policies to smooth the transition from a command to a market economy. Historical pollution exposure may correlate with such place-based policies. For example, counties with lignite industries might have received more subsidies when these industries became obsolete during the transformation process. Place-based policies might affect the labor market outcomes of individuals who had

[15] Employment, unemployment and retirement age are measured in days, but expressed as fractions of years.

[16] Occupational choice was largely determined by socialist planning committees. Job turnover in East Germany was considerably lower than in the West and largely encompassed two channels: job turnover within the same large industrial conglomerate (*Kombinat*) or the compulsory re-employment of men returning from military service and women returning from maternity leave (Findeisen et al. 2021).

lived in those places under socialism. In this case, we may underestimate the effect of air pollution on individual outcomes if it is offset by targeted policies. Moreover, reunification caused rapid economic transformation across all of East Germany (Mergele et al. 2025), which may have affected regions unevenly and further complicates inference in a simple OLS framework.

A further concern relates to migration. Mobility restrictions were lifted when reunification became official in October 1990. If vulnerability to air pollution correlates with individuals' relocation decisions, estimates might be biased. For instance, if those whose health had been particularly affected by Socialist-era pollution moved to less polluted, rural areas—where air quality is better but employment prospects are weaker—we might overestimate the long-term labor market effects of air pollution exposure due to spatial sorting.

To address these types of concerns, we isolate the long-term causal effect of air pollution exposure in a movers design. Specifically, we *invert* the canonical movers design used in health economics (Baum, Wisnivesky, Basu, Siu, and Schwartz 2020; Deryugina and Molitor 2021; Finkelstein et al. 2021) to identify causal effects of differential origin rather than differential destination. The intuition is as follows: Consider two individuals who are comparable on observables but grow up in different counties in socialist East Germany. Individual *A* grows up in a county close to a lignite mine and is exposed to an exogenous increase in air pollution following the 1982 shock. Individual *B* grows up in a county not exposed to the air pollution shock. After German reunification both individual *A* and individual *B* immediately move to an identical, third *destination* county where they spend the rest of their careers. Comparing the labor market outcomes of A and B isolates the effect of their differential exposure before relocation. Importantly, because we assume that neither A and B nor their parents ended up in their origin county due to residential sorting, there is no reason to suspect that this comparison is affected by selection into treatment. Under socialism, individuals live in the counties where they have randomly been born. We use the population of East German workers and compare all such pairs of individuals conditional on their observable characteristics. This comparison allows us to estimate the causal effect of the historical air pollution shock for individuals that select into migration to the same destination at the same time. Controlling for both individuals' joint destination county allows us to account for any variation related to pull factors of migration. Controlling for destination further allows us to account for any differential place-based effects in post-reunification Germany that vary between rather than within the narrow geographical units we consider. This includes place-based policies, differences in regional labor market developments and regional differences in post-reunification air pollution exposure. Specifically, we estimate

$$Y_i = \alpha_i + \beta D_{origin} + \mathbf{X_i'b} + \eta_{destination} + \epsilon_i \tag{3}$$

on a sample of 144,334 East German workers who move to a new county exactly once during their post-reunification life, whose one move happens exactly in the first year after reunification, and who move to destinations that receive at least one individual each from treatment and control origins. The main coefficient, β, measures the effect of growing up in a treatment region. Equation 3 further includes destination fixed effects η. Destination fixed effects capture place-based effects that treatment and control movers are jointly exposed to after relocation. Because we condition on the migration destination county, this specification captures a particularly tight comparison on the sample of East Germans who select into migration, at the same time and to the same place, but from different origins.

In Equation 3, we cannot include origin county fixed effects because treatment assignment is a time-invariant characteristic of origin counties. To show that our results are robust to controls for time-invariant unobservables, we estimate an additional specification that exploits differences in vulnerability to air pollution due to age at exposure. In this specification, we compare individuals who move between the same pair of origin and destination counties but at different ages. Destination-by-origin county fixed effects control for variation that is specific to each county pair and absorb any potential pre-reunification differences between origin counties mentioned above. We also conduct extensive balance tests and other robustness tests.

Finally, we rule out that differential selection into migration between treatment and control counties drives out results. To implement this robustness test, we predict individuals' decision to move with the binary treatment variable. While the resulting coefficient is statistically significant, the economic magnitude of the effect remains very small and corresponds to a 0.5 percentage point increase in the likelihood of migrating in the first year after reunification. We take this as confirmation that, in the context of German reunification, historical exposure to air pollution was not a first order determinant of migration decisions.

Long-Term Effects on Health and Healthcare Utilization

To investigate mechanisms, we compare the health outcomes of East Germans from treatment counties with those who lived in control counties. We estimate OLS regressions:

$$Y_{i,t} = \alpha + \beta \times D_{i,t=1990} + \mathbf{X_i}'b + \epsilon \tag{4}$$

The outcome variable $Y_{i,t}$ measures health outcomes for individual i in survey year t. The treatment variable $D_{i,t=1990}$ indicates individuals who lived in a treated county in 1990. The vector X_i includes birth year fixed effects, a gender fixed effect, and birth year-by-gender fixed effects. These control variables capture age- and gender-specific variation in health that may vary between individuals but is shared across treatment and control counties. We further control for county fixed effects that capture individual i's location in survey year t, so that we always compare individuals who live in the same county but were differentially exposed to air pollution under socialism.[17] We use survey weights provided by the G-SOEP and we compute two-way

[17] These specifications are comparable to the movers design that we estimate on labor market outcomes. The survey data, however, do not have enough statistical power to restrict the comparison to individuals who moved to the same place *at the same time*.

clustered standard errors on the household and 1990-residence county level.[18] We estimate the effect of air pollution on healthcare utilization in survey year 1992 and the effect on long-term health outcomes in survey year 2011. As a robustness test, we also estimate the effect of air pollution on long-term health outcomes on the pooled 2011, 2013, 2015, and 2017 cross sections. In these pooled regressions, we include survey year fixed effects as an additional control.

4 Results

4.1 Short-Term Effects on Air Pollution and Migration

Effects on Air Pollution

So far, we have argued that intensified lignite usage in parts of East Germany increased local air pollution. Now, we use historical air pollution data to test this argument (Figure 4). Before 1982, levels of sulfur dioxide air pollution in counties close to lignite mines were comparable to those farther away. After 1982, air pollution levels are significantly higher in counties closer to lignite mines. Event study estimates confirm that air pollution increased significantly in East German counties close to lignite mines after 1982 (Figure 4 (b)). In counties within 60 kilometers of a lignite mine, air pollution increases by 17.9% in 1983, 8.3% in 1984, 20.0% in 1985, 11.1% in 1986, 14.2% in 1987, 13.3% in 1988, and 15.2% in 1989. While the estimates coefficients fluctuate in the first years 1983-1985, counties close to lignite mines stabilize at a persistently higher pollution level than other counties. The change in pollution level coincides with the evolution of lignite mining output (Figure 2). Before 1982, coefficient estimates are small and not statistically different from zero.

Effects on Migration

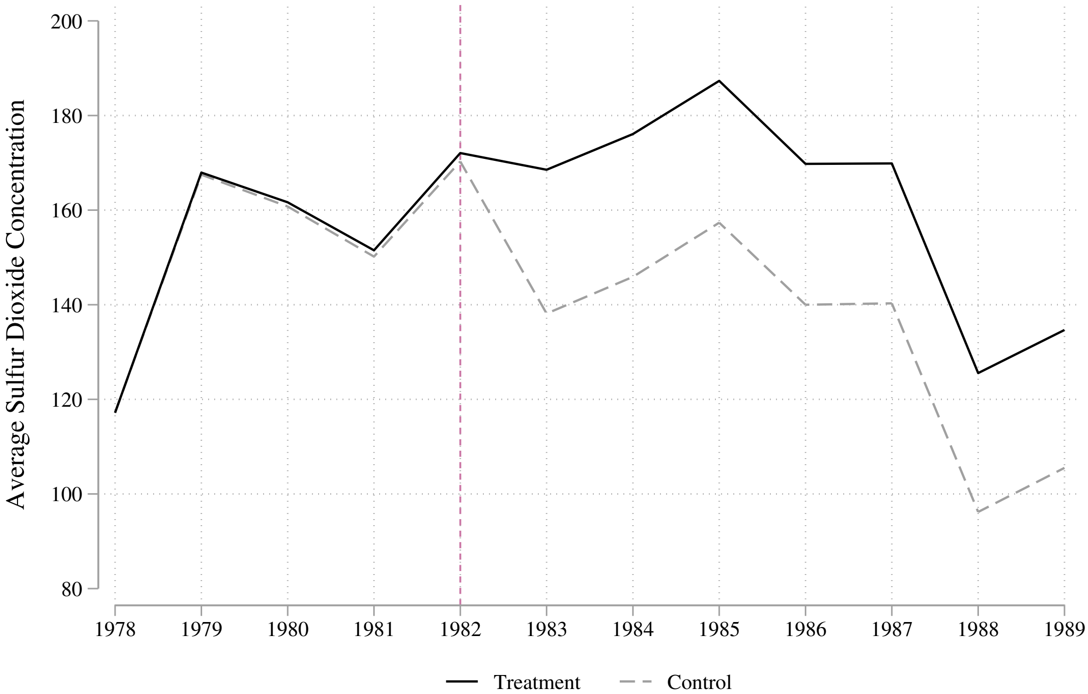
A key identifying assumption in our research design is that individuals could not endogenously move away from pollution exposure in East Germany. We provide a direct test of this assumption based on county-level demographics. Specifically, we measure migration as the difference in county population between t and $t + 1$, net of deaths and births. OLS estimates (Table 2, column (2)), indicate no significant increase in net migration out of counties close to lignite mines after 1982. The estimated coefficient is negative but reassuringly small and insignificant.[19] Figure A.3 shows a difference-in-differences event study plot for net migration. These results support our assumption that individuals could not respond to changes in local pollution by changing residence.

[18] Households are effectively nested within 1990-residence counties so that standard errors are equivalent to clustering on the county level. While we define treatment as a binary variable, we cluster at the 1990-residence county level to allow for correlation in the unobserved intensity of pollution exposure under socialism.

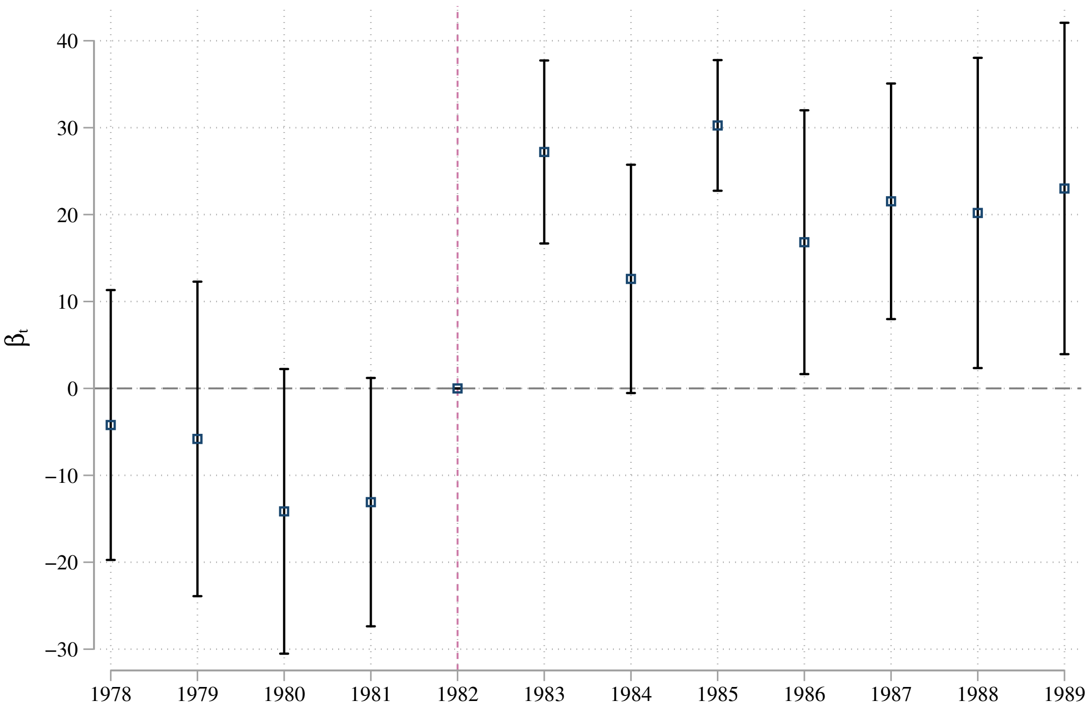
[19] In 1981, the last year before treatment, counties in East Germany had a mean population size of 76,404 and a median population size of 58,449. The OLS point estimate of 88 additional migrants corresponds to 0.12% additional population change.

Figure 4: Sulfur Dioxide Air Pollution

(a) Smoothed and Centered Means



(b) Event Study



Panel (a): Mean annual SO_2 concentration in counties within 60 km of a lignite mine (treatment) or other counties (control), smoothed and centered means. Panel (b): OLS event study coefficients that compare mean annual sulfur dioxide concentration in East German counties within 60 km of a lignite mine to other counties. 1982 omitted as reference category. β_t measured in $\mu g/m^3$ of sulfur dioxide.

4.2 Short-Term Health Effects

Effects on Infant Birth Weights

Until 1982, infants born in municipalities close to lignite mines and in other municipalities had comparable average weight at birth. By 1983, the average birth weight of infants born in municipalities close to lignite mines is lower than in other municipalities (Figure 5 (a)). While birth weights on average increase across both groups, birth weights are persistently lower in municipalities close to lignite mines after the air pollution increase.

Event study estimates confirm that birth weights decrease significantly in municipalities close to lignite mines (Figure 5 (b)). In counties within 60 kilometers of a lignite mine, average birth weights decrease by 0.4% in 1983, by 0.8% in 1984, 0.2% in 1985, 0.4% in 1986, 0.6% in 1987, 0.5% in 1988, and 0.5% in 1989. Before the air pollution increase, coefficient estimates are close to zero and insignificant. Notably, average birth weights are persistently lower in municipalities close to lignite mines, mirroring the change in air pollution levels.

Reductions in average infant birth weights are relatively small in magnitude. Medical literature suggests that these average effects mask heterogeneity throughout the birth weight distribution (Almond, Chay, and Lee 2005; Smith, Reich, Herring, Langlois, and Fuentes 2015; Schwarz, Bruckner, Ilango, Sheridan, Basu, and Benmarhnia 2019; Fong, Kosheleva, Kloog, Koutrakis, Laden, Coull, and Schwartz 2019). Next, we examine changes in effect size across the distribution by estimating separate difference-in-differences regressions at each ventile (Figure 6).[20]

Estimates confirm that municipalities close to lignite mines experience larger reductions in birth weights at lower levels of the birth weight distribution. Point estimates are negative at every ventile and become significant below the 65th percentile of the birth weight distribution. The birth weight effect doubles in magnitude from -7.357 grams at the 60th to -16.737 grams at the 10th percentile of the distribution. Infants from municipalities close to lignite mines after 1982 are also significantly more likely to have birth weights below the critical value of $2,500$ gram (Table 1).

Effects on Infant Mortality

Until 1982, infant mortality rates - like air pollution and birth weights - were comparable between counties that are close to lignite mines and those further away (Figure 7). When air pollution increases after 1982, however, an immediate and persistent gap opens: From 1982 onwards, and for the remainder of Socialism in East Germany, infant mortality is significantly higher in counties close to lignite mines. Similar to birth weights, infant mortality rates increase by a persistent deviation from the joint trend.

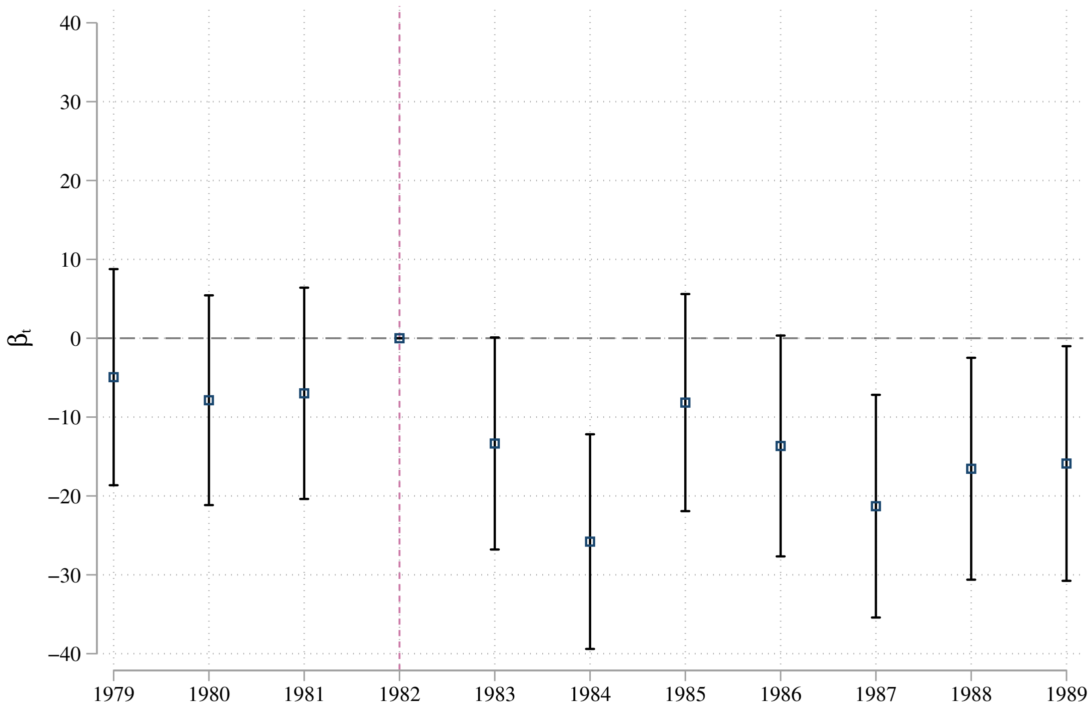
Event study estimates confirm that mortality is meaningfully higher in counties close to mines after air pollution increases even though the coefficients are less precisely estimated. In counties

[20] Effects at specific percentiles are identified if the identifying assumptions hold for each percentile series across municipalities. We test these assumptions, including parallel trends at each percentile, in Table B.12.

Figure 5: Average Infant Birth Weights

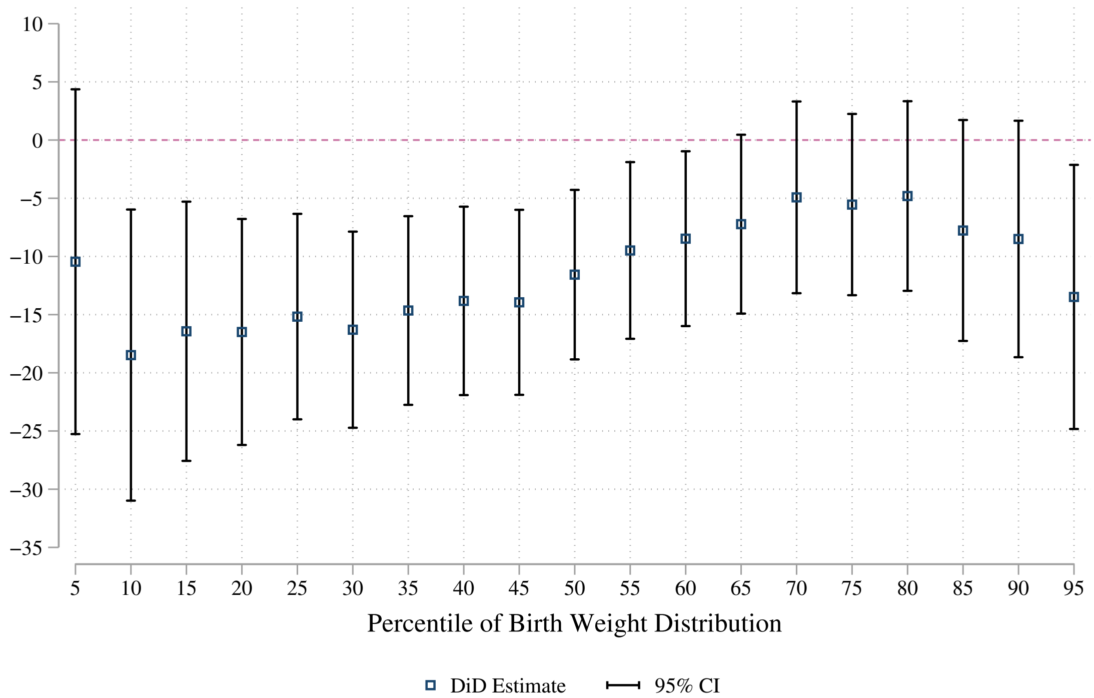
(a) Smoothed and Centered Means

(b) Event Study



Panel (a): Mean birth weight for children born in municipalities within 60 km of a lignite mine (treatment) or other municipalities (control) by birth year. Smoothed and centered means. Panel (b): OLS event study coefficients that compare average birth weights in East German municipalities within 60 km of a lignite mine to other municipalities. 1982 omitted as reference category. β_t measured in gram.

Figure 6: Average Treatment Effects Along the Birth Weight Distribution



Average treatment effect on birth weight for children born in municipalities within 60 km of a lignite mine after 1982. OLS estimates from running separate difference-in-differences regressions at each ventile of the birth weight distribution. We report tests of the identifying assumptions at each ventile in Table B.12.

within 60 kilometers of a lignite mine, infant mortality increases by 9.7% in 1983 (p = 0.114), 3.6% in 1984 (p = 0.544), 15.3% in 1985 (p = 0.016), 9.6% in 1986 (p = 0.087), 7.8% in 1987 (p = 0.204), and 4.6% in 1989 (p = 0.462). Coefficient estimates are small and close to zero before 1982.

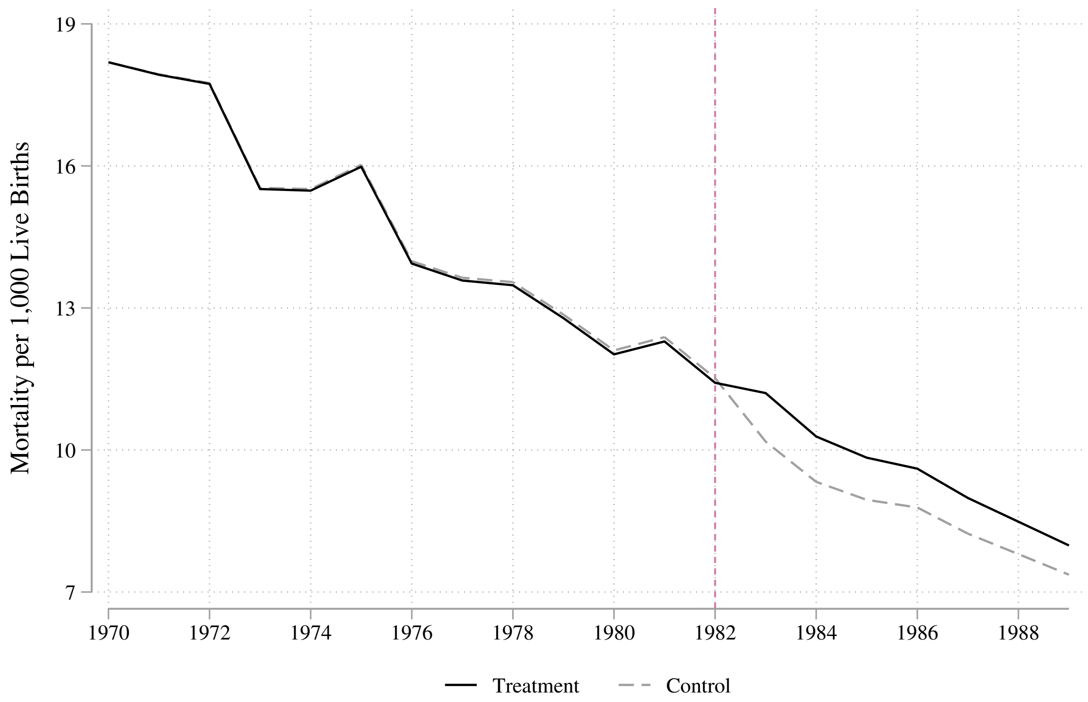
Difference-in-Differences: Average Treatment Effects on the Treated

Difference-in-differences regressions aggregate the dynamic event study estimates to average treatment effects on the treated counties and municipalities (Table 1). OLS estimates indicate large and significant treatment effects in counties and municipalities close to lignite mines. After 1982, air pollution rises by 28.808 $\mu g/m^3$ per year in counties within 60 kilometers of a lignite mine (Table 1, Column 1). This effect corresponds to the difference between the 10th to the 90th percentile of SO_2 monitor locations in the United States in 2017.[21] The estimate indicates a persistent 19.0% increase relative to the treatment group mean in 1981 and a 177% increase relative to the pre-treatment baseline difference between treatment and control counties in 1981. Sulfur dioxide pollution in East Germany in 1981 was comparable to pollution in the United States at the same time: on average, sulfur dioxide monitors in East Germany measured 157.32 ppb pollution in 1981 compared with 165.43 ppb for US monitors according to the EPA.

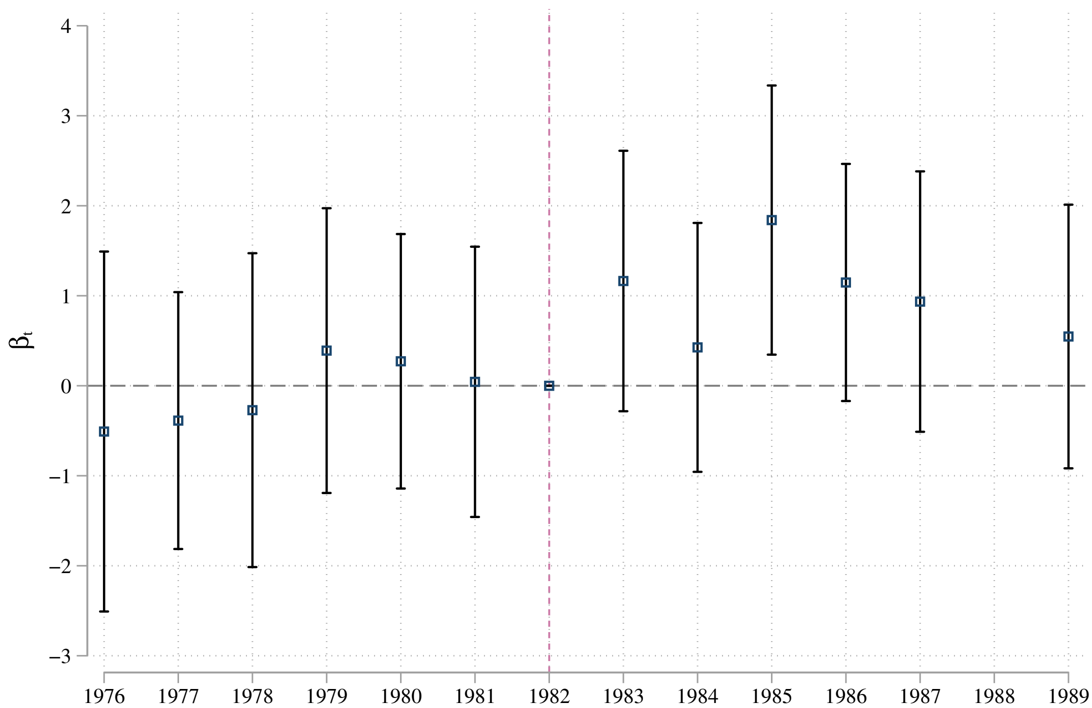
[21]https://www.epa.gov/air-trends/sulfur-dioxide-trends, last accessed 26 July 2025. The 90th percentile of US measurement stations is below 20 $\mu g/m^3$ since 2020.

Figure 7: Average Infant Mortality Rate

(a) Smoothed and Centered Means



(b) Event Study



Panel (a): Mean infant mortality rates per 1,000 live births in counties within 60 km of a lignite mine (treatment) or other counties (control) by birth year. Smoothed and centered means. Panel (b): OLS event study coefficients that compare average infant mortality in East German counties within 60 km of a lignite mine to other counties. 1982 omitted as reference category. β_t measured in deaths per $1,000$ live births.

The average birth weight of infants born within 60 kilometers of a lignite mine is 8.745 grams lower after 1982 (Table 1, Column (2)).[22] The estimated coefficient corresponds to a 0.27% reduction relative to the 1981 average for treated municipalities and 82.2% of the difference between treatment and control averages. Notably, effects are larger at the lower end of the birth weight distribution. Relative to 1981, an infant's risk to be born below the critical value of 2,500 gram increases by 2.7% in municipalities close to lignite mines (Table 1, Column (3)).

Table 1: Difference-in-Differences: Pollution, Birth Weights, Mortality

	SO_2 Pollution	Infant Birth Weights	1[Low Birth Weight]	Infant Mortality
	(1)	(2)	(3)	(4)
1[t>1982] $\times D_i$	28.808***	-8.745***	0.002**	1.076***
	(3.342)	(2.853)	(0.001)	(0.343)
Mean Y	151.583	3,279.517	0.061	11.178
Observations	2,274	2,095,887	2,095,887	2,808
R-squared	0.845	0.013	0.005	0.301

OLS estimates of Equation 1. D_i is an indicator for locations within 60 kilometers of a lignite mine. Robust errors clustered at county-level in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

On average, 1.076 additional infants die per 1,000 live births per year in counties close to lignite mines after 1982. Relative to the 1981 baseline, the infant mortality rate in treated counties rises by 9.0%. The estimated effect corresponds to 149% of the baseline difference between treatment and control counties. In contemporary terms, the increase in infant mortality is equivalent to the difference in infant mortality between the United States and Chile in 2024.[23]

Next, we investigate whether counties with increased air pollution experienced effects on stillbirths (Table 2). In East Germany, stillbirths were defined as infant births without detectable breath or heartbeat. In our context, effects on stillborn infants help isolate the impact of in utero exposure to air pollution from exposure during the first few months after birth. OLS estimates indicate no significant effect on stillbirths. While stillbirths are rare and mothers with increased risk for stillbirths might receive additional medical attention, we do not have evidence that these additional margins would vary between counties close to lignite mines and those further away. Rather, in light of the birth weight and mortality effects, the estimates seem to imply that increased pollution impacted infant health mainly post-birth rather than in utero.

Infant Mortality Elasticities

To compare the magnitude of our findings with other settings, we estimate the elasticity of infant mortality to air pollution. To implement this analysis, we regress the natural logarithm of the infant mortality rate on the natural logarithm of sulfur dioxide pollution readings. We use an indicator for counties close to lignite mines after 1982 as an instrument for pollution levels.

[22] We estimate the average treatment effect on the treated infants using individual level data but aggregate the data to the municipality level in event studies.

[23] https://www.cia.gov/the-world-factbook/field/infant-mortality-rate/country-comparison, accessed 26 July 2025.

Table 2: Difference-in-Differences: Stillbirths, Net Migration

	Stillbirths	Net Migration
	(1)	(2)
1[t>1982] $\times D_i$	-0.159	-87.976
	(0.242)	(70.683)
Mean Y	6.165	-64.811
Observations	2,808	2,604
R-squared	0.208	0.748

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located within 60 kilometer's straight-line distance of the nearest lignite mine. Robust errors clustered at county-level in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table 3: DiD-IV: Infant Mortality Elasticities

Y =	(1) ln(Infant Mort.)	(2) ln(SO_2)	(3) ln(Infant Mort.)	(4) ln(Infant Mort.)	(5) ln(Infant Mort.)
	Reduced Form	First Stage	IV	IV	IV
ln(SO_2)			0.265**	0.501*	0.816*
			(0.118)	(0.269)	(0.434)
$1[t > 1982] \times D_i$	0.097**	0.366***			
	(0.044)	(0.034)			
Unit	deaths per 1,000 births	$\mu g/m^3$	deaths per 1,000 births	deaths per 1,000 births	deaths per 1,000 births
Mean Y	2.114	4.940	2.114	2.114	2.114
Observations	2,070	2,070	2,070	2,070	2,070
Cragg-Donald F			72.846	11.813	25.440
Kleibergen-Paap F			118.347	35.722	26.677

Note: IV estimates of the elasticity of infant mortality to air pollution using $1[t > 1982] \times D_i$ as an instrument for $ln(SO_2)$. Sample restricted to years in which air quality monitor readings are available (1979-89). Column (4) includes a treatment group-specific time trend. Column (5) includes a full set of county-specific time trends. D_i is a binary variable indicating whether a county is located within 60 kilometer's straight-line distance of the nearest lignite mine. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Reduced form estimates of log infant mortality on an indicator for counties close to lignite mines show that infant mortality rates increase by 9.7% in these counties after 1982 (Table 3, Column 1).[24] The first stage results show that air pollution in counties close to lignite mines increases by 36.6% after 1982. First stage results also suggest that the instrument is a strong and meaningful shifter of local air pollution levels with a Kleibergen-Paap F-statistic of 118.35. Finally, Columns (3) to (5) report our results on the infant mortality elasticity, estimated with two-stage least squares. We find that a 1% increase in local SO_2 pollution in East Germany leads to an increase in local infant mortality rates ranging between 0.26% and 0.82%, depending on whether and how we adjust for time trends in the dependent variable. The magnitude of the estimated elasticity is large, also in comparison to other estimates

[24]This coefficient differs from the 9.0% effect we estimate in Table 1 because the IV sample is limited to counties within 100 kilometers of an air quality monitor and years after 1977 when monitor readings are available.

from the literature. Luechinger (2014) reports an elasticity of 0.07 – 0.13 in a sample from Germany, shortly after reunification.[25] Currie, Neidell, and Schmieder (2009) estimate an elasticity for CO pollution in the US of 0.04, Knittel et al. (2016) of 0.146. Our estimates are more comparable in magnitude to estimates from developing countries today. Arceo, Hanna, and Oliva (2016) report an infant mortality elasticity to CO pollution of 0.27 – 0.33 in Mexico City and Chay and Greenstone (2003) report an elasticity of 0.28 for TSP pollution. These results emphasize that - in a setting with little room for mitigation via technological solutions, via relocation or both, the impact of air pollution on infant health is especially high.

Robustness: Short-Term Results

We show that our results are robust to alternative treatment definitions. We vary the threshold distance to the nearest lignite mine from 60 kilometers to 40, 50, 70, and 80 kilometers in Tables B.1 to B.5. Across the board, our estimates remain largely unaffected. The only exceptions are that the birth weights results become statistically insignificant at 80 kilometers and infant mortality results become insignificant at 40 kilometers. These changes in precision appear to be driven by treatment spillover, as shifting the threshold distance reassigns counties between the treatment and control group. When we omit counties instead of allowing them to switch treatment assignment in these analyses, all results remain significant and precisely estimated.

We estimate the specifications for infant mortality using non-parametric distance bins in 15 kilometer increments instead of binary treatment variables in Figure A.4 and find treatment effects until about 60 kilometers.

Table B.6 reports difference-in-differences results with continuous measures of treatment that capture the intensity of local exposure to increased air pollution. First, we measure treatment by the kilometer distance to the nearest lignite mine. Second, we measure treatment by the number of lignite mines that are located within 60 kilometers of the focal area. Both continuous treatment measures confirm our previous findings, although estimates are less precise.

We further find that results are robust to excluding the border regions between East Germany, Poland, and the Czech Republic, suggesting that the effects are not driven by cross-border spillovers between Eastern Bloc members (Tables B.7 and B.8). When we repeat our estimation dropping each of the 15 GDR states one at a time, we find the results unchanged (Figures A.5, A.6, and A.7).

Robustness: Placebo Estimates from Potash Mines and Pseudo-Treatment in Time

The only other abundant natural resource in East Germany was potash. Like lignite mining, potash mining is an exploitative mining industry associated with industrial agglomeration but *without* effects on local air pollution. First, we assign a placebo-treatment as being close to a

[25] Luechinger (2014) studies the infant mortality elasticity to SO_2 pollution using variation from mandated power plant desulfurization on a sample mainly covering reunified Germany – when restrictions limiting individual scope for endogenous mitigation were not longer in place.

potash mine. In a second falsification test, we conduct placebo-tests in time where we assign the oil imports cap to different years before the actual treatment. This way, we assess whether our results are driven by general differences between the treated and control counties rather than the difference-in-differences interaction of timing and lignite proximity. Due to the required pre-treatment data, we only test this for infant mortality.

Tables B.9 and B.10 report the results from placebo-tests on the 1982 air pollution shock, based on distance to potash mines. All coefficients are statistically insignificant and considerably smaller than the effects of our main analyses. Table B.11 reports the results from testing our infant mortality results both with a placebo-in-space and a placebo-in-time. Here, too, none of the estimated coefficients is statistically different from zero and all of them remain economically insignificant.

Robustness: Evidence from Cold Years

Domestic heating with lignite briquettes was an important source of pollution in East Germany. To show that increased lignite usage was the main driver behind increased air pollution, we isolate variation in heating in a triple difference analysis (Gruber 1994; Olden and Møen 2022). Specifically, we use colder years with more frost days as a shifter in the demand for domestic heating. If people heat more in colder winters, years with more frost days should coincide with increased exposure to air pollution – but only *after* the 1982 shock when lignite briquettes are more common in heating. We estimate OLS regressions:

$$Y_{ct} = \alpha_c + \phi_t + \gamma F_{ct} + \beta_1(D_c \times 1[t > 1982]) + \beta_2(D_c \times F_{ct}) \tag{5}$$

$$+ \beta_3(1[t > 1982] \times F_{ct}) + \beta_4(D_c \times 1[t > 1982] \times F_{ct}) + \varepsilon_{ct} \tag{6}$$

The outcome variable Y_{ct} measures average sulfur dioxide pollution, infant birth weights, a low birth weight indicator, or infant mortality in county c (municipalities for birth weight) and year t. County fixed effects α_c capture variation that is county-specific but time invariant, e.g. due to differences in economic activity or geographic characteristics. Year fixed effects ϕ_t capture time variant variation that is shared across counties. F_{ct} counts frost days in county c in year t. Specifically, we measure annual frost days as the deviation from the median number of frost days across East German counties and years (91 days/year). Measuring frost days in deviation from the median has two distinct advantages. First, the omitted category now describes years with average frost. Second, estimated coefficients express effects in days above or below the median (which is relevant in many years) rather than in days above zero (which falls outside the support of our data). We show that the distribution of frost days across counties after 1982 is comparable to the distribution we observe before (Figure A.14). We further include two- and three-way interactions between F_{ct} and D_c, an indicator for counties close to lignite mines, and $1[t > 1982]$, an indicator for years after the 1982 trade shock. The estimated coefficients relate deviations from median frost to the outcome variable: γ captures the effect of an additional day of frost in counties far away from lignite mines before 1983, β_2 captures the difference in this slope in counties close to lignite mines, and β_3 captures nationwide changes to the frost slope in counties far from lignite mines after 1982. The remaining interactions capture treatment effects: β_1 captures level differences in average outcomes in counties close to lignite mines after 1982 for years with an average number of frost days – a proxy for the ATT estimated in our difference-in-difference analysis above. Finally, the triple-difference coefficient β_4 captures extra effects of colder winters in treated counties after the shock, beyond the average treatment effect β_1. Coefficients are identified under the treatment effect homogeneity assumption that the effects of frost are concentrated in the year it occurs. We compute two-way clustered standard errors at the county and year levels to allow for correlation in a county's frost exposure over time and across counties in a given winter.

OLS estimates of Equation 5 replicate the results from our difference-in-differences analysis (Table 4). Specifically, ATT estimates of β_1 show that counties and municipalities close to lignite mines experienced significantly higher air pollution, significantly lower infant birth weights, and significantly higher infant mortality rates after 1982. We also find a numerically comparable effect on the probability of low birth weight, though the coefficient is less precisely estimated (p = 0.14). Triple-difference estimation suggests that the effects of pollution exposure are exacerbated during colder years when demand for heating is higher. After 1982, infant mortality increases by an additional 0.24% per additional frost day in counties close to lignite mines. An additional frost day is also associated with a 0.3 ppb increase in air pollution, though the effect is not statistically significant.

Table 4: Triple Difference Estimates: Colder Years

	SO_2 Pollution	Infant Birth Weights	1[Low Birth Weight]	Infant Mortality
	(1)	(2)	(3)	(4)
γ	0.941	-0.056	0.000	-0.030**
	(1.006)	(0.200)	(0.000)	(0.013)
β_1	24.311**	-7.405**	0.002	1.128***
	(10.402)	(2.829)	(0.001)	(0.312)
β_2	-0.340	0.080	0.000	-0.018**
	(0.459)	(0.137)	(0.000)	(0.007)
β_3	-0.384	-0.005	0.000	0.038***
	(0.713)	(0.117)	(0.000)	(0.013)
β_4	0.302	0.084	-0.000	0.027**
	(0.507)	(0.158)	(0.000)	(0.013)
Mean Y	153.520	3279.514	0.061	12.958
Observations	2,109	2,095,703	2,095,703	4,104
R-squared	0.853	0.013	0.005	0.389

OLS estimates of Equation 5. D_i is an indicator for locations within 60 kilometers of a lignite mine. Robust errors clustered at county-level in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

4.3 Long-Term Effects

Ordinary Least Squares Regressions

We now examine the long-term effects of air pollution on individual labour market outcomes of East German workers. We begin with all individuals we observe in East German social security data in 1991 and 1992. We separate this pooled cohort in two groups: individuals who spent life under socialism in counties within 60 kilometers of a lignite mine and those who did not. Tracing employment and unemployment in these groups over time, we find that the post-reunification period is generally marked by economic difficulties for East German workers (Figure 8). Employment rapidly decreases and unemployment drastically rises. These developments are stronger for individuals who spent life under socialism in counties close to lignite mines. They experience a larger increase in relative unemployment and a slightly larger decrease in relative employment.

Next, we compare individuals' labor market trajectories between 1992 and 2020 conditional on observable characteristics (age, sex, education, occupation, industry, state) at worker-level at baseline (Table 5). In the first 28 years after German reunification, individuals who, under socialism, lived close to lignite mines spend significantly less time in employment: 0.241 years or almost 3 months relative to the individuals from areas far away from lignite mines (Column 1). They instead spend more time in unemployment (Column (2)) and retire slightly earlier (Column (3)). We find no significant effect on wages (Column (4) and (5)). We report results with alternate sets of covariates in Tables B.13 to B.17.

Table 5: OLS Regression Results for Long-Term Outcomes

	Employment	Unemployment	Retirement Age	Wages	ln Wages
	(1)	(2)	(3)	(4)	(5)
D_c	-0.241***	0.178***	-0.047**	0.108	0.001
	(0.0679)	(0.053)	(0.020)	(0.489)	(0.008)
Unit	Years	Years	Years	EUR	ln EUR
Mean Y	15.849	3.865	62.272	63.844	4.023
Observations	6,108,837	6,108,837	6,108,837	6,108,837	6,095,277
R-squared	0.440	0.133	0.063	0.350	0.374

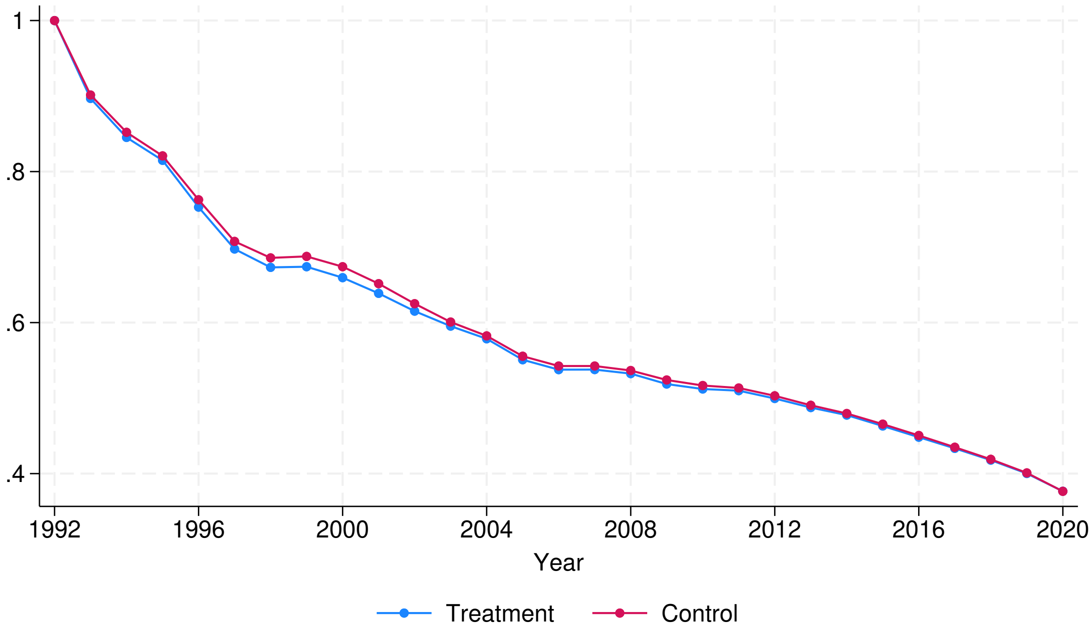
OLS estimates of Equation 2. D_c is a binary variable indicating whether a county is located within 60 kilometer's straight-line distance of the nearest lignite mine. All Columns include additional controls for year-of-birth, a state fixed effect, level of education, sex, five-digit occupational code fixed effects, and three-digit industry code fixed effects. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Inverse Movers Design

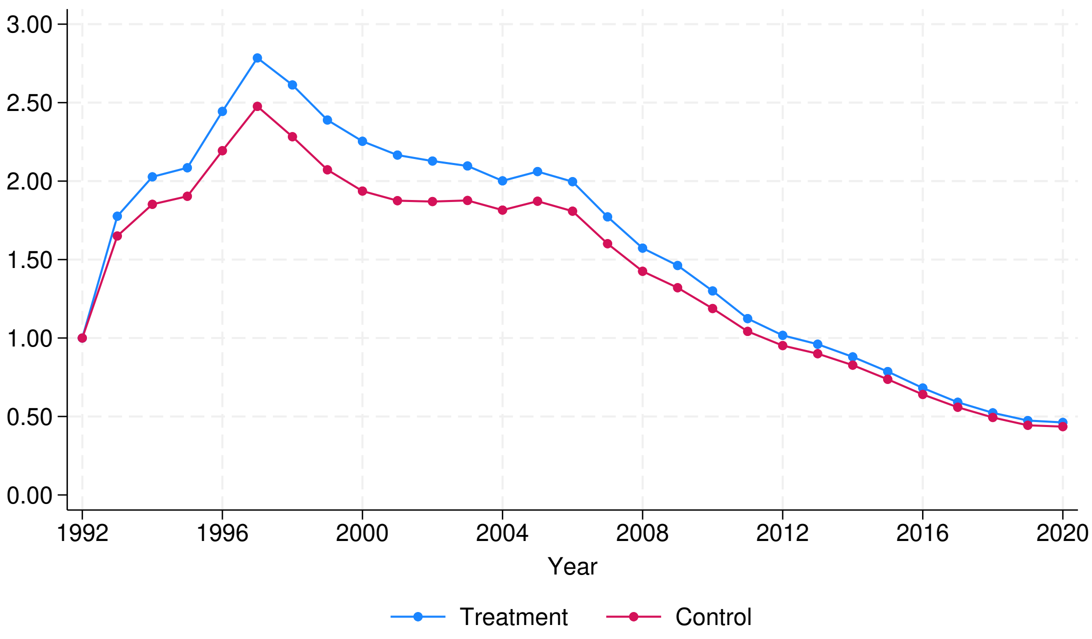
The OLS estimates in Table 5 only reflect the causal effect of air pollution on labor market outcomes if treatment and control groups are comparable except for their pollution exposure. As we discuss in Section 3, this might not be the case due to (1) post-reunification and (2) pre-reunification differences.

Figure 8: Indexed Relative Labor Market Performance, 1991/92 Cohort

(a) Employment



(b) Unemployment



Employment and unemployment figures for the 1991/92 cohort relative to the 1992 baseline over time. Individuals are assigned to the treatment or control group depending on whether their first location in post-reunification social security data is within 60 kilometer's straight-line distance from the nearest lignite mine or not.

To overcome these identification challenges, we use an inverse movers design (Section 3). Specifically, we isolate historical variation in pollution exposure on individuals who migrate immediately after German reunification. Intuitively, we compare pairs and groups of individuals who move to the same destination region directly after reunification. This allows us to account for potentially differential effects of reunification and economic transformation with a destination county fixed effect.[26] Each group consists of at least one worker who moves from a treated origin, exposed to historical air pollution increases, and one who moves from a control origin. Comparing the universe of these groups conditional on a rich set of baseline observables allows us to estimate the causal effect of historical air pollution on labor market outcomes. In this initial analysis, coefficients are identified under the assumption of no unobservable pre-reunification differences between origin regions that correlate with future labor market success. We relax this assumption in additional analyses.

Estimating the movers design on the 144,338 East Germans who move to another county in Germany by 1993, we find that historical air pollution significantly decreases employment in the long-term (Table 6, Column (1)). The estimated employment reduction over the 28 years after German reunification is 54% larger than OLS estimates. Treated individuals spend 0.372 fewer years, almost 4.5 months, in employment compared with individuals in the same destination county but from an origin county in the control region. The estimated effect on unemployment is considerably smaller than the OLS estimate and is not statistically significant (Column (2)). We find, however, that pollution exposure has a large effect on retirement (Column (3)). Treated individuals retire at a significantly younger age than individuals from control counties, on average 0.163 years or two months earlier. The estimated coefficient is almost four times larger than the OLS estimate and may in part explain the insignificant estimate for unemployment. Jointly, the results indicate that individuals exposed to air pollution in East Germany leave the labor force altogether instead of spending time in unemployment. The movers design also shows that treated individuals earn significantly lower wages than individuals who live in the same county but are from the control regions. Over the 28 post-reunification years, treated individuals earn 2.05 EUR (Column (4)), or 3.0% (Column (5)) lower daily wages than the control group. We report results with alternate sets of covariates in Tables B.18 to B.23.

Back-of-the-Envelope Calculation: Social Cost of Long-Term Air Pollution

What is the social cost of the labor market effects of air pollution? To answer this question, we conduct a back-of-the-envelope calculation of the total cost to the social security system in East Germany. Based on our estimates (Table 6), workers average 0.372 fewer years in employment 1993-2020. At 220 working days/year, individuals receive transfers from the social security system on 81.84 additional days rather than paying into it. At an assumed tax rate of 25% on counterfactual average daily wages (EUR 70.05), EUR 1,433.10 of tax revenue is lost per worker. If workers instead receive benefits at 40% of their observed average daily wages (EUR 68.00), additional transfers sum to EUR 2,225.70 per worker. Since half of the 6.1 million workers in our sample are exposed to the pollution shock, social security costs of lost employment alone sum to EUR 10.976 billion (in 2015 prices) – close to 1% of West Germany's gross domestic

[26] As we condition on pairs of individuals who spend their entire post-reunification life (1993 onwards) in a fixed location, we absorb any place-based variation related to post-reunification location choices.

Table 6: Inverse Movers Design Results for Long-Term Outcomes

	Employment	Unemployment	Retirement Age	Wages	ln Wages
	(1)	(2)	(3)	(4)	(5)
D_c	-0.372***	0.088	-0.163**	-2.054**	-0.030***
	(0.129)	(0.059)	(0.068)	(0.807)	(0.010)
Unit	Years	Years	Years	EUR	ln EUR
Mean Y	14.185	3.497	61.490	67.996	4.020
Observations	144,338	144,338	144,338	144,322	144,338
R-squared	0.369	0.140	0.086	0.373	0.356

OLS estimates of Equation 3. D_c is a binary variable indicating whether a county is located within 60 kilometer's straight-line distance of the nearest lignite mine. All columns include a set of destination county fixed effects to account for differential post-reunification effects. All Columns include additional controls for year-of-birth, level of education, sex, five-digit occupational code fixed effects, and three-digit industry code fixed effects. County-level clustered standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

product in 1989, the last year before reunification. Notably, this estimate is a lower bound that does not account for the welfare cost of personal loss of income, dynamic wage penalties, costs to the health sector, losses in quality of life, losses in the non-worker population, or welfare losses that already occurred under socialism.

Effect Heterogeneity by Age

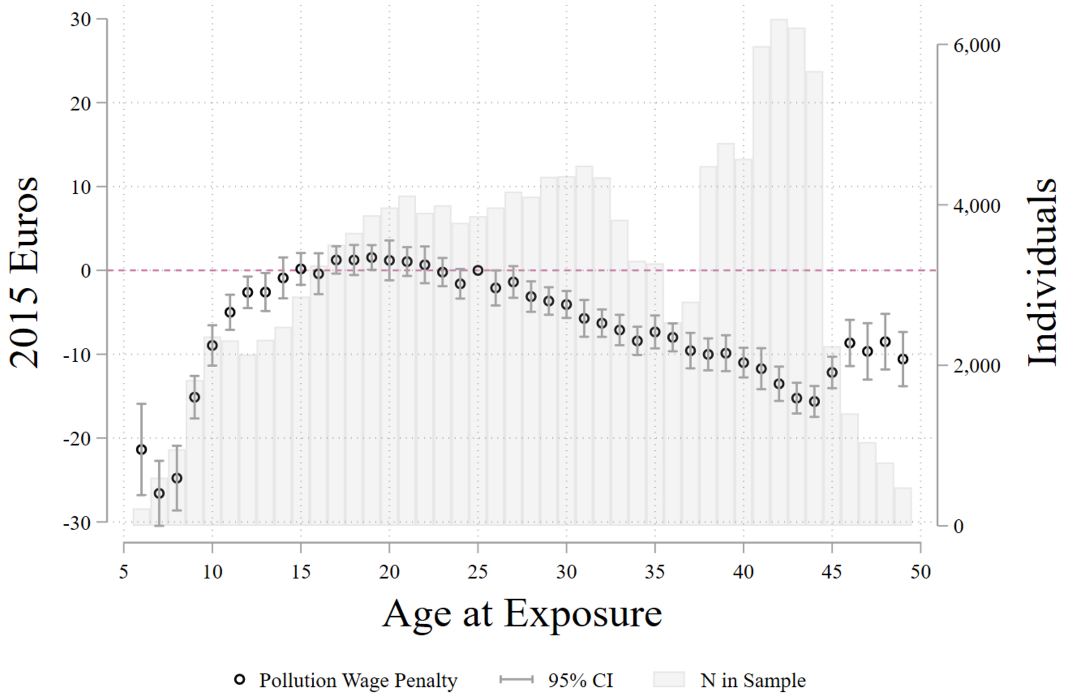
Studies on the long-term labor market effects of air pollution typically compare individuals born just before or after specific cutoff dates to generate variation in early life exposure (Isen et al. 2017; Voorheis et al. 2017; Colmer et al. 2022). Our setting extends this evidence in two ways. First, our setting captures sustained differences in pollution exposure that last years rather than weeks or months. Second, our setting allows us to identify effects across the entire age distribution, including on individuals who were already adults when first exposed.

We estimate treatment effect heterogeneity by age in a modified version of our movers design. Specifically, we interact the treatment variable, indicating individuals who spent their life under socialism in areas exposed to the lignite shock, with individuals' age in 1982. Estimates of variation in air pollution wage penalties with age show an inverse U-shape relationship (Figure 9). We find the largest estimated air pollution effects for the youngest *and* oldest individuals in our sample. Importantly, the effect is not restricted to infants and the elderly. Instead, estimates show significant effects for children up to 13 years of age and for adults as young as 28 years.

Effects on Individuals at Vulnerable Ages

The finding that pollution exposure effects vary by age allows us to strengthen identification in the movers design. Specifically, we can use variation in age at first exposure to control for destination-by-origin fixed effects that absorb place-based variation both before and after migration. These 'flow' controls exploit variation in vulnerability to air pollution within groups of movers in the same origin-destination county pair. These estimates account for identification

Figure 9: Treatment Effect Heterogeneity by Age



OLS coefficients from movers design with treatment exposure interacted with individuals' age in 1982.

concerns related to both (1) post-reunification and (2) pre-reunification differences between treatment and control groups.

To explicitly rule out that our findings are driven by differences between origin or destination counties, we leverage effect heterogeneity by individuals' age at first exposure. We define age cutoffs to capture when individuals are particularly vulnerable to air pollution based on the inverse U-shaped relationship between age at first exposure and wage effects (Figure 9). We define two versions of the vulnerability treatment. First, we define individuals as vulnerable if their birth year is below the 5th (at most 11 years old in 1982) or above the 95th percentile of the birth year distribution (at least 44 years old). Second, we account for the longer tail of the age distribution and lower the upper age restriction to the 75th percentile (at least 39 years old). Because the vulnerability treatment varies within migration origin regions, we can use it to estimate the inverse movers design with destination-by-origin fixed effects. These fixed effects absorb both push and pull factors of migration. Intuitively, we now compare individuals who move from the same origin to the same destination region ('flow'). The treatment variable captures whether movers were of particularly vulnerable age when first exposed. OLS estimates with destination-by-origin fixed effects indicate that movers of vulnerable age earn significantly lower wages than other movers (Table 7). Individuals of vulnerable age average up to 12.7% lower daily wages in their post-reunification career.[27]

[27] Because the vulnerability treatment is based on individuals' year of birth, we can only estimate these effects for wages and not for retirement and time spent in the labor force.

Table 7: Inverse Movers Design: Age Treatment with Destination-by-Origin FE

	(1) Wages	(2) Wages	(3) ln Wages	(4) ln Wages
< p5 or > p75	-7.229***		-0.127***	
	(0.265)		(0.005)	
< p5 or > p95		-6.135***		-0.120***
		(0.312)		(0.005)
Origin-by-Destination FE	✓	✓	✓	✓
Level of Education FE	✓	✓	✓	✓
Sex FE	✓	✓	✓	✓
Occupation FE	✓	✓	✓	✓
WZ08 3-digit FE	✓	✓	✓	✓
Mean Y	65.953	65.953	3.987	3.987
Observations	76,929	76,929	76,919	76,919
R-squared	0.451	0.448	0.405	0.403

OLS estimates of Equation 3. $VulnerableAge_i$ is an indicator for individuals in an age group particularly vulnerable to air pollution exposure. All columns include a set of destination-by-origin county fixed effects to account for migration push, migration pull, and post-reunification place-based effects. All columns include additional controls for year-of-birth, level of education, sex, five-digit occupational code fixed effects, and three-digit industry code fixed effects. The sample is limited to counties within 60 kilometers of a lignite mine. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Origin County Characteristics

Next, we compare the observable characteristics of treatment and control counties (Table 8). We compare movers' origin counties in OLS regressions, weighted by each origin counties' share of movers in our sample. Using data for 1989, the last full year of socialism in East Germany, we regress county characteristics on an indicator for counties within 60 km of a lignite mine. Estimates show that the treatment and control origin counties are relatively well balanced. We find some differences in counties' age and labor force composition (Column (1)). Treatment origin counties have a significantly share of local workers in the energy, chemicals and engineering industries. All but one of these differences, however, disappear, when we condition on state fixed effects (Column (2)). The remaining difference is that treatment counties have a higher energy sector employment share. To confirm that this difference does not affect our results, we estimate movers design regressions that exclude all energy sector workers (power plant workers, coal miners, etc.). Estimates that exclude energy workers are almost identical to the main estimates and, if anything, slightly larger (Table B.22).

We use data on government investment by sector to study the Socialists' policy response to the sudden reduction in Soviet oil exports. After 1982, the government spent more on installations and equipment for the coal mining, electricity generation, and heating sectors. In contrast, we observe no meaningful change in labor force composition. Comparing employment in coal mining with employment in other types of mining, we find that employment in coal

Table 8: Balance Tests: Characteristics of Movers' Origin Counties

	(1)	(2)
Demographics		
Log Mean Population, 1980-88	0.039	-0.077
	(0.386)	(0.174)
Log County Size, 1989	-0.122	-0.106
	(0.208)	(0.379)
Share of Population < 15y, 1989	-1.875***	-0.250
	(0.342)	(0.347)
Share of Population > 64y, 1989	2.928***	0.661
	(0.442)	(0.509)
Political Favouritism		
SED Party Membership	0.560	1.718
	(2.006)	(2.207)
March '90 Voter Turnout	0.222	-0.243
	(0.506)	(0.524)
December '90 Voter Turnout	0.340	-0.276
	(0.856)	(1.152)
Industrial Production, Education and Innovation		
Log Industrial Output, 1989	0.408	-0.132
	(0.429)	(0.298)
Patents Filed, 1989	-63.860	45.583
	(222.474)	(32.074)
I[University or College]	0.008	-0.006
	(0.156)	(0.189)
Number of Universities or Colleges	0.056	-0.235
	(1.091)	(0.461)
Labor Force Composition		
Employment Share Agriculture, 09/89	-1.558	1.061
	(2.267)	(3.155)
Employment Share Energy, 09/89	2.929**	4.462**
	(1.291)	(2.230)
Employment Share Chemicals, 09/89	2.324**	1.229
	(1.151)	(1.077)
Employment Share Metal, 09/89	-0.084	-1.475
	(0.666)	(0.994)
Employment Share Engineering, 09/89	4.029***	0.312
	(1.498)	(1.839)
Employment Share Water and Sewage, 09/89	-0.054	-0.006
	(0.075)	(0.068)
Employment Share Electronics, 09/89	-1.128	-1.904
	(1.660)	(2.000)
Employment Share Mgmt, Planning and R&D, 09/89	0.308	0.070
	(0.545)	(0.604)
Share of Cooperative Workers, 09/89	-0.725	0.497
	(1.709)	(2.350)
Observations	216	216
State FE		✓

OLS estimates for each characteristic on an indicator for counties located within 60 km of a lignite mine, weighted by the share of movers that originate from each county. *** p < 0.01 ** p < 0.05 * p < 0.1

mining increased by 5.6% after 1982 (Figure A.9). This increase, however, is stretched out over multiple years. We do not observe any changes in employment in the electricity and heating sectors around 1982 (Figure A.10). Instead of increasing employment, the government raised investment in installations and equipment for the coal mining, electricity and heating sectors. While public investment in coal mining and in other mining follows an identical trend until 1982, investment in coal mining increases significantly after the shock (Figure A.11). Similarly, public investment in electricity and heating increased relative to other utilities like water and gas supply (Figure A.12).

Selection into Migration

A potential challenge to the identifying assumptions of the movers design is differential selection into migration between treatment and control origins. If historical air pollution exposure is correlated with migration decisions, the individuals emigrating from treatment origins may not be comparable to those from control origins. Specifically, if emigrants from treatment origins have lower latent labor market potential than individuals from control counties irrespective of historical air pollution exposure, we may misattribute differences in innate preconditions for labor market success to air pollution. In the logic of a spatial sorting model, this challenge concerns the differential impact of *push* and *pull* factors of migration. The inverse movers design accounts for pull factors with migration-destination fixed effects. As we explicitly limit comparisons to individuals who move to the exact same place at the same time, there is no scope for differential pull to affect our results. Similarly, we include destination-by-origin fixed effects when we estimate the movers design with treatment defined by age-related vulnerability. In this case, we condition on migration between the same origin-destination county pair so that differential push and pull factors are absorbed by fixed effects.

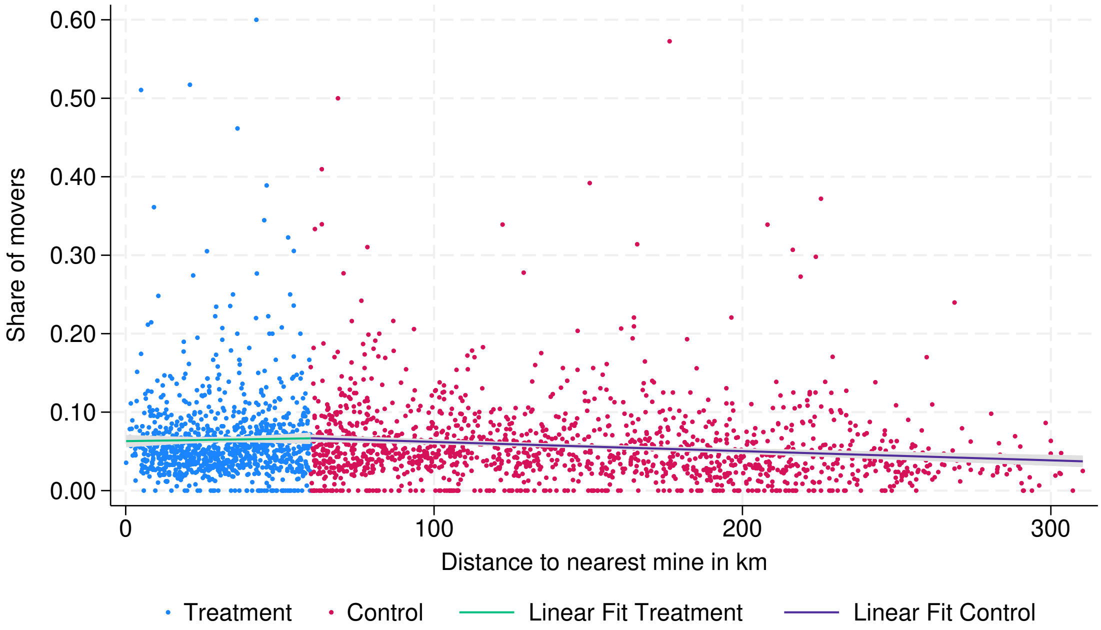
To further investigate selection into migration, we study municipality-level outmigration rates after reunification by distance to the nearest lignite mine (Figure 10). We observe no changes in emigration rates around the distance cutoffs we use to define treatment (60 km). While the probability that workers migrate marginally decreases for municipalities that are more than 150 kilometers from a lignite mine, this change is driven by lower migration rates in coastal regions.

We also assess variation in the timing of migration for individuals moving exactly once over the post-reunification timeline (Figure A.8). The frequency of these migration patterns peaks in the first post-reunification year which suggests that most relocation choices are driven by the macro push effect of reunification and reinstated freedom of movement, rather than later realizations of pollution effects.

Robustness: Placebo Estimates from Potash Mines

Beyond lignite, potash was the other domestically available natural resource in East Germany. Like lignite, potash mining is an exploitative industry associated with industrial agglomeration. Unlike lignite, potash is not associated with air pollution. To validate that our results are not

Figure 10: Selection into Migration



Share of East German workers moving to a different county by 1993 plotted against distance to each municipality's nearest lignite mine. Three municipalities with a mover share above 0.60 omitted.

driven by place-based effects of mining culture or exploitative industries, we construct a placebo treatment based on counties' distance to potash rather than lignite mines. We assign counties to the placebo if their geographical centroid is within 60 kilometers of the nearest potash mine (Figure A.2). We then estimate an inverse movers design, equivalent to our estimates for lignite (Table 6), that restricts our sample to groups of individuals who are comparable on observables, are differentially exposed to our placebo treatment under socialism, and move to the exact same migration destination right after German reunification. OLS estimates for all coefficients are statistically insignificant and substantially smaller than the estimates for lignite (Table 9).

Table 9: Movers Design: Placebo Effect of Distance to Potash Mines in East Germany

	(1) Employment	(2) Unemployment	(3) Retirement Age	(4) Wages	(5) Ln Wages
D_p	-0.147	0.075	-0.057	-0.502	-0.002
	(0.139)	(0.067)	(0.064)	(0.661)	(0.009)
Year of Birth FE	✓	✓	✓	✓	✓
Destination FE	✓	✓	✓	✓	✓
Level of Education FE	✓	✓	✓	✓	✓
Sex FE	✓	✓	✓	✓	✓
Occupation FE	✓	✓	✓	✓	✓
WZ08 3-digit FE	✓	✓	✓	✓	✓
Mean Y	14.187	3.497	61.490	68.002	4.020
Observations	144,352	144,352	144,352	144,352	144,336
R-squared	0.369	0.140	0.086	0.373	0.355

Note: OLS placebo estimates of Equation 3. P_c is a placebo treatment indicator for counties within 60 kilometer's straight-line distance of the nearest potash mine. All columns include destination county fixed effects to account for differential post-reunification effects. All columns include additional controls for year-of-birth, level of education, sex, five-digit occupational code fixed effects, and three digit industry code fixed effects. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

5 Mechanism: Air Pollution and Long-Term Health

Research in medicine and economics documents that air pollution affects health, including our results in section 4.2 above. To test health as the mechanism behind long-term labor market effects (Section 4.3), we estimate OLS regressions on healthcare utilization and health diagnoses using survey data from the German Socio-Economic Panel.

OLS estimates indicate that individuals who lived in treatment counties under socialism have significantly higher healthcare utilization in 1992, ten years after the initial shock (Table 10). Compared with individuals from control counties, treated individuals are 20.9 percentage points more likely to have visited a GP in the three months before they were surveyed. These individuals are also 21.5 percentage points more likely to have taken at least one sick day at work.

Next, we estimate the long-term health effects of sustained air pollution exposure. To implement this analysis, we study health diagnoses surveyed in 2011, 29 years after the initial shock. OLS estimates indicate that, conditional on place of residence in the survey year, individuals who lived in treated counties under socialism are significantly more likely to suffer from pollution related conditions than individuals from control counties (Table 11). Specifically, we find that treated individuals are 12.6 percentage points more likely to have a diagnosis for asthma and 12.8 percentage points more likely to have a diagnosis for cardiopathy by 2011. Relative to the sample average, the asthma incidence is 229% higher for individuals from treated counties relative to individuals from control counties. The cardiopathy incidence is 83% higher.

Importantly, we do not find differential health effects when we investigate medical conditions that should be *un*related to air pollution. Specifically, we do not find that individuals from treated

Table 10: G-SOEP: OLS Healthcare Utilization 1992

	GP Visit (1/0)	Sick Day at Work (1/0)
	(1)	(2)
D_i	0.209**	0.215**
	(0.098)	(0.105)
YoB-by-gender FE	✓	✓
County FE	✓	✓
Mean Y	0.690	0.233
Observations	3,635	3,635
R-squared	0.201	0.225

OLS estimates. The treatment variable D_i indicates individuals who lived in a county within 60 kilometers of a lignite mine in 1990. GP visits refer to GP visits in the last 3 months before the survey data, sick days refer to the year before the survey date. Column (2) includes an additional control for employment. All specifications are weighted by G-SOEP survey weights. All specifications control for residence in the survey year. Standard errors two-way clustered at the household and 1990 residence county level. *** p < 0.01 ** p < 0.05 * p < 0.1

counties are more likely to have a diagnosis for diabetes or chronic back pain. We show that these findings are robust to pooling the 2011 cross section with the 2013, 2015, and 2017 cross sections in Table B.24. Lastly, we show that survey responses indicate economic effects that are comparable to what we find in our movers analyses using social security data (Table B.25).

Table 11: G-SOEP: OLS Health Outcomes 2011

	Asthma	Cardiopathy	Diabetes	Chronic Back Pain
	(1)	(2)	(3)	(4)
D_i	0.126*	0.128*	-0.032	0.000
	(0.064)	(0.074)	(0.057)	(0.071)
YoB-by-gender FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Mean Y	0.055	0.154	0.143	0.249
Observations	1,329	1,329	1,329	1,329
R-squared	0.259	0.341	0.279	0.255

Note: Cross-sectional OLS estimates. The treatment variable D_i is whether individuals lived in a county within 60 kilometers of a lignite mine in 1990. All specifications are weighted by G-SOEP survey weights. All specifications control for residence in 2011. Standard errors two-way clustered at the household and 1990 residence county level. *** p < 0.01 ** p < 0.05 * p < 0.1

6 Conclusions

When the Soviet Union capped East German access to imported oil in 1982, East Germany rapidly ramped up the domestic production of lignite for use in private households and industrial production. Lignite is a low-quality, highly polluting type of coal that is costly to transport due to its high moisture content and crumbly texture. We have shown that increased used of lignite caused a large increase in air pollution in areas close to lignite mines. Due to the authoritarian nature of the East German regime, individuals could not respond by moving away, changing jobs, or sorting through the housing market. We have shown that this air pollution shock had large and persistent negative effects on the local population's health and long-term labor market outcomes. Pollution caused an immediate increase in infant mortality and low birth weight probabilities, as well as an immediate decrease in average infant birth weights compared with areas further away from lignite mines. Effect magnitudes are larger than those found in other industrialized countries and comparable to effects found in developing countries today. These findings show that that a lack of technology to mitigate the adverse effects of air pollution and the inability to move away to cleaners air increases the adverse effects of air pollution. Pollution effects may be underestimated in alternative settings where individuals can respond to exposure, for instance through residential sorting. We have found that there are also large long-term effects on the health and labor market outcomes of the affected individuals. A conservative back-of-the-envelope calculation indicates that the economic damage from the long-term labour market effects alone amount to almost one percent of West Germany's GDP in 1989.

Our paper also carries an important lesson for contemporary politics. Similar to the early 1980s, the Russian invasion of Ukraine on 24 February 2022 forced countries dependent on imports of Russian fossil fuels to rapidly adjust their energy mix. The experience of Socialist East Germany in the early 1980s is a cautionary tale of how dependence on unreliable trade partners can carry substantial negative side effects. It also illustrates that replacing one fossil fuel with another may result in additional externalities imposed on the local population.

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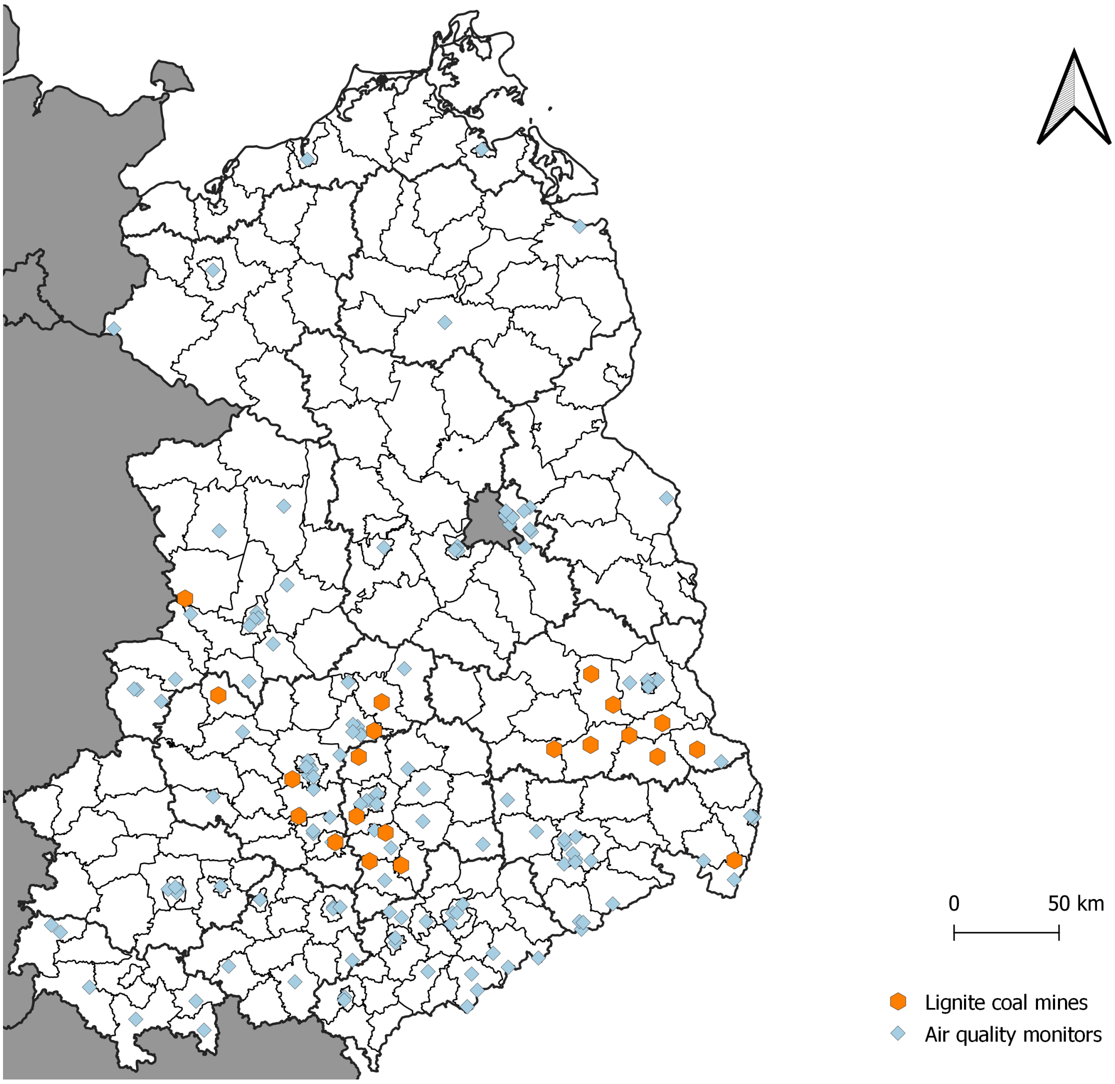
Supplemental Appendix

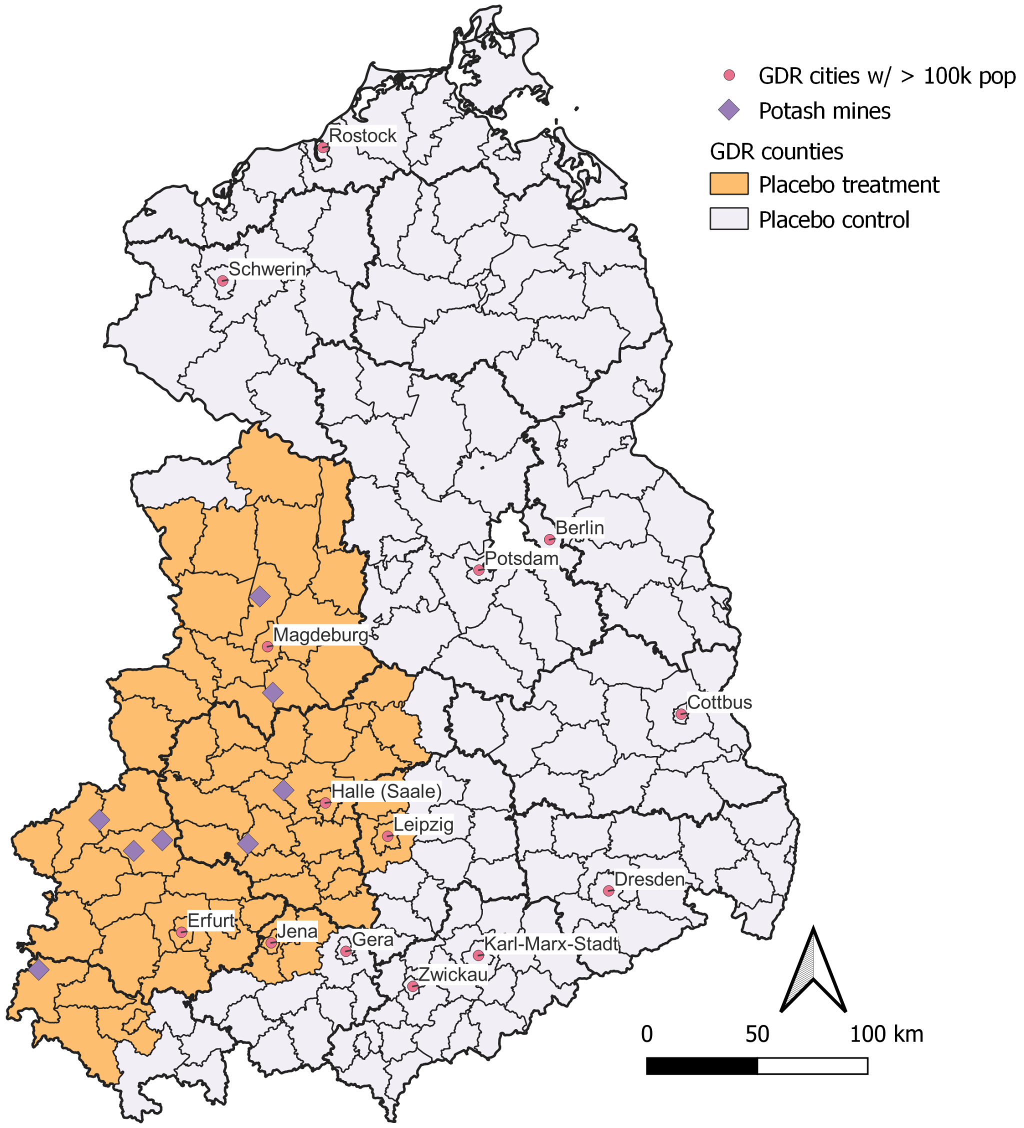
A Additional Figures

Figure A.1: Location of Air Quality Monitors and Lignite Mines



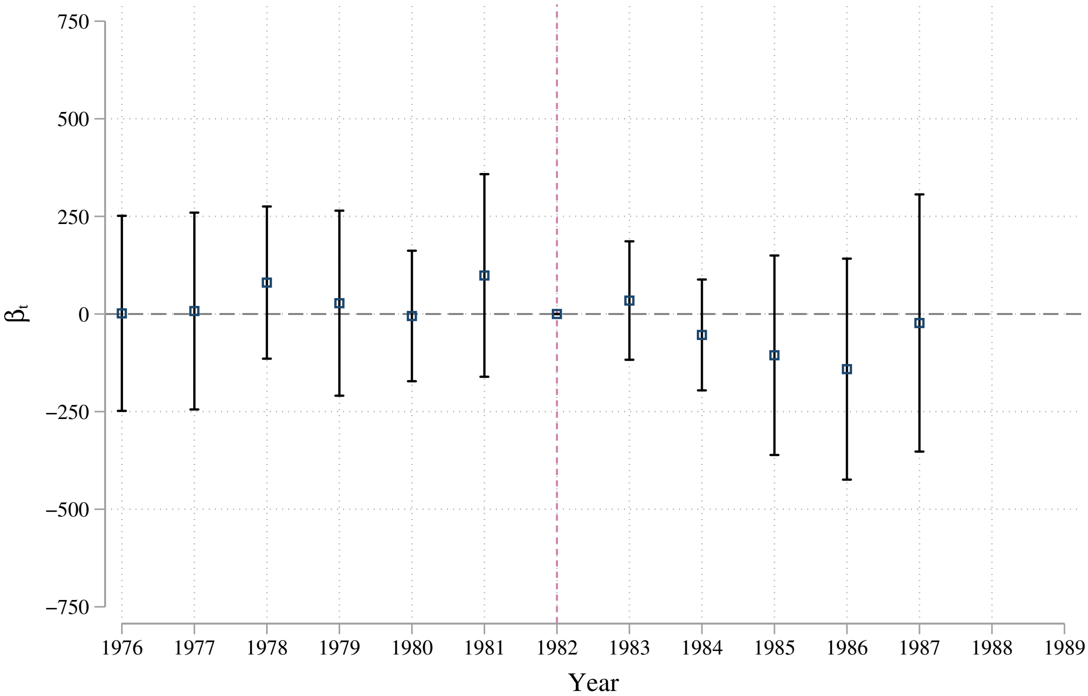
Geocoded locations of lignite mines and air quality monitors in East Germany. We use inverse distance-weighted interpolation to assign annual average sulfur dioxide concentration readings to the geographical centroids of all counties that have at least one air quality monitor within 100 km.

Figure A.2: Placebo Test: Location of Potash Mines in East Germany



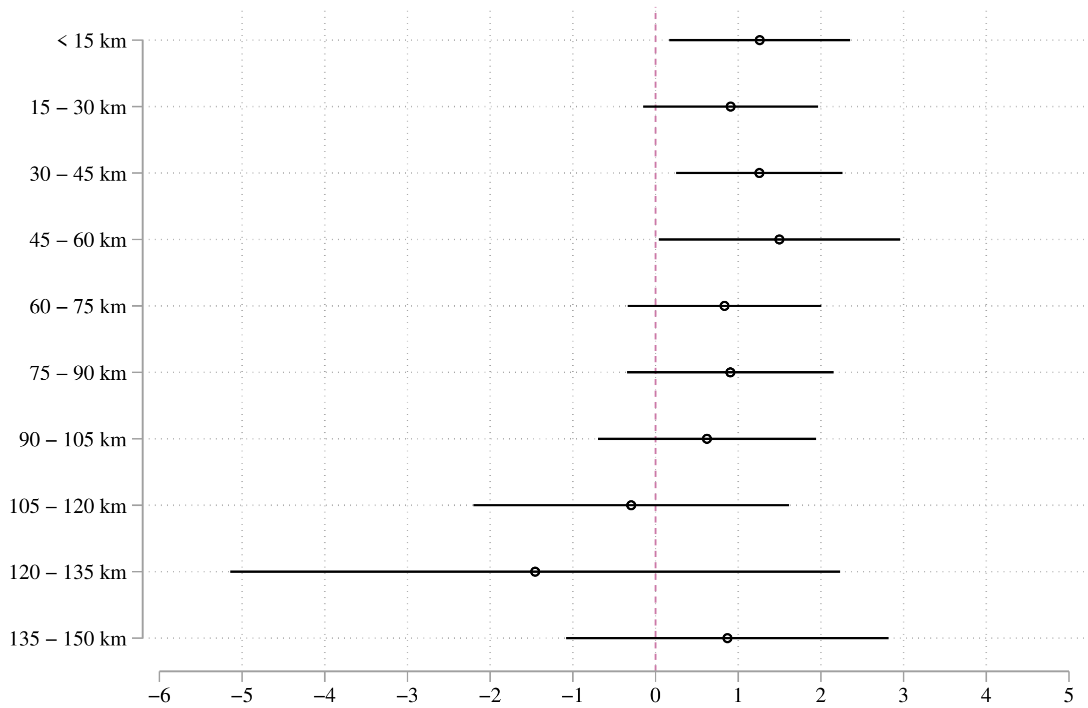
We assign counties to a placebo treatment of control status depending on whether their geographical centroid is within 60 km of a potash mine.

Figure A.3: Average Net Migration



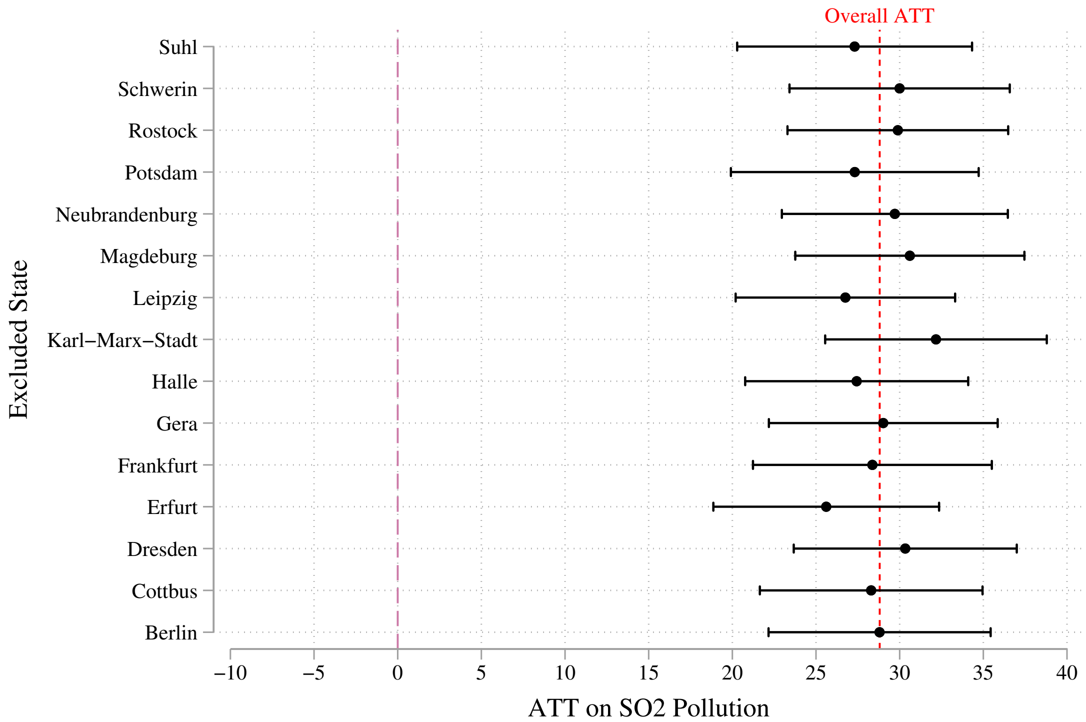
Coefficient plot for an event study difference-in-differences regression of mean net migration in GDR counties on a binary treatment. Treatment is defined as whether or not a county's geographical centroid is within 60 kilometers of the nearest lignite mine and whether the year is greater than 1982. The coefficient for 1982 is omitted as a reference category. The coefficients β_t are measured in individual migrants to treatment counties.

Figure A.4: Non-Parametric 15 km Distance Bins: Infant Mortality per 1,000 Live Births



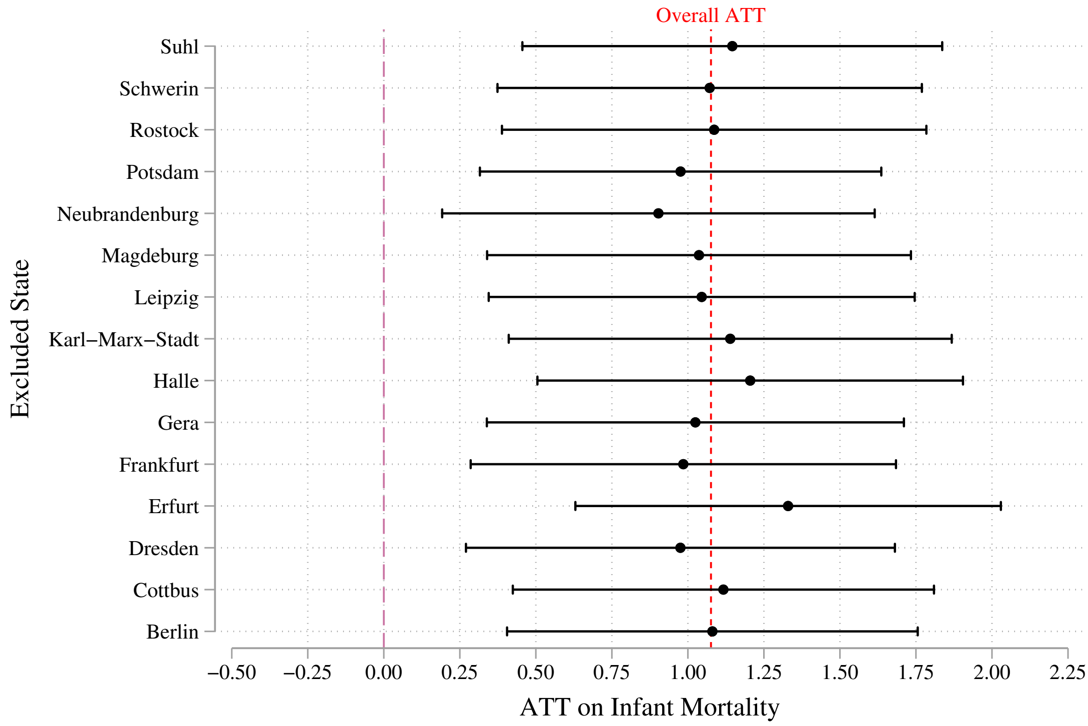
Coefficient plot for a difference-in-differences regression of mean infant mortality in GDR counties on a treatment timing indicator interacted 15 kilometer distance bins.

Figure A.5: SO_2 Pollution Excluding One State at a Time



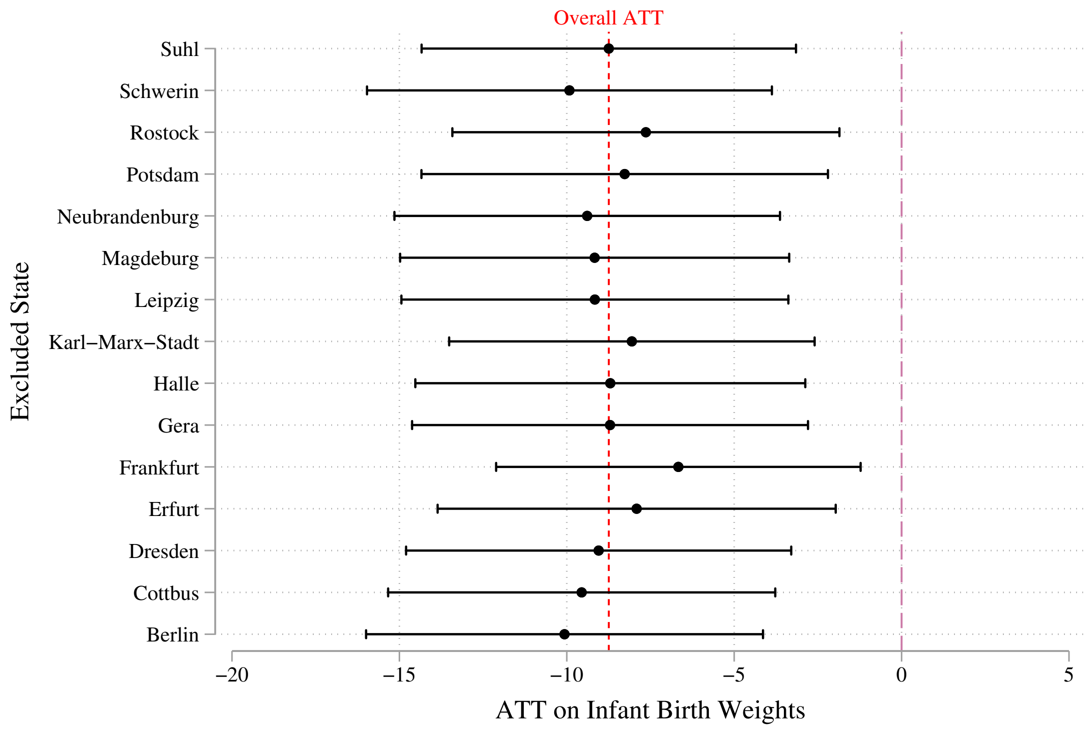
Point estimates and 95% CI for counties within 60 km of a lignite mine after 1982 in diff-in-diffs regressions of mean air pollution in East German counties. We iteratively omit one of the 15 states of East Germany.

Figure A.6: Infant Mortality Excluding One State at a Time



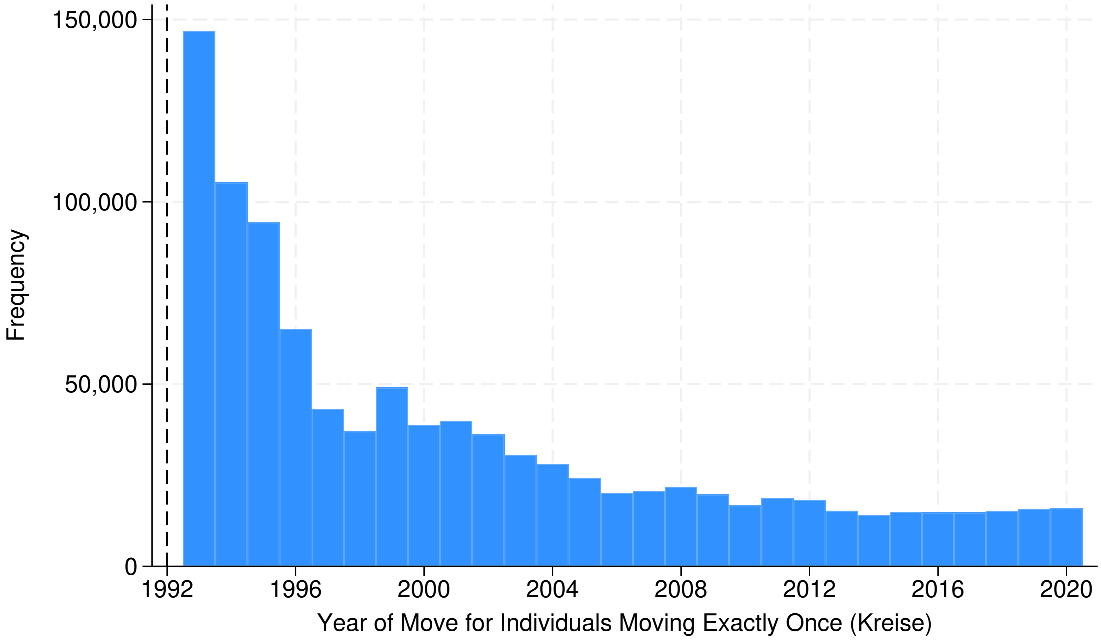
Point estimates and 95% CI for counties within 60 km of a lignite mine after 1982 in diff-in-diffs regressions of mean infant mortality in East German counties. We iteratively omit one of the 15 states of East Germany.

Figure A.7: Infant Birth Weights Excluding One State at a Time



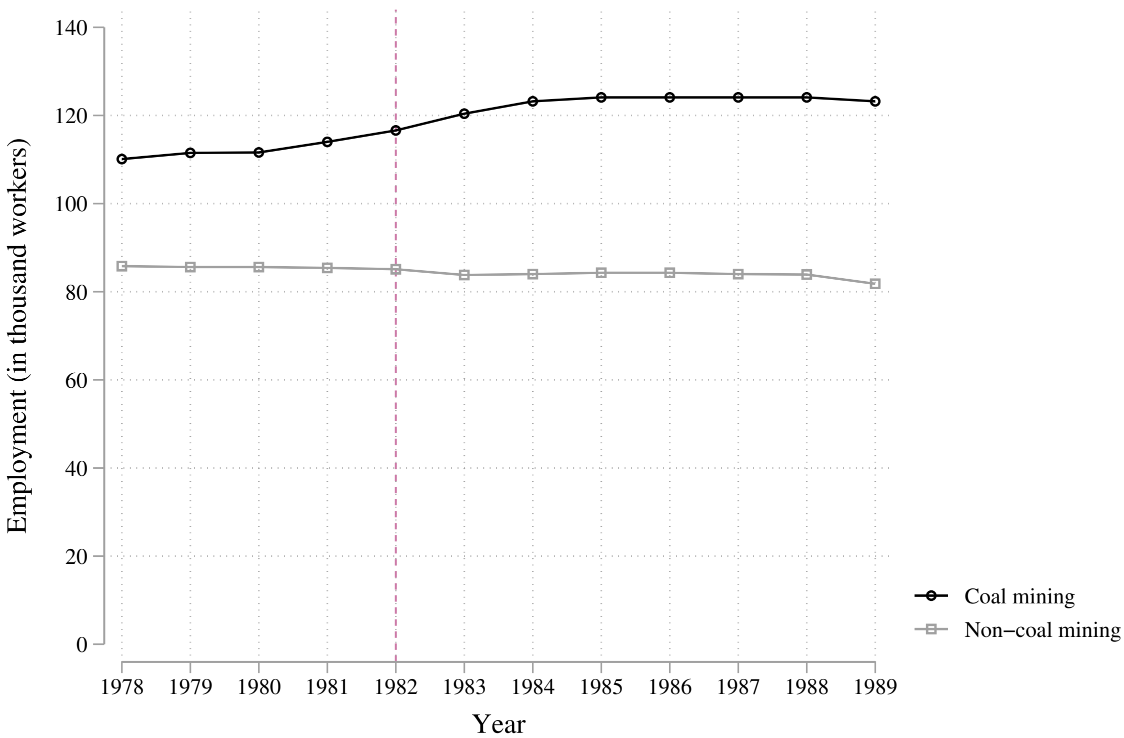
Point estimates and 95% CI for counties within 60 km of a lignite mine after 1982 in diff-in-diffs regressions of mean infant birth weights in East German counties. We iteratively omit one of the 15 states of East Germany.

Figure A.8: Year of Move for Individuals Moving Exactly Once



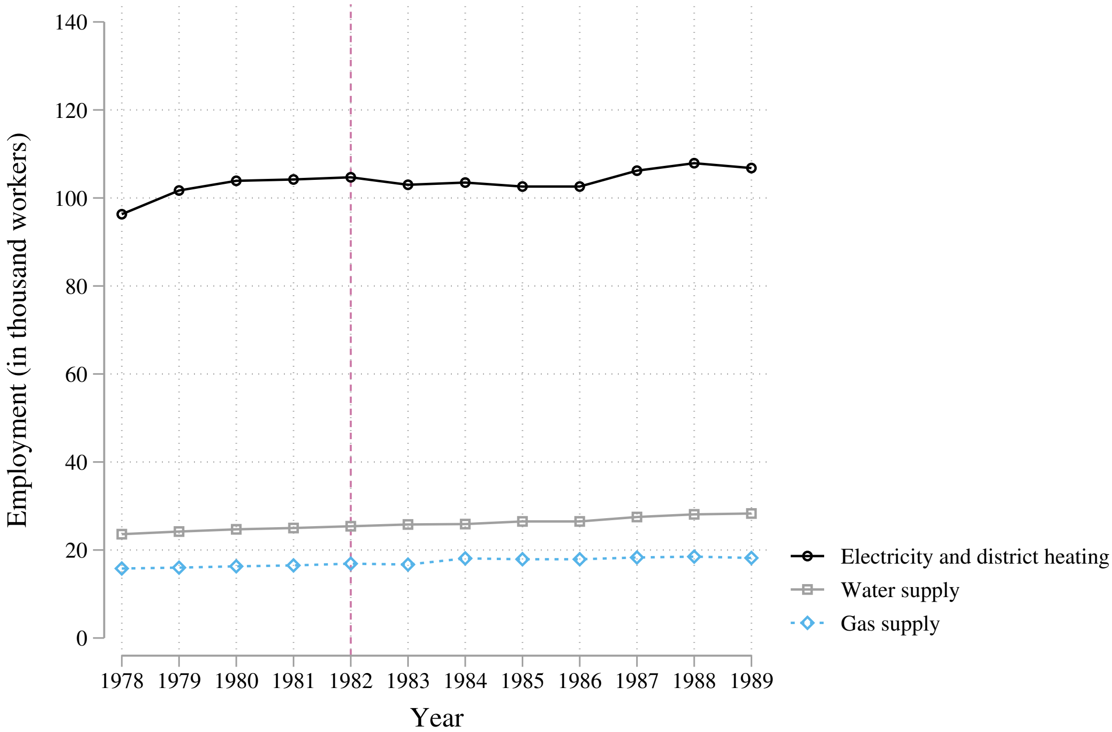
Histogram of movers' migration timing. Sample restricted to individuals moving exactly once during their post-reunification career.

Figure A.9: Total Employment in Mining Sectors, 1978-1989



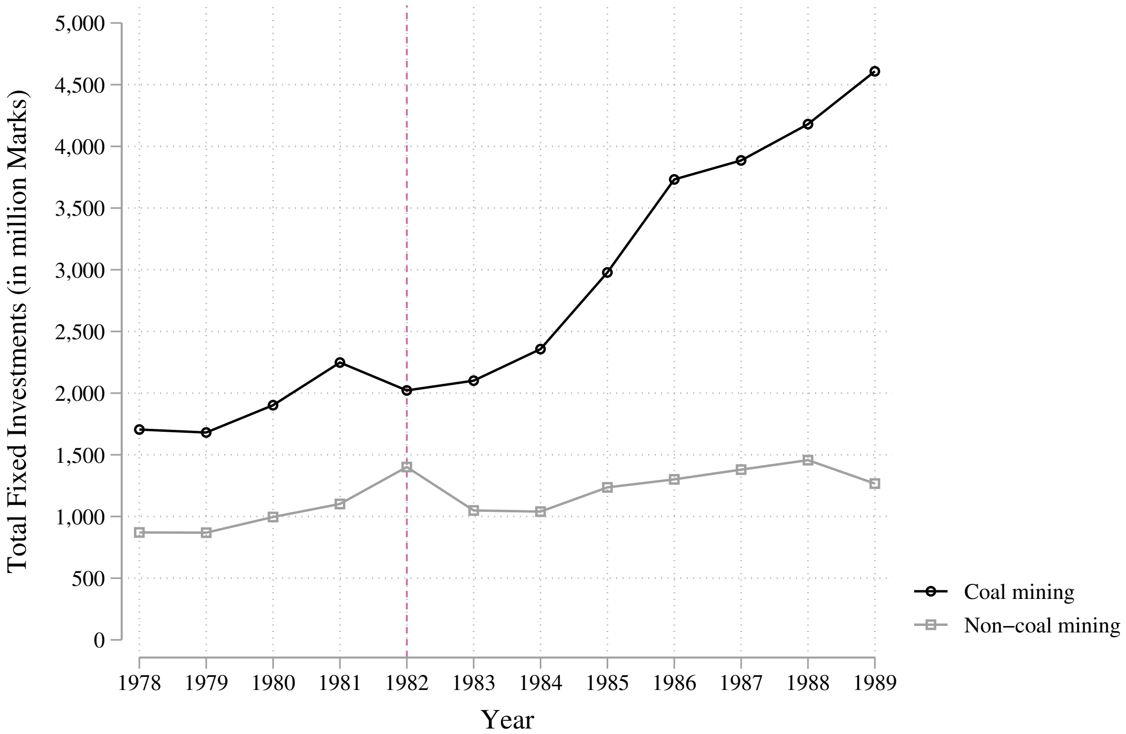
Sector level data on annual employment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A.10: Total Employment in Utility Sectors, 1978-1989



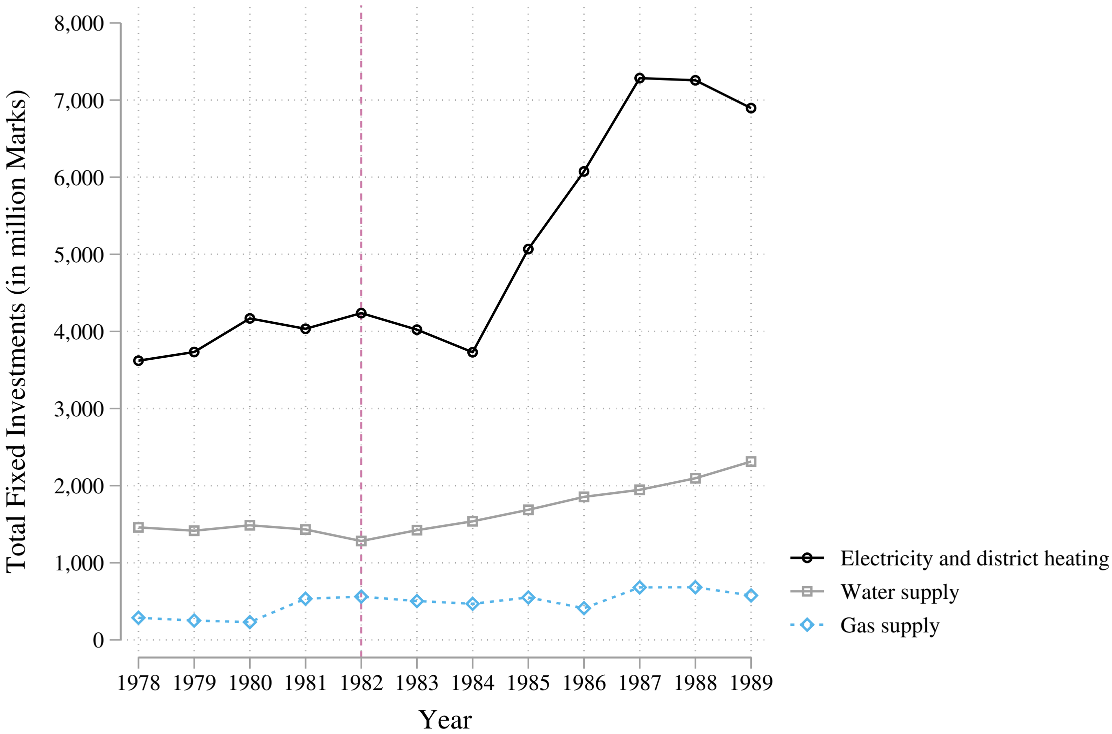
Sector level data on annual employment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A.11: Total Fixed Investments in Mining Sectors, 1978-1989



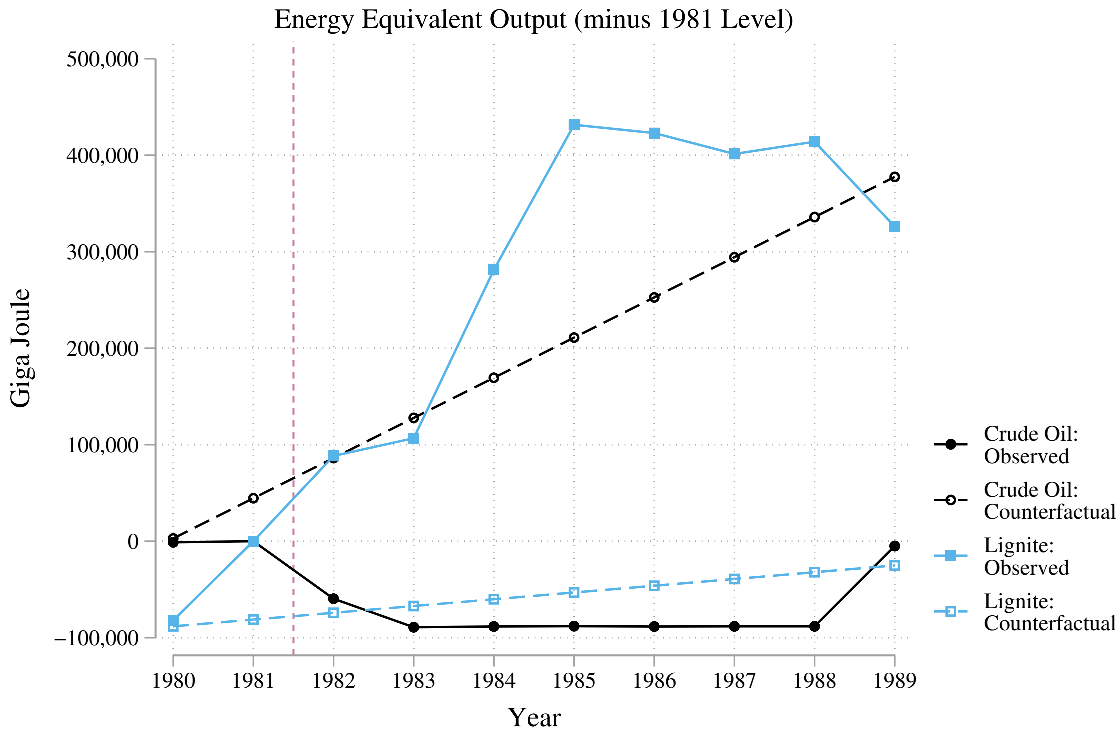
Sector level data on annual public investment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A.12: Total Fixed Investments in Utility Sectors, 1978-1989



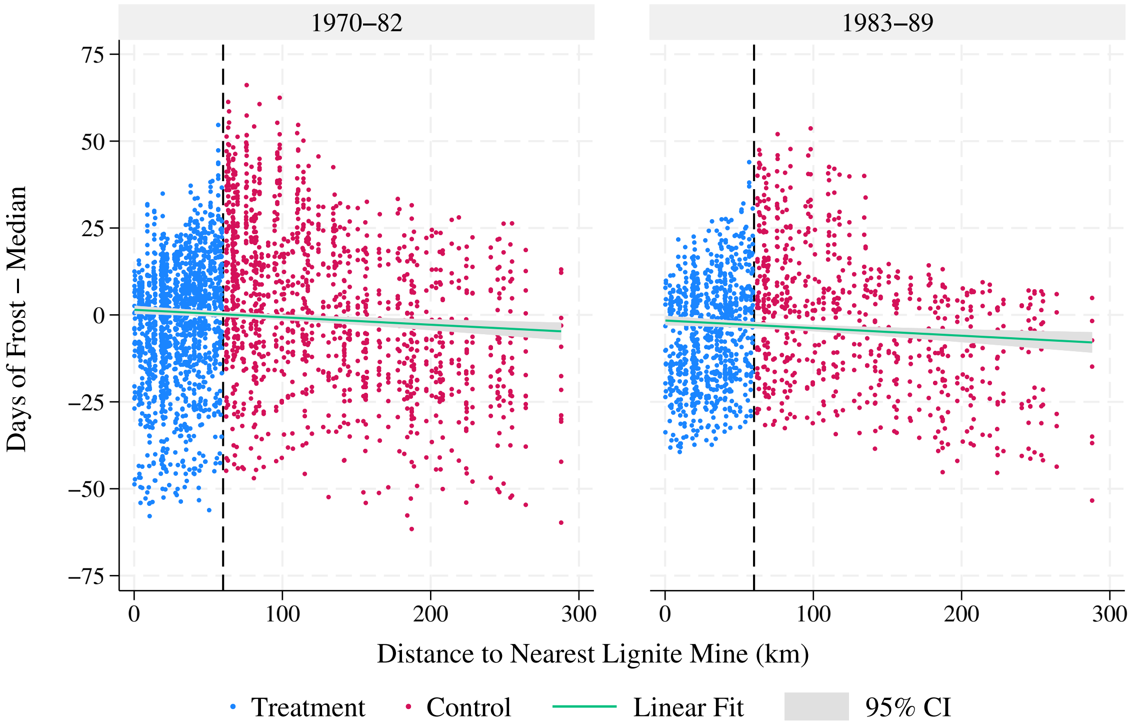
Sector level data on annual public investment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A.13: Trade Shock Effect on Oil and Lignite Accounting for Energy Density



Energy equivalent reduction in oil imports and increase in lignite production in East Germany. We use fixed conversion rates to transform outputs into energy equivalents. Specifically, we assume that a metric ton of crude oil corresponds to 44.9 GJ in energy and that a metric ton of lignite corresponds to 9.5 GJ. We difference all time series by their 1981 level to account for pre-existing differences in absolute production and import levels.

Figure A.14: Frost Days by Distance to Nearest Lignite Mine



Deviations from median frost days per year by distance to nearest lignite mine before and after the 1982 trade shock. Median computed across all counties and years (91 days of frost/year).

B Additional Tables

Table B.1: Difference-in-Differences: SO_2 Concentration with Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	28.808***	21.073***	24.784***	28.956***	30.291***
	(3.342)	(2.986)	(3.136)	(3.473)	(3.626)
Mean Y	151.583	151.583	151.583	151.583	151.583
Observations	2,274	2,274	2,274	2,274	2,274
R-squared	0.845	0.840	0.843	0.844	0.844

Note: OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a lignite mine. Threshold distance varies between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table B.2: Difference-in-Differences: Birth Weight with Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	-8.745***	-7.277**	-8.938***	-5.510*	-2.305
	(2.853)	(3.199)	(3.079)	(2.926)	(2.894)
Mean Y	3279.517	3279.517	3279.517	3279.517	3279.517
Observations	2,095,887	2,095,887	2,095,887	2,095,887	2,095,887
R-squared	0.013	0.013	0.013	0.013	0.013

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a lignite mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table B.3: Difference-in-Differences: Infant Mortality with Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	1.076***	0.348	0.744**	1.196***	1.178***
	(0.343)	(0.337)	(0.339)	(0.352)	(0.366)
Mean Y	11.178	11.178	11.178	11.178	11.178
Observations	2,808	2,808	2,808	2,808	2,808
R-squared	0.301	0.298	0.299	0.301	0.301

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a lignite mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table B.4: Difference-in-Differences: Stillbirth with Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	-0.159	-0.059	-0.160	-0.346	-0.480*
	(0.242)	(0.244)	(0.236)	(0.255)	(0.266)
Mean Y	6.165	6.165	6.165	6.165	6.165
Observations	2,808	2,808	2,808	2,808	2,808
R-squared	0.208	0.208	0.208	0.208	0.209

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a lignite mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.5: Difference-in-Differences: Net Migration with Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	-87.976	-47.583	-63.573	-93.682	-94.096
	(70.683)	(74.891)	(70.026)	(73.910)	(77.667)
Mean Y	-64.811	-64.811	-64.811	-64.811	-64.811
Observations	2,604	2,604	2,604	2,604	2,604
R-squared	0.748	0.748	0.748	0.748	0.748

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a lignite mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.6: Continuous Difference-in-Differences: Short-Term Health Outcomes

	Infant Mortality		Infant Birth Weights	
	(1)	(2)	(3)	(4)
# km to nearest mine	-0.008***		0.021	
	(0.003)		(0.019)	
# mines within 60km		0.100*		-1.214**
		(0.052)		(0.482)
Unit	deaths per 1,000 births	deaths per 1,000 births	gram	gram
Mean Y	11.178	11.178	3279.517	3279.517
Observations	2,808	2,808	2,095,887	2,095,887
R-squared	0.301	0.299	0.013	0.013

OLS estimates of Equation 1. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.7: Difference-in-Differences: SO_2 Air Pollution Excluding Border Regions

	Base	Excl. CZ	Excl. PL	Excl. CZ + PL
	(1)	(2)	(3)	(4)
$1[t > 1982] \times D_i$	28.808***	32.978***	29.304***	34.060***
	(3.342)	(3.259)	(3.542)	(3.428)
Mean Y	151.583	154.351	156.840	160.269
Observations	2,274	2,118	2,105	1,949
R-squared	0.845	0.848	0.841	0.844

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a potash mine. Column 1 includes all counties, Column 2 omits counties along the border to the Czech Republic, Column 3 omits counties along the border to Poland, Column 4 omits counties along both borders. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.8: Difference-in-Differences: Infant Mortality Excluding Border Regions

	Base	Excl. CZ	Excl. PL	Excl. CZ + PL
	(1)	(2)	(3)	(4)
$1[t > 1982] \times D_i$	1.076***	1.056***	1.062***	1.044***
	(0.343)	(0.356)	(0.360)	(0.376)
Mean Y	11.178	11.252	11.136	11.214
Observations	2,808	2,639	2,600	2,431
R-squared	0.301	0.301	0.293	0.292

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a potash mine. Column 1 includes all counties, Column 2 omits counties along the border to the Czech Republic, Column 3 omits counties along the border to Poland, Column 4 omits counties along both borders. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.9: Difference-in-Differences: SO_2 Air Pollution Placebo-in-Space Potash Mines

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	0.384	-0.687	-1.846	-1.109	0.896
	(3.969)	(4.242)	(4.079)	(3.759)	(3.696)
Mean Y	151.583	151.583	151.583	151.583	151.583
Observations	2,274	2,274	2,274	2,274	2,274
R-squared	0.834	0.834	0.834	0.834	0.834

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a potash mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.10: Difference-in-Differences: Birth Weights Placebo-in-Space Potash Mines

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
$1[t > 1982] \times D_i$	-1.016	5.326	0.338	-0.624	-1.498
	(3.735)	(4.320)	(4.001)	(3.416)	(3.383)
Mean Y	3279.517	3279.517	3279.517	3279.517	3279.517
Observations	2,095,887	2,095,887	2,095,887	2,095,887	2,095,887
R-squared	0.013	0.013	0.013	0.013	0.013

OLS estimates of Equation 1. D_i is a binary variable indicating whether a county is located close to a potash mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.11: Difference-in-Differences: Infant Mortality Placebo Tests

Placebo Treatment	60km	40km	50km	70km	80km	
	(1)	(2)	(3)	(4)	(5)	N
Panel A: Placebo-in-Time						
Lignite, 1973	-0.241	-0.208	0.037	-0.369	-0.528	2808
	(0.492)	(0.494)	(0.479)	(0.513)	(0.529)	
Lignite, 1976	-0.395	-0.001	0.013	-0.502	-0.445	2808
	(0.405)	(0.394)	(0.396)	(0.417)	(0.434)	
Lignite, 1979	0.087	0.300	0.334	0.011	0.186	2808
	(0.400)	(0.390)	(0.387)	(0.416)	(0.433)	
Panel B: Placebo-in-Space						
Potash	-0.047	0.069	-0.311	-0.010	0.194	2808
	(0.369)	(0.429)	(0.380)	(0.355)	(0.350)	

OLS estimates of Equation 1. Panel A discards all data after 1982, the actual treatment. Instead, difference-in-differences regressions are repeated using pseudo treatment dates in 1973, 1976, and 1979, respectively. Panel B estimates the original 1982 treatment date, but with treatment being assigned as whether or not a county is located close to a potash mine. The threshold distance is varied between 40 and 80 kilometers. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.12: Difference-in-Differences Coefficients: Birth Weight Distribution

Percentile	DiD Effect	H0: Linear trends parallel		H0: No anticipation effect		Mean Y
	β	*F-statistic*	*p-value*	*F-statistic*	*p-value*	*grams*
10th	-16.737***	1.09	0.2970	0.46	0.7073	2,709.80
	(6.094)					
20th	-13.350***	0.00	0.9721	0.19	0.9056	2,925.84
	(4.651)					
30th	-12.552***	0.28	0.5983	0.71	0.5464	3,075.81
	(4.116)					
40th	-11.496***	0.21	0.6470	1.09	0.3506	3,204.73
	(3.674)					
50th	-9.848***	0.16	0.6852	0.52	0.6670	3,319.81
	(3.33)					
60th	-7.357**	0.12	0.7269	0.48	0.6947	3,434.15
	(3.476)					
70th	-5.306	1.51	0.2193	1.42	0.2353	3,555.89
	(3.716)					
80th	-4.225	0.75	0.3858	1.60	0.1862	3,691.27
	(3.889)					
90th	-6.216	0.01	0.9032	0.40	0.7532	3,867.30
	(4.593)					

OLS estimates of Equation 1 at each decile of the birth weight distribution and test statistics and p-values for testing the identifying assumptions for running separate difference-in-differences regressions at each decile. Municipality-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.13: OLS: Effect of Distance to Lignite under Socialism on Years of Employment

	(1)	(2)	(3)	(4)
D_i	-0.101	-0.224***	-0.214***	-0.241***
	(0.098)	(0.085)	(0.076)	(0.068)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y	15.849	15.849	15.849	15.849
Observations	6,108,837	6,108,837	6,108,837	6,108,837
R-squared	0.000	0.403	0.407	0.440

Note: Results from OLS regressions of years of employment between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite mine in 1992. Standard errors clustered on the level of individuals' 1992 municipality (2,403 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.14: OLS: Effect of Distance to Lignite under Socialism on Years of Unemployment

	(1)	(2)	(3)	(4)
D_i	0.226	0.205**	0.211***	0.178***
	(0.138)	(0.095)	(0.080)	(0.053)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y	3.865	3.865	3.865	3.865
Observations	6,108,837	6,108,837	6,108,837	6,108,837
R-squared	0.001	0.025	0.047	0.133

Note: Results from OLS regressions of years of unemployment between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite mine in 1992. Standard errors clustered on the level of individuals' 1992 municipality (2,403 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.15: OLS: Effect of Distance to Lignite under Socialism on Wages

	(1)	(2)	(3)	(4)
D_i	-2.039	1.476	0.806	0.108
	(2.290)	(1.156)	(0.911)	(0.489)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y	63.844	63.844	63.844	63.844
Observations	6,108,837	6,108,837	6,108,837	6,108,837
R-squared	0.001	0.041	0.194	0.350

Note: Results from OLS regressions of wages (in 2015 Euros) between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite mine in 1992. Standard errors clustered on the level of individuals' 1992 municipality (2,403 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.16: OLS: Effect of Distance to Lignite under Socialism on Log Wages

	(1)	(2)	(3)	(4)
D_i	-0.036	0.022	0.013	0.001
	(0.036)	(0.018)	(0.014)	(0.008)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y	4.023	4.023	4.023	4.023
Observations	6,095,277	6,095,277	6,095,277	6,095,277
R-squared	0.001	0.050	0.192	0.374

Note: Results from OLS regressions of log wages (in 2015 Euros) between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite mine in 1992. Standard errors clustered on the level of individuals' 1992 municipality (2,403 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.17: OLS: Effect of Distance to Lignite under Socialism on Retirement Age

	(1)	(2)	(3)	(4)
D_i	0.207***	-0.030	-0.021	-0.047**
	(0.032)	(0.025)	(0.027)	(0.020)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y	62.272	62.272	62.272	62.272
Observations	6,108,837	6,108,837	6,108,837	6,108,837
R-squared	0.000	0.040	0.041	0.063

Note: Results from OLS regressions of age of retirement for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite mine in 1992. Individuals with missing retirement age are assumed to retire at 65. Standard errors clustered on the level of individuals' 1992 municipality (2,403 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.18: Movers Design: Effect of Distance to Lignite in Origin on Years of Employment

Y =	Years of Employment				
	(1)	(2)	(3)	(4)	(5)
D_i	-0.360**	-0.410**	-0.366**	-0.372***	-0.341*
	(0.175)	(0.180)	(0.162)	(0.129)	(0.195)
Destination FE	✓	✓	✓	✓	✓
Year of Birth FE		✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
Origin State FE					✓
Mean Y	14.185	14.185	14.185	14.185	14.185
Observations	144,338	144,338	144,338	144,338	144,338
R-squared	0.039	0.331	0.338	0.369	0.370

Note: Movers design OLS regression of years of employment between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite coal mine under socialism. Sample restricted to individuals moving in 1993. Standard errors clustered on the level of individuals' 1992 county (77 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.19: Movers Design: Effect of Distance to Lignite in Origin on Years of Unemployment

Y =	Years of Unemployment				
	(1)	(2)	(3)	(4)	(5)
D_i	0.203	0.188	0.162	0.088	0.045
	(0.137)	(0.126)	(0.109)	(0.059)	(0.069)
Destination FE	✓	✓	✓	✓	✓
Year of Birth FE		✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
Origin State FE					✓
Mean Y	3.497	3.497	3.497	3.497	3.497
Observations	144,338	144,338	144,338	144,338	144,338
R-squared	0.025	0.050	0.078	0.140	0.140

Note: Movers design OLS regression of years of unemployment between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite coal mine under socialism. Sample restricted to individuals moving in 1993. Standard errors clustered on the level of individuals' 1992 county (77 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.20: Movers Design: Effect of Distance to Lignite in Origin on Age at Retirement

Y =	Retirement Age				
	(1)	(2)	(3)	(4)	(5)
D_i	-0.147	-0.144*	-0.133	-0.163**	-0.211*
	(0.089)	(0.082)	(0.083)	(0.068)	(0.110)
Destination FE	✓	✓	✓	✓	✓
Year of Birth FE		✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
Origin State FE					✓
Mean Y	61.490	61.490	61.490	61.490	61.490
Observations	144,338	144,338	144,338	144,338	144,338
R-squared	0.011	0.059	0.061	0.086	0.086

Note: Movers design OLS regression of age at retirement between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite coal mine under socialism. Sample restricted to individuals moving in 1993. Standard errors clustered on the level of individuals' 1992 county (77 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.21: Movers Design: Effect of Distance to Lignite in Origin on Wages

Y =	Daily Wages				
	(1)	(2)	(3)	(4)	(5)
D_i	-2.141	-2.078	-2.126*	-2.054**	-1.488*
	(1.885)	(1.835)	(1.213)	(0.807)	(0.852)
Destination FE	✓	✓	✓	✓	✓
Year of Birth FE		✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
Origin State FE					✓
Mean Y	67.996	67.996	67.996	67.996	67.996
Observations	144,338	144,338	144,338	144,338	144,338
R-squared	0.070	0.097	0.248	0.373	0.373

Note: Movers design OLS regression of mean daily wages between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite coal mine under socialism. Daily wages are measured in constant 2015-Euros. Sample restricted to individuals moving in 1993. Standard errors clustered on the level of individuals' 1992 county (77 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.22: Inverse Movers Design Results Excluding Energy Sector Workers

	(1) Employment Years	(2) Unemployment Years	(3) Retirement Age	(4) 2015 Wages	(5) Ln(2015 Wages)
D_i	-0.377***	0.095	-0.165**	-2.101**	-0.031***
	(0.131)	(0.060)	(0.068)	(0.820)	(0.011)
Year of Birth FE	✓	✓	✓	✓	✓
Destination FE	✓	✓	✓	✓	✓
Level of Education FE	✓	✓	✓	✓	✓
Sex FE	✓	✓	✓	✓	✓
Occupation FE	✓	✓	✓	✓	✓
WZ08 3-digit FE	✓	✓	✓	✓	✓
Mean Y	14.145	3.475	61.474	67.331	4.009
Observations	137,175	137,175	137,175	137,175	137,159
R-squared	0.365	0.139	0.088	0.370	0.355

We exclude all workers in the following 3-digit WZ08 industry codes: 52, 351, 352, 353. OLS estimates of Equation 3. D_c is an indicator for counties within 60 kilometer's straight-line distance of the nearest lignite mine. All columns include a set of destination county fixed effects to account for differential post-reunification effects. All Columns include additional controls for year-of-birth, level of education, sex, five-digit occupational code fixed effects, and three-digit industry code fixed effects. County-level clustered standard errors in parentheses. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.23: Movers Design: Effect of Distance to Lignite in Origin on Ln Wages

Y =			Ln Wages		
	(1)	(2)	(3)	(4)	(5)
D_i	-0.030	-0.029	-0.031*	-0.030***	-0.026**
	(0.023)	(0.023)	(0.017)	(0.010)	(0.013)
Destination FE	✓	✓	✓	✓	✓
Year of Birth FE		✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
Origin State FE					✓
Mean Y	4.020	4.020	4.020	4.020	4.020
Observations	144,322	144,322	144,322	144,322	144,322
R-squared	0.059	0.092	0.220	0.356	0.356

Note: Movers design OLS regression of logged mean daily wages between 1992 and 2020 for the 1992 cohort on an indicator of whether an individual was based within 60 kilometers of a lignite coal mine under socialism. Daily wages are measured in constant 2015-Euros. Sample restricted to individuals moving in 1993. Standard errors clustered on the level of individuals' 1992 county (77 clusters). *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.24: G-SOEP: OLS Health Outcomes Pooled 2011, 2013, 2015, 2017

	(1) Asthma	(2) Cardiopathy	(3) Diabetes	(4) Chronic Back Pain
D_i	0.117*	0.133**	-0.046	0.018
	(0.060)	(0.052)	(0.049)	(0.056)
YoB-by-gender FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Mean Y	0.058	0.167	0.155	0.244
Observations	4,472	4,472	4,472	4,472
R-squared	0.237	0.264	0.273	0.205

Note: Pooled cross-sectional OLS estimates for survey years 2011, 2013, 2015, and 2017. The treatment variable D_i is whether individuals lived in a county within 60 kilometers of a lignite mine in 1990. All specifications are weighted by G-SOEP survey weights. All specifications control for current residence. Errors two-way clustered at the individual and 1990 residence county level. *** p < 0.01 ** p < 0.05 * p < 0.1

Table B.25: G-SOEP: OLS Labor Market Outcomes Pooled 2011, 2013, 2015, 2017

	(1) Gross Wages	(2) ln(Wages)	(3) 1[Retired]	(4) 1[Employed]
D_i	-401.219**	-0.303*	0.081*	-0.089
	(187.063)	(0.156)	(0.043)	(0.054)
YoB-by-gender FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Survey year FE	✓	✓	✓	✓
Mean Y	970.689	7.537	0.516	0.329
Observations	4,472	2,022	4,472	4,472
R-squared	0.522	0.335	0.799	0.589

Note: Pooled cross-sectional OLS estimates for survey years 2011, 2013, 2015, and 2017. The treatment variable D_i is whether individuals lived in a county within 60 kilometers of a lignite mine in 1990. All specifications are weighted by G-SOEP survey weights. All specifications control for current residence. Errors two-way clustered at the household and 1990 residence county level. *** p < 0.01 ** p < 0.05 * p < 0.1