The Long-Term Effects of Air Pollution: Evidence from Socialist East Germany*

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Abstract

We measure the causal effects of long-term exposure to air pollution on individuals' health and labor market outcomes. When the Soviet Union – the main provider of fossil fuels to Socialist East Germany after World War II – unexpectedly cut oil exports in 1982, East Germany had to rapidly substitute oil with highly-polluting lignite coal. Exploiting the spatial distribution of lignite mines within East Germany, we show that exposed counties experienced a large and persistent increase in air pollution. Over the next four decades, individuals from exposed counties earned significantly lower wages, spent less time in employment and retired earlier. Comparing effects along the age distribution, we find large negative effects for children below age 12 and adults above age 27. We identify these effects in an inverse movers design that leverages, first, authoritarian restrictions on individual freedom of movement and the non-competitive housing and labor markets of East Germany's command economy to rule out endogenous pollution mitigation and, second, the sudden lapse of these restrictions after German Reunification in 1990. Using survey data, we identify declining individual health as the mechanism driving these labor market effects.

Keywords: Air pollution, labor supply, migration, place effects

JEL Classification: I15, J24, J60, N54, Q53

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1 Introduction

Place-based effects are major determinants of individuals' well-being, productivity, and welfare (Chetty and Hendren 2018b,a; Bergman et al. 2024). The health consequences of environmental risks like air pollution are important drivers of these effects (Aguilar-Gomez et al. 2022). Economic and medical research documents that air pollution has important short- and medium-term effects on health and labor market outcomes.[1] Yet, little is known about long-term effects and sustained exposure. This lack of evidence contrasts with the importance of long-term effects for understanding the full impact of environmental risk: Depending on how long these effects persist over time, they may constitute a large part of the overall cost of pollution.

These concerns are heightened when individuals are exposed to air pollution for a sustained period of time. Measuring the causal effects of sustained exposure to air pollution, however, is particularly challenging: Individuals endogenously respond to changes in pollution exposure with adjustments to their behavior and their housing and labor market decisions (Banzhaf and Walsh 2008). Mitigation incentives may also induce selection and spatial sorting if the degree to which individuals can adjust to pollution varies with their income or socioeconomic status (Currie 2011; Chen et al. 2022). Causal evidence on the effects of sustained pollution exposure remains scarce[2] because directly estimating these effects requires quasi-experimental variation that accounts for behavioral responses (Deryugina and Reif 2023). In this paper, we provide such evidence.

We study air pollution in a unique setting which – due to political constraints – did not permit latent sorting and related behavioral responses and therefore allows us to identify the effects of sustained pollution exposure on health and labor market outcomes. Specifically, we use a natural experiment that occurred in socialist East Germany (the German Democratic Republic or GDR)[3] in the 1980s. When Germany split into East and West after World War II, the GDR was cut off from almost all natural resource deposits. The only natural resources available in East Germany were underground deposits of lignite ('brown coal') and potash. As a result, the GDR became dependent on imports of natural resources and fossil fuels from the Soviet Union. In 1982, however, the Soviet Union suddenly and unexpectedly ended East Germany's preferential access to these imports (Schürer, 1999; Pfaff, 2006). Initially fearing the trade shock would see "the existence of the country endangered"[4] (Schürer 1999), GDR authorities were forced to rapidly substitute imported oil with lignite. The abrupt increase in the usage of lignite led to a large and persistent increase in air pollution in *some* areas of the country. Because lignite is generally not suited for trade and transport, this increase was concentrated in counties close to minable

[1] See, among others, Currie and Walker (2011); Graff Zivin and Neidell (2012); Luechinger (2014); Beach and Hanlon (2018); Graff Zivin and Neidell (2018); Alexander and Schwandt (2022); Adhvaryu et al. (2022); Borgschulte et al. (2022); Duque and Gilraine (2022); Hoffmann and Rud (2024).

[2] The few papers providing evidence on the effects of long-term exposure focus on mortality outcomes (Chen et al. 2013; Ebenstein et al. 2017; Anderson 2020; Barreca et al. 2021).

[3] We use the terms East Germany and GDR interchangeably throughout the paper.

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

lignite deposits. The resulting regional differences in air pollution persisted until the end of the GDR in 1990.

Socialist East Germany provides a unique setting to identify the long-term effects of sustained exposure to air pollution because the country's authoritarian regime constrained individuals' ability to endogenously respond to pollution exposure. The setting precludes identification concerns present in most other settings: The socialist government restricted individual freedom of movement, the country's command economy did not allow for competitive housing or labor markets, the decoupling of prices from market mechanisms prevented latent sorting through pollution pricing, and there was no job-to-job mobility in the labor force. Historical evidence shows that these restrictions affected almost all members of society, including bureaucrats and party officers – most shortages, for instance in housing, were driven by genuine scarcity.[5] We corroborate the assumption that mobility restrictions in the GDR did not allow the population to move across counties and show that there was no detectable spatial sorting response to the air pollution shock.[6]

Our results document that the air pollution shock significantly altered the health and labor market outcomes of individuals in treated areas after 1982 – both in the short and long run. First, we use historical maps of lignite coal fields and previously classified administrative records of GDR air quality monitors to show that intensified lignite usage caused an immediate, large, and persistent increase in sulfur dioxide pollution.[7] Difference-in-differences estimates show that counties close to lignite mines experienced an average pollution increase of 27.553 $\mu g/m^3$ – an effect comparable in magnitude to moving from the 10th to the 90th percentile of sulfur dioxide pollution in the contemporary United States (EPA, 2019).

Second, we use administrative data on infant mortality and birth weights to examine the immediate health effects of the lignite-induced air pollution shock. Difference-in-differences estimates suggest that counties close to lignite mines experienced a significant and persistent increase in infant mortality rates and a significant and persistent decrease in infant birth weights. Notably, the infant health effects we estimate are large and immediate – with infant mortality increasing between 7 and 11% relative to the pre-treatment baseline – and create a persistent mortality gap between treatment and control counties over the post-treatment period, until the end of the GDR in 1990. Birth weight effects are smaller in magnitude at the mean (8 grams), but increase towards the bottom of the birth weight distribution. Instrumenting air quality monitor readings with a county's distance

[5] See Lichter et al. (2021) for another study leveraging restrictions on the freedom of movement in socialist East Germany for identification.

[6] Studies on air pollution effects in the short-term frequently leverage day-to-day variations in predominant winds 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. Wind directions are highly persistent in the long-term as they are caused by differences in atmospheric pressure. These arise due to temperature differences that stem from the difference between heating at the equator and the poles, and by the Earth's rotation.

[7] These monitor readings were classified information under socialism and encryption protected. Researchers at the German Federal Environmental Agency (*Umweltbundesamt*) decrypted these data after German reunification.

to the nearest lignite mine, we estimate the infant mortality elasticity of air pollution to lie between 0.26 and 0.82 – considerably higher than estimates from comparable settings in which endogenous mitigation is possible.

Next, we investigate the impact of long-term exposure to air pollution on individuals' labor market outcomes. We use social security data on the universe of East German workers to trace individual-level labor market trajectories between 1991, when they first appear in reunified Germany's administrative records, and 2020. We measure individuals' baseline characteristics and location under socialism using each worker's first individual records in 1991 and 1992.

OLS regressions indicate that individuals who lived in treated counties under socialism have significantly worse labor market outcomes compared to other East Germans. OLS estimates, however, may be biased due to omitted variable concerns. Labor market trajectories of individuals from treated counties may systematically differ from those of individuals from control counties in post-reunification Germany. For instance, reunification changed the industrial structure of East Germany substantially (Mergele et al. 2025). If treatment and control counties differed in their exposure to structural change, OLS might overestimate the effects of air pollution on long-term labor market outcomes. Moreover, counties historically exposed to air pollution may have been subject to different place-based subsidies after reunification because of the discontinuation of obsolete industries such as lignite mining. If place-based policies offset some of the adverse effects of environmental pollution on individuals and counties, our estimates may be biased downwards.

To identify the true effect of air pollution, we isolate long-run effects from pollution under socialism in a movers design (Deryugina et al. 2020; Finkelstein et al. 2021; Deryugina and Molitor 2021). Specifically, we *invert* the standard approach to migration-based causal inference and propose a novel design that allows for identification of differential origin rather than differential destination effects. To identify the impact of historical pollution, we study East Germans who move between German counties right after the collapse of socialism. We leverage sudden migration waves after reunification in a comparison of pairs of individuals who move to the exact same post-reunification county at the same time, but from different origin counties: One from a treatment county – subject to exogenous increases in air pollution – and one from a control county. Conditioning on destination county fixed effects allows us to capture both pull factor determinants of migration and place-specific post-reunification effects – including differences in local labor markets, place-based policies, and variation in subsequent air pollution exposure.

This design identifies the long-term effect of sustained pollution exposure under the assumption that unobservable differences between origin regions are not correlated with later-life labor market outcomes. To assess this assumption, we show that there are no structural differences between treatment and control origin regions under socialism in a rich set of balanced regional characteristics. We further estimate our model with destination-by-origin fixed effects to rule out that our results are affected by differential selection into migration between treatment and control origin regions. Moreover, we use the locations of non-lignite mines in East Germany as a placebo-treatment to show that our findings on long-term effects are not driven by mining culture or place-based effects of exposure to exploitative industries.

We estimate the effects of the lignite-induced pollution shock on individuals' aggregate labor market outcomes on this movers sample. Up to four decades after the initial shock, individuals who lived in treatment regions under socialism experience significantly worse labor market outcomes than other East Germans who moved to the exact same location immediately after the end of socialism. On average, exposed individuals spend 0.372 fewer years (4.46 months) in employment, retire significantly earlier (1.96 months on average), and suffer a significant wage penalty over their working life, amounting to almost 3% of average daily earnings. In a back-of-the-envelope calculation, we show that the post-exposure employment effects of air pollution in East Germany alone account for social security costs of about 1% of German GDP in 1989.

Our setting allows us to uncover important heterogeneity in treatment effects because we are able to estimate air pollution exposure effects for the population of workers in East Germany. Specifically, we estimate how pollution effects differ along the age distribution. The results show that, in contrast to findings in the literature, pollution exposure affects not only the young ($<$ 12 years) but also individuals above the age of 25. Our results document that negative effects of air pollution matter for a much larger share of the population than previously assumed: Individuals of almost all ages are affected, while the effects are largest for the youngest and the oldest individuals in our sample.

Finally, we show that the main mechanism driving these labor market effects is a pollution-induced decline in individual health. Using individual-level survey data from the German Socio-Economic Panel, we show that individuals who resided in treated counties before 1990 have significantly worse health outcomes and significantly higher healthcare utilization than individuals from control counties. In particular, three decades after the initial shock, treated individuals are significantly more likely to have been diagnosed with pollution-related health issues such as asthma (+229.1%) or cardiopathy (+83.1%), but *not* more likely to have been diagnosed with pollution-*un*related issues like diabetes or chronic back pain. These health effects are a relevant channel through which air pollution affects long-term labor market outcomes, but also constitute important outcomes in their own right.

This paper contributes to 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 effects of air pollution on labor market outcomes is scarce. Studies show that air pollution exposure *in utero* or early in life have detrimental effects on college attendance (Voorheis et al. 2017; Colmer et al. 2022), income, and employment (Isen et al. 2017). All of these studies use decreases in air pollution induced by changes in environmental regulations and compare beginning-of-career outcomes for infants born around a cut-off. By construction, these studies are focused on the effects of pollution in early life.

To our knowledge, no studies exist that produce estimates comparable to the detrimental long-term effects of air pollution throughout the entire age distribution that we are able to identify in our setting.

A small numbers of papers provide evidence on the effect of long-term exposure to air pollution. (Beach and Hanlon 2018; Anderson 2020) use variation in air pollution induced by persistent wind patterns to provide evidence on the mortality effects of long-term exposure. Wind patterns themselves, however, have been shown to factor into neighborhood sorting (Heblich et al. 2021). (Barreca et al. 2021) use variation from the Acid Rain Program to study the effects of air pollution on adult mortality. Chen et al. (2013) and Ebenstein et al. (2017) leverage the Huai River discontinuity in China to study the effects of sustained air pollution exposure on life expectancy. We contribute to this literature by studying the effect of air pollution in a unique setting where – for political reasons – spatial sorting was minimized and latent sorting through pollution pricing impossible allowing us to identify long-term effects of pollution exposure on both short- and long-term health and labor market outcomes.

Most literature on the effects of air pollution has focused 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, our estimate of the air pollution elasticity of infant mortality is considerably larger than effect sizes considered in the relevant literature. These results suggest that endogenous mitigation and sorting may downward bias effect sizes in other settings. In addition, these results also identify the air pollution impact on health as a relevant channel underlying our long-term results.

Finally, our study complements existing studies illustrating the long-term effects of life under authoritarianism on individuals. Authoritarian regimes frequently limit the freedoms of their citizens, such as the freedom of expression, the freedom of assembly, and the freedom of movement. These limitations to their freedoms impose externalities on the individuals affected. Not only are individuals under authoritarian rule not able to pursue their comparative advantage and maximize their welfare, they are also limited in their ability to offset the adverse effects of authoritarian policies. Focusing on East Germany, we illustrate the costs of being subject to place-based externalities when repressed freedom of movement prevents mitigating responses with an example of an environmental disamenity like air pollution. We show that individuals affected by place-based externalities suffer from these immediately, severely, and for decades after the resulting policy changes have been passed. This way, we add health-related consequences to the existing literature showing that the lasting effects of life under socialism impact socioeconomic aspects such as trust (Lichter et al. 2021), preferences (Alesina and Fuchs-Schündeln 2007), or savings decisions (Fuchs-Schündeln and Schündeln 2005).

2 Setting

2.1 The Crude Oil Trade Shock of 1981/82

The partition of Germany after the end of World War II left the nascent East German state without notable access to natural resources. The only natural resources abundantly available within its borders were underground deposits of lignite and potash. In contrast, most industrial activity before the war had been relying on now severed supply lines from the west of the country. Quickly, the socialist economy became dependent on importing natural resources and in particular fossil fuels from abroad (Pfaff 2006). Other member countries of the Eastern Bloc became the GDR's most important trading partners: The GDR's statistical yearbooks list imports of crude oil and natural gas from the Soviet Union and imports of bituminous coal from Czechoslovakia, Poland, the Soviet Union and from West Germany. Trading within the Bloc was preferable for three reasons (Steiner 2004). First, trading was facilitated by political alignment. Second, the Eastern Bloc relied on a lagged, moving average pricing mechanism that allowed the procurement of resources below world market prices. This pricing structure was meant to reduce the impact of price variation on the world market and proved to be an important economic advantage for importers within the Eastern Bloc during the oil crises of the 1970s. Third, imports from the Eastern Bloc did not require payments in costly foreign exchange but could be off-set with exports of goods produced within the GDR.

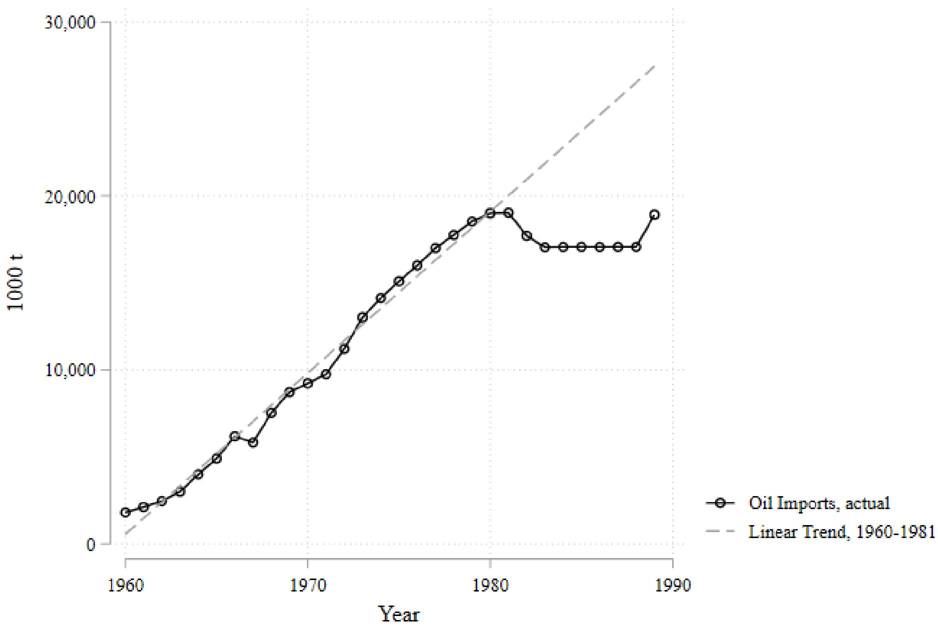
The GDR's central planners put the availability of subsidized crude oil imports from the Soviet Union at the heart of their economic strategy (Pfaff (2006), p.36). The GDR had benefited from importing continuously increasing amounts of oil along a linear schedule of available volumes. Accordingly, the country was not prepared when, in 1981/82, the Soviet Union suddenly and unexpectedly announced a reduction in the amount of oil available for import to the GDR (Schürer 1999). In fact, the GDR's central planners were so surprised that they initially feared "the existence of the country endangered".[8] Over night, the Soviet Union capped the amount of oil available at 17 million tons a year. Figure 1 illustrates East German fossil fuel imports between 1960 and 1989. These follow a linear schedule until 1981/82. From 1982 onwards, imports of crude oil remain capped for the remainder of East Germany's existence as an independent country. In addition to reduced supply, oil imports after 1981/82 had to be compensated by sending higher-valued goods to the Soviet Union in return.[9] These goods, while already constituting an increase in the price of oil, came with the added opportunity cost of no longer being able to exchange them for foreign exchange on the world market. The Soviet Union's decision to reduce the oil supply to East Germany relied on three separate concerns. First, the Soviet economy itself was experiencing a downturn after a string of poor harvests and adverse productivity shocks (Steiner 2004). Second, the intensified geopolitical situation following the Soviet invasion of Afghanistan in 1979 and resulting military investments put an additional strain on the

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

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

Soviet budget. Third, the GDR was using a large portion of the subsidized oil imports to refine crude into higher quality petrochemicals and selling them for foreign exchange in the West (Stokes 2013). The fact that East Germany essentially conducted arbitrage on Soviet subsidies proved to outweigh international political goodwill, even if the obtained foreign exchange was increasingly indispensable for East German fiscal stability (Steiner 2004). With imports of bituminous coal also on a continued decline, the increasing energy needs of the East German economy left the socialists with but one response: the country had to swiftly extend the exploitation of its domestic lignite deposits.

Figure 1: East German Imports of Crude Oil, 1960-1989



East German imports of crude oil from the Soviet Union per year between 1960 and 1989. Dashed line indicates the linear fit of running an OLS regression on imports between 1960 and 1981 and extrapolating the resulting schedule to the years between 1982 and 1989.

2.2 Lignite Production and Usage

To compensate for the suddenly reduced availability of oil, East Germany rapidly increased the share of lignite in its energy mix.[10] Increases in lignite production, retooling of industrial plants and power plants allowed them to switch from oil to lignite in the industrial sector, domestic heating switched from using oil to using lignite briquettes. The rapid adjustment was possible due to the existence of substitution strategies that had been crafted but not properly implemented in the late 1970s (Steiner 2004). To maintain the growing foreign exchange benefits from exporting refined petrochemicals, the substitution of oil even went beyond the 2 million tons that crude oil imports had been reduced by (Stokes

[10]In Figure A13, we provide an alternative comparison of East Germany's energy mix that accounts for the different fuel energy density of oil and lignite coal. We find that the increase in lignite production, relative to the linear pre-1982 trend, accounts for 118.4% of the energy density lost due to the oil trade shock.

2013). Lignite was primarily produced from open pit strip mines in the southern regions of East Germany. Figure 2 illustrates lignite production between 1960 and 1989, with a substantial increase in output from 1981/82 onwards.

Figure 2: East German Production of Lignite, 1960-1989

Total lignite production in East Germany between 1960 and 1989. Dashed line indicates the linear fit of running an OLS regression on imports between 1960 and 1981 and extrapolating the resulting schedule to the years between 1982 and 1989.
Sources: GDR Statistical Yearbook.

While the GDR was able to use lignite to compensate for the reduced supply of imported crude oil, lignite itself is far from an ideal energy carrier. The U.S. Energy Information Administration describes lignite as "the lowest rank of coal".[11] Originating from compressed natural peat, lignite is characterized by its high moisture content, its proclivity to crumble, and the fact that it rapidly deteriorates when exposed to air. Jointly, these characteristics render lignite impractical and unsuitable for long-distance trade and transport. Adding the large amounts of sulfur dioxide and carbon dioxide pollution emitted under combustion, lignite is generally considered a low-quality fossil fuel. Nonetheless, and predominantly for lack of available alternatives, East Germany became the world's largest lignite producer by 1989 and covered close to 70% of its energy demand out of domestic lignite mines. Only after German reunification, when alternative fuels became available and the extension of stricter environmental regulations reduced the profitability of lignite-based plants, lignite finally became a less prominent part of German energy policy (Luechinger 2014).

[11]https://www.eia.gov/tools/glossary/?id=coal, last accessed 11 November 2022.

2.3 Lignite, Air Pollution, Health, and Productivity

Relative to other energy carriers, lignite has a particularly high pollutant to heat generation ratio. Upon combustion, lignite emits both sulfur dioxide and carbon dioxide. These pollutants, or their secondary residuals, can be carried in the air over dozens of kilometers (Almeida et al. 2020). Both sulfur dioxide and carbon dioxide are precursor pollutants that, when airborne, decay to smaller particulate matter pollution (mainly $PM_{2.5}$). $PM_{2.5}$ consists of micro particles that have been shown to penetrate indoors even when they are emitted outdoors (Hoek et al. (2008), see the corresponding section in Holub and Thies (2022) for a review of the role of particulate matter pollution). Air pollution impacts individuals largely through the effects of breathing in particulate matter. In their review of the relevant medical literature, Aguilar-Gomez et al. (2022) highlight three crucial aspects: First, air pollution impacts the respiratory and cardiovascular systems by causing acute and/or chronic inflammations. These injuries, particularly if they are repeated, may lead to reductions in exertable physical effort and increasing fatigue. They can also negatively impact cognitive performance, including concentration, focus or memory. Second, air pollution can harm the brain and the nervous system. By causing neuro-inflammations and oxidative stress, air pollution can impair brain functionality and the development of longer-term cognitive capabilities - particularly so in infants whose nervous system is still developing. Third, air pollution can impact gene expression by causing mutations in the expressions of given 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 as they manifest with acute exposure and in the long run when health impacts can turn into chronic conditions.

Existing research in economics has shown that these types of effects are generally present in short-term and acute exposure episodes. In most of these settings, the health and cognition effects of air pollution have impacts on the measurable labor productivity of the affected individuals. The short-term productivity depressing effects of air pollution exposure have been shown in settings ranging from low-skilled labor like agricultural work (Graff Zivin and Neidell 2012) or fruit picking (Chang et al. 2016), to sports (Lichter et al. 2017; Archsmith et al. 2018), politics (Heyes et al. 2019), high-skill jobs like software development (Holub and Thies 2022) and cognitively demanding work environments like call centers (Chang et al. 2019). While these studies confirm the existence of labor productivity channels, as of date there is no compelling evidence of how these channels manifest in longer-term labor marker outcomes.

3 Data

3.1 Lignite Mining Locations

To measure the causal impact of rising air pollution after the 1981/82 shock, we compare individuals and counties close to lignite deposits with those far away from lignite deposits.

To construct this distance-to-lignite-based research design, we begin by digitizing two maps that were published in the GDR's statistical yearbooks. The first map, from 1970, provides information on the locations and extent of underground coal fields in East Germany. We combine this with a second map, from 1972, which provides details on the extraction points of natural resources. From this map, we retrieve the geographical coordinates of all 21 lignite mines and 8 potash mines that were active in the country at the time of the 1981/82 shock.[12] We obtain historical shape files of East German administrative regions from für Kartographie und Geodäsie (2020). For each of the GDR's 217 counties, we calculate the straight-line distance between the county's geographical centroid and the full set of lignite and potash mining locations in kilometers. We repeat this exercise for the 7,565 GDR municipalities, as well as for all contemporary counties and municipalities observed in reunified Germany in the year 2021. We then define a treatment indicator based on whether a county or municipality centroid is within 60 kilometers of its nearest lignite mine or not. We use the locations of the potash mines analogously to define a placebo-treatment for exploitative industries and agglomeration which we use for robustness tests.

3.2 Air Quality Data

To measure the air pollution effect of increased lignite mining, we use data on historic sulfur dioxide air quality monitors operated under socialism. These data are provided by reunified Germany's Federal Environmental Agency (*Umweltbundesamt*). Confidential under socialism, the data were first de-encrypted, digitized and quality-assessed by environmental researchers there. In total, we retrieve annual mean readings from 131 air quality monitors along with their geolocations. While individual stations have readings starting in 1969, the data offer a wide coverage of sulfur dioxide levels in East Germany only from 1978 onwards. Between 1978 and 1988, we observe non-missing readings for, on average, 37 monitors per year. We use the geolocations of the measuring stations to assign air quality levels to East German counties using an inverse-distance weighted imputation algorithm. We calculate straight-line distances between each county centroid and each air quality monitor location and consider all stations within 100 kilometers of the centroid for averaging. Each centroid is then assigned the inverse-distance weighted average of air quality monitors in its area. We use a linear weight-decay function to assign less importance to monitors that are further away from the county centroid under consideration. Figure A1 in Appendix A plots the geocoded locations of air quality monitors and lignite mines.

3.3 Administrative Data on Output and Infant Health

We make extensive use of the GDR's official statistical yearbooks, an annual periodical containing a wide range of aggregate statistics (see Glitz and Meyersson (2020) for addi-

[12] We do not find any indications that East Germany opened additional mines between 1972 and 1982. We use the available information on the locations and extent of coal fields to construct a measure of all potential mining spots available within East Germany.

tional information on the yearbooks).[13] [14] Aggregate production statistics have already been digitized by Glitz and Meyersson (2020). We extend these data with time series on the bilateral trade of crude oil and bituminous coal between the GDR and other countries which we digitize from the yearbooks. We further use the data from the statistical yearbooks to build a yearly panel on the county-level for the short-term outcomes we study between 1970 and 1989. We obtain data on infant mortality and live births on the county level from Class and Driesch (2017). We enrich this data by also digitizing the time series on stillbirths, county-level population and overall mortality. We use these data to calculate a measure of net migration on the county-level by computing year-on-year population change net of births and deaths. This measure allows us to show that there were indeed no residential sorting reactions to increased air pollution in East Germany. We obtain a digitized and anonymized copy of the GDR's administrative birth register from the German Federal Archives (*Bundesarchiv*). The birth register is available between 1979 and 1989 and provides information on 2,210,149 individual births. For each of these births, we observe the location of birth at the municipality level, the exact birth date, the weight, height and gender of the newly born, as well as the newly born's parents' age, occupation and education.

3.4 Social Security Data

To investigate the long-term effects of air pollution exposure in socialist East Germany, we rely on administrative data from the social security records of the German Federal Employment Agency. After German reunification became official in October 1990, East German workers were required to be registered in unified social security records from January 1991 onwards. We obtain the universe of these social security records and construct a baseline for each individual East German worker. Using the first available record per individual, we are able to measure the occupational profile, education and location of 6,222,849 individuals as they emerge from socialism.[15] We follow Dauth et al. (2019), Boelmann et al. (2021) and Heise and Porzio (2022) in assuming that the first recorded location of individuals in the social security data corresponds to individuals' location under socialism.[16] We create individual labor market trajectories by following workers between 1992 and 2020. For each worker, we use detailed data on social security contributions and benefits to calculate

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

[14]Existing literature documents concerns about the trustworthiness of data reported by authoritarian regimes (Martínez 2022). We circumvent these issues by using disaggregate micro data whenever possible. In addition, we mainly use the data reported by the socialist regime 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).

[15]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.

[16]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 very small (Hunt 2006; Burchardi and Hassan 2013).

which share of these years they spend in employment, unemployment and retirement, respectively.[17] We also calculate their average daily wage over this time span by converting all earnings into constant 2015-Euros using a concatenated CPI series from the German Federal Statistical Office (*Statistisches Bundesamt*). To measure retirement, we create a measure of when individuals leave the labor force. Retirement information is explicitly recorded for individuals who are unemployed at the time of retirement or who retire early. For all other individuals, we follow Haywood et al. (2021) in assigning 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 record individuals' moves across space for inference in a movers design. We measure each individual's location each year and define a move as any incidence where an individual is registered in a different county than the previous year. We disregard moves between different municipalities of the same county. For each origin location that individuals are moving away from, we use the calculated straight-line distance between the respective municipality's centroid and the nearest lignite mine to determine whether an origin was a treatment or control region for exogenous air pollution exposure under socialism. We take information on GDR counties' local characteristics from Lichter et al. (2021) to show that there are no structural differences between treatment and control regions. We restrict the movers' sample to those individuals who move exactly once during their post-reunification life and whose one single move took place at exactly the first time we can observe such a move after reunification: between 1992 and 1993. We record each individuals' destination and origin county and municipality, we then restrict the analysis to all destinations that experience at least two migration inflows: one each from a treatment and from a control origin. These restrictions result in a final sample of $144,338$ individuals moving from 71 origin counties in East Germany to 391 destination counties in all of reunified Germany by 1993. 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 working life.

3.5 Survey Data

We use survey data from the German Socio-Economic Panel (G-SOEP, v38.1) to investigate the impact of pollution exposure on individual-level health outcomes. The G-SOEP is an annual panel of German households and individuals and was first established in 1984. The survey extended to East Germany right after German Reunification in the summer of 1990. Our analyses focus on the sample of individuals who confirm that they lived in East Germany in 1989 (Q: *Where did you live in 1989?*). From this sample, we select all individuals who participated in the summer 1990 survey and for whom we can observe their county of residence in East Germany in 1990. We assume that individuals' county

[17] The data also provide records of worker death, as long as death occurred before retirement. This implies that both death and retirement constitute absorbing states for individuals in our data. As it seems likely that individuals who die prematurely might also be more likely to retire earlier, we do not consider this outcome in our analyses.

of residence in the summer of 1990 corresponds to their location under socialism and use these counties to delineate individuals from treatment and control counties.

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 inquire 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 having visited a medical practitioner in the survey year (Q: *Have you seen a GP in the last three months?*) and an indicator for having missed time at work due to illness (Q: *How many days did you not work due to illness in 1992?*). In total, we observe these 1992 outcomes for 3,635 individuals who lived in East Germany under socialism.

Second, to measure long-term health effects, we use survey items that inquire about whether individuals have been diagnosed with a granular set of medical conditions (Q: *Have you ever been diagnosed by a doctor with one or more of the following diseases? (...)*). 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 et al. 2018) and two of which are placebo outcomes that are *un*related to pollution exposure (diabetes and chronic back pain). The full set of diagnoses items was first introduced to the G-SOEP in 2011 – 29 years after the air pollution shock in East Germany. In total, we observe diagnoses for 1,329 individuals for who we know where they lift in East Germany under socialism. As diagnoses were also surveyed in 2013, 2015, and 2017 we pool the 2011-17 cross sections as an additional robustness test to improve statistical power (pooling the cross sections results in 4,472 observations).

Lastly, we use G-SOEP to show that the labor market effects we observe in social security data are also present in the 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 in all analyses of survey data.

4 Research Design

4.1 Treatment Definition

We identify the causal effects of air pollution exposure in East Germany by comparing counties that are located close to lignite mines with counties that are not. This variation leverages the fact that it was easier to substitute lignite for oil in counties close to mines because lignite coal is unsuited 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, this relevant economic activity encompassed three broad categories: electricity generation, heat generation in industrial applications, and domestic heating with lignite briquettes. In Section 5.1.1 we to confirm that our treatment definition directly maps to large and persistent differences in local levels of air pollution. To this end, we use historic air quality monitor readings as a catch-all measure of pollution exposure that is independent of the definition of emission sources.

4.2 Short-Term Effects: Difference-in-Differences

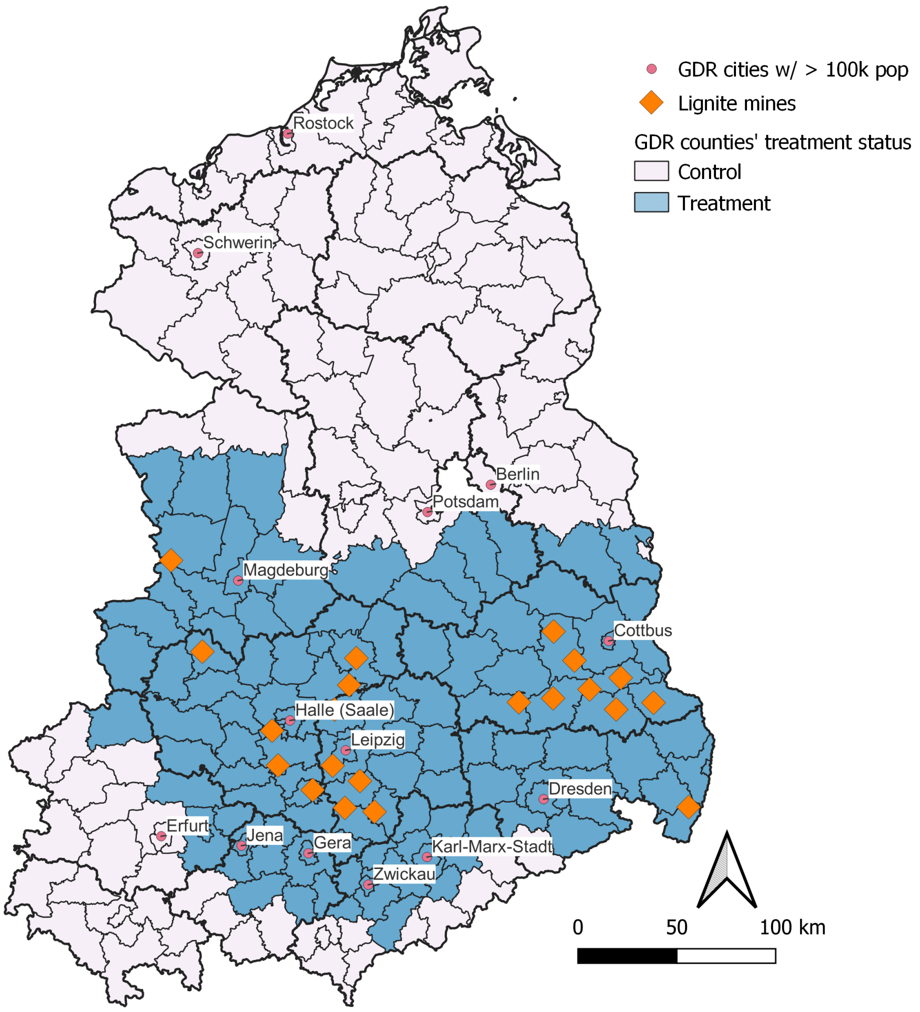
To identify the short-term effects of the lignite shock and its impacts on air pollution, we use a difference-in-differences analysis. We compare the development of counties over time based on whether they are located close to lignite mines, and could therefore easily substitute lignite for oil, or not. Difference-in-differences estimation allows us to control for other characteristics of the treated counties that might potentially correlate with air pollution, including unobserved time-invariant covariates. For instance, air pollution may predominantly be present 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

$$Y_{it} = \alpha_i + \phi_t + \big(1[t > 1982] \times D_i\big)\beta + \epsilon_{it} \tag{1}$$

where the outcome variable Y_{it} is either a county's interpolated mean annual sulfur dioxide pollution, the county's annual infant mortality rate per 1,000 live births or the corresponding average infant birth weight. Each outcome is regressed on a county fixed effect α_i, a year fixed effect ϕ_t and a post-treatment indicator $1[t > 1982]$ interacted with an indicator of treatment status D_i. Treatment status is assigned to counties with at least one lignite mining operation within 60 kilometers of the county's geographical centroid, as depicted in Figure 3. Equation 1 is a standard two-way fixed effect regression specification common in difference-in-differences settings. To account for potentially diverging trends, we further run a specification based on Equation 1 including a county-specific linear time trend. Given this setting of a single uniform policy change, all treatment counties are treated at the same time and the β-coefficient from estimating Equation 1 with OLS on the balanced panel of counties will yield a straight-forward estimate of the average treatment effect on the treated counties (Roth et al. 2022). In the appendix, we report results using alternative treatment definitions. We vary the threshold distance between 40 and 80 kilometers and repeat our analyses using non-parametric distance bins in 15 kilometer intervals. Generally, the direction and magnitude of our estimates remains unaffected and the specifications using non-parametric distance bins confirm that significant treatment effects can be observed right up to the 60 kilometer delineation. We assign treatment based on the distance of counties to mines, rather than industrial installations such as power plants, because this assignment allows us to capture a wider range of lignite usage applications. Aside from electricity generation, lignite was also used in heating systems and industrial applications (e.g. in the petrochemical industry). While most power plants were located close to lignite mines, disregarding alternative use cases for lignite could potentially bias our results.

Moreover, the placement of power plants maybe an endogenous policy variable. In contrast, the presence of lignite deposits suitable for mining was outside the realm of political control. In Chapter 5.1.5, we augment our difference-in-differences approach to estimate the infant mortality elasticity of air pollution. To this end, we use the binary treatment indicator described above as an instrument for the actual level of local air pollution, as measured by air quality monitors in East Germany. Then, taking both infant mortality rates and air pollution readings in logs, we estimate the elasticity based on this DiD-IV specification.

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. Map depicts treatment counties as blue-shaded areas and marks the 21 lignite mines considered in our analysis. We report alternative maps that account for local wind directions in the appendix.

The key challenge to identification in difference-in-differences designs is that the true counterfactual outcome for treated observations, in this case counties, can never be observed. Difference-in-differences estimation overcomes this challenge by assuming 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). To test this assumption of parallel trends for the increase in lignite usage in the GDR, we report the results of event-study regressions in Chapter 5. In these regressions, we introduce lags and leads of the interaction term between post-treatment indicator and treatment status. We plot these coefficients relative to the last pre-treatment time period and show that none of the coefficients prior to 1982 is statistically different from zero. This suggests that in this application the assumption of parallel trends is likely to hold.

To probe the robustness of our results, we carry out two falsification exercises in the appendix. First, we conduct placebo tests in space where we assign treatment status to counties not based on their distance to lignite mines but based on their distance to potash mines. Using potash mines allows us to rule out that our results are driven by the broader impacts of exploitative industries and agglomeration. Second, we conduct placebo tests in time where we assign treatment to the same set of counties but three, six, or nine years earlier. To do so, we discard all data post the actual treatment date in 1982 and shift our binary treatment indicator to different years. This timing variation allows us to rule out that our results are driven by general differences between the groups of counties which we compare against each other.

In the spirit of Rambachan and Roth (2020), we are applying a 'design-based' approach to difference-in-differences. Our analysis includes all counties of the GDR and therefore is not sampled from a larger super-population. As such, we do not consider the population of counties studied a random draw but rather assume that, conditional on fixed effects, only treatment assignment is random. Accordingly, we cluster standard errors at the county-level for valid inference.

4.3 Long-Term Effects: Linear Regression

To identify the long-term effects of air pollution exposure, we leverage the fact that individuals were not able to endogenously sort across space under socialism. Due to restrictions on the freedom of movement, insufficient housing supply and the non-existence of a housing market in the first place, individuals were exposed to the lignite-induced air pollution shock based on their pre-determined and exogenous placement in counties of East Germany. We corroborate this assumption by showing that there was no statistically significant migration response to the air pollution shock while the socialist party dictatorship remained in power. Looking at long-term labor market outcomes, we compare individuals who were exposed to the exogenous air pollution shock under socialism with those who were not. We assign treatment and control status to individuals in post-reunification admin data based on recorded locations of East Germans when they appear in social security records for the very first time. We define individuals to have been subject to the historic air pollution shock if they spent their life under socialism in one of the counties that is located within 60 kilometers of a lignite mine. As a first step, we estimate

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

with OLS on 6,222,849 East German workers who appear in social security data after German reunification. In Equation 2, the outcome variable Y_i is one of the following measures of individual labor market trajectories: the cumulative number of days individual i spends in employment between 1992 and 2020, the cumulative number of days individual i spends in unemployment, the age at which individual i retires from the labor force, and the average daily wage individual i incurs between 1992 and 2020 measured in constant 2015-euros.[18] D_c is a binary variable indicating whether the county in which individual i lived under socialism was exposed to the air pollution shock and β is our treatment coefficient of interest. We also control for a vector of baseline characteristics describing each individual worker's occupational profile as they emerge from socialism. Baseline characteristics include a full set of year-of-birth fixed effects, a full set of state-of-residence fixed effects, a set of dummy variables for each individuals' highest level of education, a full set of five-digit occupational code-fixed effects, and a full set of three-digit industry code-fixed effects. We use these baseline characteristics to condition on individuals' initial characteristics at the end of socialism. Some of these characteristics, for instance educational attainment, may become endogenous after socialism. However, during socialism, access to (higher) education and occupational sorting was predominantly determined by political considerations rather than merit.[19] We therefore freeze individual baseline characteristics at each worker's first record, rather than updating the information from social security data moving forward in time. As a robustness test, we also report specifications without including control variables that may become endogenous to air pollution exposure after German reunification. We cluster standard errors on the county-level for each individual's location in 1991/92.

4.4 Long-Term Effects: Inverse Movers Design

Equation 2 allows us to compare the long-term labor market outcomes of individuals who lived in areas exposed to the exogenous air pollution shock under socialism with the labor market performance of those who do not. However, these labor market outcomes constitute a weighted-average effect of the labor market capital that individuals have accumulated both under socialism and in reunified Germany. As such, the labor market outcomes we observe may be impacted both by the historic air pollution shock as well as by other

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

[19] See the 'historical background' section in Dauth et al. (2019) for an overview of occupational choice under socialism. As Dauth et al. (2019) argue, 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.

variation that impacts individuals' exposure to air pollution and related place-based effects after German reunification. Importantly, when German reunification becomes official in the fall of 1990, restrictions on movement have also been lifted. Post-reunification migration behavior is unconstrained and may therefore induce sorting on exposure. If individuals relocate in space after reunification and their choice of destination is impacted by their vulnerability to air pollution, we may underestimate the true impact of historic pollution exposure. Similarly, if place-based policies after reunification correlate with the pollution shock, this may bias our estimates. For instance, if counties with now obsolete lignite industries receive disproportionate amounts of subsidies in the transformation process, this may impact the labor market outcomes of the individuals who lived in these areas under socialism. German reunification also started a major process of economic transformation across all areas of East Germany (Mergele et al. 2025). The potentially differential effects of transformation across regional labor markets further complicate inference in a simple regression framework.

To isolate the true long-term causal effect of air pollution exposure, we build a movers design leveraging post-reunification social security data. In particular, we extend the canonical movers design used in health economics (Baum et al. 2020; Deryugina and Molitor 2021; Finkelstein et al. 2021) by *inverting* it 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 two separate counties of socialist East Germany. Individual A grows up in a county that is close to a lignite mine and therefore exposed to an exogenous increase in air pollution following the 1981/82 shock. Individual B grows up in a county that is 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. 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. Using the population of East German workers and comparing all pairs of such individuals, conditional on their observable characteristics, allows us to estimate the causal effect of the historic air pollution shock. Controlling for both individuals' joint destination county allows us to account for any variation related to pull factors of migration as we are conditioning on the decision whether and where to migrate to. Controlling for destination further allows us to account for any variation related to differential place-based effects in post-reunification Germany, as long as these effects 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. Here, β is the coefficient of interest measuring the effect of having lived in a county exposed to the air pollution shock under socialism. Equation 3 also includes destination fixed effects η. Notably, because treatment assignment is a time-invariant characteristic of origin counties, we cannot use origin county fixed effects in Equation 3. We rerun our results with specifications including state level fixed effects as an additional robustness test.

The identifying assumption underlying Equation 3 is that no unobservable differences between treatment and control origin counties correlate with the later-life labor market outcomes of the migrating individuals. To assess this assumption, we conduct extensive balance tests between treatment and control counties in the results section. We further rule out that our results are impacted by differential selection into migration between treatment and control counties by predicting individuals' decision to move with the binary treatment variable. While the resulting coefficient is statistically significant, the economic magnitude of the effect remains minuscule and corresponds to a 0.5% increase in the likelihood of migrating in the first year after reunification. We take this as confirmation that, in the context of German reunification, historic exposure to air pollution was a determinant of migration decisions but not a first order concern. In additional robustness tests, we rerun our specifications either excluding or only considering coal miners and power plant workers. That way, we can show that coal miners and power plant workers were neither driving the observed effects nor subject to particularly large effect sizes.

4.5 Mechanism: Health and Healthcare Effects of Air Pollution

To investigate the mechanism behind long-term effects of sustained air pollution exposure in the labor market, we compare the health outcomes of East Germans who lived in treatment counties, exposed to the air pollution shock, with those who lived in control counties. We estimate the following equation with OLS:

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

where $Y_{i,t}$ measures health outcomes for individual i in survey year t. The indicator variable $D_{i,t=1990}$ is equal to one for individuals who lived in a treated county in 1990 and zero otherwise. The vector X_i includes a full set of 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 common to those from treatment and control counties. X_i further includes a set of 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.[20] We use survey weights provided by the G-SOEP and we compute two-way clustered standard errors on the household and 1990-residence county level.[21] 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 variable.

5 Results

5.1 Short-Term Effects

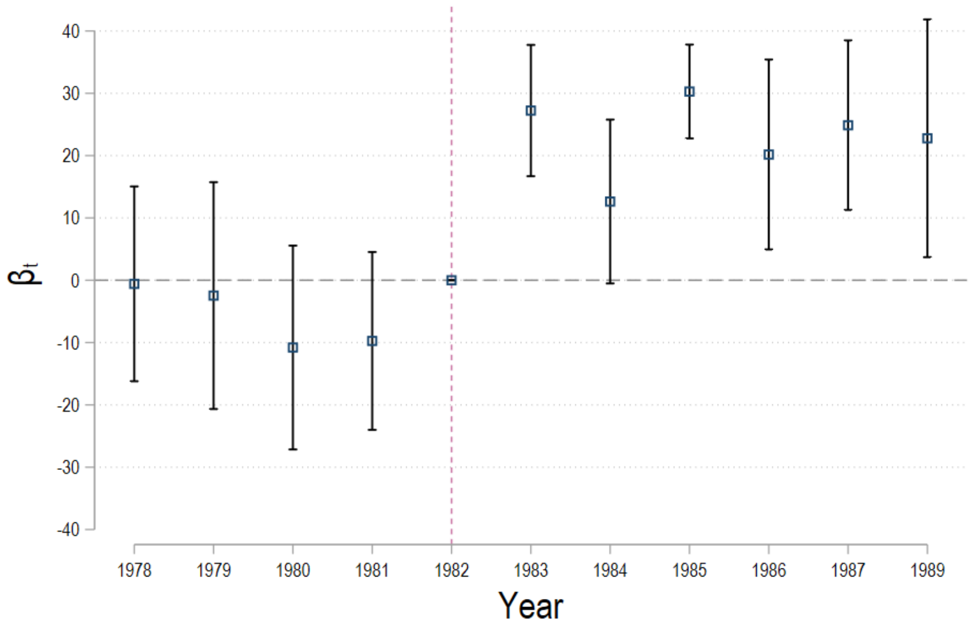
5.1.1 Effect on Air Pollution

Figure 4 reports the coefficients of an event study difference-in-differences regression of annual mean sulfur dioxide air pollution. While the yearly coefficients for East German counties close to lignite mines are insignificant and close to zero before 1982, there is a large and significant increase in air pollution for the years after 1982. The lack of differential effects before the trade shock supports the causal interpretation of these coefficients. Rather than being driven by underlying differences between the treatment and control counties, the difference in air pollution is caused by the natural experiment resulting from the lignite shock. While the magnitude of the individual year effects varies in the first years after the trade shock, differences in air pollution between treatment and control counties stabilise over time. This pattern is consistent with the evolution of lignite production outputs in Figure 2. However, as we drop counties with insufficient air quality monitor coverage, it does not trace the development of lignite output exactly. Lignite output shows a rapid but unsteady increase in production immediately after the shock before plateauing at a level of around 310 million tons per year after 1985. Importantly, Figure 4 documents that differences in air pollution exposure between treatment and control counties remained at significant and persistent levels for the remainder of the GDR's existence as a sovereign country.

[20] These specifications are comparable to the movers design that we estimate on labor market outcomes. The survey data, however, do not allow us to further restrict the comparison to individuals who moved to the same place *at the same time*.

[21] 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.

Figure 4: Sulfur Dioxide Air Pollution



Coefficient plot for an event study difference-in-differences regression of mean annual sulfur dioxide air pollution in GDR counties on a binary treatment interacted with yearly dummies. Treatment is defined as binary indicator for whether a county's geographical centroid is within 60 kilometers of the nearest lignite mine. The coefficient for 1982 is omitted as a reference category. The coefficients β_t are measured in $\mu g/m^3$ of sulfur dioxide.

5.1.2 Effect on Infant Birth Weights

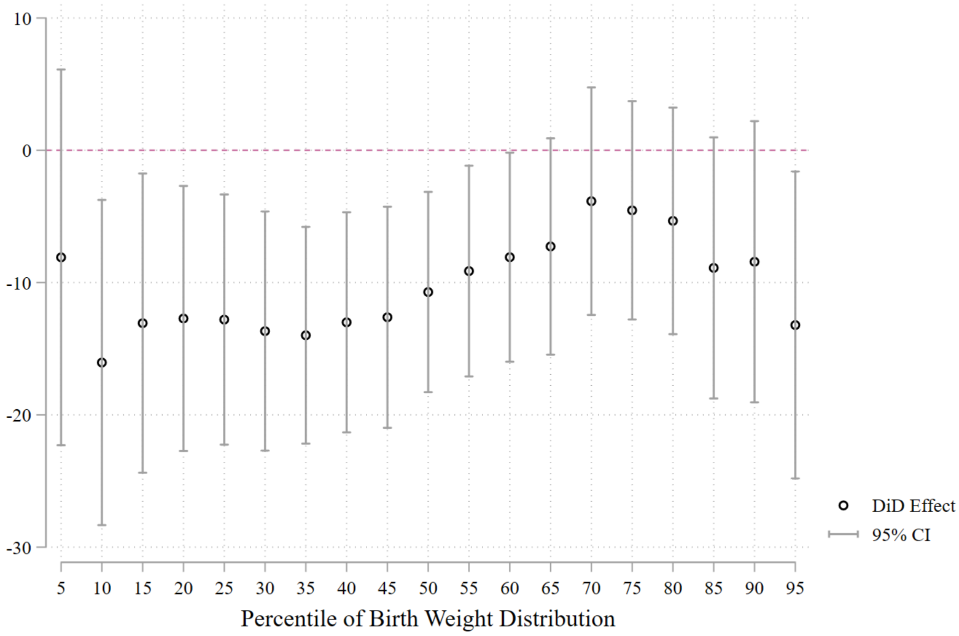
We report the development of average infant weights at birth over time in Figure 5. The dashed line indicates mean birth weights by year of birth in counties within 60 kilometers of a lignite mine. The solid line indicates the corresponding time series for the control counties. The two time series follow a parallel trend until 1982, supporting the notion that there are no structural differences in average birth weights between treatment and control counties before the trade shock. For the birth years after 1982, a large and persistent gap opens between births in treatment versus control counties. Infants who are born in treated counties after the pollution shock, from that point in time onward, have persistently lower average weight at birth. This difference between treatment and control counties creates a wedge between the two time series and remains largely unchanged until the end of the GDR in 1989. We interpret this as support for our assumption that the authoritarian nature of the socialist regime prevented mitigating responses by affected individuals in the treated counties. Had individuals been able to endogenously adjust to reduce the impact of the air pollution shock on newly born infants, the gap would likely have narrowed, or even closed entirely, over time. In general, the birth weights time series in both treatment and control counties follow a positive macro health trend. With the notable exception of the years between 1980 and 1982, average birth weights are increasing across the board. Average birth weights continue to increase even after the trade shock, but now with a clear level difference between treatment and control.

Figure 5: Infant Birth Weights

Mean birth weight for children born in counties that are within 60 kilometers of a lignite mine (treatment) or more than 60 kilometers away from the nearest lignite mine (control), by children's' year of birth. The two time series are adjusted to start at a common origin value and smoothed with a linear trend per group. Figure A3 reports a difference-in-differences event plot.

The medical literature suggest that the mean effects of air pollution on birth weights may be heterogeneous throughout the birth weight distribution (Almond et al. 2005; Smith et al. 2015; Schwarz et al. 2019; Fong et al. 2019). To account for this heterogeneity, we repeat our analysis of the effect of air pollution on birth weights in East Germany in Figure 6. Figure 6 reports coefficients and 95% confidence intervals for separate difference-in-differences regressions of birth weights on ventiles of the birth weight distribution. The results confirm our previous analysis and the point estimates for the effect of air pollution on birth weights are negative at every point of the distribution. However, we also see substantial effect heterogeneity. Individual effects are significant at and below the 60th percentile of the birth weight distribution. The negative effect of air pollution exposure on birth weights is larger at lower ventiles and increases in absolute size from -7.357 grams at the 60th to -16.737 grams at the 10th percentile of the distribution. We report corresponding regression estimates and difference-in-differences identification tests for distribution deciles in Table B.10.

Figure 6: Air Pollution Effects Throughout the Infant Birth Weights Distribution

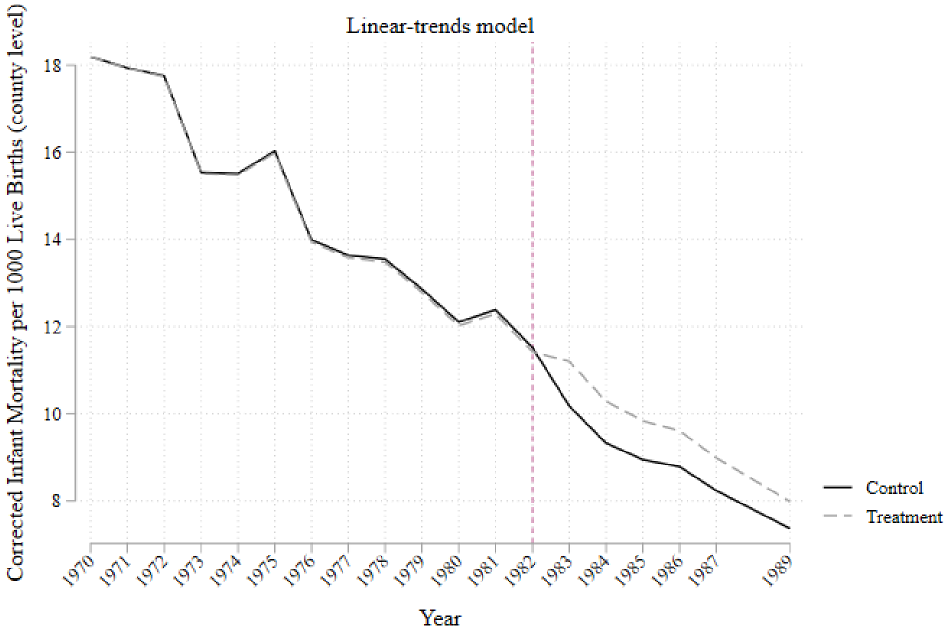


Air pollution treatment effect on birth weight for children born in counties that are within 60 kilometers of a lignite mine rather than more than 60 kilometers away from the nearest lignite mine after 1982. Coefficient estimates from running separate difference-in-differences regressions at each percentile.

5.1.3 Effect on Infant Mortality

We report the development of average infant mortality rates by county group in Figure 7. The dashed line indicates mean annual infant mortality rates for counties within 60 kilometers of a lignite mine, while the solid line indicates the corresponding figures for control counties more than 60 kilometers away from the nearest lignite mine. Because we take information on county-level infant mortality from the GDR's statistical yearbook rather than from the administrative birth register (see Chapter 3), we observe the development of this time series over a longer pre-period than the birth weights series. Before 1982, the development of infant mortality rates is virtually indistinguishable between treatment and control counties. However, an immediate and persistent gap opens following the air pollution shock. After 1982 and for the remainder of the GDR's existence, counties that are within 60 kilometers of a lignite mine display substantially higher rates of infant mortality than counties further away from a mine. Similar to infant birth weights in Figure 5, infant mortality rates display a persistent wedge between both time series after the trade shock. Here as well, the non-dynamic effect of the air pollution shock supports the assumption that authoritarian government policies in East Germany did not leave individuals with sufficient freedom to endogenously respond and mitigate the adverse effects of the pollution shock. Similar to birth weights, infant mortality rates also follow a macro health trend throughout our entire sample. While average infant mortality is decreasing in both treatment and control counties before and after the trade shock, the level at which infant mortality declines in treatment counties is substantially lower after the shock.

Figure 7: Infant Mortality Rates



Mean infant mortality rates per 1,000 live births in counties that are within 60 kilometers of a lignite mine (treatment) or more than 60 kilometers away from the nearest lignite mine (control), by children's' year of birth. The two time series are adjusted to start at a common origin value and smoothed with a linear trend per group. Figure A4 reports a difference-in-differences event plot.

5.1.4 Difference-in-Differences: Regression Results

We report the results from estimating difference-in-differences regressions of Equation 1 for our main short-term outcomes in Table 1. Comparing counties with and without lignite mines over time reveals large and significant treatment effects. Column (1) reports the results for estimating the difference-in-differences regression for sulfur dioxide air pollution. After the trade shock, counties close to lignite mines experience a relative increase in mean annual sulfur dioxide pollution of 27.553 $\mu g/m^3$. The effect is large, corresponding to moving from the 10th to the 90th percentile of SO_2 monitor locations in the 2019 US.[22] The effect is also sizeable by the standards of its own time and context. It corresponds to a persistent shock of almost 18% relative to the all-time mean of counties in the GDR or 169% of the pre-treatment baseline difference between treatment and control counties in 1981.

Column (2) reports the estimated effect on birth weights. Unlike the other outcomes presented in Table 1, we use individual level data to estimate the difference-in-differences effect for birth weights as it allows us to use additional control variables on the individual level in the regression. These additional controls include the parents' age and socioeconomic status (occupation and education), as well as for the infant's sex and exact date of birth.

[22] https://www.epa.gov/air-trends/sulfur-dioxide-trends, last accessed 18 November 2022.

We find that the treatment has a strongly significant negative effect on average birth weights in the affected counties. That said, the magnitude of the effect is relatively small. Infants born in the treatment counties after 1982 are on average 8.33 grams lighter than comparable infants born in the control counties. This corresponds to an effect size of about 0.25% relative to the all-time mean across GDR counties. Relative to the pre-treatment baseline difference between treatment and control counties in 1981, this still constitutes an effect size of about 78%. Generally, multiple channels may be important to put this effect size in this context. First, there is a strong macro health trend of average birth weights increasing over time. This trend holds for both treatment and control group, and it holds both before and after treatment. Second, as we show in Section 5.1.2, the effect of air pollution on birth weights does not predominantly manifest at the mean but at different points in the lower half of the birth weight. Third, it may also be the case that air pollution effects on infant health are more severe after birth than in utero. We investigate this last point further by looking at the treatment effect on stillbirths in Table 2 below.

Table 1: Difference-in-Differences: Regression Results on Short-Term Outcomes

	SO_2 Air Pollution	Infant Birth Weights	Infant Mortality
	(1)	(2)	(3)
$1[t>1982] \times D_i$	27.553***	-8.328***	0.879***
	(3.178)	(0.226)	(0.324)
Unit	$\mu g/m^3$	gram	per 1,000 births
Mean Y	156.002	3,282.4	12.955
Observations	2,069	2,210,149	4,104
R-squared	0.850	0.030	0.386

Note: Table 1 reports results from estimating Equation 1 using ordinary least squares. Column (2) includes additional control variables. 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. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Column (3) in Table 1 reports difference-in-differences results for infant mortality. Counties close to a lignite mine see a large and significant increase in infant mortality relative to counties far away from lignite mines. On average, treated counties see an additional 0.88 infant deaths per 1,000 live births per year after treatment. Relative to the 1981 baseline, this constitutes a 7.44% increase in annual infant mortality. Some of the alternative specifications we consider for robustness tests suggest that this effect may increase up to 11% relative to the pre-treatment baseline. The effect is about 121% of the baseline difference between treatment and control counties. In contemporary terms, 0.88 additional infant deaths per 1,000 live births is equivalent to moving from the 2019 US mainland average infant mortality rate to the 2019 Puerto Rico average infant mortality rate.[23]

We extend this analysis by looking at two additional short-term variables in Table 2. First, in Column (1), we rerun our difference-in-differences specification to investigate the effect of air pollution on stillbirths. Stillbirths in East Germany were defined as infants born without detectable breath or heartbeat. As such, air pollution effects on stillborn

[23] https://www.cia.gov/the-world-factbook/field/infant-mortality-rate/country-comparison, last accessed 18 November 2022.

infants will reflect effects of in utero exposure to air pollution rather than exposure in the first few months of life after birth. While Column (1) returns a positive coefficient, the result is not statistically significant. This small effect size may in principle be driven by stillbirths being a very rare event or mothers with increased stillbirth risk being given additional medical attention. However, in the context of the results on birth weights and early life mortality, the results on stillbirths may also support the interpretation of air pollution in East Germany seemingly affecting infant health primarily through post-birth rather than in utero exposure.

Table 2: Difference-in-Differences: Regression Results on Supplemental Outcomes

	Stillbirths	Net Migration
	(1)	(2)
1[t>1982] × D_i	0.109	-118.66
	(0.544)	(92.69)
Unit	individuals	individuals
Mean Y	7.147	-83.252
Observations	4,100	3,668
R-squared	0.277	0.373

Note: Table 2 reports results from estimating Equation 1 using ordinary least squares. 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

Lastly, Column (2) of Table 2 reports results from running difference-in-differences regressions on net migration in East German counties close to lignite mines. This regression functions as the most direct test of whether or not individuals were able to endogenously sort away from pollution exposure in East Germany. We construct this measure of migration as the difference in a county's total population between t and $t+1$, net of deaths and births. The resulting coefficient is negative - indicating relative excess out-migration - but remains statistically indistinguishable from zero. The effect is not only insignificant but also reassuringly small. It is not considerably larger than the all-time mean of county-level out-migration and it is small relative to the average population size of East German counties.[24] We present a difference-in-differences event study plot for net migration in Figure A5. The absence of a pollution effect on net migration supports the notion that the restrictions on freedom of movement in the GDR did indeed limit individuals' ability to endogenously mitigate adverse pollution exposure effects - a crucial prerequisite for our analyses of both short-term and long-term outcomes.

5.1.5 Estimating Infant Mortality Elasticities

Next, we complement our previous analyses by estimating the elasticity of infant mortality to air pollution. Directly estimating the infant mortality elasticity provides us with a continuous, rather than additive, measure of the effect of air pollution on short-term health

[24] 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.

outcomes. Crucially, estimating the infant mortality elasticity also allows us to compare the effect of air pollution on infant mortality in East Germany to results from other settings. Given that East Germans were not able to endogenously mitigate air pollution effects, we expect to find an elasticity that is larger than comparable figures in the literature if these other estimates are potentially biased downwards by endogenous mitigation. To estimate the infant mortality elasticity, we regress the natural logarithm of the infant mortality rate on the natural logarithm of sulfur dioxide pollution readings. In turn, we generate exogenous variation in the air pollution readings by instrumenting them with our binary treatment indicator for counties that are close to lignite mines or not.

Table 3: DiD-IV: Estimating Infant Mortality Elasticities

	Reduced Form	First Stage	Two-Stage Least Squares	Two-Stage Least Squares	Two-Stage Least Squares
	(1)	(2)	(3)	(4)	(5)
$\ln(SO_2)$			0.263**	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
Observations	2,070	2,070	2,070	2,070	2,070
R-squared	0.090	0.146	0.893	0.887	0.966
Kleibergen-Paap F		118.35	118.35	35.72	26.67

Note: Table 3 reports results from estimating the infant mortality elasticity to air pollution, using $1[t > 1982] \times D_i$ as an instrument for $ln(SO_2)$. The dependent variable in Columns (1), (3), (4), and (5) is the natural logarithm of infant deaths per $1,000$ live births per county per year in East Germany. The dependent variable in Column (2) is $ln(SO_2)$. The analysis uses data from the years 1978 to 1989 in which air quality monitor readings are available. 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

Table 3 reports the results from running difference-in-differences specifications using proximity to lignite mines as an instrumental variable for SO_2 air pollution. Column (1) reports the estimated reduced form relationship between infant mortality rates and our binary treatment indicator. The reduced form estimate mirrors the results from Table 1 but, due to the log-linear specification, we can now interpret β as the percentage change in local infant mortality rates in treatment regions after the lignite shock. After the shock, counties that are close to lignite mines on average experience a 9.7% increase in the infant mortality rate relative to counties that are far away.[25] Column (2) reports the first stage results relating the instrument, being close to a lignite mine after the lignite shock, to the endogenous variable, $ln(SO_2)$. The first stage results show that the treatment counties experience a 36.6% increase in SO_2 air pollution over the implied counterfactual. Importantly, the first stage results also suggest that the instrument is capturing strong variation in local air pollution levels with a Kleibergen-Paap F-statistic of 118.35. Finally,

[25]The coefficient differs slightly from the estimated percentage effect reported for Table 1 because the sample used for Table 3 only covers the time period beginning in 1978 when air pollution data is available.

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 from the relevant literature. Our elasticity estimate is considerably higher than the $0.07 - 0.13$ reported by Luechinger (2014), the setting closest to our study.[26] Our estimates are also considerably larger than elasticities for CO pollution in the US, estimated between 0.04 (Currie et al. 2009) and 0.146 (Knittel et al. 2016). Estimates of comparable magnitude are in turn found in setting with higher base levels of infant mortality and higher pollution exposure: Arceo et al. (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. We interpret our comparatively large elasticity estimates as additional support for the notion that East Germany is a unique setting to study air pollution effects because authoritarian restrictions on individual freedoms close the endogenous mitigation channel.

5.1.6 Robustness Short-Term Results

We probe the robustness of our regression results with a number of alternative specifications in Appendix B. First, we re-estimate our difference-in-differences specifications using alternative treatment delineations. Tables B.1 to B.5 repeat our estimations from Tables 1 and 2 but vary the threshold distance to the nearest lignite mine from 60 kilometers to 40, 50, 70, and 80 kilometers instead. 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, all results remain significant and precisely estimated. We re-estimate the specifications for infant mortality using non-parametric distance bins in 15 kilometer increments instead of binary treatment variables in Figure A6. Using distance bins confirms that treatment effects indeed are significant until about 60 kilometers. Table B.6 reports difference-in-differences results using continuous measures of treatment. We define two such continuous treatment measures that are supposed to 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, albeit with some loss of precision. Next, we conduct two sets of falsification tests. First, we assign a placebo-treatment by varying the geographical treatment assignment. Instead

[26] 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.

of lignite mines, we assign treatment as being close to a potash mine - the only other abundantly available natural resource in East Germany. This way, we test whether our results are driven by the lignite-induced air pollution shock or whether they represent the effects of exploitative industries and industrial agglomeration more generally. Second, 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.7 and B.8 report the results from placebo-testing the air pollution shock using distance to potash mines. Reassuringly, all coefficients are statistically insignificant and also much smaller in magnitude than the effects of our main analyses. Table B.9 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. One of the likely pollution emission sources in East Germany is domestic heating with lignite briquettes. We explicitly consider this pollution channel by leveraging variation in within-county deviations from temperature averages in a triple difference analysis (Gruber 1994; Olden and Møen 2022). In particular, we generate a third difference by redefining our control group according to whether counties experienced an above median number of frost days in a given year. If colder years go along with more domestic heating, these years should also result in more exposure to air pollution as lignite heating becomes more common after the lignite shock. Table B.11 confirms our previous findings of detrimental effects of air pollution on infant mortality and birth weights using an according triple difference specification. We further find that all reported results are robust to excluding the border regions between East Germany and its neighbor countries, suggesting that the effects are not driven by cross-border spillovers between Eastern Bloc members. When we repeat our estimation dropping each of the 15 GDR states one at a time, we find the results virtually unchanged. Our results are robust to re-estimating the difference-in-differences models with county-specific linear trends, although precision is slightly lower across the board.[27]

5.2 Long-Term Effects

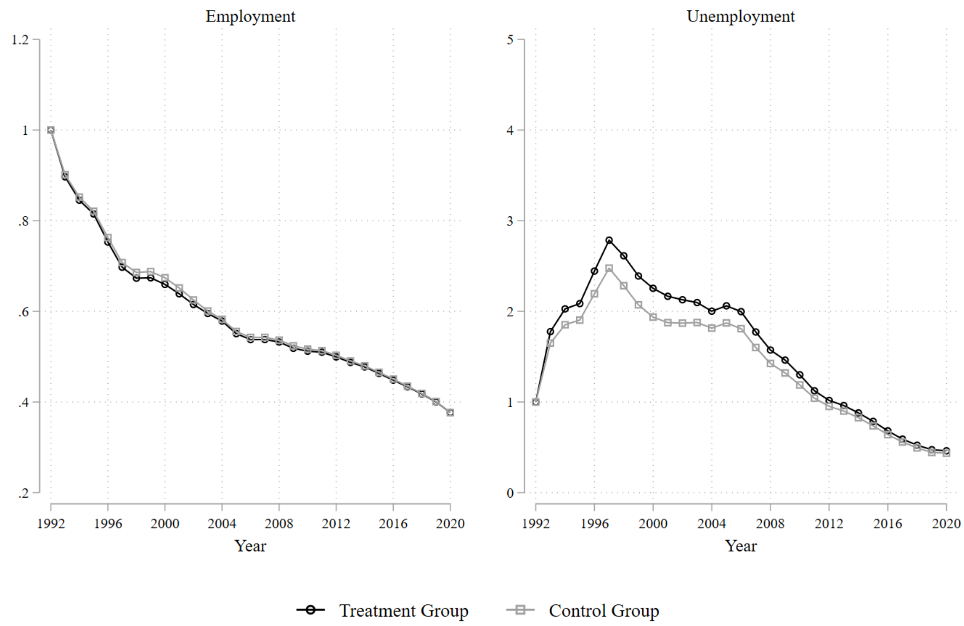
5.2.1 Ordinary Least Squares Regressions

We explore the long-term effects of air pollution on workers in East Germany by first analysing the labor market careers of the pooled 1991 and 1992 cohorts. We separate this pooled cohort in two groups, one composed of individuals who spent life under socialism in counties within 60 kilometers of a lignite mine and one composed of those who did not. Figure 8 traces the employment and unemployment time series of these two groups of individuals over time. The post-reunification period is marked by general economic dif-

[27]Results for these last three robustness tests are available upon request.

ficulty for workers in formerly socialist East Germany. Employment rapidly decreases and unemployment drastically rises. These developments are more pronounced for individuals who spent life under socialism in counties exposed to the air pollution shock. Individuals from counties close to lignite mines experience a larger increase in relative unemployment and a slightly larger reduction in relative employment figures.

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



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.

We report results from estimating Equation 2 with OLS in Table 4. Table 4 compares individuals' labor market trajectories between 1992 and 2020 conditional on baseline observable characteristics (age, sex, education, occupation, industry, state). Column (1) shows that, in the 28 years following German reunification, individuals who spent life under socialism in counties close to lignite mines spend significantly less time in employment. The total time they spend in employment is 0.241 or almost 3 months lower relative to the individuals from areas far away from lignite mines. These individuals instead spend more time in unemployment (Column (2)) and retire slightly earlier (Column (3)). There is no evidence of a significant impact on daily wages earned (Column (4)). We report the full results for the regressions presented in Table 4 in Tables B.12 to B.14 in Appendix B.

5.2.2 Inverse Movers Design

The OLS results in Table 4 are unlikely to reflect the true causal effect of air pollution on the affected individuals' labor market outcomes. There are three main concerns complicating causal inference in this case. First, both the results in Figure 8 and Table 4 may be

Table 4: OLS Regression Results for Long-Term Outcomes

	Employment	Unemployment	Retirement Age	Wages
	(1)	(2)	(3)	(4)
D_c	-0.241***	0.178***	-0.046**	0.111
	(0.0679)	(0.053)	(0.020)	(0.488)
Unit	Years	Years	Years	EUR
Mean Y	15.849	3.865	62.272	63.844
Observations	6,108,781	6,108,781	6,108,781	6,108,781
R-squared	0.440	0.133	0.063	0.350

Note: Table 4 reports results from estimating Equation 2 using ordinary least squares. 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

impacted by the fact that post-reunification labor markets may have differed, substantially, between treatment and control counties. The industrial structure of East Germany underwent a rapid and radical transformation process (Dauth et al. 2019; Mergele et al. 2025; Hennicke 2022). As some industries became obsolete, individual labor market trajectories may have been affected by differential trends in structural economic reform across regions. Obsolete industrial activities included some of the GDR's lignite mining and processing facilities such as lignite briquette factories. The presence of differential structural trends may lead us to overestimate the effect of historic air pollution exposure on individual labor market outcomes. At the same time, we may also be underestimating the long-term effects of air pollution. Restrictions on freedom of movement were lifted and housing markets became competitive after German reunification. This afforded individuals with more adaptability to endogenously influence their subsequent air pollution exposure. Vulnerable individuals engaging in residential sorting may make it difficult to capture the full extent of historic exposure effects in OLS regressions. Similarly, large-scale place-based subsidy programs after German reunification may differentially impact the economic context in which individuals' post-reunification labor market trajectories manifest.[28] Public investments that disproportionally target areas close to lignite deposits could then offset some of the adverse effects of air pollution and downward-bias our estimates. In contrast, if public investments disproportionally target areas far away from lignite deposits, we may be overestimating the consequences of historic air pollution.

To overcome these identification challenges, we leverage the inverse movers design described in Chapter 4. We isolate historic variation in air pollution exposure by focusing on individuals who migrate directly after German reunification. Intuitively, we want to compare pairs of individuals who move to the same destination region directly after reunification – as this will allow us to account for the differential effects of reunification and transformation described above with a destination county fixed effect.[29] Each pair of indi-

[28] See Etzel et al. (2021) for a study of the GRW, the largest post-reunification place-based subsidy program in reunified Germany.

[29] As we condition on pairs of individuals spending their entire post-reunification life (from 1993 onward)

viduals consists of one moving from an origin exposed to historic air pollution increases and one moving from a control origin. Comparing the universe of these pairs and conditioning on their post-socialism baseline observables will allow us to estimate the causal effect of historic air pollution on labor market outcomes, as long as there are no unobservable differences between origin regions that also correlate with future labor market success.

Table 5: Inverse Movers Design Results for Long-Term Outcomes

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

Note: Table 5 reports results from estimating Equation 3 using ordinary least squares. 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. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 5 reports the results from estimating the movers design-regression on our sample of 144,334 East German movers who migrate to another county in Germany in 1993. As in Table 4, we find a large and significant negative effect of historic air pollution exposure on long-term employment in Column (1). The reduction in employment over the 28 years following German reunification is 54% larger relative to the OLS results. Treated individuals spend 0.372 fewer years, or close to 4.5 months, in employment than comparable individuals who live in the same destination but moved there from a control origin. Column (2) repeats this analysis looking at the amount of years individuals spend in unemployment instead. In contrast to the OLS results, we find a considerably smaller coefficient that is not statistically significant. We do, however, find a much larger effect on retirement in Column (3). Individuals exposed to historic air pollution retire at a significantly younger age than individuals who spent life under socialism in control counties. The effect is almost four times as large as the corresponding OLS effect and may in part explain the insignificant result on employment in Column (2). Jointly, the results suggest that individuals exposed to air pollution in the GDR leave the labor force altogether rather than spending more time in unemployment. On average, individuals from treatment origins are 0.163 or two months younger when they retire. Intriguingly, the movers design reveals that individuals from treatment origins also incur a statistically significant wage penalty relative to individuals from control origins. Over the 28 post-reunification years, the average daily wage of treated individuals is 2.05 EUR/day below that of control individuals - even conditional on observable characteristics and both individuals spending the 28 years in the same destination region. This effect corresponds to a 2.94% wage penalty relative to the all-time mean

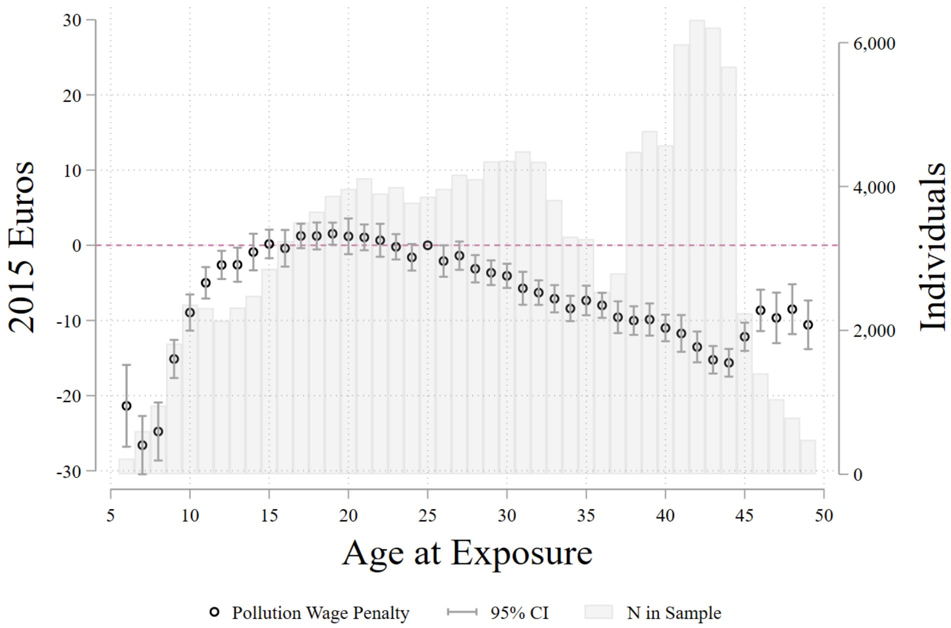
in the same location, we are able to control for any place-based variation of this nature.

daily wage of movers in our data. We report the full regression specifications underlying the results presented in Table 5 in Tables B.15 to B.18 in Appendix B.

5.2.3 Effect Heterogeneity by Age

Our findings report significant detrimental effects of long-term exposure to air pollution in individuals. But which individuals are most affected? To explore effect heterogeneity, we study how labor market effects of prolonged air pollution exposure vary throughout the age distribution. The few extant studies on long-term labor market effects of air pollution exclusively focus on variation from early life exposure, typically in designs comparing individuals born right before or right after a specific cutoff date (Isen et al. 2017; Voorheis et al. 2017; Colmer et al. 2022). Studying the causal effects of air pollution in East Germany allows us to extend this evidence by studying effects throughout the entire age distribution, including individuals who are already in adulthood at the time they are first exposed.

Figure 9: Treatment Effect Heterogeneity by Age



Coefficient plot of inverse movers design regression interacting treatment exposure with individuals' age in 1982.

We estimate treatment effect heterogeneity by age in a modified version of our inverse movers design. Specifically, we interact the binary treatment indicator, delineating individuals who spent their life under socialism in areas exposed to the lignite shock, with individuals' age in 1982. Figure 9 plots the coefficients from estimating the heterogeneous treatment effect of long-term air pollution exposure on individuals' subsequent ages. The resulting curve describes the evolution of the air pollution wage penalty throughout the age distribution. Strikingly, the curve follows an inverse U-shape with large negative effects on wages at both ends of the age-at-exposure distribution. This distribution shows that the largest air pollution effects occur for the youngest *and* oldest in our sample. We find significant effects for children from birth to 12 years of age and for adults of 27 years and older. This finding contrasts with previous literature who generally only finds effects in early age and not beyond adulthood. Tentatively, the shape of the treatment effects heterogeneity curve suggests that the long-term effect of air pollution exposure on individuals may depend on their underlying health capital, which typically will also evolve in an inverse U-shape across the age distribution. This interpretation is supported by an additional detail: Figure 9 shows a discontinuous jump in the wage penalty by age for individuals who are in their mid-40s in 1982. This cutoff delineates individuals who were born right before rather than during World War II. Assuming that individuals born during the war – relative to individuals born right before – have greater difficulties in building up health capital in early life, the jump in individual pollution penalties is consistent with the effect size being a function of underlying individual health at first exposure.

5.2.4 Robustness Long-Term Effects

The identifying assumption underlying the results in Table 5 is that there are no unobservable differences between treatment and control origin counties that correlate with individuals' future labor market success. While this assumption is not testable, we compare treatment and control origins for differences in observables in Table 6. To do so, we assign movers to their origin GDR counties and compare these counties in weighted regressions on a large number of balancing characteristics. We use data for the last full year of the GDR's existence in 1989. We then regress these measures on our binary treatment variable, weighted by the share of individual movers in our sample from the respective county.[30] We report the coefficients and standard errors of the treatment variable for each characteristic in Column (1). We repeat this exercise including state fixed effects in Column (2). We find that the treatment and origin counties in our analysis are relatively well balanced. In Column (1), we find some differences in counties' population age composition between treatment and control group. We also find differences in the labor force composition of treatment and control origins. Treatment origin counties have a significantly share of local workers in the energy, chemicals and engineering industries. This is not surprising: all these occupations are tightly connected to the lignite mining industry. When including state-level fixed effects in Column (2), almost all differences between treatment and control origins become insignificant. The only remaining structural difference relates to the energy sector employment share. This, again, is not surprising as lignite was the GDR's main energy source and we would therefore expect a large portion energy workers to be based close to lignite mines. To ensure that this does not affect our results, we rerun the inverse movers design in two alternative versions. First, we rerun our estimations while excluding all energy sector workers (power plant workers, coal miners, etc.). This does not affect our results and suggests that the differences we find in Table 5 are not driven by

[30] We take most measures of county characteristics from Lichter et al. (2021) who conduct a comparable balancing exercise.

energy workers. Second, we rerun our estimations only looking at energy sector workers. Again, we find that there is no large difference between our overall results and those for energy sector workers, suggesting that they are not disproportionally affected relative to other workers from the same origin counties. We explicitly assess occupational effect heterogeneity in Figure A8. We use information from the US Department of Labor's O*NET database to differentiate occupations depending on how much time workers spend outdoors according to standardized work context scores.[31] Consistent with outdoor workers being more exposed to air pollution, we find that point estimates of the detrimental effects of air pollution on employment, unemployment, and retirement are larger for these workers – albeit insignificantly so.[32]

We use data on public investments by sector from the GDR's statistical yearbook to illustrate the socialist government's policy response to the sudden reduction in Soviet oil exports. We find that the government largely responded by spending more money on installations and equipment for the coal mining, electricity generation, and heating sectors. In contrast, there was no substantial change in the country's labor force composition that could influence our analysis of long-term labor market outcomes after the end of socialism. In Figure A9, we compare employment in coal mining over time with the development of total employment in other types of mining. In comparison, we see that there is moderate increase in coal mining employment of about 5.6% between 1982 and later years, but this increase is stretched out over multiple years. We do not see any changes in employment in the electricity and heating sectors around 1982, as shown in Figure A10. En lieu of an employment response, we see that the socialist government substantially increases their investment in installations and equipment for the coal mining, electricity and heating sectors. Figure A11 plots investment in coal relative to other types of mining over time. While both timelines follow an identical trend before 1982, coal mining sees a significant inflow of investments after the shock. Similarly, Figure A12 shows that the government also substantially increased investments in electricity and heating, in particular after 1984 and relative to its investment in the water or gas supply system.

An additional concern for causal identification in Table 5 is the possibility of differential selection into migration between treatment and control origins. If, for instance, historic air pollution exposure is a major motivation for individuals to migrate, the individuals emigrating from treatment origins may not be comparable to the ones emigrating from control origins. If out-migrants from treatment origins have lower latent labor market potential than individuals migrating from control counties irrespective of historic air pollution exposure, then we may be misattributing differences in their innate prerequisites for labor market success to air pollution. Thinking about this problem through the lens of a spatial sorting model, we should in general be concerned with the differential impact of both *push* and *pull* factors of migration. The inverse movers design of Table 5 accounts for

[31] We crosswalk the 2019 O*NET work context database to five-digit occupational codes in German social security data. https://www.onetonline.org/find/descriptor/browse/4.C, last accessed July 27, 2023.

[32] We find wage effects to be lower for workers with more outdoor exposure. However, this difference partly reflects differences in the types of jobs in which outdoor exposure is high (e.g. forestry workers or mail carriers) rather than low (e.g. auditors or dentists).

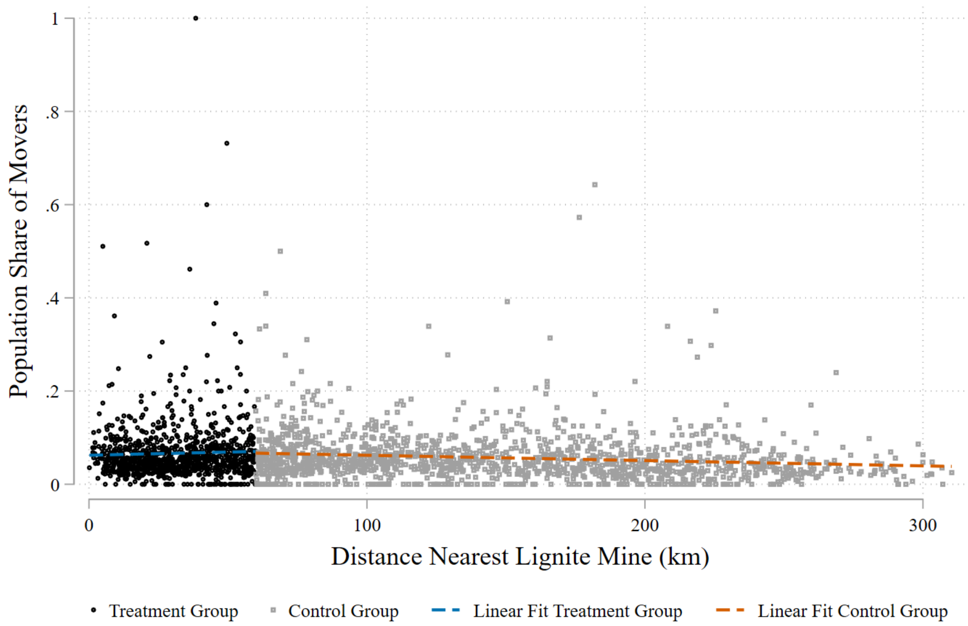
Table 6: Balance Tests: Characteristics of Movers' Origin Counties

	GDR 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		✓

Note: Table 6 reports coefficients from separately regressing each county characteristic on an indicator variable of whether the county is located within 60 kilometers of a lignite coal mine. Observations are weighted by the share of individuals in the movers-to-movers comparison sample that are located in the county in 1992. Post-reunification location data were allocated to GDR counties using GIS software. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

pull factors through the presence of migration-destination fixed effects. As we exclusively compare individuals who move to the exact same place at the same time, there is no scope for differential pull to affect our results. Our first defense against differential push factors follows a similar path: by definition we have restricted our sample to comparing individuals who have selected both into migrating and into migrating to an identical destination. This leaves relatively little margin for additional push selection. We test the potential bias from differential push by taking the population sample of East German workers emerging from socialism and running a prediction exercise to determine which characteristics differentiate movers from non-movers. Our main characteristic of interest in this regard is the binary treatment variable of being from an origin county located close to a lignite mine. For the population on East German workers in 1992, we predict which of them will be migrating to a new home by 1993 based on historic air pollution. While we find the treatment indicator is a statistically significant predictor of migrating by 1993, the resulting effect is economically minuscule. The likelihood of moving rises by about 0.5%. We take this result as evidence that historic air pollution was a factor, but not a first order concern determining individuals' relocation decisions.

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.

We complement this analysis by plotting mover rates for each East German municipality in Figure 10. For each municipality in East Germany, Figure 10 plots the share of workers emerging from socialism in 1991/92 who have moved to another county in Germany by 1993 versus the respective municipality's distance to the most proximate lignite mine. While there is a slight decline in workers probability to migrate the further municipalities are away from lignite mines, this result is predominantly driven by marginally lower migration rates in coastal regions of far north-east Germany. We report the distribution of move years for individuals moving exactly once over the post-reunification timeline in Appendix-Figure A7. Migration notably peaks in the first post-reunification year, suggesting that the universal push effect of reunification and the reinstatement of freedom of movement may have outweighed regional differentials.

To explicitly rule out that our findings are driven by selection into migration, we build an alternative version of our inverse movers design that accounts for both push and pull factors of migration using destination-by-origin fixed effects. To implement this alternative treatment, we leverage effect heterogeneity by individuals' age at first exposure documented in Section 5.2.3. Based on the inverse U-shaped relationship between age and pollution wage penalties, we define cutoffs for ages in which individuals are particularly susceptible to the adverse effects of air pollution exposure. We propose two alternative definitions for this vulnerable age treatment. First, we assign individuals to the vulnerable treatment group if their birth year is below the 5th or above the 95th percentile of the birth year distribution. This way, individuals are treated if they are at most 11 or at least 44 years old in 1982. Second, we account for the longer tail of the age distribution and lower the upper treatment bound to the 75th percentile of the age distribution (age 39 and up in 1982). Because this age-dependent treatment is no longer congruent with a subset of migration origin regions, we can use the vulnerable age treatment to re-estimate the inverse movers design including a full set of destination-by-origin fixed effects. The inclusion of these fixed effects fully accounts for push and pull factors driving migration decisions. Intuitively, we are now comparing individuals who move from the same origin to the same destination region. The treatment variable than relates to whether the observed movers were in a particularly vulnerable age when first exposed or not. We report the results from estimating the inverse movers design for pollution wage effects with destination-by-origin fixed effects in Table 7. The results show that the negative effect of pollution exposure is large and significant even when accounting for all migration push and pull factors. Because treatment is based on individuals' year of birth this analysis is only feasible for wage effects and not for retirement and time spent in the labor force.

Finally, we re-estimate our inverse movers design as a falsification exercise. To validate that the movers design captures relevant variation and is not 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 as being placebo-treated if their geographical centroid is within 60 kilometers of the nearest potash mine, as depicted in Figure A2. We then construct an inverse movers design equivalent to Table 5 by restricting our sample to pairs 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. The results in Table B.19 show that all coefficients are statistically insignificant and substantially smaller than the coefficients we estimate in our main analysis.

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

	Wages	Wages
	(1)	(2)
Vulnerable Age$_i$	-6.135***	-7.289***
	(0.312)	(0.265)
Treatment	$<$ p(5) OR $>$ p(95)	$<$ p(5) OR $>$ p(75)
Unit	EUR	EUR
Mean Y	65.952	65.952
Observations	76,929	76,929
R-squared	0.448	0.451

Note: Table 5 reports results from estimating an extended version of Equation 3 using ordinary least squares. $VulnerableAge_i$ is a binary variable indicating whether an individual is 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.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

6 Mechanism: Air Pollution and Long-Term Health

To test mechanisms behind long-run economic effects of sustained exposure to air pollution, 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 counties exposed to the air pollution shock under socialism have significantly higher healthcare utilization in 1992, ten years after the initial shock (Table 8). Compared with individuals who lived in non-exposed counties in East Germany, 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.

We then estimate the long-term health effects of sustained air pollution exposure by analysing health diagnoses surveyed in 2011, 29 years after the initial shock. OLS estimates indicate that, conditional on current place of residence, individuals who lived in treated counties under socialism are significantly more likely to be suffering from pollution related conditions than individuals who lived in control counties (Table 9). Specifically, we find that treated individuals are 12.6 percentage points more likely to have been diagnosed with asthma and 12.8 percentage points more likely to have been diagnosed with cardiopathy by 2011. Relative to the sample average, the increase in the asthma incidence is 229% higher for individuals from treated counties relative to individuals from control counties. For cardiopathy, the 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. We do not find that individuals from treated counties are more likely to have been diagnosed with diabetes or chronic back pain. We show that these findings are robust to pooling the 2011 cross section with the 2013,

Table 8: G-SOEP: OLS Health Outcomes 1992

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

Note: Cross-sectional 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.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

2015, and 2017 cross sections in Table B.20. 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.21).

Table 9: G-SOEP: OLS Health Outcomes 2011

	Asthma (1/0)	Cardiopathy (1/0)	Diabetes (1/0)	Chronic Back Pain (1/0)
	(1)	(2)	(3)	(4)
D_i	0.126*	0.128*	-0.032	0.000
	(0.064)	(0.074)	(0.057)	(0.071)
Model	OLS	OLS	OLS	OLS
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.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

7 A Back-of-the-Envelope Calculation for the Total Social Cost of Long-Term Air Pollution

What was the societal cost of air pollution in East Germany? To answer this question, we conduct a conservative back-of-the-envelope calculation to approximate the total social security cost on the German post-reunification economy. Based on Table 5, workers spent on average 0.372 years less in employment between 1993 and 2020. At 220 working days a year on average, this translates to 81.84 days on which individuals receive transfers out of, rather than paying contributions into, the social security system. Assuming a tax rate of 25% on wages and counterfactual average daily wages of EUR 70.05 this translates into EUR 1,433.10 of lost tax revenue per worker. Similarly, assuming that workers instead receive benefits at 40% of their actual average daily wages of EUR 68.00 on those 81.84 days translates into additional transfers of EUR 2,225.70 per worker. Seeing that close to half of the 6.1 million workers we observe in our sample are exposed to the air pollution shock, social security costs of lost employment alone are estimated at around EUR 10.976 billion measured in 2015 prices – close to 1% of West Germany's gross domestic product in 1989,[33] the last year before reunification. Notably, this estimate constitutes a conservative lower bound and does not account for the welfare losses from 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.

8 Conclusions

When the Soviet Union capped East German access to imported oil overnight, East Germany responded by rapidly ramping up the domestic production and utilisation of lignite. Lignite is a low-quality coal generating large amounts of ambient air pollution upon combustion. Lignite is also difficult to trade or transport. We use this natural experiment to show that the switch to lignite cause a large increase in air pollution in areas of East Germany in which lignite could be extracted. The authoritarian nature of the East German regime meant that individuals could not respond to the air pollution shock by moving away, changing jobs, or sorting through the housing market. We show that this air pollution shock had large and persistent negative effects on the local population. Affected areas experienced large increases in infant mortality and reductions in infant birth weights. In the long-term, individuals living in the affected areas displayed significantly worse labor market outcomes. Affected individuals spend less time in employment, earn lower wages and retire earlier in the four decades following the initial pollution shock.

Our paper provides novel evidence on the causal effects of air pollution on individuals. We show that long-term exposure carries substantial costs for the affected individuals, even decades after the end of exposure. We also show that air pollution effects are particularly large in our setting, suggesting that pollution effects may be underestimated in alternative settings where individuals can respond to exposure, for instance through residential sorting.

Our paper also carries an important lesson for contemporary politics. Similar to the early 1980s, the Russian invasion of Ukraine on 24 February 2022 resulted in countries dependent on Russian fossil fuel exports having to rapidly find ways of adjusting their

[33] According to estimates of the German Federal Statistical Offices in current prices in 2023. https://www.destatis.de/DE/Themen/Wirtschaft/Volkswirtschaftliche-Gesamtrechnungen-Inlandsprodukt/Publikationen/Downloads-Inlandsprodukt/statistischer-bericht-2180120233225.html?nn=214136, last accessed on July 29, 2023.

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 equally illustrates that replacing one fossil fuel with another may result in additional externalities imposed on the local population.

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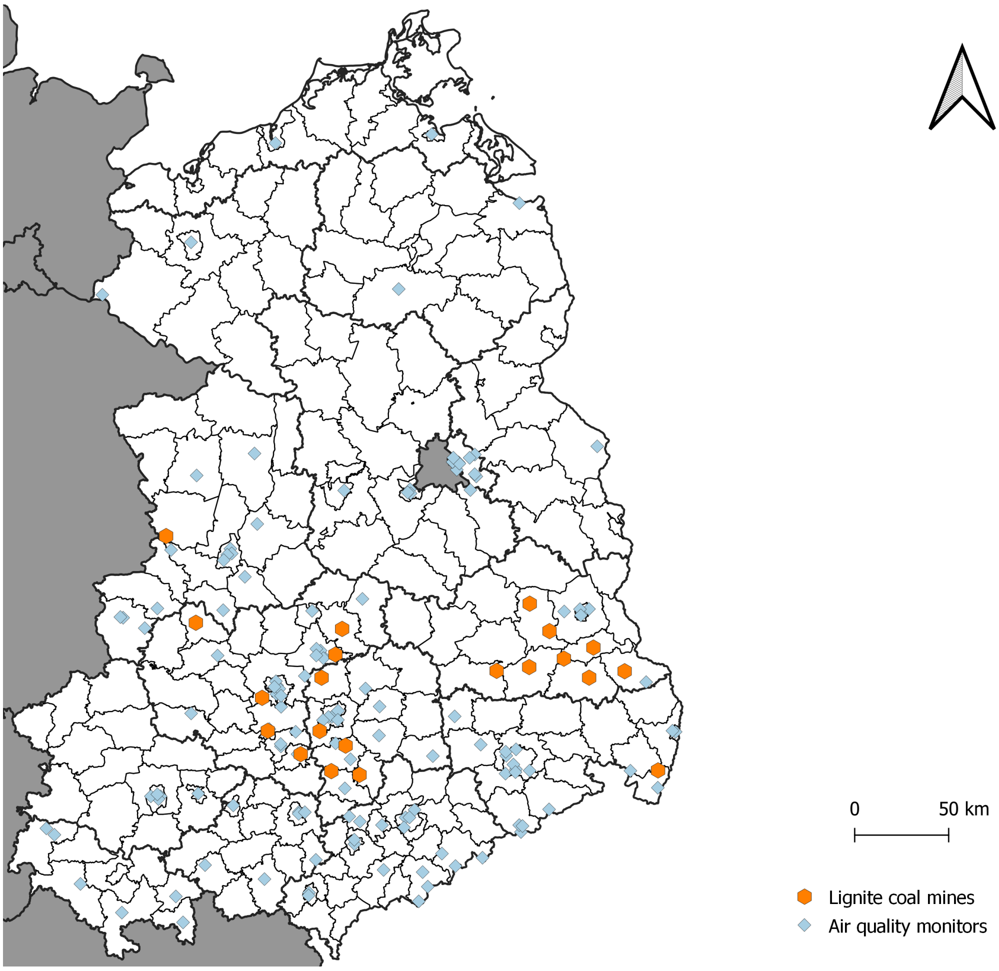
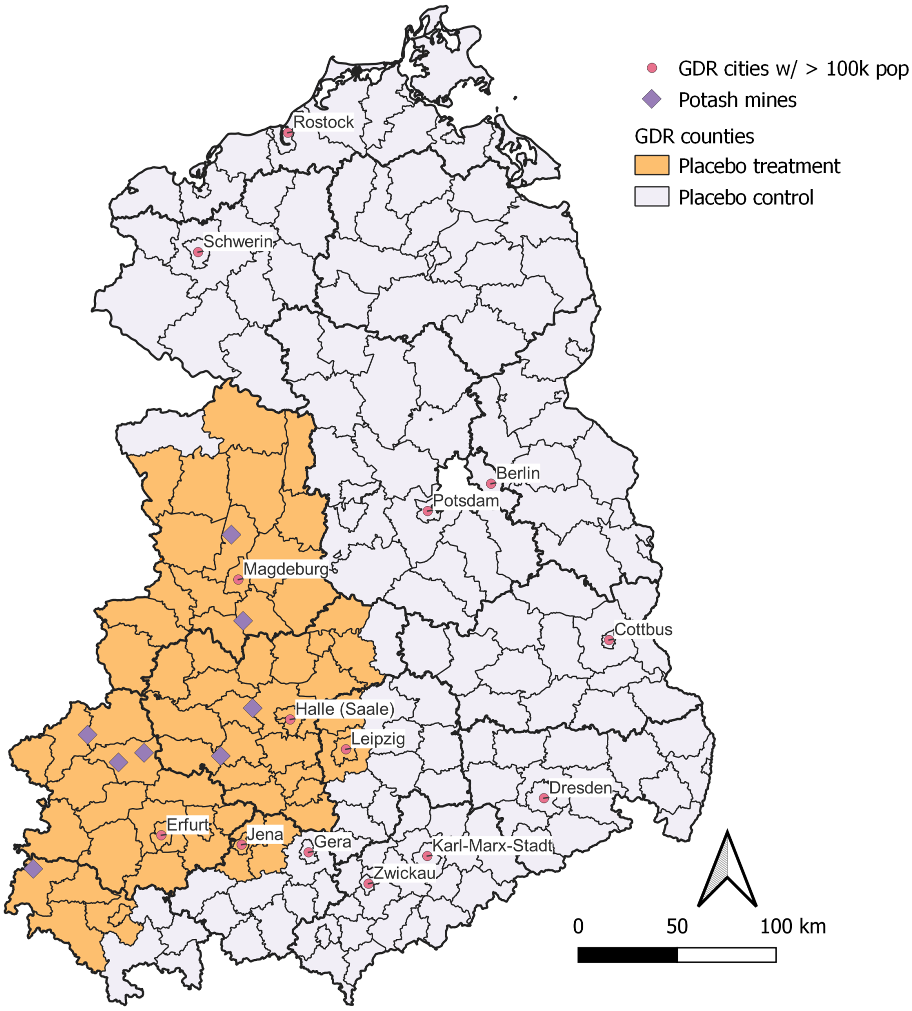
A Additional Figures

Figure A1: Location of Air Quality Monitors and Lignite Mines



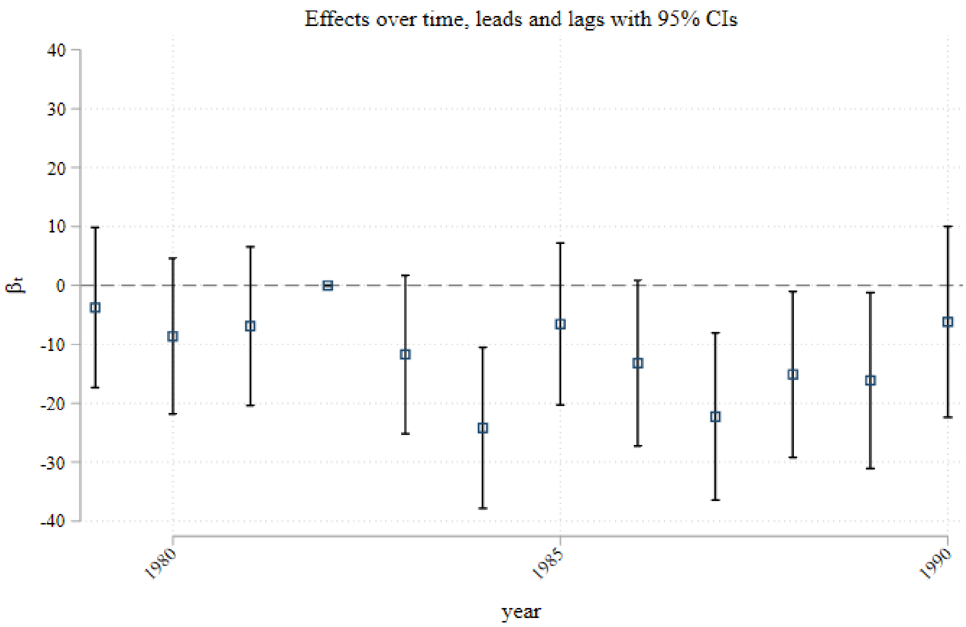
The map depicts the geocoded locations of lignite mines and air quality monitors in East Germany. Both mines and monitors are generally concentrated in the south of the country. We use inverse distance-weighted interpolation to assign mean air pollution readings (for sulfur dioxide) to the geographical centroids of all counties that have at least one air quality monitor within 100 kilometers' distance.

Figure A2: Placebo Test: Location of Potash Mines in East Germany



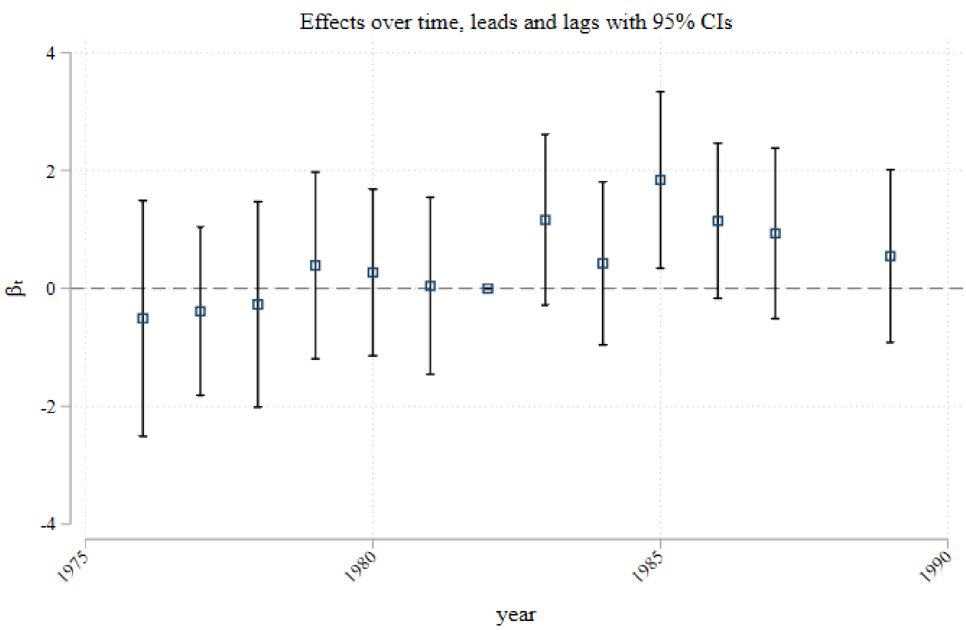
We assign counties to a placebo treatment of control status depending on whether their geographical centroid is within 60 kilometers of a potash mine. The map depicts pseudo-treatment counties as orange-shaded areas and marks the 8 potash mines considered in our falsification exercises.

Figure A3: Average Infant Birth Weights



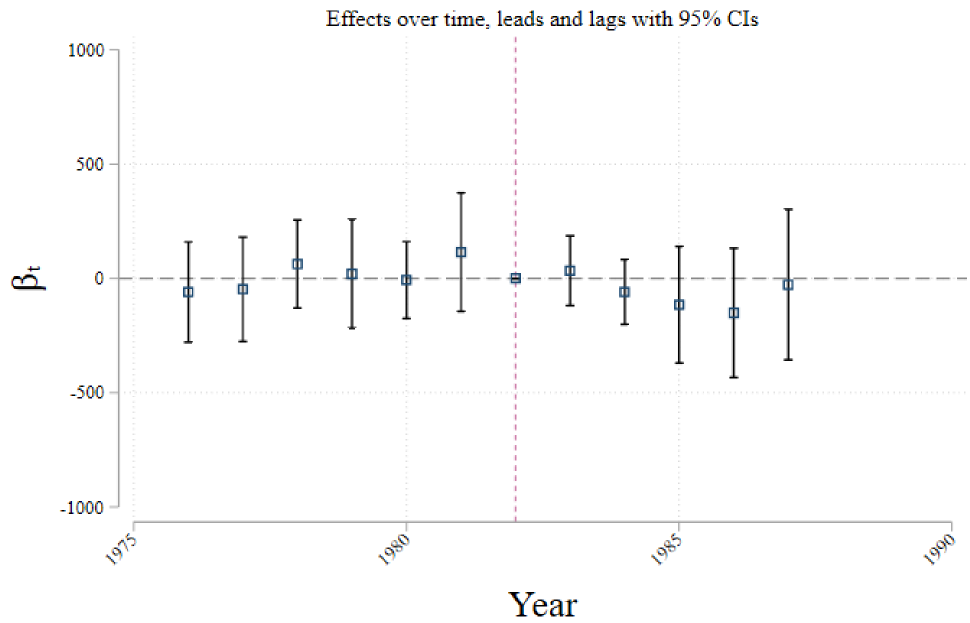
Coefficient plot for an event study difference-in-differences regression of mean birth weights in GDR counties on a binary treatment. Treatment is defined as whether or not a county's geographical centroid is within 60 kilometers of the nearest lignite mine and whether the year is greater than 1982.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

Figure A4: Average Infant Mortality Rates



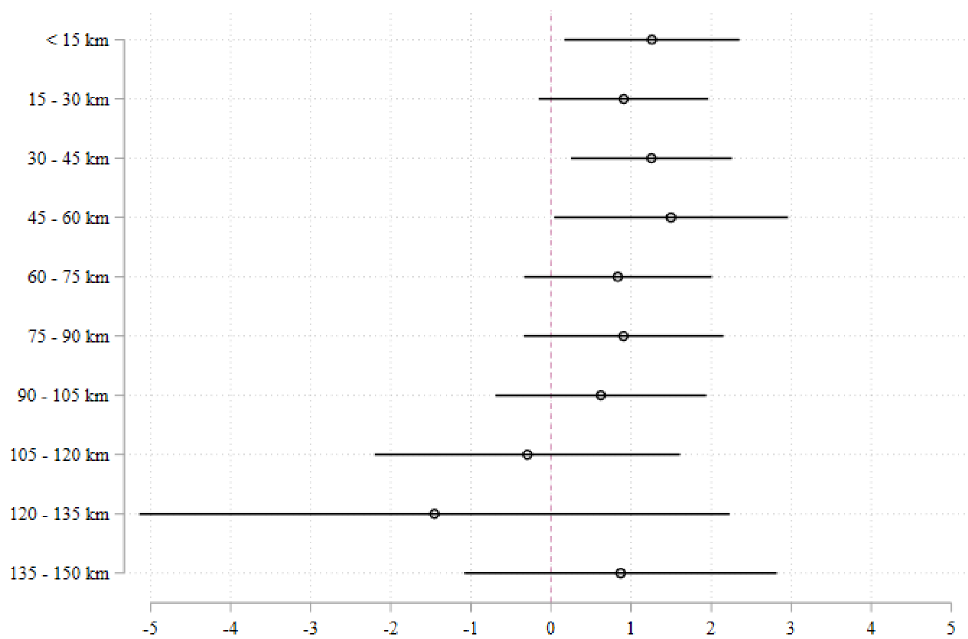
Coefficient plot for an event study difference-in-differences regression of mean infant mortality rates in GDR counties on a binary treatment. Treatment is defined as whether or not a county's geographical centroid is within 60 kilometers of the nearest lignite mine and whether the year is greater than 1982. The coefficient for 1982 is omitted as a reference category. The coefficients β_t are measured in infants deceased below 1 year of age per 1,000 live births.

Figure A5: Average Net Migration



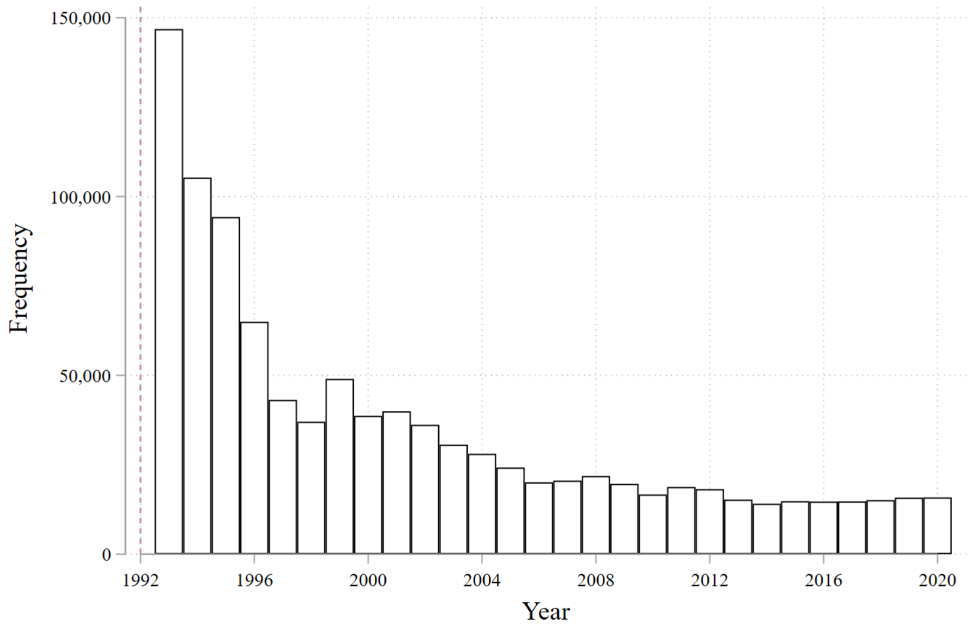
Coefficient plot for an event study difference-in-differences regression of mean net migration in GDR counties on a binary treatment. Treatment is defined as whether or not a county's geographical centroid is within 60 kilometers of the nearest lignite mine and whether the year is greater than 1982.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

Figure A6: Non-Parametric 15 km Distance Bins: Infant Mortality per 1,000 Live Births



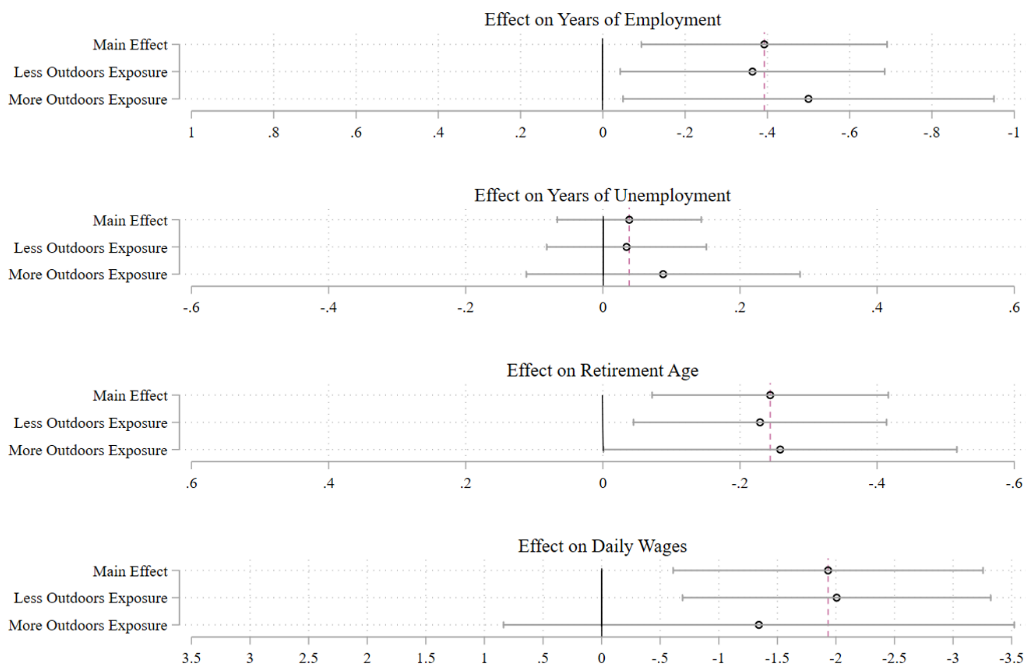
Coefficient plot for a difference-in-differences regression of mean infant mortality in GDR counties on a treatment timing indicator interacted 15 kilometer distance bins.

Figure A7: Year of Move for Individuals Moving Exactly Once



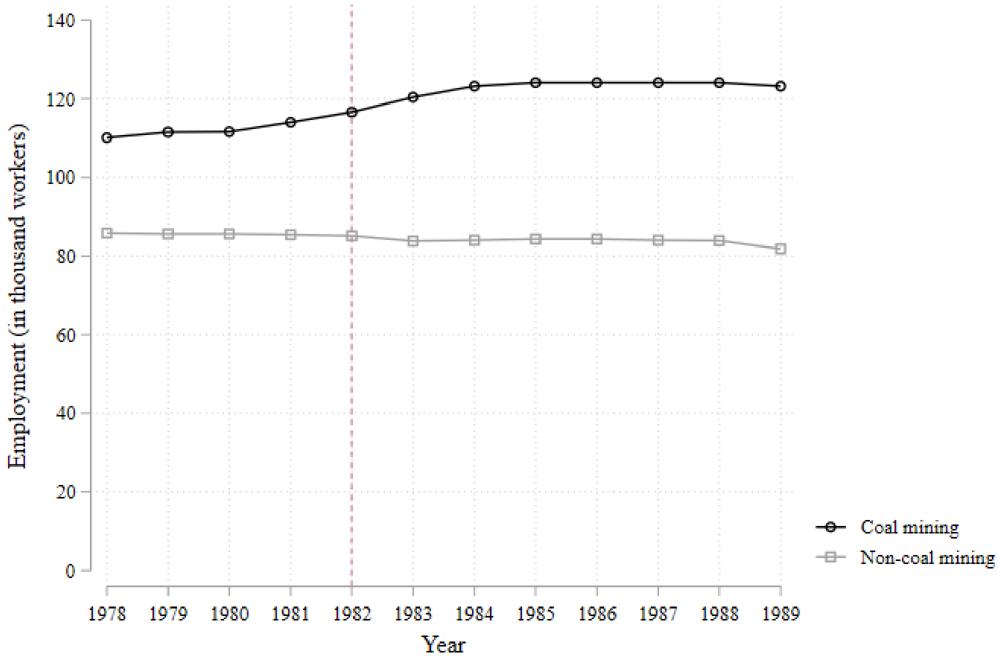
Histogram of movers migration timing. Sample restricted to individuals moving exactly once during their post-reunification career.

Figure A8: Effect Heterogeneity by Occupational Outdoor Exposure



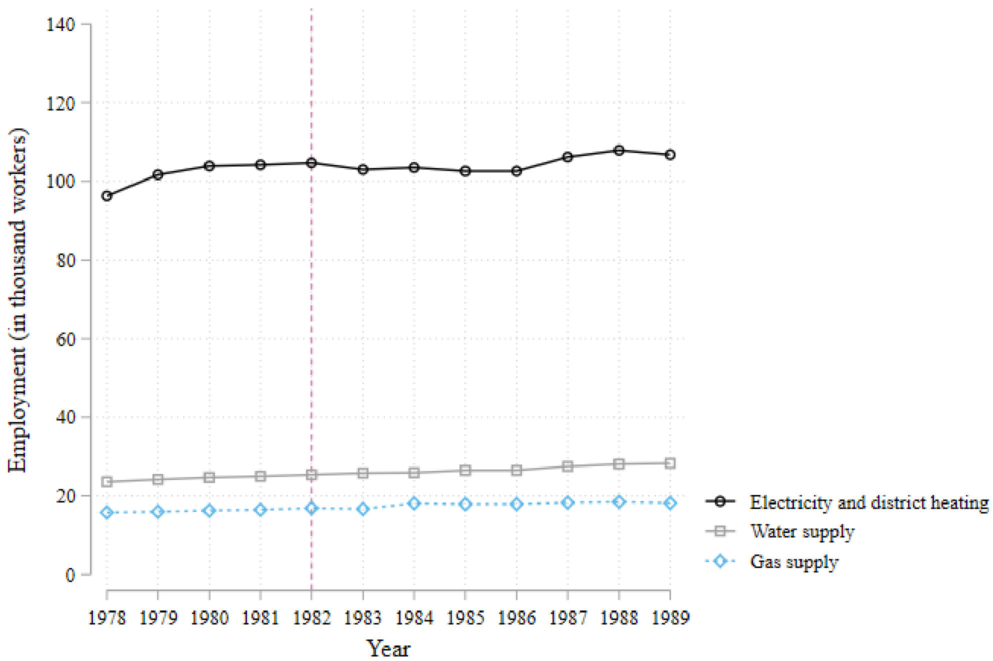
Movers design effects differentiated by occupational outdoor exposure. Outdoor exposure is measured on a five item Likert scale according to the 2019 O*NET work conditions database. We generate a multi-classification crosswalk between O*NET data and five-digit occupational codes in German social security data (so-called KldB 2010).

Figure A9: Total Employment in Mining Sectors, 1978-1989



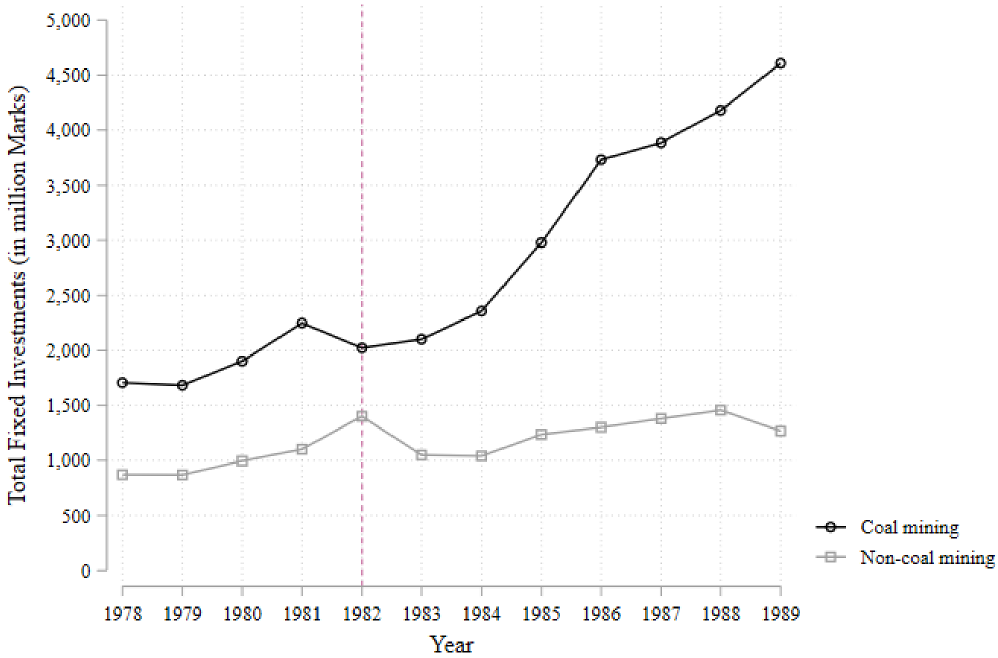
Sector level data on annual employment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A10: Total Employment in Utility Sectors, 1978-1989



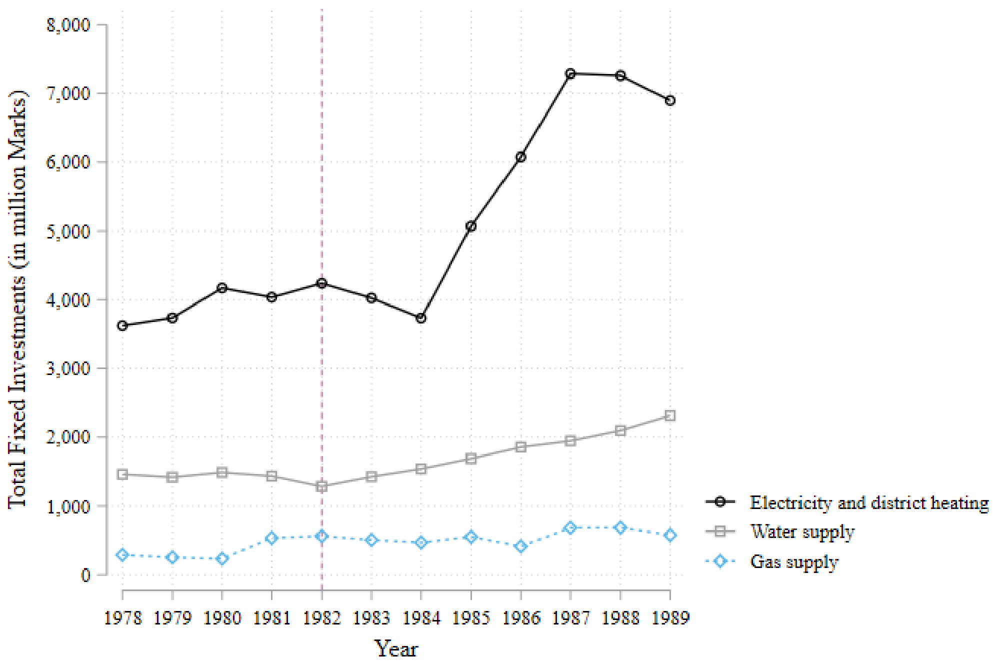
Sector level data on annual employment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A11: Total Fixed Investments in Mining Sectors, 1978-1989



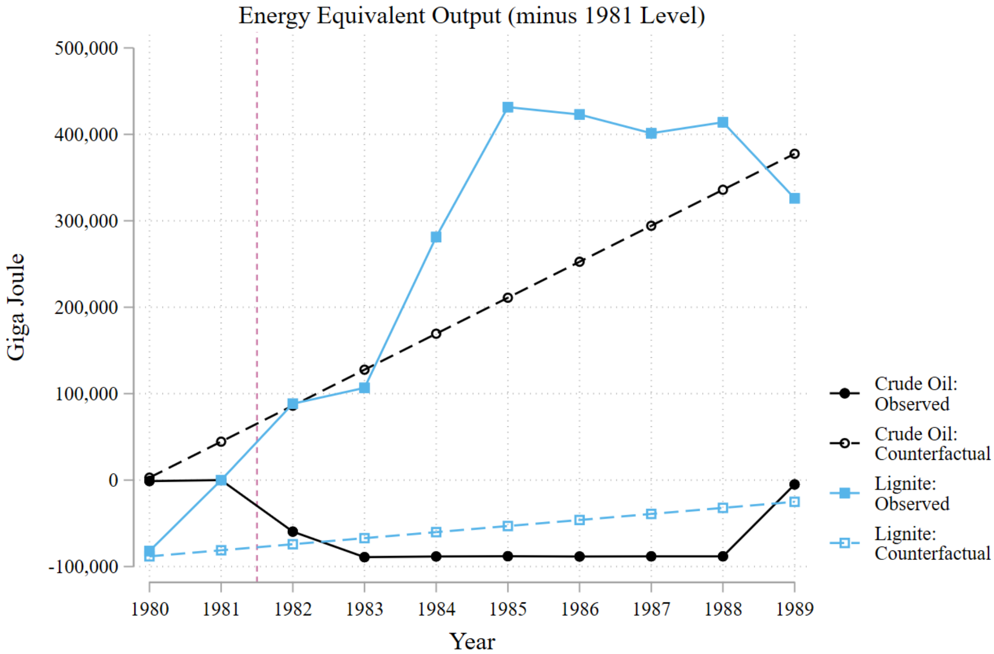
Sector level data on annual public investment figures from the GDR's statistical yearbooks. The dashed vertical line delineates the last year before the sudden reduction of Soviet oil exports triggered the lignite shock.

Figure A12: 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 A13: 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.

B Additional Tables

Table B.1: Difference-in-Differences: SO_2 Air Pollution Results for Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D_i$	27.553***	19.974***	23.706***	27.878***	29.183***
	(3.178)	(2.942)	(3.014)	(3.306)	(3.472)
Mean Y	156.002	156.002	156.002	156.002	156.002
Observations	2,069	2,069	2,069	2,069	2,069
R-squared	0.850	0.845	0.848	0.849	0.849

Note: Table B.1 reports results from estimating Equation 1 using ordinary least squares. 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.2: Difference-in-Differences: Birth Weight Results for Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D_i$	-8.328***	-7.512**	-8.713***	-5.346*	-2.112
	(2.931)	(3.293)	(3.166)	(2.987)	(2.943)
Mean Y	3282.414	3282.414	3282.414	3282.414	3282.414
Observations	2,210,149	2,210,149	2,210,149	2,210,149	2,210,149
R-squared	0.030	0.030	0.030	0.030	0.030

Note: Table B.2 reports results from estimating Equation 1 using ordinary least squares. 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 Results for Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D_i$	0.894***	0.348	0.750**	0.964***	0.972***
	(0.322)	(0.322)	(0.321)	(0.331)	(0.343)
Mean Y	12.958	12.958	12.958	12.958	12.958
Observations	4,104	4,104	4,104	4,104	4,104
R-squared	0.386	0.385	0.386	0.386	0.386

Note: Table B.3 reports results from estimating Equation 1 using ordinary least squares. 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 Results for Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D$	0.109	0.262	0.155	-0.056	-0.164
	(0.226)	(0.223)	(0.219)	(0.243)	(0.254)
Mean Y	7.147	7.147	7.147	7.147	7.147
Observations	4,100	4,100	4,100	4,100	4,100
R-squared	0.277	0.277	0.277	0.277	0.277

Note: Table B.4 reports results from estimating Equation 1 using ordinary least squares. 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 Results for Alternative Treatment Thresholds

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D_i$	-118.659	-39.256	-76.019	-139.080	-144.826
	(92.686)	(87.875)	(87.705)	(101.687)	(109.106)
Mean Y	-83.252	-83.252	-83.252	-83.252	-83.252
Observations	3,668	3,668	3,668	3,668	3,668
R-squared	0.373	0.373	0.373	0.374	0.374

Note: Table B.5 reports results from estimating Equation 1 using ordinary least squares. 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: Difference-in-Differences: Short-Term Health Outcomes with Continuous Measures of Treatment

	Infant Mortality		Infant Birth Weights	
	(1)	(2)	(3)	(4)
# km to nearest mine	-0.007***		0.019	
	(0.003)		(0.019)	
# mines within 60km		0.087*		-1.199**
		(0.049)		(0.497)
Unit	per 1,000 births	per 1,000 births	gram	gram
Mean Y	12.958	12.958	3,282.41	3,282.41
Observations	4,104	4,104	2,210,149	2,210,149
R-squared	0.386	0.385	0.013	0.013

Note: Table B.6 reports results from estimating variations of Equation 1 using ordinary least squares. Instead of a binary variable indicating whether a county is located close to a lignite mine, we employ two continuous measures capturing the intensity treatment. First, we use the straight-line distance between each county (infant mortality) and municipality (infant birth weights) centroid to the nearest lignite mine. Second, we use the number of lignite mines that are within 60 kilometers of a county or municipality centroid. Note that the two treatment measures are coded on reverse scales: Treatment intensity is decreasing in the number of kilometers to the nearest mine and treatment intensity is increasing in the number of mines within 60 kilometers. 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 Placebo-in-Space Potash Mines

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D_i$	4.254	3.069	2.055	3.087	5.088
	(3.796)	(4.130)	(3.905)	(3.588)	(3.516)
Mean Y	156.002	156.002	156.002	156.002	156.002
Observations	2,069	2,069	2,069	2,069	2,069
R-squared	0.840	0.840	0.840	0.840	0.840

Note: Table B.7 reports results from estimating Equation 1 using ordinary least squares. 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.8: Difference-in-Differences: Birth Weights Placebo-in-Space Potash Mines

	60km	40km	50km	70km	80km
	(1)	(2)	(3)	(4)	(5)
1[t>1982] $\times D_i$	-0.592	5.798	0.632	-0.255	-1.163
	(3.821)	(4.367)	(4.099)	(3.504)	(3.467)
Mean Y	3282.414	3282.414	3282.414	3282.414	3282.414
Observations	2,210,149	2,210,149	2,210,149	2,210,149	2,210,149
R-squared	0.013	0.013	0.013	0.013	0.013

Note: Table B.8 reports results from estimating Equation 1 using ordinary least squares. 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.9: 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	0.036	-0.369	-0.527	2,808
	(0.492)	(0.493)	(0.479)	(0.512)	(0.528)	
Lignite, 1976	-0.394	-0.001	0.013	-0.501	-0.445	2,808
	(0.404)	(0.394)	(0.395)	(0.416)	(0.433)	
Lignite, 1979	0.086	0.299	0.334	0.010	0.186	2,808
	(0.399)	(0.389)	(0.387)	(0.415)	(0.432)	
Panel B: Placebo-in-Space						
Potash	-0.028	0.220	-0.361	-0.032	.0484	4,104
	(0.571)	(0.677)	(0.585)	(0.548)	(0.542)	

Note: Table B.9 reports estimated β-coefficients from estimating Equation 1 using ordinary least squares. 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.10: 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)					

Note: Table B.10 reports estimated β-coefficients from estimating Equation 1 using ordinary least squares at each decile of the birth weight distribution. Table B.10 further reports 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.11: Triple Difference Estimation: Regression Results on Short-Term Health Outcomes

	Infant Mortality	Infant Birth Weights
	(1)	(2)
DiDiD	0.755**	-8.187**
	(0.047)	(0.153)
Unit	per 1,000 births	gram
Mean Y	12.958	3,304.1
Observations	4,104	87,886

Note: Table B.11 reports results from estimating triple difference specifications using ordinary least squares. The first difference delineates the pre-post split in time relative to the lignite shock. The second difference is a binary variable indicating whether a county is located within 60 kilometer's straight-line distance of the nearest lignite mine. The third difference is whether an area experienced above-median (relative to its own time series) days of frost in a given year. County-level clustered standard errors in parentheses. In this analysis, we aggregate infant birth weight data from the level of individual births to municipality-year averages for ease of computation. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table B.12: OLS: Effect of Distance to Lignite under Socialism on Years of Employment

	(1)	(2)	(3)	(4)
D_i	-0.082	-0.215**	-0.213***	-0.241***
	(0.105)	(0.085)	(0.076)	(0.067)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y				
Observations	6,222,849	6,222,849	6,108,786	6,108,781
R-squared	0.000	0.408	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.13: OLS: Effect of Distance to Lignite under Socialism on Years of Unemployment

	(1)	(2)	(3)	(4)
D_i	0.229*	0.198**	0.211***	0.178***
	(0.139)	(0.093)	(0.080)	(0.053)
Year of Birth FE		✓	✓	✓
State FE		✓	✓	✓
Level of Education FE			✓	✓
Sex FE			✓	✓
Occupation FE				✓
WZ08 3-digit FE				✓
Mean Y	3.831	3.831	3.865	3.865
Observations	6,222,849	6,222,849	6,108,786	6,108,781
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.14: OLS: Effect of Distance to Lignite under Socialism on Retirement Age

	(1)	(2)	(3)	(4)
D_i	0.214***	-0.026	-0.020	-0.046**
	(0.033)	(0.026)	(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	6,222,849	6,222,849	6,108,786	6,108,781
R-squared	0.000	0.042	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.15: Movers Design: Effect of Distance to Lignite in Origin County on Years of Employment

	(1)	(2)	(3)	(4)	(5)
D_i	-0.367**	-0.419**	-0.366**	-0.372***	-0.341*
	(0.173)	(0.175)	(0.162)	(0.129)	(0.195)
Year of Birth FE		✓	✓	✓	✓
Destination FE	✓	✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
State FE					✓
Mean Y	14.087	14.087	14.185	14.185	14.185
Observations	146,787	146,786	144,348	144,338	144,338
R-squared	0.039	0.330	0.338	0.369	0.370

Note: Table B.15 reports 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 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.16: Movers Design: Effect of Distance to Lignite in Origin County on Years of Unemployment

	(1)	(2)	(3)	(4)	(5)
D_i	0.193	0.179	0.161	0.088	0.045
	(0.134)	(0.123)	(0.109)	(0.059)	(0.069)
Year of Birth FE		✓	✓	✓	✓
Destination FE	✓	✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
State FE					✓
Mean Y	3.462	3.462	3.497	3.497	3.497
Observations	146,787	146,786	144,348	144,338	144,338
R-squared	0.024	0.049	0.078	0.140	0.140

Note: Table B.16 reports 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 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.17: Movers Design: Effect of Distance to Lignite in Origin County on Age at Retirement

	(1)	(2)	(3)	(4)	(5)
D_i	-0.153*	-0.148*	-0.133	-0.163**	-0.211*
	(0.086)	(0.080)	(0.083)	(0.068)	(0.110)
Year of Birth FE		✓	✓	✓	✓
Destination FE	✓	✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
State FE					✓
Mean Y	61.458	61.458	61.490	61.490	61.490
Observations	146,787	146,786	144,348	144,338	144,338
R-squared	0.011	0.059	0.061	0.086	0.086

Note: Table B.17 reports results from OLS regressions 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.18: Movers Design: Effect of Distance to Lignite in Origin County on Wages

	(1)	(2)	(3)	(4)	(5)
D_i	-2.126	-2.064	-2.126*	-2.054**	-1.488*
	(1.886)	(1.840)	(1.212)	(0.807)	(0.852)
Year of Birth FE		✓	✓	✓	✓
Destination FE	✓	✓	✓	✓	✓
Level of Education FE			✓	✓	✓
Sex FE			✓	✓	✓
Occupation FE				✓	✓
WZ08 3-digit FE				✓	✓
State FE					✓
Mean Y	67.901	67.901	67.995	67.996	67.996
Observations	146,787	146,786	144,348	144,338	144,338
R-squared	0.069	0.097	0.248	0.373	0.373

Note: Table B.18 reports results from OLS regressions 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.19: Movers Design: Placebo Effect of Distance to Potash Mines in East Germany

	Employment	Unemployment	Retirement Age	Wages
	(1)	(2)	(3)	(4)
P_c	-0.147	0.075	-0.057	-0.502
	(0.139)	(0.067)	(0.064)	(0.661)
Unit	Years	Years	Years	EUR
Mean Y	14.187	3.497	61.490	68.002
Observations	144,352	144,352	144,352	144,352
R-squared	0.370	0.140	0.086	0.373

Note: Table B.19 reports results from estimating a falsification exercise based on Equation 3 using ordinary least squares. P_c is a placebo treatment indicating whether a county is located within 60 kilometer's straight-line distance of the nearest potash 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.20: 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)
Model	OLS	OLS	OLS	OLS
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.21: 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)
Model	OLS	OLS	OLS	OLS
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.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$