The Effects of Air Pollution on Educational Outcomes: A Natural Experiment from the GDR

Matthias Brachert*

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Abstract

What are the educational costs of sustained air pollution during late secondary schooling? I exploit the 1982 Soviet oil supply cut, which forced the German Democratic Republic to substitute toward domestic lignite and sharply increased ambient sulfur dioxide (SO_2) near pre-established mining areas. Institutional constraints on residential mobility and school assignment in the GDR limited sorting and private avoidance responses, allowing the regional pollution shock to be studied in a setting with unusual institutional controls. Combining validated historical monitoring data with administrative university admissions records (STUBEW) and the 1989 employment register (GAV), I estimate difference-in-differences and event-study models. Counties near pre-established lignite mining areas experienced an increase in annual mean SO_2 of 28.8 μg/m^3 after 1982 (roughly 19 percent), with no indication of differential pre-trends. Among university applicants, transcript grades deteriorated by approximately 0.03 grade points (0.05 standard deviations), concentrated in cognitively demanding academic subjects and consistent with cumulative learning impairment under sustained exposure. Post-shock cohorts in treated counties are 2.6–2.7 percentage points less likely to have entered tertiary pathways by 1989. The results indicate that pollution costs extend to human capital formation during adolescence, with educational and early occupational consequences falling disproportionately on communities near energy infrastructure.

Keywords: air pollution; human capital; education; natural experiment; East Germany; lignite

JEL: I24; Q53; J24; N34; R12

*Halle Institute for Economic Research, Kleine Märkerstr. 8, 06108 Halle (Saale), Germany. E-mail: [address].

1 Introduction

In 1982, the Soviet Union unexpectedly cut crude oil deliveries to East Germany by one third. Unable to source alternative fuels, the GDR rapidly switched to domestic lignite coal, sharply increasing air pollution in regions near mining areas. This paper uses this energy shock as a natural experiment to study how sustained pollution exposure during the final years of secondary schooling affects educational performance and early labor-market outcomes. The question matters beyond this historical episode: air pollution is geographically concentrated near energy and industrial infrastructure, and if chronic exposure impairs learning during adolescence, environmental policy is also human-capital policy—with distributional consequences that fall unevenly across regions.

Identifying the educational costs of pollution requires a setting where exposure varies exogenously and households cannot sort away from it. The GDR provides both. Housing was allocated by the state, residential mobility was administratively restricted, and school assignment was tied to place of residence. These institutional features imply that the regional pollution shock induced by the lignite pivot translated into realized exposure for local students with limited behavioral attenuation, and that changes in the composition of student cohorts are unlikely to confound the estimates.

The analysis combines three administrative data sources. First, validated air-quality monitoring series reconstructed by the German Environment Agency (UBA) covering 1969–1990 provide county-by-year SO_2 concentrations. Second, individual-level records from the GDR's Central Office for University Admissions (STUBEW) contain secondary-school transcript grades for applicants to higher education from certificate cohorts issued between 1980 and 1989, allowing me to study academic performance among students at the end of upper-secondary schooling. Treatment is defined by proximity to active open-cast lignite mining areas, which strongly predicted the location of lignite-fired power generation and large combustion capacity. Third, the 1989 GDR employment register (GAV), a cross-section of approximately 7.25 million workers, provides information on labor-market entry channels and occupations. The empirical strategy exploits spatial variation in lignite proximity and cohort variation around the 1982 policy onset, estimating difference-in-differences (DiD) models with county and year (or cohort) fixed effects, complemented by event-study specifications. Educational outcomes are analyzed at the student-by-subject level; the preferred specification includes school-by-subject fixed effects.

The analysis yields three main results. In the first stage, counties within 60 km of an active lignite mining site experienced an increase in annual mean SO_2 concentrations of approximately 28.8 $\mu g/m^3$ after 1982—roughly 19 percent relative to the pre-treatment mean—and event-study estimates show no indication of differential pre-trends. This pollution shock translates into a deterioration of roughly 0.03 grade points in transcript performance (on a scale where higher values denote worse grades), equivalent to approximately 0.05 standard deviations; for context, studies of transitory exam-day pollution exposure report effects of broadly similar order of magnitude (Ebenstein, Lavy, and Roth, 2016), though direct comparisons across settings and exposure types are difficult. While modest in absolute terms, this shift is meaningful in the GDR context: the centralized admissions system assigned study fields and institutions on the basis of small performance differences, so even marginal grade losses could redirect students away from their preferred programs. The effects are concentrated in STEM subjects—chemistry, physics, and mathematics—and are close to zero in music and visual arts, consistent with a learning-impairment channel rather than a general change in grading standards. Effects are somewhat larger for male students (0.038 grade points) than for female students (0.028 grade points), though both groups show deterioration. Consistent with downstream effects of these performance losses, exposed Abitur cohorts from treated counties are 2.6 percentage points less likely to enter the labor market via a tertiary-track channel and 2.7 percentage points less likely to have completed tertiary education by 1989, among cohorts old enough for this outcome to be observed.

Three questions motivate the analysis and define its contributions. First, are the educational costs of air pollution driven by acute exposure on specific assessment days, or does chronic multi-year exposure generate additional cumulative impairment? The GDR setting separates these by design: the pollution shock is sustained over multiple school years, and the dynamic event-study pattern shows effects growing with exposure duration rather than appearing as an immediate one-time shift. Second, how large are pollution effects on education when households cannot adapt through sorting or private mitigation? Standard estimates may be attenuated by avoidance behaviour; the institutional constraints of the GDR provide near-unattenuated estimates that are informative about incidence when adaptation is limited (Isen, Rossin-Slater, and Walker, 2017). Third, how does an environmental shock propagate from classroom performance into early occupational trajectories, and what does this imply for the regional distribution of human-capital losses? By linking transcript data to an employment register, the paper traces this channel directly, and documents that the resulting costs are spatially concentrated near energy-generation infrastructure—a finding with direct implications for place-based policy evaluation and the welfare assessment of industrial siting decisions. The paper is complementary to Lubczyk and Waldinger (2025), who document long-run health and labor-market effects of the same shock; this paper focuses on the educational mechanism and early occupational sorting, and provides the upstream evidence on performance that motivates their downstream findings.

The paper proceeds as follows. After describing the historical background, data, and empirical strategy (Sections 2–4), Section 5 presents the main results, Section 6 discusses mechanisms and robustness, and Section 7 concludes.

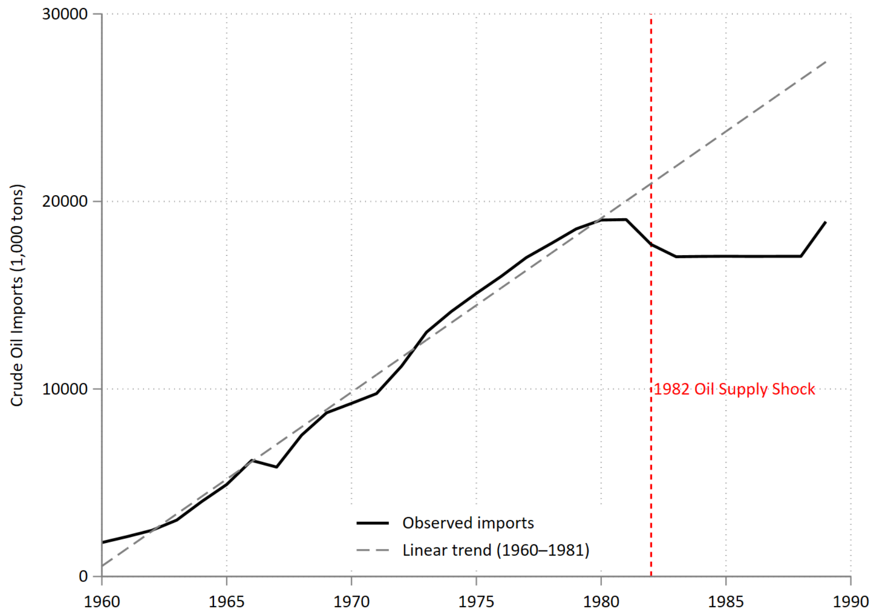
2 Historical Background and Institutional Context

2.1 The 1982 Soviet Oil Supply Shock and the Lignite Pivot

Throughout the 1960s and 1970s, the GDR relied heavily on energy imports from the Soviet Union and other Council for Mutual Economic Assistance (CMEA) countries. Long-term contracts and administratively set prices insulated the GDR from world-market fluctuations and allowed the regime to sustain an energy-intensive industrial structure despite limited domestic oil and gas resources. Within its borders, lignite was the only fossil fuel available at scale; energy security therefore depended critically on imported crude oil.

This dependence became a liability in 1982, when the Soviet Union unexpectedly reduced crude oil deliveries by roughly one-third and maintained a binding cap on exports for the remainder of the socialist period. Figure 1 shows the resulting break in GDR crude oil imports from the Soviet Union: after tracking a steady upward trend throughout the 1960s and 1970s, imports fall sharply in 1982 and remain persistently below the pre-1982 trend for the remainder of the decade. The cut was sudden and imposed from outside the GDR. Because hard-currency imports from Western markets were constrained and alternative suppliers were largely unavailable, the regime faced an immediate energy shortfall. The policy response was rapid and large-scale: mining capacity was expanded, industrial boilers and power plants were converted to lignite combustion, and lignite briquettes replaced oil in residential heating. This substitution occurred within a narrow time window and persisted through the late 1980s, making 1982–83 a natural break in the GDR's local pollution environment.

Figure 1: GDR crude oil imports from the Soviet Union, 1960–1989



Notes: The dashed line extrapolates the 1960–1981 linear trend. Imports fall sharply in 1982 and remain persistently below trend through 1989.

The exogenous and non-negotiable character of the cut is documented in the confidential exchange between the Soviet and East German leaderships that preceded it. In a letter dated 27 August 1981, Soviet General Secretary Leonid Brezhnev informed GDR leader Erich Honecker that the Politburo of the CPSU had decided to cap energy deliveries to CMEA member states for the 1982–1985 plan period, citing worsening extraction conditions and the need to redirect crude oil exports toward hard-currency markets.[1] Honecker's reply of 4 September 1981 acknowledged the Soviet Union's difficulties but warned in unusually frank terms that the proposed cuts would "undermine the very foundations of the existence of the German Democratic Republic", given the GDR's near-total dependence on Soviet oil and its domestic endowment of only lignite and uranium.[2] Despite this appeal, the cap was implemented as announced. This exchange illustrates that the timing and magnitude of the 1982 shock were determined by Soviet domestic planning constraints and bargaining within the CMEA, not by conditions internal to the GDR.

Two features of this episode are central for identification. First, as the archival correspondence above illustrates, the timing of the 1982 shock was externally imposed and unrelated to contemporaneous local educational trends. While lignite regions differed from other GDR counties along several historical dimensions—differences absorbed by the county fixed effects in the empirical specifications below—the *timing* of the shock itself was not driven by conditions internal to these regions, a claim assessed empirically through

[1]Letter from L. Brezhnev to E. Honecker, 27 August 1981, SAPMO-BA, DY 30/J IV 2/2A/2422.
[2]Letter from E. Honecker to L. Brezhnev, 4 September 1981, SAPMO-BA, DY 30/J IV 2/2A/2422.

pre-trend tests in Section 5.1. Second, the substitution margin was sharply constrained—the GDR's only feasible adjustment was domestic lignite, implying a predictable increase in emissions in regions connected to lignite extraction and combustion.

2.2 Lignite, SO_2, and the Geography of the Pollution Shock

Lignite is among the most emission-intensive fossil fuels. Its low calorific value means that larger quantities must be combusted per unit of useful energy, and its high sulfur content generates substantial sulfur dioxide (SO_2) emissions. In the GDR, lignite-fired power plants were not equipped with flue-gas desulfurization, so increases in lignite combustion translated into substantial increases in ambient SO_2 concentrations, especially near lignite-intensive combustion sites.

The geography of the resulting pollution shock is shaped by the economics of lignite transport. Because lignite is costly to move over long distances, large combustion facilities were located close to open-cast mining areas. In the GDR, lignite-based power generation was concentrated in and around the Lusatian coal basin in the south-east and the Central German basin near Leipzig and Halle. Following the 1982 pivot, the expansion of lignite extraction and combustion produced a spatially concentrated increase in SO_2 emissions near these regions.

I focus on SO_2 for two reasons. First, it is tightly linked to lignite combustion in this historical context and therefore serves as a direct proxy for the pollution shock induced by the energy substitution. Second, the reconstructed GDR monitoring archive provides substantially better spatial and temporal coverage for SO_2 than for other pollutants, enabling credible panel-based identification using county-by-year variation. Section 5.1 shows that this geographic logic is borne out empirically: proximity to lignite mining areas strongly predicts post-1982 increases in ambient SO_2, with no indication of differential pre-trends.

2.3 The GDR Education System

The GDR maintained a highly structured and stratified education system. After completing basic schooling in the *Polytechnische Oberschule* (POS), a selective subset of students entered the *Erweiterte Oberschule* (EOS), which provided upper-secondary education culminating in the *Abitur*, the university-qualifying diploma. Admission to the EOS track was centrally regulated and based on prior academic performance alongside political and social criteria.

University admission was administered centrally through the *Zentralstelle für Studienbewerbungen*, the institution behind the STUBEW records used in this paper. Study fields and institutions were allocated on the basis of applicants' transcript grades and other criteria, in a system with limited surplus capacity. This structure implies that small differences in transcript performance could determine access to preferred programs: in particular, admission to technically and scientifically demanding fields depended heavily on grades in mathematics and natural-science subjects.

Crucially for interpretation, the EOS curriculum and grading guidelines were centrally prescribed, limiting—but not eliminating—the scope for region-specific changes in grading standards. This institutional structure makes subject-specific transcript grades an economically meaningful outcome: grades in mathematics and natural-science subjects directly shaped access to technically demanding study fields in the centralized admissions system—a point returned to in the mechanisms discussion in Section 6.

2.4 Constraints on Residential Mobility and Private Avoidance

A central identification challenge in pollution research is that households may respond to deteriorating environmental conditions by relocating or by privately mitigating exposure. The institutional environment of the GDR substantially curtailed both responses.

Residential mobility was constrained by the state-controlled housing allocation system. Housing shortages were chronic, and administrative rules governed assignment to dwellings. Moving across localities required official approval and was practically difficult for most households. For families with school-age children, these constraints were compounded by school assignment rules: children were enrolled in schools based on their registered residence, and transfers between schools were rare. Parents therefore had limited ability to reduce their children's exposure through residential choice or school selection. Private pollution-mitigation technologies were largely unavailable, so increases in ambient SO_2 translated into increases in realized exposure with limited behavioral attenuation.

These institutional features support two empirical implications that underpin the identification strategy. First, the composition of student cohorts in a given county is unlikely to have shifted endogenously in response to the pollution shock, supporting the parallel-trends assumption underlying the difference-in-differences design. Second, proximity-based treatment captures changes in realized, rather than merely potential, exposure. Both implications are examined empirically: Section 5.2 documents no evidence of differential out-migration from treated counties following the 1982 shock.

3 Data and Exposure Measurement

3.1 Air Quality Data (UBA Reconstruction, 1969–1990)

To measure local environmental conditions, I use validated air-quality monitoring data from the former GDR covering the period 1969–1990. These data were compiled, harmonized across heterogeneous legacy files, and subjected to systematic plausibility checks as part of a reconstruction project commissioned by the German Environment Agency

(*Umweltbundesamt*, UBA).[3] Observations flagged as implausible in the UBA validation procedure are treated as missing, consistent with the reconstruction protocol.

I focus on annual mean SO_2 concentrations for two reasons noted in Section 2.2: the tight link to lignite combustion and the superior monitoring coverage relative to other pollutants. Annual means capture sustained exposure relevant for learning and health accumulation, while reducing sensitivity to short-run meteorological fluctuations.

Pollution is measured at the monitoring-station level and must be assigned to counties for the main analysis. When a county hosts one or more monitoring stations, I construct county-year SO_2 as the within-county average of station annual means. For counties without monitors, I impute county-year SO_2 using inverse-distance weighted (IDW) interpolation across all stations within a 100 km radius, with a linear weight-decay function that assigns zero weight at 100 km.[4] This procedure maximizes county-year coverage while retaining the spatial information embedded in the monitoring network; in practice, nearly all county-years within GDR territory are assigned a valid SO_2 measure. In robustness checks, I verify that results are not sensitive to changes in monitor availability over time by replicating the first-stage and reduced-form estimates on a balanced subsample of counties with consistently observed monitors and using alternative interpolation radii (Appendix A).

3.2 Treatment Assignment: Proximity to Lignite

The baseline treatment indicator D_c equals one if a county's geographic centroid lies within 60 km of a lignite open-cast mining area that was already established before the 1982 shock and remained operationally relevant during the subsequent lignite expansion, and zero otherwise. Mine locations and activity periods are obtained from the geodata portal of the Lausitzer und Mitteldeutsche Bergbau-Verwaltungsgesellschaft mbH (LMBV). Treatment assignment is based on sites recorded as active before 1982; the post-1982 activity information serves to confirm continued relevance rather than to identify new sites, guarding against the concern that some locations were opened or expanded as a direct consequence of the shock. Distances are computed as straight-line (great-circle) distances between county centroids and mine polygon centroids.[5] The post-shock period is defined as $\text{Post}_t \equiv \mathbb{1}[t \geq 1983]$, reflecting the onset of large-scale lignite substitution.

This proximity-based definition is historically grounded in the logistics of the GDR energy system: lignite combustion capacity was located close to extraction sites, so mining proximity captures the geography of the pollution shock without relying on monitor

[3] `https://www.umweltbundesamt.de/publikationen/aufarbeitung-analyse-von-luftqualitaetsdaten-der`

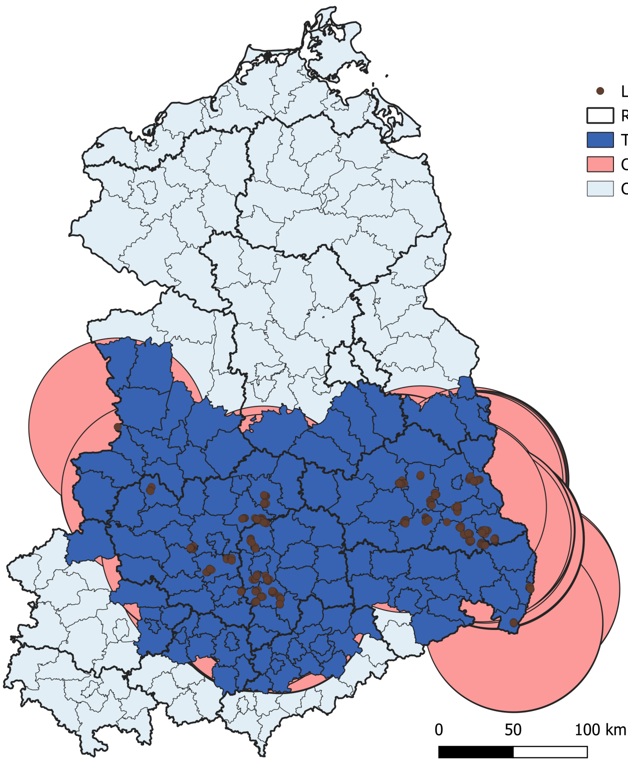
[4] Formally, $\widetilde{P}_{ct} = \left(\sum_s w_{cs}\, \overline{\text{SO}_2}_{st}\right) / \left(\sum_s w_{cs}\right)$, where $w_{cs} = \max\{0,\, 1 - d(c,s)/100\}$ and $d(c,s)$ is the distance in km from county centroid c to station s. Robustness to alternative kernels and radii is reported in the Appendix.

[5] Where a mining area consists of multiple pits, I use the centroid of the union polygon. Appendix A shows that results are robust to using polygon boundary distances and population-weighted county centroids.

coverage.

Figure 2 maps county treatment status together with the locations of pre-established lignite mining areas and the 60 km buffer used to define treatment.

Figure 2: County treatment status under the 60 km lignite-mining definition. Brown dots mark active lignite mines (1982–1988); pink shading shows the 60 km buffer around mine centroids; dark blue counties are classified as treated.



As a robustness check, I also define treatment using proximity to lignite-fired power plants with installed capacity exceeding 100 MW, geocoded from Bethkenhagen (1987) and supplementary archival sources. This alternative definition more directly proxies SO_2 emission sources. Results are stable across treatment radii of 40–80 km and under the plant-based definition (Section 6).

3.3 Educational Outcomes: STUBEW University Admissions Records

The main educational outcome data come from the administrative records of the GDR's Central Office for University Admissions (*Zentralstelle für Studienbewerbungen*; archive stock Bundesarchiv DR 306 MD/001, project "STUBEW"). The archive covers university applications submitted between 1985 and 1989 and contains individual-level information on applicants' sociodemographics, secondary-school transcripts, requested study fields and institutions, and admissions decisions.

A key feature of the data is that transcripts are recorded on standardized certificates issued between 1980 and 1989, while applications are observed only from 1985 onward. The gap arises because applicants could apply several years after school completion—for instance after completing vocational training or military service—so early-1980s certificates enter the archive via later applications. I anchor exposure timing to the *transcript year* rather than the application year, aligning treatment with the schooling phase plausibly affected by pollution. Applicants are assigned to counties based on the location of the school issuing the certificate. In robustness checks, I restrict the sample to applicants whose application closely follows certificate issuance (lag ≤ 2 years) to verify that the pre-period estimates are not driven by compositionally distinct late applicants.

On the GDR grading scale, higher numerical values denote worse performance. The two main outcome categories are: (i) overall GPA, defined as the mean of available subject grades; and (ii) individual subject grades, analyzed at the student-by-subject level after reshaping the transcript data. Study-orientation outcomes (e.g., field-of-study applications) are examined as supplementary outcomes in Section 5.3.

The estimation sample imposes the following restrictions: valid sex and birth information, valid certificate type and schooling track, GDR citizenship, age between 17 and 30 at the time of application, at least eight recorded subject grades, and no duplicate records with identical demographic and transcript information. The preferred grade specifications are estimated on approximately 1.7 million student-by-subject observations from transcript cohorts 1980–1989. Because STUBEW covers applicants to higher education rather than the full population of *Abitur* holders, estimates are conditional on appearing in the admissions system; Section 5.3 discusses the implications for interpretation.

3.4 Early Labor-Market Outcomes: GAV Register (1989)

To study early labor-market and tertiary-track outcomes, I use a centralized employment register of the GDR, the *Datenspeicher Gesellschaftliches Arbeitsvermögen* (GAV). The GAV was collected annually, but earlier snapshots were deleted for storage reasons; the surviving archive corresponds to the reference date of 31 December 1989 and contains approximately 7.25 million individual records. Each record includes demographics, educational attainment, occupational classification, and labor-market entry channel.

Because the GAV is a single cross-section, I exploit cohort variation in exposure by assigning individuals to expected *Abitur* graduation cohorts using month and year of birth together with the GDR school-entry cutoff (children entered school if they had turned six by 31 May):

$$\text{Cohort}_i = \begin{cases} \text{BirthYear}_i + 18, & \text{if BirthMonth}_i \in \{1, \ldots, 5\}, \\ \text{BirthYear}_i + 19, & \text{if BirthMonth}_i \in \{6, \ldots, 12\}. \end{cases}$$

This assignment aligns individuals with the schooling phase during which the pollution shock occurred.

I construct three binary outcomes that proxy tertiary progression and early occupational sorting: (i) an indicator for entering the labor market via a tertiary-track entry channel; (ii) an indicator for entry after completed tertiary education; and (iii) an occupation-based proxy for academic qualification derived from occupation codes. The third outcome is less mechanically tied to the timing of degree completion and therefore provides a partial check against the interpretation that results are driven purely by delayed transitions in a single cross-section.

Individuals are assigned to counties based on their place of residence as recorded in the 1989 register.[6]

A key limitation is right-censoring: individuals from later cohorts may still be enrolled in tertiary education in 1989 and therefore not observed at labor-market entry. To mitigate this concern, the baseline sample restricts to individuals with an expected *Abitur* year of 1985 or earlier—cohorts plausibly observed completing tertiary education by the reference date. The baseline estimation sample of approximately 60,000 observations reflects the following restrictions applied to the full 7.25 million-record register: retention of regular *Abitur* holders with valid county, education, entry-channel, and occupation information; exclusion of entry channels corresponding to retirement, health-related exits, or politically selected pathways not comparable to standard education-to-work transitions; and restriction to expected *Abitur* cohorts of 1985 or earlier. Standard errors are clustered at the county level throughout.

4 Empirical Strategy

4.1 First Stage: Lignite Proximity and Ambient SO_2

To document that the 1982 energy shock generated a substantial increase in local air pollution, I estimate two-way fixed effects difference-in-differences models on a county-by-year panel:

$$P_{ct} = \alpha_c + \phi_t + \beta\big(\text{Post}_t \times D_c\big) + u_{ct} \quad (1)$$

where P_{ct} is annual mean SO_2 in county c and year t. County fixed effects α_c absorb time-invariant differences in baseline pollution levels, geography, and industrial structure. Year fixed effects ϕ_t capture aggregate shocks and nationwide trends common to all counties. Under the parallel-trends assumption, β can be interpreted as the reduced-form effect of

[6]The GAV does not contain information on school of origin or educational county. Residence in 1989 may partly reflect post-school sorting; the GAV estimates are therefore interpreted as evidence on early occupational sorting rather than a pure origin-exposure effect. Section 5.2 provides supporting evidence that inter-county migration in treated cohorts was limited during the GDR period.

lignite proximity on ambient SO_2 after 1982.

To assess this assumption and characterize post-shock dynamics, I complement equation (1) with an event-study specification:

$$P_{ct} = \alpha_c + \phi_t + \sum_{\tau \neq -1} \beta_\tau \big(\mathbb{1}[t - 1983 = \tau] \times D_c\big) + u_{ct} \tag{2}$$

where τ indexes event time relative to 1983, so that $\tau = -1$, corresponding to year 1982, is the omitted baseline. Pre-treatment coefficients β_τ for $\tau < -1$ test for differential pre-trends; post-treatment coefficients trace the persistence of the pollution increase. Standard errors are clustered at the county level throughout.

4.2 Sorting and Migration Diagnostics

A key threat to identification is that households may have responded to the pollution shock by relocating, thereby changing the composition of the student population in treated counties. To assess this empirically, I construct a county-level net migration rate from the GDR statistical yearbooks:

$$\text{NetMigRate}_{ct} \;\equiv\; 1000 \times \frac{\Delta\text{Pop}_{ct} - \text{Births}_{ct} + \text{Deaths}_{ct}}{\text{Pop}_{c,t-1}},$$

and estimate equation (1) with NetMigRate_{ct} as the outcome. A null result supports the interpretation that the shock changed realized exposure rather than population composition. I also test for pre-1982 balance and stability in observable county characteristics drawn from the yearbook panels, and examine whether the composition of STUBEW applicants shifts discontinuously in treated regions after 1982.

4.3 Educational Outcomes: Micro-Level DiD in STUBEW

For educational outcomes, I estimate micro-level difference-in-differences models using the student-by-subject structure of the STUBEW transcripts:

$$Y_{isct} = \eta_\kappa + \lambda_t + \beta\big(D_c \times \text{Post}_t\big) + X_i'\gamma + \varepsilon_{isct} \tag{3}$$

where Y_{isct} is the grade for student i in subject s, assigned to county c and transcript cohort t. Transcript-year fixed effects λ_t control for common time trends. X_i includes individual controls: gender, month-of-birth dummies, organizational membership indicators, and available social-background proxies.

The term η_κ denotes high-dimensional fixed effects that vary across specifications. The baseline includes school fixed effects and subject fixed effects; the preferred specification uses *school-by-subject* fixed effects, which subsume both and compare grading within the same school and subject across transcript cohorts. This absorbs any persistent school-specific differences in grading levels and isolates variation from within-school, within-subject changes over time—the most demanding available test of a grading-standards explanation for the results.

Under parallel trends, β can be interpreted as the intent-to-treat effect of schooling in a lignite-proximate county on transcript grades after 1982. To examine pre-trends and dynamics, I estimate an event-study version of equation (3), replacing Post_t with indicators for each transcript year relative to 1982. Heterogeneous effects by subject group (STEM versus non-STEM) and by gender are estimated by interacting $D_c \times \text{Post}_t$ with the relevant indicators. Standard errors are clustered at the county level, corresponding to the level of treatment assignment; robustness to school-level clustering and two-way clustering by county and cohort is reported in the Appendix.

4.4 Early Labor-Market Outcomes: Cohort-Based DiD in GAV

Because the GAV is a single cross-section, I exploit cohort variation in exposure. Using the expected *Abitur* cohorts constructed in Section 3.4, the baseline specification is:

$$Y_{ick} = \alpha_c + \delta_k + \beta\big(D_c \times \text{Post}_k\big) + X_i'\gamma + \varepsilon_{ick} \tag{4}$$

where Y_{ick} is one of the three binary outcomes defined in Section 3.4. County fixed effects α_c and cohort fixed effects δ_k absorb time-invariant county differences and common cohort trends. $\text{Post}_k \equiv \mathbb{1}[\text{Cohort}_i \geq 1983]$ indicates cohorts whose expected *Abitur* year falls at or after the onset of the pollution shock. Under parallel trends, β captures the difference-in-differences effect of being in a treated county for cohorts exposed to the post-1982 pollution environment during secondary schooling. Identification is driven by comparing cohorts expected to complete the *Abitur* before and after 1983 across treated and control counties, with the sharpest variation coming from cohorts close to the 1983 cutoff. Because the identifying assumption here is that cohort trends within counties would have been parallel absent the shock—a stronger requirement than county-level parallel trends alone—I verify this assumption through cohort-level pre-trend tests where feasible. Given the 1989-residence county assignment discussed in Section 3.4, these estimates are interpreted as evidence on early occupational sorting rather than as pure origin-based exposure effects. Standard errors are clustered at the county level.

Equation (1) delivers a reduced-form estimate of the effect of lignite proximity on realized ambient pollution. Equations (3) and (4) deliver intent-to-treat estimates of the effect of proximity-based exposure on educational and early labor-market outcomes. The core identifying assumption in each case is parallel trends absent the lignite pivot, assessed through pre-trend tests in the event-study specifications. The sorting diagnostics in Section 5.2 provide supporting evidence that population composition did not shift differentially in response to the shock.

5 Results

5.1 First Stage: The Lignite Pivot Increased Ambient SO_2

Table 1: Baseline estimate: effect of lignite proximity on annual mean SO_2 (county-by-year DiD)

	Treatment threshold: 60km
$1[t > 1982] \times D_i$	28.820***
	(3.211)
Mean Y	151.583
Observations	2,298
Number of districts	217
Treated / Control group	115 / 102
Parallel-trends test (pre 1982)	$F(1, 216) = 0.76, \quad p = 0.3849$

Notes: Column (1) reports the ATET for the 60km threshold, adjusting for unit and time effects. Standard errors are clustered at the county level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 1 reports the baseline DiD estimate from equation (1). Counties within 60 km of a pre-established lignite mining site experienced an increase in annual mean SO_2 of 28.8 μg/m^3 after 1982 (s.e. 3.21, $p < 0.01$). Relative to a pre-treatment county mean of 151.6 μg/m^3, this corresponds to an increase of approximately 19 percent. The formal pre-trend test yields $F(1, 216) = 0.76$ ($p = 0.385$), providing no indication of differential trends prior to the shock.

Figure 3: Event-study: effect of lignite proximity on annual mean SO_2 (60 km treatment, baseline 1982)

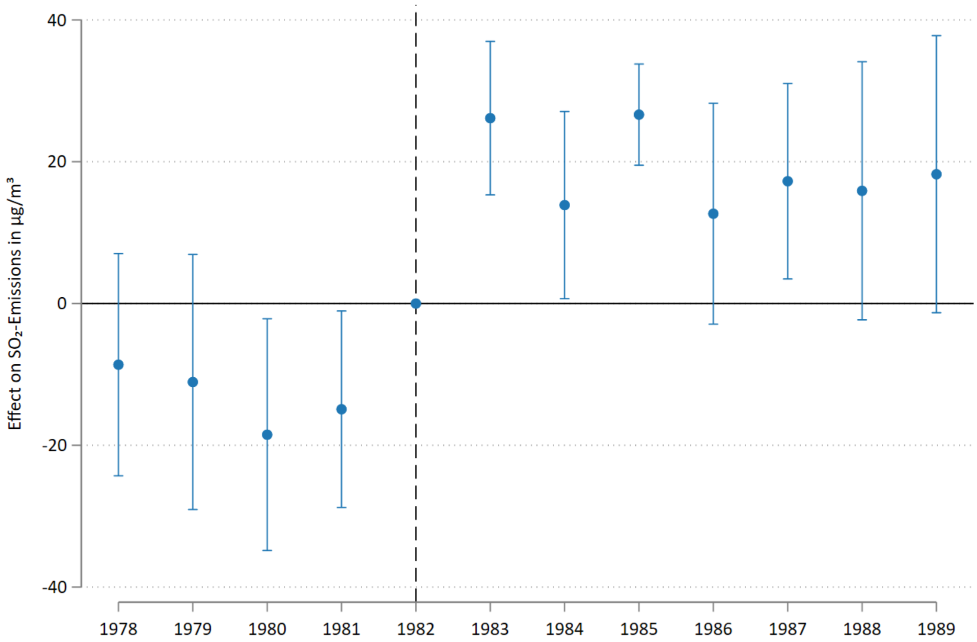


Figure 3 plots the event-study coefficients from equation (2). Pre-treatment coefficients (event years $\tau < -1$) are small and statistically indistinguishable from zero, consistent with the parallel-trends assumption. Post-treatment coefficients turn sharply positive in 1983 and remain elevated throughout the late 1980s, reflecting the sustained character of the lignite substitution. The discrete post-1982 break with a flat pre-period provides strong first-stage evidence that the energy shock induced a large and persistent regional increase in SO_2.

The estimate is robust to the choice of treatment radius. Appendix Table A1 shows that the post-1982 increase in SO_2 ranges from 24.5 to 33.4 $\mu g/m^3$ when the treatment threshold is varied between 40 and 80 km, indicating that the pollution gradient around lignite regions is broad enough that results do not hinge on a specific distance cutoff.

Beyond this internal robustness check, the magnitude of the estimated increase can also be benchmarked against current regulatory thresholds for SO_2. In December 2024, the U.S. Environmental Protection Agency finalized a revised secondary National Ambient Air Quality Standard for SO_2 of 10 ppb as an annual average (89 FR 105692), corresponding to approximately 26 $\mu g/m^3$ under standard atmospheric conditions. The EU Ambient Air Quality Directive (2008/50/EC, Annex XIII) specifies a critical level for the protection of vegetation of 20 $\mu g/m^3$ as a calendar-year average. The estimated treatment effect of 28.8 $\mu g/m^3$ therefore exceeds both thresholds on its own—that is, the *increase* induced by the lignite shock alone is larger than the levels that current U.S. and EU frameworks consider protective of ecological welfare, on top of the substantial pre-treatment baseline of 151.6 μg/m^3. This comparison underscores that the pollution shock studied here corresponds to an environmentally consequential change in ambient air quality by the standards of contemporary regulatory practice.

5.2 Sorting and Migration

Estimating equation (1) with the net migration rate as the outcome yields a coefficient near zero and statistically insignificant at conventional levels (Appendix Table A2). Treated counties did not experience differential out-migration following the 1982 shock, consistent with the institutional mobility constraints described in Section 2.4. Pre-1982 county characteristics—including population density, age structure, and broad indicators of industrial activity drawn from the statistical yearbooks—are balanced across treated and control counties, supporting the parallel-trends assumption. Within STUBEW, the observable composition of applicants (sex ratio, age distribution, and organizational membership indicators) shows no discontinuous shift in treated regions after 1982, suggesting that selection into the applicant pool did not change systematically in response to the pollution shock.

5.3 Transcript Grades

Table 2: Difference-in-differences estimates: transcript grades

	(1)	(2)	(3)
Treatment region × Post-1982	0.0257**	0.0303***	0.0298***
	(0.0116)	(0.0108)	(0.0108)
Observations	1,774,252	1,706,630	1,705,861
R^2	0.0225	0.1231	0.1493
Specification details			
Year fixed effects	Yes	Yes	Yes
School fixed effects	Yes	Yes	No
School × subject fixed effects	No	No	Yes
Controls	No	Yes	Yes

Notes: The table reports difference-in-differences estimates of the effect of attending school in treatment regions on transcript grades. The sample consists of university applications submitted between 1985 and 1989; the outcome is the average grade on the submitted transcript, issued between 1980 and 1989. Treatment region is an indicator for schools located within 60 km of a lignite-mining area, and Post-1982 equals one for transcripts issued for the school year beginning in 1983 or later. Standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2 presents DiD estimates from equation (3) under progressively demanding fixed-effects specifications. The preferred specification—school-by-subject fixed effects and individual controls—yields an estimate of 0.0298 (s.e. 0.0108, $p < 0.01$), implying that students in treated regions experienced a deterioration of roughly 0.03 grade points after 1982, equivalent to approximately 0.05 standard deviations. The estimate is stable as additional fixed effects are added, suggesting that the result is not driven by persistent school-level or subject-level differences in grading.

Figure 4: Event-study: effect of lignite proximity on transcript grades (60 km treatment, baseline 1982)

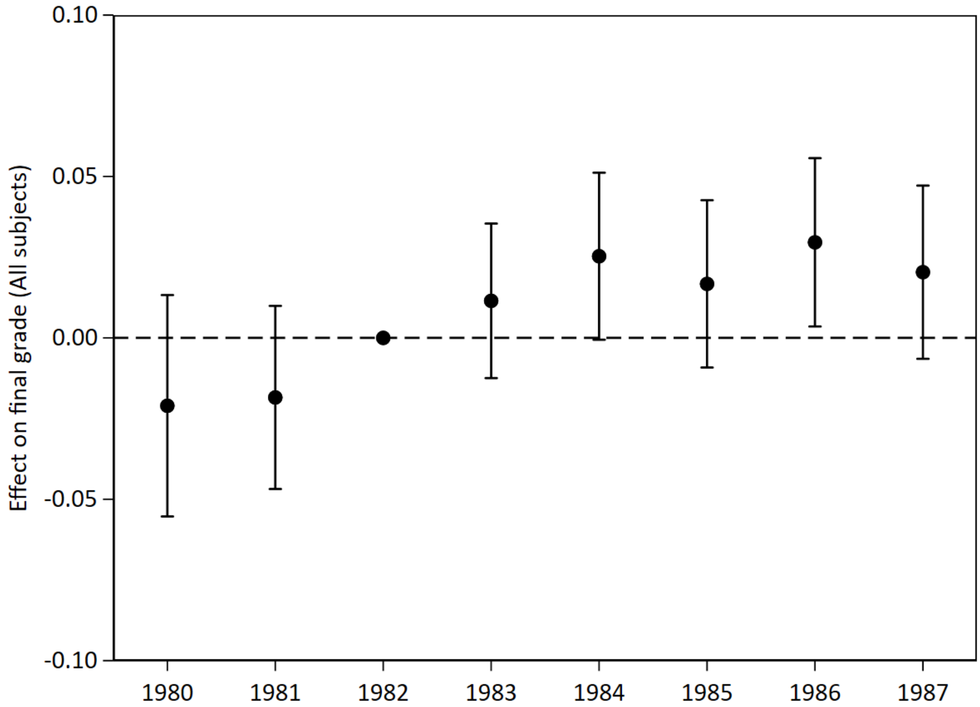


Figure 4 plots the event-study coefficients for transcript grades. Pre-treatment coefficients (1980–1981) are negative but not statistically distinguishable from zero, and a formal joint test for the pre-period coefficients fails to reject the null of no differential pre-trend, supporting the parallel-trends assumption. Post-1982 coefficients are positive in every cohort and broadly increase relative to the pre-period, with the largest point estimates for cohorts whose final years of secondary schooling coincide with the peak post-shock period (1984 and 1986). While the post-period coefficients fluctuate somewhat from cohort to cohort, the overall pattern—a shift from negative or zero pre-period estimates to consistently positive post-period estimates—is consistent with a cumulative learning-impairment mechanism in which the pollution shock depresses performance as exposure accumulates, rather than with an immediate one-time level shift.

Because STUBEW covers applicants to higher education rather than the full *Abitur* population, these estimates are conditional on applying to university. If exposure to the lignite-related pollution shock also reduced the propensity to apply, the estimates should be interpreted as effects within the applicant pool rather than as population-wide effects for all *Abitur* holders; the direction of any resulting selection bias is ambiguous and depends on which students are differentially deterred from applying.

5.4 Heterogeneity: Subject Pattern and Gender

Figure 5: Heterogeneity by subject: DiD estimates for individual transcript grades (60 km treatment, 90% confidence intervals)

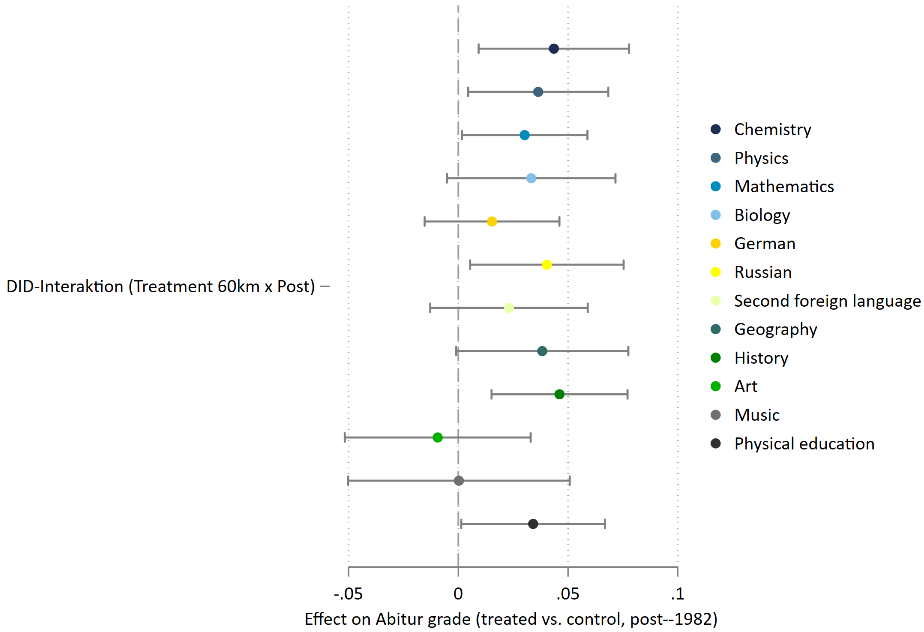


Table 3: Difference-in-differences estimates: transcript grades by gender

	(1) All students	(2) Male students	(3) Female students
Treatment region × Post-1982	0.0307***	0.0378***	0.0284*
	(0.0109)	(0.0120)	(0.0168)
Observations	1,667,660	907,822	759,838
R^2	0.1226	0.0676	0.0625
Specification details			
Year fixed effects	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The table reports difference-in-differences estimates of the effect of attending school in treatment regions on transcript grades, separately by gender. Column (1) reports the baseline estimate for the full sample, while columns (2) and (3) restrict the sample to male and female students, respectively. The sample consists of university applications submitted between 1985 and 1989, with transcripts issued between 1980 and 1989. Treatment region is an indicator for schools located within 60 km of a lignite-mining area, and Post-1982 equals one for transcripts issued for the school year beginning in 1983 or later. Standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5 plots subject-specific DiD estimates for all twelve subjects recorded on the transcripts. The subject pattern is informative about mechanisms, and I interpret it as evidence on heterogeneity across subject types rather than as a set of separate hypothesis tests for individual subjects; Appendix Table A4 reports grouped estimates for academic core subjects and other subject groups.[7] Adverse effects are concentrated in cognitively demanding academic subjects requiring cumulative mastery and sustained preparation. The largest impacts occur in history ($\hat{\beta} = 0.046$, $p < 0.01$), chemistry (0.044, $p < 0.05$), Russian (0.040, $p < 0.05$), geography (0.038, $p < 0.10$), physics (0.036, $p < 0.05$), biology (0.033, $p < 0.10$), and mathematics (0.030, $p < 0.05$). The presence of history, Russian, and geography alongside STEM subjects suggests that the relevant dimension is not STEM per se, but the combination of high cognitive demand, sequential content, and exam-relevant preparation that characterizes these subjects in the GDR curriculum.

By contrast, effects are close to zero and not statistically significant for German, the second foreign language, music, and visual arts. German and the second foreign language were taught and examined throughout the EOS curriculum but, unlike Russian, were not weighted as heavily in admissions decisions for technically oriented fields, while music and visual arts were assessed by different criteria and carried less academic weight overall. The contrast between the academic core subjects and this group of subjects is difficult to reconcile with a uniform change in grading standards, which would predict broadly similar shifts across all subjects within the same schools. These near-zero effects provide a useful internal contrast, though they should not be interpreted as pure placebos since grading practices and curricular demands differ systematically across subject types.

Physical education is an informative exception to this pattern. The estimated effect on physical education grades (0.033, $p < 0.05$) is comparable in magnitude to the effects on physics and geography, despite physical education not requiring the cumulative academic mastery that characterizes the core subjects above. In the GDR, physical education grades reflected measured physical performance (e.g., fitness and endurance tests) in addition to participation, so a deterioration in this subject is more naturally read as evidence of a direct physical channel—reduced respiratory or physical capacity under sustained SO_2 exposure—than as evidence of classroom learning impairment. I therefore treat physical education as a separate piece of evidence that complements, rather than forms part of, the academic-core learning-impairment pattern: it is excluded from the set $\mathcal{A}$ and from its comparison group in the formal interaction test in Section 6.1, and is discussed further as evidence on the respiratory-health sub-channel noted in the mechanisms discussion.

Together, the pattern across academic core subjects, lower-stakes academic subjects, arts subjects, and physical education is most consistent with two complementary channels: a learning-impairment channel that reduces students' capacity to acquire and retain

[7] With twelve subjects, some individual estimates will reach conventional significance thresholds by chance. The grouped comparisons are less susceptible to this concern.

material in cognitively demanding academic domains, and a physical channel that directly affects performance in physically demanding assessments.

Turning to heterogeneity along a different dimension, Table 3 reports gender-specific estimates. The deterioration in grades is present for both groups. The estimated effect is somewhat larger for male students (0.038, s.e. 0.012, $p < 0.01$) than for female students (0.028, s.e. 0.017, $p < 0.10$), though the difference is not statistically significant at conventional levels and both groups show meaningful deterioration.

5.5 Early Labor-Market and Tertiary Outcomes (GAV)

Table 4: Difference-in-differences estimates: tertiary outcomes (GAV)

	(1) Tertiary entry	(2) Tertiary completed	(3) Academic occupation
Treatment county × Post-1982	-0.0257^{**}	-0.0267^{***}	-0.0104^{**}
	(0.0120)	(0.0090)	(0.0051)
Observations	60,024	60,024	60,024
R^2	0.0684	0.2106	0.5674
Specification details			
County fixed effects	Yes	Yes	Yes
Expected Abitur cohort fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The table reports difference-in-differences estimates using the 1989 cross-section of the GDR register *Gesellschaftliches Arbeitsvermögen* (GAV), restricted to individuals with regular Abitur and cohorts with expected Abitur graduation year $\leq$ 1985. Treatment counties are defined as counties whose centroids lie within 60 km of a pre-established lignite mining area. Post-1982 equals one for cohorts with expected Abitur graduation year $\geq$ 1983 (constructed from birth month and year using the May 31 school-entry cutoff). All specifications include controls for gender, month of birth, number of children, number of dependent relatives, and marital status. Standard errors are clustered at the county level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4 presents cohort-based DiD estimates from equation (4) for the three binary outcomes. Among post-shock cohorts residing in treated counties in the 1989 GAV register, those with an expected *Abitur* year of 1985 or earlier are 2.6 percentage points less likely to be observed entering the labor market via a tertiary-track channel (column 1: -0.026, s.e. 0.012, $p < 0.05$) and 2.7 percentage points less likely to have entered after completing tertiary education by 1989 (column 2: -0.027, s.e. 0.009, $p < 0.01$). Column 3, using the occupation-based academic qualification proxy, shows a smaller but statistically significant decline of 1.0 percentage point (-0.010, s.e. 0.005, $p < 0.05$).

Two interpretive caveats apply. First, because the GAV is observed only in 1989, columns 1 and 2 capture tertiary completion *by* 1989 rather than over the full life cycle; later-graduating cohorts may simply not yet have entered the labor market at the reference date. The cohort-window restriction mitigates but does not eliminate this concern. Second, county assignment uses 1989 residence, which may partly reflect post-school sorting; these estimates should be read as evidence on early occupational sorting rather than causal effects of origin exposure. The occupation-based outcome in column 3 is less mechanically tied to completion timing and provides supportive corroborating evidence that the shock affected early occupational trajectories beyond a purely mechanical delay in labor-market entry.

5.6 From Pollution to Performance to Early Trajectories

The evidence across the three datasets is internally consistent and points to a pollution-to-performance-to-trajectory sequence. The first link—lignite proximity to SO_2—is strongly supported by the externally timed shock and the DiD evidence: a large, persistent, and spatially concentrated increase in ambient pollution with no prior differential trend. The second link—the proximity-induced pollution shock to transcript grades—is identified through the same DiD design applied to educational outcomes, with no pre-trends and a dynamic pattern of growing effects consistent with cumulative learning impairment rather than a one-time grading shift. The third link—grade losses to early occupational sorting—is supported by consistent patterns in the GAV register, but should be interpreted as suggestive: the GAV and STUBEW designs are independent, and a causal chain from grades to labor-market outcomes is not directly identified within a single dataset. The following section examines the mechanisms underlying the second link more closely and presents robustness evidence that speaks to the main threats to identification.

6 Mechanisms and Robustness

6.1 Mechanisms

The results in Section 5 are consistent with a learning-impairment channel in which sustained pollution exposure reduces students' ability to acquire and retain material in cognitively demanding academic subjects. This section provides three additional diagnostics that strengthen this interpretation and speak against the main alternative explanation—region-specific changes in grading standards.

Diagnostic 1: Subject heterogeneity as a formal test. The subject-specific pattern documented in Section 5.4 can be formalized by augmenting equation (3) with an interaction term:

$$Y_{isct} = \eta_\kappa + \lambda_t + \beta_1\big(D_c \times \text{Post}_t\big) + \beta_2\big(D_c \times \text{Post}_t \times \mathbb{1}[s \in \mathcal{A}]\big) + X_i'\gamma + \varepsilon_{isct} \qquad (5)$$

I define $\mathcal{A}$ ex ante as the set of academic core subjects in the EOS curriculum—mathematics, natural sciences (chemistry, physics, biology), Russian, history, and geography—based on their status as written, academically weighted compulsory subjects in the GDR's upper-secondary program, not based on which subjects happen to show significant estimates. German, the second foreign language, music, and visual arts—which were assessed by different criteria and carried less academic weight in the admissions process—form the comparison group. Physical education is excluded from both $\mathcal{A}$ and the comparison group, since Section 5.4 interprets its grade effect as evidence on a separate physical-health channel rather than on academic learning impairment. A positive and significant $\hat{\beta}_2$ indicates that the deterioration is disproportionately concentrated in academic core subjects relative to this comparison group. [$\hat{\beta}_2 =$ **[to be filled]**; $\hat{\beta}_1 =$ **[to be filled]**.] A uniform school-wide change in grading stringency would not generate this pattern; the remaining concern would be subject-specific grading changes concentrated in academic core subjects, which the school-by-year fixed effects in Diagnostic 3 speak to directly.

Diagnostic 2: Cumulative exposure. The event-study evidence in Figure 4 shows that grade effects grow over time rather than appearing as an immediate level shift at the treatment onset. To quantify this trajectory, I estimate a variant of equation (3) that replaces the binary post indicator with a continuous exposure measure:

$$Y_{isct} = \eta_{\kappa} + \lambda_t + \theta\big(D_c \times \text{YearsSince}_t\big) + X_i'\gamma + \varepsilon_{isct} \tag{6}$$

where $\text{YearsSince}_t \equiv \max(0,\, t - 1982)$ counts years of post-shock exposure, so that the first post-shock transcript cohort (1983) receives a value of one, 1984 a value of two, and so forth. A positive $\hat{\theta}$ implies that performance losses accumulate with each additional year of sustained exposure, as expected under a cumulative learning-impairment model. [$\hat{\theta} =$ **[to be filled]**.]

Diagnostic 3: Against a grading-standards explanation. Three features of the design and results argue against the interpretation that the post-1982 grade deterioration reflects a change in regional grading stringency rather than student performance. First, the GDR's EOS curriculum and grading guidelines were centrally prescribed, limiting—though not eliminating—the scope for persistent region-specific deviations in grading standards (Section 2.3). Second, the preferred specification includes school-by-subject fixed effects, which absorb any time-invariant differences in grading levels across schools and subjects. Third, I estimate specifications that additionally include school-by-year fixed effects. In these specifications, the main treatment effect $D_c \times \text{Post}_t$ is absorbed; identification comes exclusively from whether academic core subjects deteriorate *more* than the non-core comparison subjects (German, the second foreign language, music, and visual arts) within the same school and year. This design rules out any school-level, year-specific shock to grading—whether uniform or subject-specific—that affects all subjects in the same direction. The persistence of a positive $\hat{\beta}_2$ in this specification provides the strongest available evidence that the result reflects performance rather than grading standards.

Limitation: channels within the learning-impairment hypothesis. The available data do not allow full disaggregation of the channels through which sustained pollution exposure affects transcript performance. In particular, the paper cannot separately identify whether the effects on academic core subjects operate through respiratory health and school absenteeism, direct neurological or cognitive impairment under sustained exposure, fatigue and reduced concentration, or some combination of these pathways. The result for physical education in Section 5.4 is suggestive of a respiratory or physical-capacity channel operating alongside the academic learning-impairment pattern, but this is one indicative estimate rather than a validated mechanism test. Establishing the relative importance of these sub-channels would require individual-level health or attendance data from the GDR period, which are not available for the present analysis.

6.2 Robustness

The results are robust across a broad set of alternative specifications and exposure definitions; this section summarizes the main checks, with details in Appendix A.

Alternative treatment definition. Redefining treatment by proximity to lignite-fired power plants (capacity > 100 MW) rather than mining areas yields qualitatively similar first-stage and grade effects. Because combustion facilities were the direct source of SO_2 emissions, this alternative addresses concerns that mining proximity proxies non-pollution characteristics of lignite regions. Appendix Figures A1 and A2 map the power-plant-based treatment definition and its overlap with the mining-based definition; the two definitions classify the large majority of counties identically.

Threshold sensitivity and continuous exposure. Appendix Table A1 shows first-stage stability across radii of 40–80 km. Grade and GAV results are similarly stable across these thresholds (Appendix A). As a complementary check, I replace the binary treatment indicator with continuous county-year SO_2 from the monitoring data. Because observed SO_2 may reflect other local shocks beyond the lignite pivot, I interpret these estimates as supportive dose-response evidence rather than as the primary causal specification; they are consistent with the intent-to-treat results and point to a pollution mechanism.

Composition and selection. Appendix Table A3 shows no discontinuous shift in the observable composition of STUBEW applicants—sex ratio, age at application, and organizational membership—in treated regions after 1982, reducing the concern that the grade results reflect changes in the applicant pool rather than changes in performance. Grouped estimates for academic core subjects versus music and visual arts are reported in Appendix Table A4 and confirm the pattern in Figure 5.

GAV right-censoring. Restricting the GAV sample to cohorts with an expected *Abitur* year of 1984 or earlier yields qualitatively similar patterns with slightly attenuated magnitudes, confirming that the baseline results are not solely driven by mechanical censoring of the most recent cohorts (Appendix Table A5).

Spatial placebo. Replacing lignite mining proximity with proximity to potash mines—an extractive industry without the same combustion-related SO_2 signature—yields no meaningful post-1982 increase in ambient SO_2 and no systematic effects on transcript grades (Appendix Table A6). This placebo test supports the interpretation that the estimated effects are specific to the lignite-driven pollution shock rather than reflecting generic characteristics of mining regions.

Spatial spillovers and inference. Excluding counties within a 10–20 km buffer around the 60 km treatment boundary leaves results unchanged, addressing the concern that SO_2 spillovers into nearby control counties attenuate the treatment contrast. Inference is robust to spatial HAC standard errors and to alternative clustering choices (Appendix A).

7 Conclusion

This paper shows that the costs of air pollution extend beyond health and contemporaneous productivity to the formation of human capital during a critical educational transition. Using the 1982 Soviet oil supply cut as a source of exogenous variation, I document that the rapid shift to lignite combustion generated a large and persistent regional increase in ambient SO_2 in counties near pre-established lignite mining areas. The GDR's institutional constraints on residential mobility and private avoidance—state housing allocation, residence-based school assignment, and the scarcity of mitigation technologies—allow this regional pollution shock to be studied in a setting with unusually limited scope for sorting and behavioral adaptation.

Three sets of results emerge. First, counties within 60 km of pre-established lignite mining areas experienced an increase in annual mean SO_2 of approximately 28.8 $\mu g/m^3$ after 1982, with no indication of differential pre-trends. Second, the same DiD design indicates that the proximity-induced pollution shock worsened transcript performance by roughly 0.03 grade points (0.05 standard deviations), with effects concentrated in cognitively demanding academic core subjects and a dynamic pattern consistent with cumulative learning impairment under sustained exposure. Third, post-shock cohorts residing in treated counties are 2.6–2.7 percentage points less likely to have entered tertiary-linked pathways by 1989, and approximately 1 percentage point less likely to be observed in an academically classified occupation—patterns consistent with, though not directly identifying, downstream consequences of the performance losses during secondary schooling.

The findings have implications that extend beyond the historical episode. The costs of air pollution are not limited to contemporaneous health and productivity—they include meaningful losses in academic performance during a critical phase of credential accumulation. In education systems where access to selective study fields depends on small performance differences, these losses can shift early occupational trajectories even when the average effect is modest in absolute terms. Environmental regulation is therefore also regional human-capital policy: when pollution is spatially concentrated, its educational costs are borne disproportionately by the communities living closest to energy and industrial infrastructure.

Three limitations should be noted. The STUBEW estimates are conditional on applying to higher education and therefore capture effects within the applicant pool; the direction of any selection bias from differential application propensity is ambiguous. The GAV cross-section introduces right-censoring for later cohorts and relies on 1989-residence county assignment, so the labor-market results are best read as suggestive evidence on early occupational sorting. Finally, the GDR setting featured unusually high pollution levels and very limited private adaptation; extrapolation to contemporary lower-exposure environments should account for potential non-linearities and greater scope for avoidance.

Future work can extend this analysis in two directions. Within STUBEW, richer examination of field-of-study choices and admissions outcomes can clarify how performance losses translate into shifts in postsecondary orientation. Linking the educational effects documented here to longer-run labor-market outcomes after reunification—using an inverse-movers design that compares individuals arriving at the same destination from differentially exposed origins—can assess the persistence of these early educational and occupational impacts into adult careers.

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A1 Appendix Tables

Table A1: First-stage robustness: alternative distance thresholds (40–80 km)

	Baseline	**Alternative thresholds**			
	60km (1)	**40km** (2)	**50km** (3)	**70km** (4)	**80km** (5)
$1[t > 1982] \times D_i$	28.820***	24.543***	27.007***	31.679***	33.353***
	(3.211)	(2.799)	(2.974)	(3.444)	(3.536)
Observations	2,298	2,284	2,284	2,284	2,284

Notes: Two-way fixed effects DiD estimates of equation (1) for alternative treatment radii. D_i indicates whether a county centroid lies within the stated distance of a pre-established lignite mining area. Standard errors clustered at the county level in parentheses. The sample underlying columns (1)–(5) differs marginally from Table 1 (2,284 vs. 2,298 county-year observations; pre-treatment mean SO_2 of 159.4 vs. 151.6 $\mu g/m^3$) due to a slightly different balanced-panel restriction used in the threshold-sensitivity exercise; the 60 km point estimate is identical across both samples.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A2: Sorting test: net migration proxy (county-by-year DiD)

[Table placeholder – `tab_app_migration.tex`]

Table A3: STUBEW covariate balance: applicant characteristics across cohorts in treated and control regions

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Table A4: Grouped DiD estimates: academic core subjects versus music and visual arts

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Table A5: GAV robustness: alternative cohort cutoffs (expected Abitur $\leq$ 1984)

[Table placeholder – `tab_app_gav_robust.tex`]

Table A6: Spatial placebo: proximity to potash mines

[Table placeholder – `tab_app_placebo.tex`]

A2 Appendix Figures

Figure A1: Alternative treatment definition: proximity to lignite-fired power plants (capacity $> 100\,\mathrm{MW}$, $60\,\mathrm{km}$ buffer)

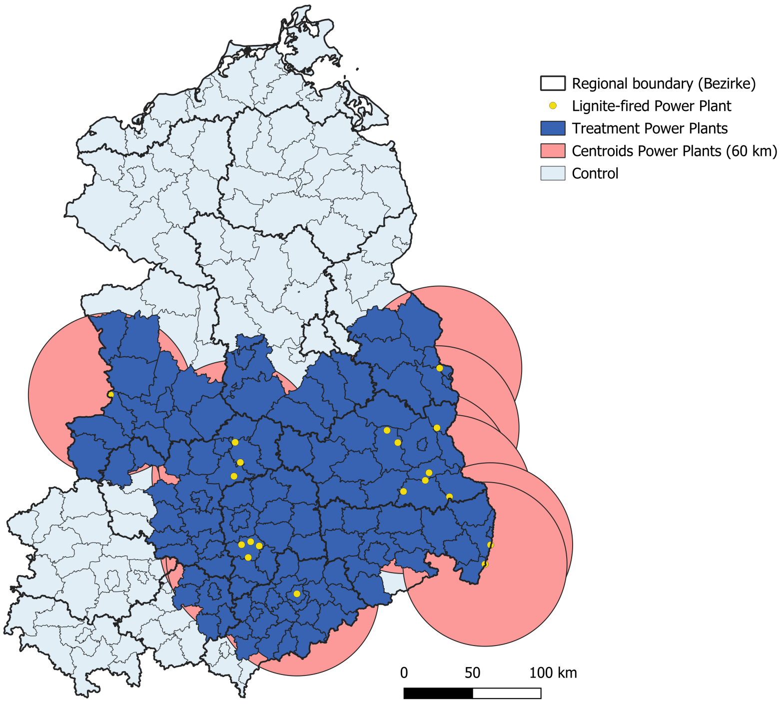

Figure A2: Overlap of the mining-based and power-plant-based treatment definitions (60 km buffers)