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Environmental Inequality and Residential Sorting in Germany: A Spatial Time-Series Analysis of the Demographic Consequences of Industrial Sites

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ABSTRACT Previous research has shown that low-income households bear a higher exposure to environmental pollution than high-income households. Some scholars have argued that selective siting of industrial facilities accounts for such environmental inequality, while others have argued that those citizens who can afford to move out of polluted regions do so, and the socioeconomically disadvantaged are sorted into polluted areas. Yet empirical evidence regarding the processes of environmental inequality is not conclusive. We build on an original data set that includes annual georeferenced data of 6,570 highly polluting industrial facilities in Germany from 2008 to 2017 and validate the fluctuation in facilities with geographical land-use data. We then connect the facilities to income and demographic data for 4,455 municipalities and investigate sociodemographic changes before and after the appearance of new facilities. Spatial models are employed to measure local relative changes, and fixed-effects individual slopes estimators are used to account for selection on economic trajectories. Results provide only limited support for the selective siting thesis but show that an area's average income decreases after the appearance of new industrial facilities, thereby resonating with the selective migration hypothesis. In contrast, facility closure does not attract, or reattract, more affluent households.

KEYWORDS Environmental inequality • Fixed-effects individual slopes • Population dynamics • Selective migration • Selective siting

Introduction

The exposure level to environmental pollution is not equally distributed across households. Research has shown that ethnic and racial minorities in the United States as well as in Europe are disproportionately exposed to environmental harms (e.g., Ard 2015; Glatter-Götz et al. 2019; Jünger 2021; Mohai and Saha 2015a; Pasetto et al. 2019; Pastor et al. 2005; Rüttenauer 2018, 2019a). Similarly, economically disadvantaged households tend to live in areas with higher levels of environmental pollution (e.g., Ash and Fetter 2004; Downey and Hawkins 2008; Raddatz and Mennis 2013; Wolverton 2009). Meanwhile, recent studies have documented the severe impacts

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of environmental pollution on health (European Environment Agency 2019) and long-term educational trajectories (e.g., Colmer and Voorheis 2020). Environmental inequality thus constitutes a severe dimension of social inequality, and it is important to understand the processes associated with the unequal distribution of environmental harms. With a comprehensive understanding of the underlying dynamics, policies can successfully target environmental inequalities and injustices.

Existing research points to two potential mechanisms involved: (1) firms selectively build new sites in socioeconomically disadvantaged areas or close old sites in affluent regions, and (2) households residentially sort into areas of different environmental quality on the basis of income. While the first explanation assumes that socioeconomic differences already existed prior to the siting of hazardous facilities, the second explanation hypothesizes that post-siting sorting processes induce socioeconomic changes regarding already existing sites. Longitudinal studies have provided mixed results (for a review, see Banzhaf et al. 2019a; Mohai and Saha 2015a), and it remains a puzzle whether environmental inequality stems from selective siting of facilities, selective migration of households, or a combination of both. Here, we address the question of “which came first?” (Pastor et al. 2001)—hazardous facilities or socioeconomically disadvantaged residents—and analyze the population dynamics related to the siting and closure of industrial sites.

In this longitudinal study, we provide new insights on the dynamics of environmental inequality in Germany by testing for evidence of selective siting and selective migration. We use georeferenced pollution data from the European Pollutant Release and Transfer Register (E-PRTR)—which documents the location of high-emission facilities in Germany—for the period from 2008 to 2017. We validate this information against longitudinal land-use data to measure the appearance and disappearance of industrial facilities. These facility data are then combined with annual socioeconomic and demographic data for 4,455 German municipalities. This allows us to test whether the socioeconomic composition of a municipality influences the likelihood of receiving new industrial disamenities (such as factories, power plants, or waste-processing sites) and whether these subsequently induce residential sorting processes.

We contribute to existing research in three ways. First, we use annual panel data of German municipalities to provide a detailed account of how demographic changes relate to changes in the presence of industrial disamenities, and distinguish between the demographic consequences of the siting of new facilities and the closing of old ones. Second, we disentangle the effect of changes in industrial sites from general economic trends and path dependencies. These trends may result in different income trajectories over time, which are correlated to—but not driven by—changes in environmental disamenities. We accomplish this by using fixed-effects individual slopes (FEIS) estimators that account for community-specific economic trends over time. Third, we consider an earlier critique that demographic changes depend on changes in environmental quality in the focal municipality, but also on changes in residential alternatives (Banzhaf and Walsh 2013). We use spatial modeling techniques to include changes in adjacent municipalities and also investigate changes in income after a reordering of environmental quality among neighboring municipalities. Hence, we provide a comprehensive test of selective siting and selective migration while considering potential reasons for the heterogeneity of previous findings.

Theoretical Background

Two opposing processes are often employed to explain the disproportionate exposure of socioeconomically disadvantaged households to environmental hazards: selective siting and selective migration. In the first process, pollution or industrial sites might be placed selectively close to specific groups of inhabitants. In the second, certain groups might selectively escape polluted areas and others might selectively move toward polluted areas.[1]

Selective Siting

The selective siting argument claims that hazardous facilities are disproportionately sited in neighborhoods characterized by low income (Been and Gupta 1997; Mohai and Saha 2015a; Pastor et al. 2001; Saha and Mohai 2005; Wolverton 2009). The reason for this selective siting behavior can be twofold. First, the market explanation assumes that companies seek to minimize their land and housing costs when identifying locations for new facilities. Because of lower land prices and housing costs, socioeconomically disadvantaged regions are an attractive siting location for new facilities (Downey 2005; Farber 1998; Saha and Mohai 2005; Wolverton 2009, 2012). Furthermore, low-income households have been found to express a lower "willingness to pay"—also in the sense of ability to afford—for environmental goods (Franzen and Vogl 2013; Liebe et al. 2010). Following the Coase theorem, companies would thus need to pay lower compensation costs for emissions in areas with a higher share of low-income residents (Banzhaf et al. 2019a, 2019b), and hence it would be a rational strategy for a profit-maximizing company to locate facilities in low-income areas.

Second, the social and political capital explanation assumes that the level of both social and political capital is lower in socioeconomically disadvantaged regions and that their inhabitants are therefore less likely to organize collective protests against hazardous facilities (Hamilton 1995; Pastor et al. 2001), to influence political decisions by engaging in collective action (e.g., efforts to ban hazardous facilities), or to take legal actions (Wolverton 2009). Affluent residents, in contrast, are more likely to influence political actors via social ties or political engagement and more likely to engage in legal actions. If the respective executive decision makers anticipate potential problems in affluent regions, they may choose the "path of least political resistance" (Saha and Mohai 2005) and selectively place industrial sites in socioeconomically disadvantaged regions.

Following the theory of selective siting, empirical research usually investigates whether aggregated demographic characteristics influence the likelihood of receiving industrial sites. Studies by Pastor et al. (2001), Richardson et al. (2010), Saha and Mohai (2005), Shaikh and Loomis (1999), and Wolverton (2009) support the theory of selective siting and found a negative correlation between income and the

[1] Research in the United States often focuses on the severe disadvantage of ethnic or racial minorities (Banzhaf et al. 2019a; Mohai and Saha 2015a). Because we have only limited longitudinal data on ethnic minorities in Germany, we focus on environmental inequality according to income. We hope that better longitudinal data will allow us to investigate ethnic disadvantages in the future.

likelihood of becoming a facility-hosting area. However, other studies that looked at poverty rates or income did not find any effect on the likelihood of facility siting (Been and Gupta 1997; Downey 2005; Oakes et al. 1996) or found rather inconsistent results (Mitchell et al. 1999; Wolverton 2012). Other studies that focused on the influence of racial composition on the siting of facilities (Funderburg and Laurian 2015; Mohai and Saha 2015b) found that differences in the minority share of an area's population already existed prior to the siting. Still, Elliott and Frickel (2013, 2015) showed that for a number of cities, the reuse of former industrial sites was a much stronger predictor for the location of currently operating industrial facilities than the demographic characteristics of nearby inhabitants. Overall, the empirical support for selective siting as an explanation for environmental inequality is mixed.

Selective Sorting

The selective migration or sorting argument, in contrast, assumes that socioeconomic changes in polluted areas sequentially follow the siting process. Here it is hypothesized that specific households sort into residential areas with different environmental qualities according to their income (Banzhaf and McCormick 2012; Banzhaf and Walsh 2008; Best and Rüttenauer 2018; Crowder and Downey 2010; Mohai and Saha 2015a; Pais et al. 2014; Sieg et al. 2004).

In general, the argument follows Tiebout's (1956) model of the "consumer-voter": that households can adjust the level of public goods provision to their preferences by moving between municipalities—they are "voting with their feet." Because households prefer a higher environmental quality over a lower one (Bayer et al. 2009; Currie et al. 2015), the demand for high-quality neighborhoods exceeds that for low-quality ones, thereby increasing the housing and land prices in high-quality areas (Banzhaf and McCormick 2012). Neighborhoods with low environmental quality are thus more likely to offer low-cost housing opportunities (Bayer et al. 2009; Currie et al. 2015; Farber 1998). At the same time, households are willing to pay more for environmental goods as their income increases (Franzen and Vogl, 2013; Liebe et al. 2010). It follows that high-income households have an increased likelihood of moving out of low-quality neighborhoods (selective out-migration) because they are willing and able to pay for higher housing prices. Simultaneously, low-income households are steered into low-quality neighborhoods because of the need for affordable housing. The selective out-migration of high-income households and the resulting decrease in housing demand further reinforce the process of selective in-migration of low-income households.

So far, few studies have assessed this argument by using household-level panel data. In line with the selective migration theory, Crowder and Downey (2010) showed that household income helps in reducing the proximity to industrial hazards in the neighborhood of destination when households move. Similarly, Pais et al. (2014) found that income reduces the likelihood of being in a persistently high pollution trajectory compared with a persistently low trajectory when analyzing the moving paths of households. For Germany, Best and Rüttenauer (2018) reported slightly higher reductions in households' perceived local pollution after residential moves for households with a higher income. Hence, longitudinal studies on the household level support the theory of selective migration or sorting.

Still, studies on the spatially aggregated level provide less conclusive results than those using individual-level survey data (for a detailed literature review, see Banzhaf et al. 2019a). Usually, such studies investigate if an area's socioeconomic composition changes after shifts in environmental quality, and hence whether selective migration on the micro level influences the aggregated income. If increasing pollution leads to selective sorting processes, this could be observed by a decreasing average income (and vice versa). This line of reasoning is supported by studies identifying post-siting demographic changes (Baden and Coursey 2002; Banzhaf and Walsh 2008; Depro et al. 2015; Gamper-Rabindran and Timmins 2011; Richardson et al. 2010). For instance, Banzhaf and Walsh (2008) reported lower income growth rates after an area received a new TRI (toxics release inventory) facility in California. Similarly, Gamper-Rabindran and Timmins (2011) reported an increase in local average income after the cleanup of Superfund sites in the United States. However, other studies did not find increasing poverty rates (Been and Gupta 1997) or a decreasing average income (Downey 2005), nor did they find increasing minority shares in area populations (Funderburg and Laurian 2015; Mohai and Saha 2015b; Oakes et al. 1996; Pastor et al. 2001; Shaikh and Loomis 1999) following the siting of new facilities. Empirical support for the selective migration or sorting argument on the macro level thus remains mixed, while individual-level results underpin the theory.

Identifying Selective Sorting

Banzhaf and McCormick (2012), Banzhaf and Walsh (2008, 2013), and Depro et al. (2015) have argued that relying solely on aggregated data and changes in the focal unit may fail to identify selective sorting processes. For instance, if a municipality experiences a marginal increase in pollution and some households with a relatively high income in the "treated" municipality sort into a cleaner municipality, the moving population might still have a lower income than the average of the receiving municipality (even though they are richer than the average of the municipality of origin). In this case, we would observe decreases in average income in the "treated" and the "control" municipality, thereby estimating a null effect of pollution changes on income changes when using within-estimators. Banzhaf and McCormick (2012) and Banzhaf and Walsh (2008) showed formally that we can only expect unambiguous shifts in average income if pollution changes in a way such that the hierarchy or rank in environmental quality among local alternatives is reordered. In this case, every household prefers to move to the municipality that has become better in terms of environmental quality than its local alternatives. We would then expect perfect residential sorting based on income, thereby increasing income in the improved municipality and decreasing income in the deteriorated municipality.

This argument holds two important implications for the modeling of selective migration processes on the macro level. First, it is important that only relative changes in quality matter for changes in demographics, and more importantly changes that are relative in local terms. To assess the impact of a change in pollution, we also need to control for what is happening in adjacent areas. Second, and directly following the argument by Banzhaf and McCormick (2012) and Banzhaf and Walsh (2008), only a reordering in the quality rank system of local alternatives leads to unambiguous changes in the relative socioeconomic composition of neighborhoods. Thus, we employ two spatial modeling strategies in our study that are intended to capture these two arguments. First, we incorporate the characteristics of adjacent municipalities in spatial lag models and, second, we create a measure of the environmental quality rank among these adjacent municipalities to test if a reordering of environmental quality leads to demographic changes other than a mere marginal change compared to local alternatives.

A second issue for the identification of any effect of changes in environmental disamenities on the changes in income is the visibility or perception of environmental quality (Banzhaf et al. 2019b). For instance, Messer et al. (2006) showed that initial efforts of site cleanup (e.g., accompanied by construction works) might actually increase the risk perception of local residents and thus impose adverse effects on the desirability of a neighborhood. Former hosting areas may remain stigmatized and thus not experience an inflow of more affluent residents. Similarly, Currie et al. (2015) found a decrease in housing prices after the opening of new industrial plants, but no significant increase in prices after the closing of existing plants. This contrasts with the positive consequences due to site cleanup identified elsewhere (Gamper-Rabindran and Timmins 2011). However, the simple closing of a facility does not mean that the respective site has been repurposed or properly cleaned up, and a closed facility might still constitute a signal of low environmental quality. This implies that the impacts due to a reduction in objective environmental hazards are less clear than impacts due to an increase in environmental disamenities.

We tackle these two issues in the following way. First, in the main analysis we focus on the number of (high-polluting) industrial facilities rather than pollution itself. We assume this is a more important indicator for the subjective perception of environmental quality than objective health risks due to toxic pollution. Especially on the geographic level of municipalities, it seems unlikely that residents have an accurate estimate of actual health risks, as toxic pollutants are often very localized, colorless, and odorless. For instance, Currie et al. (2015) found that the negative effect of plant openings on the housing market was independent of the level of toxicity and the amount of emissions from the respective plants. Moreover, it seems more likely that residents would oppose the construction of a new industrial facility rather than marginal increases in emissions from already existing facilities. As shown in the online supplement, we repeated our analyses with the amount of toxicity-weighted emissions from the facilities (see Supplement S6). Second, we separate the effect of newly emerging and disappearing facilities: after performing an overall analysis, we use event time functions to estimate temporal changes in income after increases and decreases in the number of facilities separately. We cannot determine if a site was properly cleaned up or mainly remained as an abandoned brownfield site. We thus expect the effect of a site closing to be less clear than the effect of newly operating sites (Currie et al. 2015; Messer et al. 2006).

Data and Methods

To test selective siting and selective migration, we build on an original data set combining socioeconomic information from all German municipalities obtained from the INKAR database (BBSR 2019) with facility-specific pollution data of the E-PRTR.

The socioeconomic information is available for 4,455 municipalities annually between 2007 and 2017. On average, a municipality comprises 18,584 inhabitants (median = 8,976) and covers an area of 79 km^2. We use stable municipality borders as of December 31, 2017, for all years. The E-PRTR contains annual information about industrial facilities within Germany; it includes all facilities falling under the 65 E-PRTR economic activities (European Commission 2006:79ff.) and exceeding a pollutant-specific threshold of emissions (European Commission 2006:83ff.). Facilities are required to report their emissions and geographic location. We restrict the register to facilities reporting industrial or waste management activities, thereby excluding all agricultural facilities. We do so because agricultural establishments in Germany often consist of multiple smaller farms or facilities in rural settings, and thus are a weaker signal of environmental disamenities. From 2007 to 2017, the data contain a total of 6,570 unique industrial facilities with an average annual number of 4,472. To validate the appearance and closing of facilities, we use georeferenced land-use data from the Leibniz Institute of Ecological Urban and Regional Development (IOER) monitor (Meinel 2011). This data set provides annual information on the share of land used for industry and trade, using a 1-km × 1-km grid. By validating the facility register against land-use data, we ensure that the fluctuation of facilities over time is not driven by changing emissions around the reporting threshold.

Demographic Variables

To approximate the socioeconomic composition, we use the average income tax revenue per capita of each municipality. Additional analyses using a higher aggregation level (county) confirm that the income tax revenue is highly correlated with actual household income. From the INKAR database, we also derived a few time-varying control variables that we include in the main analyses. These are the proportion of inhabitants aged 18 or younger, the proportion of inhabitants aged 65 or older, population density, population density squared, and a proxy for the share of foreigners (approximated by the share of foreigners in the unemployment statistics, as this is the best annual data available in INKAR). Furthermore, we use the trade tax revenue per capita as linear and squared terms to account for the economic development of municipalities. If we find an effect of industrial facilities net of economic development, this indicates that something else (such as residential sorting between place of work and place of residence) contributes to the dissolution between the economic development and inhabitants' income.

Industrial Facilities

We measure environmental quality by the number of industrial facilities. Because our empirical models rely on changes over time, it is crucial to have a reliable measure for the appearance and disappearance of industrial facilities. Thus, we need to take into account that facilities may either newly appear in the E-PRTR register because they started to exceed the reporting threshold or disappear because they dropped below the threshold. This would artificially indicate the siting of new or closing of old facilities, even though the facility was present throughout the entire period. Thus, we validate the E-PRTR register against the IOER land-use data by (1) constructing the first and last operating year for each facility based on the first and last E-PRTR report and (2) assigning the industrial land-use share from 2006 to 2018 to each facility location. Subsequently, we counted a facility as a new industrial site only if the industrial land-use share increased (a) in the year before, (b) in the recent year, or (c) in the year after the first E-PRTR operation period. Similarly, we validated the closing of facilities by a decrease in industrial land use around the last period. If land-use development is either constant or contradicting the increase or decrease of E-PRTR facilities, we assume that the facility was there from the beginning (to the end, respectively) of the observation period (for more details, see online Supplement S4). This ensures that we capture only changes in the number of facilities if such changes coincide with physical changes in buildings or land use within the area. This validation is critical, because only 1,334 out of 2,617 (51%) new facilities coincide with an increase in industrial land use, and only 632 out of 1,829 (35%) disappearing facilities coincide with a reduction. Although the validation has little influence on the first set of results, it matters for the temporal impact functions, because with the raw data we start counting in years without any actual or recognizable change in many instances (see online Supplement S5 for results with raw data).

From this validated database, we then calculate the number of industrial facilities for each municipality and year using the geolocation of E-PRTR reports and municipality borders. To account for the possibility of facilities located at administrative borders, we use a method proposed by Banzhaf and Walsh (2008) to combine E-PRTR and municipality data: we create a 1-km buffer around each facility location and allocate the number of facilities to the municipalities weighted by the proportional overlap between the buffer and each municipality's area (see also Mohai and Saha 2006, 2007).

This matching strategy results in a data set of 4,455 municipalities per year containing demographics and the proportional number of industrial sites. Note that we exclude the first year (2007) from the analyses because the data show a relatively large increase in the number of facilities from 2007 to 2008, which is likely to occur because of an underreporting throughout the first year of data collection. Additionally, our validation strategy induces missing values for 2007: we cannot measure increases in the year before 2007, as IOER data are only available from 2006 on, thereby not allowing us to measure differences around 2007. In sum, this leaves us with a final data set of 44,550 observations nested within 4,455 municipalities. Summary statistics are presented in Table S1.1 (see the online supplement), and Figure 1 illustrates the spatial distribution of our main indicators for the year 2015. The map shows that the number of industrial facilities tends to be highest in the mid-west of Germany, while high-income communities are clustered in the south. Furthermore, income levels (and trends) differ strongly between former East Germany and West Germany, so we later stratify our analysis into these historical regions.

Modeling Relativity of Changes

We employ two spatial methods to measure relative changes in pollution and income. First, we apply spatial SLX models to incorporate the pollution changes in adjacent

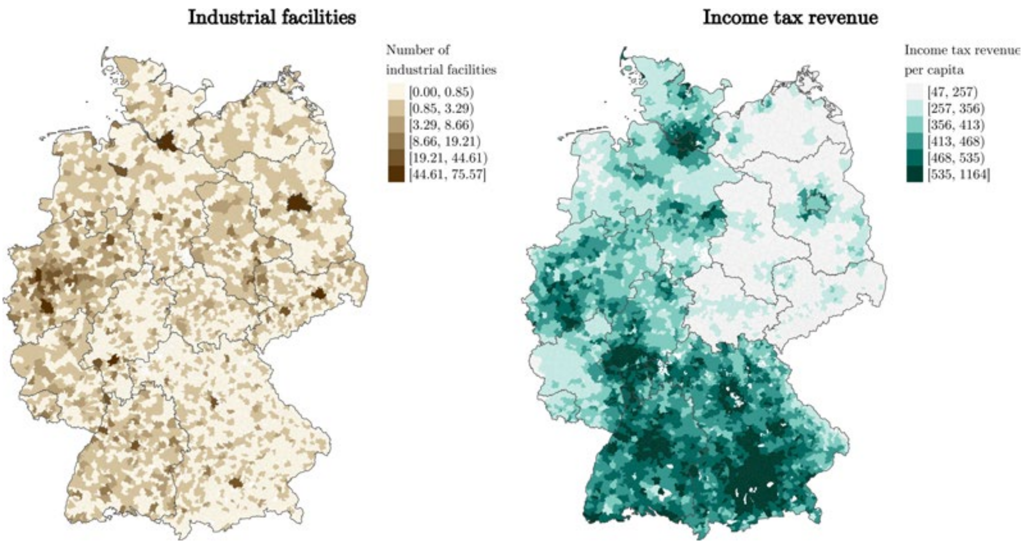

Fig. 1 Spatial distribution of industrial facilities and income tax revenue per municipality for 2015, Germany

municipalities (e.g., Halleck Vega and Elhorst 2015; Rüttenauer 2019b). We define a spatial weights matrix **W**, specifying all units as neighbors that share at least a common border or edge (row-normalized "Queens" neighbors). All elements within the $N \times N$ weights matrix are $w_{ij} > 0$ for all neighboring i and j, $w_{ij} = 0$ otherwise (for all $i \neq j$), and $w_{ii} = 0$. Subsequently, the SLX model allows us to account for the number of facilities in the focal and neighboring areas:

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{W}\mathbf{X}\boldsymbol{\theta} + \boldsymbol{\varepsilon},$$

where $\mathbf{y}$ is an NT vector of the dependent variable for $i = (1, \ldots, N)$ observations and $t = (1, \ldots, T)$ time periods per observation, $\mathbf{X}$ is an $NT \times K$ matrix of covariates $k = (1, \ldots, K)$, $\boldsymbol{\beta}$ and $\boldsymbol{\theta}$ are $K \times 1$ vectors of coefficients, and $\boldsymbol{\varepsilon}$ is an $NT \times 1$ vector of residuals.

In this model, **WX** represents the average values of the covariates in neighboring units. This means that we can estimate the effect of a change in **X** in the focal unit, while controlling for or keeping constant the **X** value of the neighboring units. Thus, changes in **X** constitute changes in **X** relative to neighboring units. It follows that we can estimate whether a change in the number of facilities (average income, respectively)—while keeping the average number of facilities (average income, respectively) in the local surrounding constant—affects the average income of a community (the number of facilities, respectively).

Still, this strategy does not capture if a municipality becomes "better" or "worse" than the neighboring alternatives. As outlined earlier, marginal relative changes might not unambiguously induce relative changes in the demographic composition (Banzhaf and McCormick 2012; Banzhaf and Walsh 2008). Thus, we apply a second strategy, shown in Figure 2, to account for the ordering of communities in a local "environmental quality rank system." For each municipality i and its neighbors as

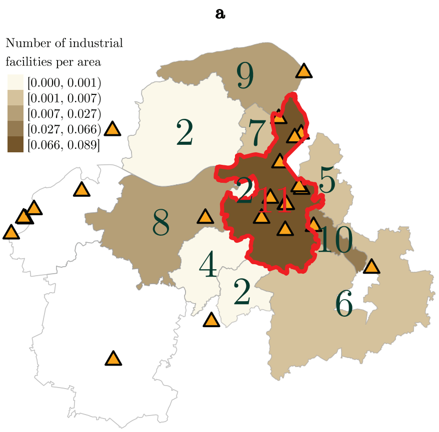

Fig. 2 Measurement of industrial facility rank for each focal community. Orange triangles represent the locations of facilities.

defined in $\mathbf{W}$, we use the number of facilities per km^2 to compute the rank in the system of local communities. For instance, in panel a of Figure 2, the focal unit (outlined in red) has 11 neighbors, and it holds the highest number of facilities per km^2 compared to its neighbors. Thus, the focal unit receives the rank of 11. This procedure is done for each unit i with its own local neighbors. For instance, the focal unit in panel b of Figure 2 does not receive the rank of 8, as measured in panel a, but rather the rank of 5, because this is its rank when comparing the community with all its adjacent units. The resulting measure has no substantial meaning in cross-sectional terms, as the absolute rank depends on the number of neighbors for each unit. Still, when measuring changes over time (see the following), this rank variable indicates if a reordering in the ranks of environmental quality occurred among local adjacent communities, which should induce unambiguous sorting processes.

Fixed-Effects Individual Slopes

To test selective siting and selective sorting, we employ panel data methods based on within-municipality variance only. More precisely, we want to estimate if changes in income in period $t - 1$ influence the number of facilities in period t (selective siting), and if changes in the number of industrial facilities in period $t - 1$ influence the average income in period t (selective migration).

Conventional two-way fixed-effects (FE) estimators rely on the assumption of parallel trends between municipalities receiving new facilities (or experiencing a decline) and those not experiencing a change, as observations without variance in facilities remain in the effective estimation sample as a "control group" for temporal shocks (Rüttenauer and Ludwig 2020). Still, different regimes of economic development likely lead to diverging trends in income and the number of facilities over time. For instance, more industrialized areas likely experience a steeper increase in facilities, and at the same time a slower increase in income, which is causally unrelated to the occurrence of new facilities. An Artificial Regression Test (ART) (Rüttenauer and

Ludwig 2020) confirms that the data exhibit substantial heterogeneity in the effect of trade tax revenue on income and that this heterogeneity is correlated with the municipality-specific variance in the number of facilities. The ART is an extension of the conventional Hausman test (of FE vs. random effects), indicating that the coefficient of interest changes significantly when relaxing the parallel trends assumption, thereby giving reason to believe that conventional FE estimates are biased.

To overcome the problem of nonparallel trends in "treated" and "untreated" municipalities, we use fixed-effects individual slopes estimators (Brüderl and Ludwig 2015; Rüttenauer and Ludwig 2020). FEIS accounts for municipality-specific time-constant differences *and* municipality-specific economic trends, which we measure using the trade tax revenue. The FEIS is given by

$$\tilde{\mathbf{y}} = \tilde{\mathbf{X}}\boldsymbol{\beta} + \tilde{\boldsymbol{\alpha}}_t + \tilde{\boldsymbol{\varepsilon}},$$

where $\tilde{\mathbf{y}}$, $\tilde{\mathbf{X}}$, and $\tilde{\boldsymbol{\varepsilon}}$ are individually "de-trended" data $\tilde{\mathbf{y}} = \mathbf{y} - \hat{\mathbf{y}}$, $\tilde{\mathbf{X}} = \mathbf{X} - \hat{\mathbf{X}}$, and $\tilde{\boldsymbol{\varepsilon}} = \boldsymbol{\varepsilon} - \hat{\boldsymbol{\varepsilon}}$, with $\hat{\mathbf{y}}$ and $\hat{\mathbf{X}}$ being stacked vectors of municipality-specific predicted values, and $\tilde{\boldsymbol{\alpha}}_t$ are residualized time fixed effects. For each municipality i, we estimate a municipality-specific trend in income, the number of industrial sites, and further controls ($\hat{\mathbf{y}}_i$ and $\hat{\mathbf{X}}_i$) on the basis of trade tax revenue and trade tax revenue squared. Subsequently, we subtract the predicted individual trend for each municipality from the original data and run a regression on the residualized data. We would obtain similar coefficients when interacting the municipality dummy variables in a least-squares dummy variable approach with trade tax revenue.

In terms of selective migration, $\boldsymbol{\beta}$ then indicates if an increase in facilities above the general trend influences income beyond the average income we would have expected based on each municipality's economic development. Relying on this residualized variance is then less likely to be driven by selection into "treatment" based on diverging trends and increases the confidence in a causal interpretation of the results. To identify an unbiased effect, we now rely on the assumption that deviations from a municipality-specific trend (which are likely influenced by unmeasured characteristics) rather than deviations from the municipality-specific mean are independent of the error term. Supplementary results based on conventional FE models (see online Figures S3.3 and S3.5) underpin this argument: conventional FE estimators return a significant "treatment effect" already in the year before "treatment," as the correlated trends are added to the effect of interest, thus producing larger effect sizes than the FEIS models in the main analysis. As in the conventional FE models, the FEIS estimator is based on municipalities exhibiting relevant within-variance, while we keep those without within-variance as a "control group" for exogenous time shocks by including time dummy variables (as in two-way FE). Obviously, we still rely on the strict exogeneity assumption of no time-varying unobserved confounders being correlated with our covariates net of included controls and municipality-specific economic trends (for more details, see Brüderl and Ludwig 2015; Rüttenauer and Ludwig 2020).

Results

Before analyzing the processes of environmental inequality, we first present results from a between model (comparing between cross-sectional units) to illustrate that

Table 1 SLX between estimator, regressing the number of industrial facilities on the income tax revenue in the focal and neighboring municipalities

	Overall		West Germany		East Germany	
	(1)	(2)	(3)	(4)	(5)	(6)
Income Tax Revenue	–0.409***	–0.175***	–0.439***	–0.224***	–0.080	0.024
	(0.052)	(0.044)	(0.054)	(0.046)	(0.172)	(0.138)
W Income Tax Revenue	0.440***	–0.101*	0.466***	–0.089†	0.712***	–0.010
	(0.056)	(0.051)	(0.061)	(0.054)	(0.209)	(0.177)
R^2	0.014	0.384	0.019	0.387	0.025	0.558
Adjusted R^2	0.014	0.382	0.019	0.384	0.023	0.550
No. of Observations	4,455	4,455	3,486	3,486	969	969

Notes: Figures are standardized coefficients, with standard errors shown in parentheses. W is the spatially lagged coefficient. Models 1, 3, and 5 are without controls, and models 2, 4, and 6 are with controls, which include percentage aged 18 or younger, percentage aged 65 or older, population density, population density squared, percentage of foreigners, trade tax revenue per capita, and trade tax revenue per capita squared (all additionally included as spatial lag).

†$p<.10$; *$p<.05$; ***$p<.001$

there is indeed a high correlation between the number of industrial facilities and the socioeconomic composition. All variables are scaled by the overall standard deviation. Full tables with results of control variables can be found in the online Supplement S7.

Table 1 shows a relatively strong and highly significant negative correlation between average income and the number of industrial sites in Germany. Controlling for additional demographic variables dramatically reduces the magnitude of this correlation, indicating that there are large-scale spatial differences (e.g., relating to economic and demographic characteristics). Furthermore, the overall correlation is mainly driven by former West Germany. In the region of former East Germany, correlations are weak and nonsignificant in the full model. This indicates that the issue of environmental inequality and income is less severe in East Germany, but is relatively strong in West Germany. To account for this regional difference, we conduct separate analyses in the following sections.

Selective Siting

Turning to the temporal processes, Table 2 tests the argument of selective siting. The dependent variable is the number of industrial facilities at t regressed on the socioeconomic composition at $t-1$. Following the argument of selective siting, we would expect a negative effect of income on the number of facilities.

When looking at the overall model, the estimates point in the expected direction: increases in income in the focal unit—while neighboring units remain unchanged—are associated with a decrease in the number of industrial facilities below the level we would expect based on the economic development of the respective municipality.

Table 2 Fixed-effects individual slopes (FEIS) estimator, regressing the number of industrial facilities on the lagged income tax revenue in the focal and neighboring municipalities

	Overall (1)	West Germany (2)	East Germany (3)
Income Tax Revenue$_{t-1}$	–0.012†	–0.009	–0.024
	(0.007)	(0.006)	(0.029)
W Income Tax Revenue$_{t-1}$	0.018†	0.011	0.092
	(0.009)	(0.009)	(0.063)
R^2	0.015	0.015	0.028
Adjusted R^2	0.015	0.014	0.026
No. of Observations	40,095	31,374	8,721
No. of Groups: ID	4,455	3,486	969

Notes: Figures are standardized coefficients, with cluster robust standard errors shown in parentheses. W is the spatially lagged coefficient. All models are with controls, which include percentage aged 18 or younger, percentage aged 65 or older, population density, population density squared, percentage of foreigners, and year dummy variables (except for year, all additionally included as spatial lag). Slopes for the FEIS: trade tax revenue per capita and trade tax revenue per capita squared.

†$p < .10$

Similarly, an increase in income within neighboring municipalities (as indicated by W) has a positive effect on the focal municipality. This resonates with the theoretical expectation that affluent surrounding communities tend to steer away industrial facilities, thereby increasing the pressure for the focal unit. Still, the effects are not statistically significant at the 5% level and are very weak in magnitude. Because of a one-standard-deviation increase in average income tax revenue (156 EUR per capita), the number of facilities is estimated to be 0.013 standard deviations (or 0.045 facilities) lower than expected in the following year.

Furthermore, when distinguishing between West and East Germany, it appears that a large part of the effect size stems from East Germany. Nevertheless, the precision of the point estimate is much lower in East Germany. The effect in West Germany is even smaller than the effect observed in the overall model and is not statistically significant. Altogether, the results provide very limited support for the idea that facilities are selectively sited in areas with a decreasing average income. At least during our observation period, selective siting does not contribute substantially to environmental inequality.

Selective Migration

An alternative explanation for environmental inequality is that households selectively move into and escape from polluted areas. In Table 3, we test the selective migration thesis by regressing the income tax revenue at time t on the number of industrial facilities at time $t-1$ in models 1–3, and on the relative facility rank compared to surrounding municipalities at $t-1$ in models 4–6.

Table 3 Fixed-effects individual slopes (FEIS) estimator, regressing the income tax revenue on the AWK facilities in the focal and neighboring municipalities and the relative neighborhood rank

	Overall (1)	West Germany (2)	East Germany (3)	Overall (4)	West Germany (5)	East Germany (6)
No. of Facilities$_{t-1}$	−0.052**	−0.080**	0.006			
	(0.020)	(0.026)	(0.015)			
W No. of Facilities$_{t-1}$	0.047	−0.033	0.213***			
	(0.044)	(0.063)	(0.046)			
Relative Rank$_{t-1}$				−0.027*	−0.031*	−0.010
				(0.011)	(0.015)	(0.009)
R^2	0.736	0.716	0.942	0.729	0.710	0.933
Adjusted R^2	0.736	0.716	0.942	0.729	0.710	0.933
No. of Observations	40,095	31,374	8,721	40,095	31,374	8,721
No. of Groups: ID	4,455	3,486	969	4,455	3,486	969

Notes: Figures are standardized coefficients, with cluster robust standard errors shown in parentheses. W is the spatially lagged coefficient. All models are with controls, which include percentage aged 18 or younger, percentage aged 65 or older, population density, population density squared, percentage of foreigners, and year dummy variables (except for year, all additionally included as spatial lag in models 1–3). Slopes for the FEIS: trade tax revenue per capita and trade tax revenue per capita squared.

*$p<.05$; **$p<.01$; ***$p<.001$

When first looking at the SLX specification in models 1–3, the number of industrial facilities exhibits a negative and significant effect on the average income in the following period. If the number of facilities increases by one standard deviation (or 3.46 facilities)—while keeping neighboring municipalities at a constant level—income tax revenue per capita is found to be 0.053 standard deviations (or 8.3 EUR) lower in the following year than we would have expected based on the municipality's economic trajectory. At the level of counties (for which income tax revenue and income data are available), this decrease in income tax revenue would translate to an approximately 20 EUR lower monthly gross income per person. This seems to be a small to moderate effect of the number of industrial facilities on the socioeconomic composition of the municipality. Still, this effect is significant and much stronger than the siting effect discussed earlier. Again, in the overall model, the spatial lag indicates a countervailing but nonsignificant effect due to industrial facilities in neighboring municipalities.

When separating by West and East, it appears that the effect of industrial facilities on subsequent income tax revenues is stronger in West Germany than in the overall model. In East Germany, in contrast, changes in industrial facilities exhibit a null effect on the income distribution within a municipality. Surprisingly, we observe a relatively strong effect of the spatial lag indicator. This might, however, result from different municipality sizes in East Germany. In total, the results support the selective migration thesis in West Germany: an increase in industrial disamenities above the expected trend leads to a decrease in income below what we would have expected based on the pure economic development of a municipality. The effect size is moderate but stronger than for the siting process. Results for East Germany, in contrast, are

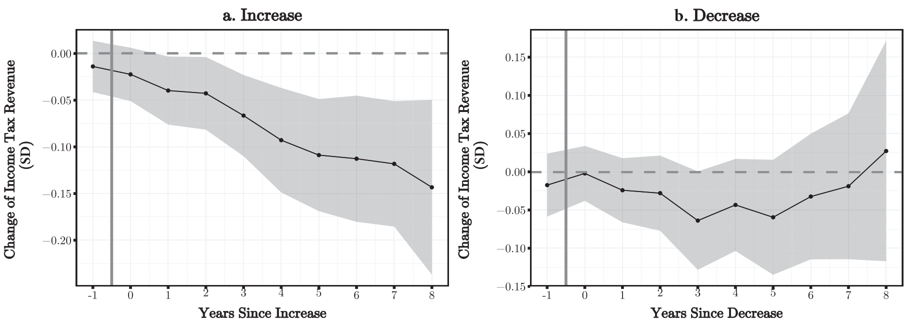

Fig. 3 Effect estimates of dichotomous shocks ($|x| \geq 0.9$) and time paths in West Germany based on FEIS SLX estimate, showing cluster robust 95% confidence intervals

less conclusive and point toward a null finding. This is consistent with the absence of a cross-sectional correlation between income tax revenue and industrial facilities in East Germany.

To gain further confidence in our results, models 4–6 repeat the same task, but use the measure of relative rank in the number of industrial facilities per area. If a municipality changes from a lower rank (fewer facilities per area than neighbors) to a higher rank (more facilities), this leads to a decrease in average income below the predicted level in the following period. The effect size is smaller than in the previous models and remains relatively stable in West Germany. Again, for East Germany, we observe only nonsignificant results. The negative impact in East Germany, as opposed to a slightly positive one in model 3, provides some support for the idea that only reordering processes lead to unambiguous demographic migration processes. Overall, this second measure of relative environmental quality fosters the previous conclusion: in West Germany, changes in the number of industrial facilities induce demographic sorting processes according to income, while this is not the case in East Germany.

Selective Migration Over Time

Although we see selective migration processes in West Germany, the magnitude of demographic changes is moderate. Still, we might underestimate the total effect, as residential sorting processes may get more severe after a temporal delay. Moreover, decreases in the number of facilities might have a lower impact than increases as we cannot identify if sites have been cleaned up or just closed. Therefore, we also estimated models using flexible event time functions, which start to count after an increase or decrease in the number of facilities. Results are shown in Figure 3 for West Germany and in Figure 4 for East Germany. The figures present averaged effects in FEIS SLX models of receiving a new industrial facility (panel a) or experiencing a reduction in facilities (panel b) between $t - 1$ and t, as indicated by the vertical line. The event time clock starts counting from the first instance of any increase or decrease observed in the data, thereby summing potentially accumulating new facilities into the later years. However, the results are robust to different specifications

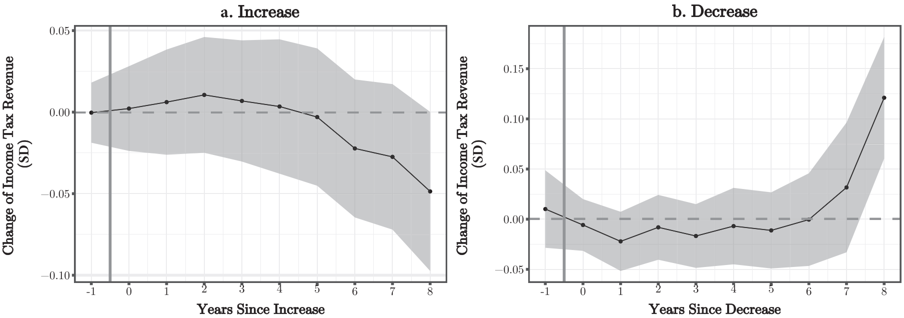

Fig. 4 Effect estimates of dichotomous shocks ($|x| \geq 0.9$) and time paths in East Germany based on FEIS SLX estimate, showing cluster robust 95% confidence intervals

because the number of municipalities with multiple increases and decreases is relatively small (15% and 5% of municipalities showing any within-change for increases and decreases, respectively). We use a threshold of ±0.9 facilities, as this comes close to an entire facility but allows for small overlaps of the 1-km buffer with neighboring communities.

In West Germany (see Figure 3), communities exhibit a continuously declining income during the years after receiving an industrial facility, in addition to the general trend (while controlling for increases and decreases in neighboring units). Five years after this increase in industrial sites relative to local neighbors, on average, hosting communities exhibit an income that is more than 0.1 standard deviations lower than we would have expected based on the respective economic development. The effect size due to an increase by at least 0.9 facilities is substantial: a new industrial facility is predicted to lower income tax revenue per capita by 15.6 EUR in year 5 after siting, which translates to an approximately 38 EUR lower monthly gross income per person. This temporal pattern is completely in line with the expectations based on selective migration or sorting theory, and documents an accumulating negative effect over a relatively short period of time.

Interestingly, we basically observe a null effect after a reduction in the number of facilities (panel b of Figure 3). If an industrial facility is closed, there is no reversal of the negative effect due to new facilities, that is, a significant increase in average income. This conforms to results from the United States indicating no positive effects on the housing market due to the closure of industrial plants (Currie et al. 2015). A reduction in the number of operating facilities does not necessarily mean that industrial sites are sufficiently cleaned up or repurposed, and it is not clear whether this decrease goes along with visible and recognizable changes in environmental quality, though obviously it goes along with a reduction in emissions. As has been argued earlier (Messer et al. 2006), areas around former industrial sites may remain stigmatized and thus not experience an inflow of wealthy households even following improvements in environmental quality.

Turning to East Germany (see Figure 4), we again observe a different picture. We do not find any influence due to an increase in facilities relative to local alternatives over time. Although the trend goes downward from year 2 on, the effect is weak and not significant. For decreases, we observe no influence on the average income either.

Though the last period shows a steep upward shock, this should be considered with caution, as the indicator for period 8 after the event is based on a very low number of cases. In general, these time patterns strengthen our previous conclusions: a continuously increasing sorting effect due to new sites in West Germany, but no sorting pattern in East Germany. Indeed, migration patterns may be different in East Germany, and thereby not exhibit the sorting of high-income households into cleaner areas and of the socioeconomically disadvantaged into more polluted areas. It is difficult to speculate about the reasons. Differences in infrastructure or the housing market might contribute to these diverging patterns in similar ways as general differences in economic conditions.

Supplement S2 in the online material presents results of the same modeling approach for the relative rank measure. In general, Figures S2.1 and S2.2 conform to the patterns presented here. The only difference is that at least within the first three years after a decline in the rank—which means an increase in relative environmental quality—we observe an increase in average income by trend (though not significant). This can be interpreted in favor of the argument by Banzhaf and McCormick (2012) and Banzhaf and Walsh (2008, 2013): only rank reordering processes unambiguously trigger selective migration processes. Still, in this case, the changes due to site closing are not statistically different from zero, which adds further confidence in the conclusions drawn earlier. Even "getting better" than neighbors does not significantly reattract affluent households beyond expectations based on the economic trends.

Discussion and Conclusions

The unequal distribution of environmental harms across society is a major dimension of social inequality given its severe impact on other domains of life (Colmer and Voorheis 2020; European Environment Agency 2019). In this study, we add new insights on the processes generating this unequal distribution by using spatially aggregated longitudinal data at the level of German municipalities. We account for potential explanations of diverging results between micro- and macro-level studies and model environmental changes in relation to changes in neighboring regions—the likely alternatives for residential choices. Moreover, we control for time-constant heterogeneity and selection on diverging economic trajectories using fixed-effects individual slopes estimators.

Our results found no support for the argument of selective siting. In former West and East Germany, we do not find significant effects of a community's socioeconomic composition on the number of industrial facilities. Although we observe a negative cross-sectional correlation between average income and the number of facilities in West Germany, within-estimators challenge the hypothesis of a causal link between income and the likelihood of receiving new industrial facilities. At least during our observation period (2008–2017), changes in income did not affect the placement or closing of industrial sites net of municipality-specific trends. This result conforms to previous results in the United States showing that other (infrastructural) characteristics are more important for the placement of new sites than demographic characteristics (Elliott and Frickel 2013, 2015). Of course, facilities might have been placed selectively in the past or may be placed selectively within municipalities. Future research should thus aim to conduct similar analyses with larger time frames and on a more fine-grained level within municipalities. However, in the recent decade and on the spatial scale employed here, selective siting does not substantially contribute to environmental inequality.

In West Germany, we find evidence for selective sorting or migration patterns. If a community experiences an increase in the number of sites while surrounding communities are kept at a constant level, the hosting municipality's average income drops in the following years. The magnitude is moderate in the first year after a change in environmental quality, but the disadvantage continuously accumulates over time. Within a period of five years, this disadvantage reaches a substantial size. In contrast, the closing of existing sites does not reattract affluent households. In the online supplement, we replicated our main analyses using the toxicity-weighted pollution instead of the number of facilities (see Supplement S6). Although low-income communities are exposed to higher levels of toxic emissions in between models, we do not find any evidence for selective sorting processes based on changes in toxic pollution. One reason for this finding might be that—at least on this spatial scale—industrial sites are a stronger and more visible sign of environmental quality than toxic pollution itself, which may be difficult to assess by residents (see also Currie et al. 2015). Still, sorting based on industrial sites likely contributes to the higher exposure of low-income municipalities to toxic pollution (see Table S6.7).

These results provide some practical implications for tackling environmental inequality. First, municipalities should be aware of the negative consequences on their economic returns attributable to population dynamics following an increase in industrial activities. Second, at least during our observation period, population dynamics play a more important role in environmental inequality than siting decisions. Successful policies should thus focus on reducing income inequalities and residential sorting mechanisms rather than on pure environmental zoning, which is directed toward industrial siting and likely to be counteracted by migration dynamics. Third, the simple closing of industrial facilities does not counterbalance the negative effect of new industrial sites. Shutting down facilities might be a less visible signal of environmental change, or former industrial areas may remain stigmatized, thereby not reattracting affluent households.

Our results also generate new questions for further research. First, studies could investigate in more detail which environmental cues influence individual perceptions of environmental quality and thus trigger residential sorting. The finding of sorting based on industrial sites but not on toxic emissions raises some doubt about the accuracy of individual perception of environmental risks. Second, the role of site cleanup should be investigated in more detail. It is important to know in which instances closing or cleanup of industrial sites triggers positive population changes, but also when these actions lead to environmental gentrification (Banzhaf et al. 2019b; Banzhaf and McCormick 2012), thereby potentially exerting greater pressure on low-income households. Indeed, our results indicate that the mere closing of potentially hazardous facilities does not significantly change the composition of the local population, and so may reduce inequalities in exposure to industrial emissions. Nevertheless, future research should compare different levels of cleanup to assess whether they may lead to more robust conclusions. Third, we find stark differences in the level of environmental inequality between West and East Germany, with industrial sites being more equally distributed in the latter. Indeed, we do not find evidence for selective migration processes in East Germany. The region of former East Germany experienced a different level of industrial development, and the average income is still below that of West Germany. However, the presence of a less pressured housing market and different infrastructural characteristics might also contribute to lower environmental inequality in the region.

Overall, we demonstrate the importance of selective sorting processes for the unequal distribution of environmental disamenities. The placement of industrial facilities leads to selective sorting processes and significantly changes the socioeconomic composition of an area, thereby steering less affluent households into areas closer to environmental hazards. Taking these negative demographic consequences and impacts on individual households into account can help to reduce social inequality and protect socioeconomically disadvantaged populations. ■

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