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Abstract

Using a natural experiment from Germany, we show that temporary place-based subsidies generate persistent effects on economic density. We identify employment and capital formation as main channels for higher income per square kilometer. As the spatial regression discontinuity design allows us to control for all spatially-continuous determinants of agglomeration (e.g. homemarket effects, knowledge spillovers), we attribute an important role to capital formation in explaining persistent spatial patterns of economic activity. However, estimates of externalities at the treatment border point to small net effects of the policy. We find strong evidence that pre-treatment land owners have benefitted substantially from the program and that transfers have shown larger effects in high-density places. Finally, accounting for regional subsidies raises the causal effect of market access for economic development as identified in Redding and Sturm (2008) by about 45 percent.

JEL-Code: H250, H400, H540, O150, O180, R120.

Keywords: place-based policy, regional policy, economic geography, persistence, regression discontinuity, locational advantage, land value capitalization.

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1 Introduction

When supporting underdeveloped regions, policy makers often hope that temporary transfers establish self-sustaining long-run economic development. The effort is substantial. For example, the EU dedicates about one third of its overall budget 2014-2020 to regional policy amounting to more than 350 billion euros (EU Commission, 2011). The US does not have a unified regional policy, but annual spending on regional development programs is estimated at about 95 billion US dollars per year (Kline and Moretti, 2014). Also China has installed regional policies that resemble those in the EU in terms of instruments and magnitude (EU Commission, 2010).

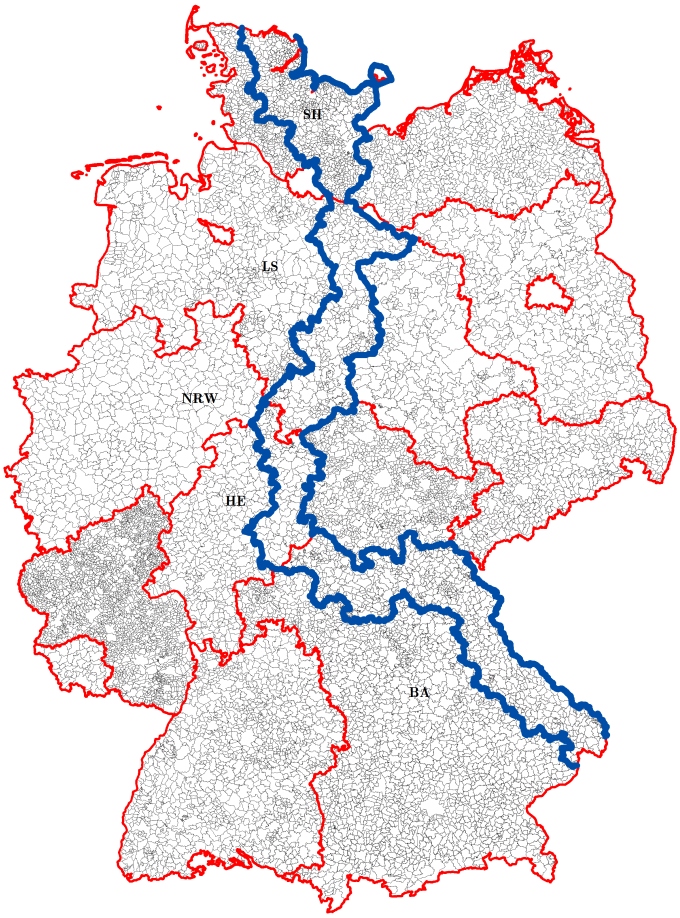
Despite these efforts, little is known about the long-term consequences of these programs and their underlying mechanisms (Neumark and Simpson, 2015).[1] Using a natural experiment from Germany allows us to make progress in this direction. We uncover the channels that create persistence and disentangle the role of capital accumulation from agglomeration economies. Second, we estimate (local) external effects of the policy to make statements about the net effect of the program. Third, we examine the distributional consequences by estimating the capitalization of transfers in land prices. Fourth, we reassess the importance of market access for economic development and look for heterogeneous treatment effects between low-density and high-density places.

In 1971, the West German government started a large scale transfer program to stimulate economic development in a well-defined geographical area adjacent to the Iron Curtain. All districts that accommodated either 50 percent of their area or population within a distance of 40 kilometers to the inner-German and Czechoslovakian border on January 1, 1971 became part of the *Zonenrandgebiet* (ZRG).[2] As shown in Figure 1, it stretched from the Danish border in the North to the Austrian border in the South running through four states (Bavaria, Hesse, Lower-Saxony, and Schleswig-Holstein). A major reason for this privileged treatment was to compensate firms and households close to the eastern border for being cut off adjacent markets on the other side of the Iron Curtain. The remoteness

[1] The literature on placed-based policies has mostly looked into the effects of transfers *during* programs, e.g. Busso, Gregory, and Kline (2013) evaluate the federal empowerment zones program in the US, Glaeser and Gottlieb (2008) examine the place-based policy Appalachian Regional Commission, Gobillon, Magnac, and Selod (2012) study the French enterprise zone program, Becker, Egger, and Ehrlich (2010, 2012) focus on income and employment effects of EU Structural Funds.

[2] See Deutscher Bundestag (1970), Drucksache VI/796 and Ziegler (1992, p.9). Zonenrandgebiet literally means area adjacent to the (Soviet occupation) zone, that became the German Democratic Republic. It was common in West Germany to refer to the German Democratic Republic as the “Zone”.

Figure 1: The German Zonenrandgebiet, 1971-1994



Note: The blue lines mark the western border of the ZRG and the Iron Curtain, respectively. The grey lines represent the municipalities according to the 1997 classification while the red lines define State borders. The border of the ZRG follows the administrative districts according to the 1971 classifications which was modified substantially in the mid-1970s. In most of our analysis we consider the states (Länder) Schleswig-Holstein (SH), Lower Saxony (LS), North Rhine-Westphalia (NRW), Hesse (HE), and Bavaria (BA).

was feared to cause substantial outmigration to the western parts of the country.[3] The program was not intended for a fixed number of years and its termination came as unexpectedly as German reunification. As transfers were redirected towards East Germany after 1990, the place-based policy was phased out until 1994.

[3]See Ziegler (1992) for a more detailed exposition.

The institutional setting of the ZRG gives rise to two types of discontinuities that we can use for identification of causal effects. First, we apply a spatial regression discontinuity design based on municipalities (and grid cells) in a close neighborhood on either side of the treatment border. If other relevant factors vary continuously at the ZRG-border, a discontinuity in economic activity at this border can be interpreted as the causal effect of the place-based policy. As the treatment border does not separate areas with different institutions, many concerns of other discontinuities that are important at country borders can be ruled out. Nevertheless, administrative borders are unlikely to be drawn randomly. To establish more credibility of our results, we also exploit the political rule which governed the location of the treatment border. As the treatment probability of districts jumps at a distance of 40 kilometers from the Iron Curtain, we apply a classical regression discontinuity design. The advantage is that the 40-kilometer rule does not coincide with any administrative boundary nor with geographic features that may cause discontinuities in relevant determinants for outcome. Depending on parametric or non-parametric estimation and the choice of the control function, we find that regional transfers led to higher income per square kilometer in the treatment area by about 30-50 percent in 1986. Undertaking the same exercise for 2010, that is 16 years after the program was eventually stopped, we find no evidence that the estimated effects have diminished.

To better understand underlying mechanisms of this outcome, we examine the program's effects on capital and labor at the treatment border. First, and foremost, transfers were dedicated to subsidize firm investments and to finance public infrastructure. We find strong and significant effects with respect to both private and public capital intensity. A second reason for higher economic activity could be migration of people into the treatment area. We find that the place-based policy led to a 20-40 percent higher population density both in the mid-1980s and in 2010. The response in employment is even higher indicating substantial commuting into the Zonenrandgebiet. However, we find no evidence that the educational composition of the workforce was affected.

There is a lively debate in the literature to what extent agglomeration economies play a role in shaping the spatial distribution of economic activity in the long run.[4] For example, Bleakley and Lin (2012) show that historical portage sites in the US serve as a good predictor for the location of cities today. As they do not find substantial differences in

[4]For syntheses of the theoretical literature on agglomeration economies see Duranton and Puga (2004) and Puga (2010).

the capital stock or capital intensity between portage and non-portage sites, Bleakley and Lin (2012) interpret their findings as evidence in favor of agglomeration economies as an important explanation for this path dependency rather than capital structures. Kline and Moretti (2014) analyze the effects of the Tennessee Valley Authority program showing that manufacturing employment continued to grow after transfers into this region were stopped in 1960. They argue that initial capital investments before 1960 are unlikely to be responsible for this long-run effect as the capital stock would have depreciated several decades later.[5]

Agglomeration economies cannot explain our findings. As long as these benefits of density (like labor-market pooling, technology spillovers, or the home-market effect, among others) dissipate continuously with distance, these determinants can be shown to cancel in our econometric approach. But why do firms have an incentive to maintain investment levels (and thus the discontinuity in the capital stock) even in the absence of subsidies? We suggest the following interpretation: Subsidies for private capital investment raised demand for premises and public infrastructure that local governments needed to provide. At the end of the treatment period, the Zonenrandgebiet was equipped not only with a larger capital stock (that would depreciate over time), but also with new industrial parks, roads, power networks, and sewage systems that generate a locational advantage in the long run. Although these investments also depreciate over time, the maintenance could be cheaper than planning new industrial parks elsewhere. What matters for firms, these investments reduce costs, provide premises to locate in the area in the first place or act as a coordination device. In this regard, our paper links to Davis and Weinstein (2002, 2008) attributing a key role to local structures in explaining the long-term spatial pattern of economic activity.

Despite these persistent discontinuities, it is not ensured that the policy generated new economic activity in the ZRG. As our identification strategy builds on municipalities that are close to each other, entrepreneurs outside the Zonenrandgebiet have an incentive to move their business into the treatment area. This incentive is *not* continuous at the border because entrepreneurs are not indifferent between locating one meter to the left or

[5]Schumann (2014) documents persistent effects of different levels of local population due to different settlement policies for refugees in the American and French occupation zones in Germany after World War II. Redding, Sturm, and Wolf (2011) regard the persistent relocation of the main German airport from Berlin to Frankfurt (initiated during the Cold War) as evidence for multiple spatial equilibria while Ahlfeldt, Maennig, and Richter (2014) find no persistent effect of urban renewal policies in Berlin.

to the right of the ZRG-border. Hence, the average treatment effect we are identifying could be driven by a relocation of economic activity from non-treated to treated regions leaving a zero net effect of the policy. In order to quantify these externalities, we follow approaches introduced by Rossi-Hansberg, Sarte, and Owens (2010) in the case of land prices and urban revitalization investments and Turner, Haughwout, and van der Klaauw (2014) in the case of land use regulation. We focus on units that did not directly benefit from regional transfers but were located in the proximity of the treated areas. Indeed, the evidence suggests that negative relocation externalities are prevalent and quantitatively important.

A further aspect of this paper deals with the beneficiaries of the regional subsidies. Policy makers often initiate place-based policies to raise wages and employment, in particular of poor households. However, there is wide-spread concern that regional transfers eventually capitalize in higher land rents (Glaeser and Gottlieb, 2008) such that the beneficiaries of the policy are those households that owned property *before* the program. Using detailed information on land prices our results confirm these concerns. We find that ZRG-transfers raised land rents by around 30 percent both in 1988 and 2010 which approximately offset the nominal per-capita income gain in the recipient regions.

Although the average treatment effect at the border cannot be explained by spatially-continuous agglomeration economies, this does not mean that they do not operate. As a further contribution, we analyze the interaction between market access and policy-induced locational advantage of ZRG municipalities. In their seminal paper, Redding and Sturm (2008) have shown that cities close to the Iron Curtain suffered from the lack of market access resulting in lower population growth rates of about one percentage point during the time of German division compared to cities located further away from the border. Although they acknowledge the relevance of regional transfers into the Zonenrandgebiet, Redding and Sturm (2008) are unable to explicitly control for this effect. As our findings point to a substantial role of federal subsidies for the location decision of firms and households, we re-estimate their regression with their (and our) data while accounting for regional transfers. We show that controlling for transfers leads to a market access effect that is about 45 percent higher. Finally, we take advantage of German reunification and EU-Enlargement in 2004 to show that access to markets in the East has led to a head start of municipalities in the former Zonenrandgebiet, arguably due to policy-generated locational advantage. This indicates higher returns of transfers in high-density places.

The paper is organized as follows. In the next section, we provide an overview of the historical and institutional background of the transfer program. In section 3, we discuss the data and descriptive statistics. Section 4 deals with contemporaneous and persistent average treatment effects of the place-based policy. We shed light on the net effect of the policy intervention in section 5 and study responses in land rents in section 6 to identify the incidence of regional transfers. Section 7 considers the role of ZRG-treatment for estimating the costs of remoteness as in Redding and Sturm (2008) and highlights interaction effects of exogenous expansions in market access with the policy. Section 8 concludes.

2 Historical background

As Germany's surrender in the Second World War became more likely, the Allied Forces started negotiations about the borders of post-war Germany and the division among the US, the UK, France and the Soviet Union in 1943. Different political ideologies caused growing tensions between the Western Allies and the Soviet Union and eventually led to the division of the country into the Federal Republic of Germany (West Germany) and the German Democratic Republic (East Germany). When the government in East Germany began to install fences and even a death strip at the inner-German border in 1952, passage of goods and people became impossible. Regular transit was only allowed between East and West Berlin until the erection of the Berlin Wall on August 13, 1961 finally closed this last loop hole for nearly 30 years.

While regional transfers in the 1950s targeted primarily former industrial centers that were heavily bombed during the war, politicians in West Germany also responded to the new situation of a divided state.[6] Districts at the inner-German border received support to prevent outmigration of residents and firms. This was a serious concern as the Iron Curtain deteriorated the living conditions for both psychological and economic reasons. At this point, West German policy makers widely regarded the division of Germany as a temporary phenomenon such that transfers were justified to preserve the economic position of the geographical center of pre-war Germany for the time after reunification.[7] Hence, politicians recognized the potentially long-lasting consequence of an event which was then

[6] See Karl (2008) for a more detailed review of regional policy in West Germany.
[7] Bundesministerium für innerdeutsche Beziehungen (1987).

still considered temporary. A further motivation for privileged treatment of the ZRG was geopolitical. An economically strong border region was expected to provide a better buffer against a potential attack of Warsaw Pact troops (Ziegler, 1992).

However, there was no clear rule yet for the allocation of resources. It was not until the late 1960s that the Federal Ministry of Economics suggested a better coordination of regional policy leading to two important laws in 1969: (i) the Joint Task "Improvement of the Regional Economic Structure" (Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur, GRW)[8] and (ii) the Investment Premium Law (Investititionszulagengesetz). While a politically-installed committee decided about the eligibility of regions to receive transfers, the Zonenrandgebiet was guaranteed privileged support by law (Zonenrandförderungsgesetz, 1971) within this framework. The federal law of 1971 provided a transparent definition of the ZRG that was never modified until ZRG treatment was eventually stopped in 1994: All districts that accommodated at least 50 percent of their area or population within 40 kilometers to the inner-German or Czechoslovakian border on January 1, 1971 became part of the Zonenrandgebiet.[9] It is remarkable that the ZRG boundaries were never modified despite substantial changes in district and municipality borders, especially in the mid-1970s. The ZRG program lost its status in 1994 when Germany was reunified and the focus of regional policy abruptly shifted to the development of the 'New Länder'.

The Zonenrandgebiet accounted for 18.6 percent of the West German territory accommodating 12.3 percent of the population (see Table A1). The ZRG transfer scheme comprised a menu of measures. A major focus was laid on subsidies for firm investment. Firms inside the Zonenrandgebiet could apply for investment subsidies of up to 25 percent. For initial investment, the total value of direct subsidies and tax deductions could even reach about 50 percent of the investment volume.[10] Further, firms were eligible for superior credit conditions of the public bank KfW (Kreditanstalt für Wiederaufbau), capital allowances were more generous, and there was a large program of public debt guarantees.

[8] See Eckey (2008) for a historical overview of the Joint Task.

[9] See Deutscher Bundestag (1970), Drucksache VI/796 and Ziegler (1992, p.9). According to a statement by state secretary Sauerborn, the 40-kilometer rule also included less needy regions, but was appealing for practical reasons in the first place (see Protocol of the 39th session of the cabinet committee of economics). It was recognized in the parliamentary debate on June 17, 1971 that the treatment border must remain fixed over time in order to rule out strategic modifications of local district borders (see Protocol of the 128th session of the Bundestag).

[10] See Ziegler (1992) and Zonenrandförderungsgesetz (ZRFG), 1971, available at http://dipbt.bundestag.de/dip21/btd/11/050/1105099.pdf (Anhang 3).

Moreover, companies located in the ZRG were treated with priority in public tendering. Beyond firm subsidies, a substantial share of the budget was dedicated towards public infrastructure projects and transfers could also be used for renovation of houses, investments in social housing, day care centers, education and cultural activities. This heterogeneity of measures makes it impossible to report a single money value of the ZRG program.

While the overall figure is unavailable, we do have data on certain parts of the ZRG program, especially the Joint Task and the Investment Premium Law. This allows us to document that the ZRG received the lion's share of the transfer budget. Between 1984 and 1987 about 60 and 85 percent of total public transfers in the states we consider was directed to the ZRG.[11] Note that data on tax deductions, the value of public tenders, and other monetary advantages that applied specifically to the ZRG are not available such that the treatment intensity of the ZRG was even higher than these numbers suggest. To get an idea about the overall size of the program, estimates range between 1.3-2.5 billion euros (at 2010 prices) per year in the 1980s which amounts to about 194-373 euros per capita.[12] This makes it comparable to the size of current EU Structural Funds amounting to about 230 euros per capita in regions with the highest transfer intensity (Becker, Egger, and Ehrlich, 2010).

3 Data

The basis of our empirical work is geographical and administrative data from municipalities and the exact location of the Zonenrandgebiet border. According to the precise definition of the ZRG, we georeference a map of West German districts in 1971 to identify the exact location of both the Iron Curtain (inner-German and Czechoslovakian border) and the ZRG border that separates the treatment from the control area.[13]

We use two different samples based on municipalities and districts, respectively (Table 1). This is required by the econometric approaches we introduce below. We consider the five states (Länder) that include or border the treated region: Schleswig-Holstein, Lower Saxony, North Rhine-Westphalia, Hesse, and Bavaria comprising in *total* 4,991 and 5,018

[11] Documentation of the Joint Task, Rahmenplan No. 13, available at www.bundestag.de.

[12] See Ziegler (1992) and Wirtschaftswoche, 1990 Nr. 99/4. About 6.7 million individuals were living in the ZRG in 1961.

[13] The map we use is provided by the former Bundesforschungsanstalt für Landeskunde und Raumforschung at a scale of 1:1,000,000.

Table 1: Observational units

	No. municipalities				No. districts	
	Total		Boundary sample			
	1986	2010	1986	2010	Total	Boundary sample
Non-ZRG	3,367	3,391	2,298	2,305	396	202
ZRG	1,573	1,576	1,572	1,576	106	107
Total	4,940	4,967	3,870	3,881	502	309

Notes: We consider the states States (Länder) Schleswig-Holstein, Lower Saxony, North Rhine-Westphalia, Hesse, and Bavaria. These five states comprise in total 4,991 and 5,018 populated municipalities in 1986 and 2010, respectively. We lose 51 municipalities due to partial treatment (i.e. ZRG border crosses the municipality) and imprecise assignment to municipal boundaries in the digital maps (see Appendix A for details). The boundary sample on the municipality level contains all municipalities with a distance to the ZRG border of less than 100km; the boundary sample on the district level includes all districts with $M_d \leq 150$ (see section 4.1). Districts are based on the 1969 classification, municipalities on the 1997 and 2010 classifications.

populated municipalities in 1986 and 2010, respectively. To georeference municipality data, we use digital maps (shape files) from the Bundesamt für Kartographie und Geodäsie. As they are only available since 1997, we assign each municipality to a district in 1971 and drop all observations where the municipality cannot be linked to a district with at least 90 percent of its area (20 municipalities or about 0.4 percent of the sample).[14] Moreover, we drop 31 municipalities due to partial treatment (i.e. ZRG-border crosses the municipality based on the 1997 or 2010 classification, see Appendix A for details). The *boundary sample* of municipalities contains all jurisdictions with a distance to the ZRG border of less than 100 kilometers. This includes all municipalities in the treated region and about 68 percent of the municipalities in the five states west of the ZRG border. For the boundary sample at the district level, we limit the observations to jurisdictions that are sufficiently close to the threshold determining transfer eligibility which will be described in detail below. This includes again all treated observations and about 50 percent of the districts outside the treated area and in the five states. Note that all our analyses are based on the 1971 district classification such that the number of districts remains constant over time.

This georeferencing provides us with relevant distance measures for each municipality and coordinates that we use as controls in several econometric specifications. We compile

[14]This may happen due to changes in administrative boundaries that were frequent especially in the 1970s. Note that all our results are robust to the exclusion of *all* municipalities that could not perfectly be assigned to a 1971 district.

a unique dataset on municipality characteristics between 1984 and 2010 and merge it with the information on location and district affiliation in 1971. Table 2 provides an overview of all outcome variables we use. While we can refer to the year 2010 for all variables in the analysis on long-run effects (*Persistent*), data availability is poorer for the treatment period 1971-1994 (*Contemporaneous*). Regional data at the municipality level are generally not available before 1975 for Germany with the exception of population. Further, as not all variables are available for each year, we take 1986 or the available date between 1984 and 1988, depending on the variable.

We use (taxable) nominal income as our main proxy of overall economic activity. Using the area of each municipality, we weight this measure by square kilometers. To obtain a better understanding of potential reasons for changes in *income per* km^2, we use data on population, employment, and human capital (share of residents with tertiary degree) to capture the labor side. To proxy capital stock responses, we distinguish between private and public capital. *public capital* measures the area share of a municipality covered by public infrastructure like streets, railway tracks, airports, seaports, public squares, or public buildings. Similarly, *private capital* represents the area share of a municipality covered by industrial parks, commercial buildings and residential homes. Relating commercial capital to total private structures (*industrial/private capital*) allows for insights into the relative importance of business activity versus residences. A further proxy for private capital stock is the *business tax base* which is defined homogeneously across all municipalities. We finally use information on *land prices* per square meter ("Bodenrichtwert") which is net of the value of built structures. In Germany, independent expert committees are commissioned by federal legislation to investigate at least biannually land values based on transactions data and adjust it according to structure quality, size, type, or layout. In contrast to common real estate offer prices these data refer to homogeneous transaction prices and do not require us to collect information on detailed property characteristics. Moreover, it is available at a fine spatial scale and for different types of land usages.[15] The data on land prices are provided by the states' expert committees and by *F&B* real estate consulting whereas all other data was supplied by the Statistical Offices of the Länder and the Federal Statistical Office (see Appendix A for more details on data sources).

[15]We focus on municipality level variation and averages across types while our results are robust to specific categories and within municipality variation.

Table 2: Descriptive statistics of outcome variables

	Contemporaneous				Persistent			
	Mean	Std.dev.	Obs.	Year	Mean	Std.dev.	Obs.	Year
area in km^2	33.042	33.378	3,870	1986	33.167	33.572	3,881	2010
income/km^2	1522.511	2965.65	3,870	1986	2848.013	4701.864	3,881	2010
population/km^2	160.083	238.896	3,870	1986	178.333	258.123	3,881	2010
employment/km^2	39.074	99.598	3,845	1986	48.798	119.153	3,672	2010
income/capita	24.716	11.010	3,870	1986	31.461	7.247	3,881	2010
human capital	0.028	0.024	1,782	1985	0.061	0.048	2,576	2010
public capital	0.044	0.020	3,856	1984	0.051	0.023	3,866	2010
private capital	0.048	0.048	3,856	1984	0.067	0.057	3,866	2010
industrial/private capital	0.094	0.083	1,315	1988	0.069	0.069	3,851	2010
business taxbase/km^2	7.606	23.929	3,709	1986	18.194	77.084	3,870	2010
business taxbase/employee	0.201	0.403	3,667	1986	0.425	0.626	3,642	2010
land price	26.197	21.016	982	1988	74.188	70.601	3,635	2010

Notes: We consider the states (Länder) Schleswig-Holstein, Lower Saxony, North Rhine-Westphalia, Hesse, and Bavaria. We restrict the sample to observations located within 100km of the ZRG border. *income* and *business taxbase* are measured in current 1,000 euros, *human capital* refers to the share of residents with tertiary education, *public capital* and *private capital* measure the area share of a municipality covered by public infrastructure (area used for streets, railway, airports, seaports, public squares, public buildings etc.) and private structures (industry, business, and housing), respectively. *industrial/private capital* refers to the ratio of industry and business structures in total private structures. Land prices correspond to current prices per m^2.

The average income per square kilometer in the boundary sample was about 1.5 million euros during the transfer program and it increased to about 2.8 million euros in 2010. Note that averages across municipalities deviate from the country average because cities receive the same weight as small municipalities. Municipalities are the smallest administrative units comprising on average about 33 square kilometers with a high standard deviation and a minimum of 0.45 square kilometers while the largest municipality stretches over 359 square kilometers. Accordingly, the municipal average of population density reaches about 160 (178 in 2010) individuals per square kilometer which is well below the German average of about 220. Likewise, per capita income and employment density average at relatively low levels of about 25,000 euros and 40 employees per square kilometer. Note also that 19 (7) municipalities exist in 1986 (2010) that have no employment and taxable business income such that they will be dropped when specifying these outcomes in logarithmic terms. Simple t-tests about the equivalence of the averages in the groups of transfer recipients and controls suggest for many variables significant differences across groups. For instance, income per square kilometer and population density are higher by about 10 and 27 percent in the control group than in the treatment group and these differences turn out significant at conventional levels. This points to the expectable selection issue and implies that an unconditional comparison may lead to false conclusions.

4 Effects on local economic activity

Regional policy usually targets very specific groups of recipients. For instance, these can be regions lagging behind in terms of economic performance, cities being confronted with a high degree of poverty and emigration, or firms lacking private funds. Hence, public subsidies are not distributed randomly impeding a causal evaluation of such programs. The major part of regional subsidies in Germany during the period we analyze was allocated to a well-defined area and according to a precise geographic rule. This unique program gives rise to two types of discontinuities that are the basis of most of our econometric exercises. First, we examine observations in a close neighborhood on either side of the treatment border. Provided that other regional characteristics vary smoothly in space, a discontinuous jump in the outcomes of interest at the ZRG border can be attributed to the place-based policy. This approach is referred to as *Spatial Discontinuity Design* or

Boundary Discontinuity Design (Holmes, 1998).[16] Second, we exploit a discontinuity in the political rule that governed the treatment eligibility of regions and allows for local randomization of transfer recipience.

4.1 Identification strategy

We denote by Y_{i0} and Y_{i1} the potential outcomes of a municipality i in the situations with and without transfers, respectively. Our aim is to identify the effect of a transfer T_i which corresponds to $\tau = Y_{i0} - Y_{i1}$. As counterfactual situations for individual units are unobservable, we aim at estimating an average treatment effect $E[\tau_i]$ for a group of comparable treated and control units. Our outset represents a special case of a two-dimensional RDD where the location of each municipality relative to the threshold is described by latitude and longitude, $\mathbf{L}_i = (L_{ix}, L_{iy})$. Similarly, the boundary between the treatment area $\mathcal{A}^t$ and the control area $\mathcal{A}^c$ consists of an infinite number of border points $\mathbf{b} = (b_x, b_y) \in \mathbf{B}$.

Due to the geographic nature of the policy measure, assignment to treatment is a discontinuous function of location, $T = 1\{\mathbf{L}_i \in \mathcal{A}^t\}$, where units east of $\mathbf{B}$ receive treatment while those to the west do not. In the spatial discontinuity design, location acts as the so-called forcing variable and we focus on the discontinuity of expected outcome at the geographical border:

$$\tau(\mathbf{b}) \equiv E[Y_{i1} - Y_{i0}|\mathbf{l} = \mathbf{b}] = \lim_{\mathbf{l}^t \to \mathbf{b}} E[Y_i|\mathbf{L_i} = \mathbf{l}^t] - \lim_{\mathbf{l}^c \to \mathbf{b}} E[Y_i|\mathbf{L}_i = \mathbf{l}^c], \tag{1}$$

where $\mathbf{l}^t \in \mathcal{A}^t$ and $\mathbf{l}^c \in \mathcal{A}^c$ refer to locations in treated and control areas, respectively. Accordingly, $\tau(\mathbf{b})$ identifies the average treatment effect at the border point $\mathbf{b}$. In contrast to a one-dimensional regression discontinuity design, our approach yields a function of treatment effects evaluated at each border point $\mathbf{b} \in \mathbf{B}$. For most of our analysis we consider the average treatment effect along the whole border while we explore variations in the treatment effects across locations for sensitivity checks as well as for the interactions with market-access variations.[17]

[16] Recent applications include Bayer, Ferreira, and McMillan (2007) focussing on school district boundaries to quantify the willingness to pay for a more educated neighborhood, Lalive (2008) identifying the effects of unemployment benefits on the duration of unemployment, Dell (2010) documenting the long-run impact of historical labor market institutions.

[17] See Papay, Willett, and Murnane (2011) for treatment effect heterogeneity in a two-dimensional RDD and non-geographic context. Importantly, this design allows us to limit the estimation to border segments

The identification strategy of a regression discontinuity rests on two comparably weak assumptions (see Hahn, Todd, and van der Klaauw, 2001). First, counterfactual outcomes $E[Y_{i0}|\mathbf{L}_i]$ and $E[Y_{i1}|\mathbf{L}_i]$ have to be continuous at the border, that is all relevant variables besides treatment must change smoothly. Second, selective sorting at the border must be ruled out to ensure that treatment is "as good as" randomly assigned (Lee and Lemieux, 2010). Hence, municipalities must not be able to (precisely) manipulate their location relative to the treatment border. Since the treatment effect in the geographic discontinuity design is identified for units converging to the boundary, we pursue robustness checks using information on land coverage and luminosity that vary at a very fine spatial scale (e.g. grid cells of $100m \times 100m$) around the border (see Appendix D).

The first assumption is fulfilled if the ZRG border was drawn randomly. However, there is reason to argue that administrative boundaries are usually not set at random, but follow some specific features such as rivers, mountains or cultural borders which may lead to discontinuities in other characteristics that matter for outcome. Common ways to address this issue include testing for discontinuities in relevant covariates (Dell, 2010) and removing border segments from the sample that seem to follow a problematic pattern (Black, 1999). While we pursue both robustness checks, we emphasize they are naturally limited in the sense that only a selection of covariates can be checked. Following this path, we thus cannot rule out a discontinuity in another relevant factor with certainty. We use two institutional features in our specific context to rebut these concerns. First, the ZRG border separates a set of 75 individual district pairs over a distance of 1,737 kilometers. These pairs may be divided according to historical routes, but there is no reason to expect that the ones in the treated area had systematically superior or inferior characteristics than the ones in the control area across *all* 75 pairs. Second, the district borders were modified substantially only a few years after the start of the ZRG-treatment whereas the ZRG border remained fully unchanged. Hence, the largest part of the ZRG border did not coincide with the relevant administrative district borders during the time we study.[18] To further improve confidence in our results, we will contrast the discontinuity at the threshold prior to the start of the program with the contemporaneous effects such that time invariant confounding discontinuities will cancel. Finally and most importantly, we will exploit the 40-kilometer rule that determined the actual treatment border, but did

where the identifying assumptions are likely to be met.

[18] Roughly 57 percent of the 1,737km ZRG border ceased to represent a district border already between 1971 and 1978.

not coincide with any administrative or geographical boundary.

The second identifying assumption requires that districts or municipalities cannot (or only imprecisely) select themselves into treatment. In practice this means that municipalities in the control area must not be able to receive transfers by merging with municipalities located inside the originally-defined ZRG or influence the location of the border. As $\mathcal{A}^t$ was never changed (despite changes in jurisdictional boundaries), this assumption is justified.[19] Note, however, that individuals and firms may choose their place of residence and thus sort across the border. This is exactly what we are interested in as it is the consequence of treatment. As in Dell (2010), migration across treated and control regions is one of the channels we study.

We implement the spatial RDD both in a parametric and in a non-parametric way. In the former case we state the conditional expectations in (1) as $E[Y_{i0}|\mathbf{L}_i] = \alpha + f(\mathbf{L}_i) + g_0(D_i)$ and $E[Y_{i1}|\mathbf{L}_i] = \alpha + \tau + f(\mathbf{L_i}) + g_1(D_i)$ where $f(\mathbf{L_i})$ represents flexible polynomials of geographic location and D_i refers to the shortest distance from i's centroid to the border ($\mathbf{B}$), i.e. the perpendicular to the closest border point. The inclusion of asymmetric distance control functions accounts for the possibility that proximity to the treatment border influences outcomes differently for transfer recipients and non-recipients.[20] Controlling for $\mathbf{L}_i$ may be important as units with the same distance to $\mathbf{B}$ may in fact be quite different if they are located in different parts of Germany (e.g. north versus south or distance to the sea, state/country borders). Thus, the regression model is given by:

$$Y_i = \alpha + g_0(D_i) + f(\mathbf{L_i}) + T_i[\tau + g_1(D_i) - g_0(D_i)] + \varepsilon_i. \tag{2}$$

Since $g_1(D_i) - g_0(D_i)$ converges to zero for observations close to the border, the average treatment effect is captured by $\hat{\tau}$. Since the credibility of the results rest on the correct specification of the control functions, we run alternative regressions with different functional forms (e.g. order of the polynomials), with and without coordinate control functions ($f(\mathbf{L_i})$), for different windows around the ZRG border, and we include border-segment fixed effects as well as state fixed effects.[21]

[19] Municipalities that were located outside the ZRG and merged with municipalities in the treatment area could not become eligible for transfers. In such cases the treatment border passes through the municipalities. The jurisdictional boundaries as of January 1, 1971 remained relevant for treatment throughout the duration of the program.

[20] By presuming symmetric functions on both sides of the RDD threshold, a kink may be misinterpreted as a discontinuity (see Lee and Lemieux, 2010).

[21] Limiting the sample to small windows around the threshold can substitute for including a higher order

Table 3: DISTANCE & ASSIGNMENT VARIABLE

	ZRG				Non-ZRG			
	Mean	Std.	Min	Max	Mean	Std.	Min	Max
Distance to **B** (D_i)	22,702	15,807	88	97,603	40,203	28,497	183	99,953
Distance to **G** (DG_i)	19,647	13,126	3	59,451	87,425	30,298	17,923	157,786
M_d	20.337	11.322	3	45	88.021	29.664	42	149

Notes: Distances are in meters and refer to municipality centroids. The assignment variable M_d is defined as the minimum distance (in km) from the Iron Curtain that includes the majority share of the district area. It is determined at the district level according to the 1971 classification. Each municipality is uniquely assigned to a district. Three districts (Schlüchtern, Einbeck, and Peine) were misassigned as they received treatment although not being eligible according to the rule. Of those districts being eligible all received treatment. We dropped all observations with a distance of more than 150km to the ZRG border and districts with $M_d > 150$.

The assumptions about the form of the geographic control functions can be further relaxed by estimating the treatment effect in a non-parametric way. To do so, we employ local linear regressions and estimate the conditional expectations at the border as stated in (1). Notice that we base our estimates for $E[Y_{i0}|\mathbf{L}_i = \mathbf{b}]$ and $E[Y_{i1}|\mathbf{L}_i = \mathbf{b}]$ only on observations in $\mathcal{A}^t$ and $\mathcal{A}^c$, respectively. As in the parametric approach, we may condition either on a one-dimensional forcing variable $D_{i\mathbf{b}}$ or on the location vector $\mathbf{L}_i$. In the former case, we estimate univariate local linear regressions for a set of 20 border points $\mathbf{b}^1, ..., \mathbf{b}^{20}$ which are allocated at equal distances along the border. The alternative approach follows Papay, Willett, and Murnane (2011) using a bivariate non-parametric regression with the arguments L_{ix} and L_{iy}. Due to the well-known curse of dimensionality bivariate local linear regressions require a much higher density of data. For this reason we favor the univariate non-parametric approach.[22] The corresponding results crucially depend on the choice of bandwidth. We derive the optimal bandwidth h^* according to the criterion suggested by Imbens and Kalyanaraman (2012) and use a triangular kernel (see Fan and Gijbels, 1996, and Imbens and Lemieux, 2008).[23]

Table 3 reports descriptive statistics on the distance of observations from the ZRG border **B** and from the Iron Curtain **G**. Although the treated area corresponds mostly to a narrow band of 40 kilometers there are treated observations in the north-east (in

control function but requires a sufficient density of observations.

[22]We check the sensitivity of our results with 10 and 30 border points. All our results are robust to the bivariate non-parametric regression approach. See Appendix B, Figure B1 for a more detailed description of the non-parametric specifications.

[23]Alternatively, we use cross-validation procedures and vary the bandwidth manually.

particular on the island Fehmarn) located at a distance of up to 100 kilometers from the ZRG border. The closest municipal centroids lie at about 88 and 183 meters from the ZRG border for the treated and control groups, respectively. The distances to the Iron Curtain **G** range from 3 meters to 158 kilometers. As an alternative to the centroids' distances from **B** and **G** – which can be very small with narrow municipalities – we approximate the location of a municipality by the average over a sufficiently large number of grid cells within the municipal boundaries. All our results are robust to this alternative.[24] Due to the nature of the transfer program the distance to the Iron Curtain is positively correlated with the distances to the ZRG border. However, as the location of **B** is determined by the districts' shape, size, and location the correlation between distance to **B** and **G** is only about 0.6 for the boundary sample and reduces to less than 0.05 when we limit the sample to a 20-kilometer window from **B**.

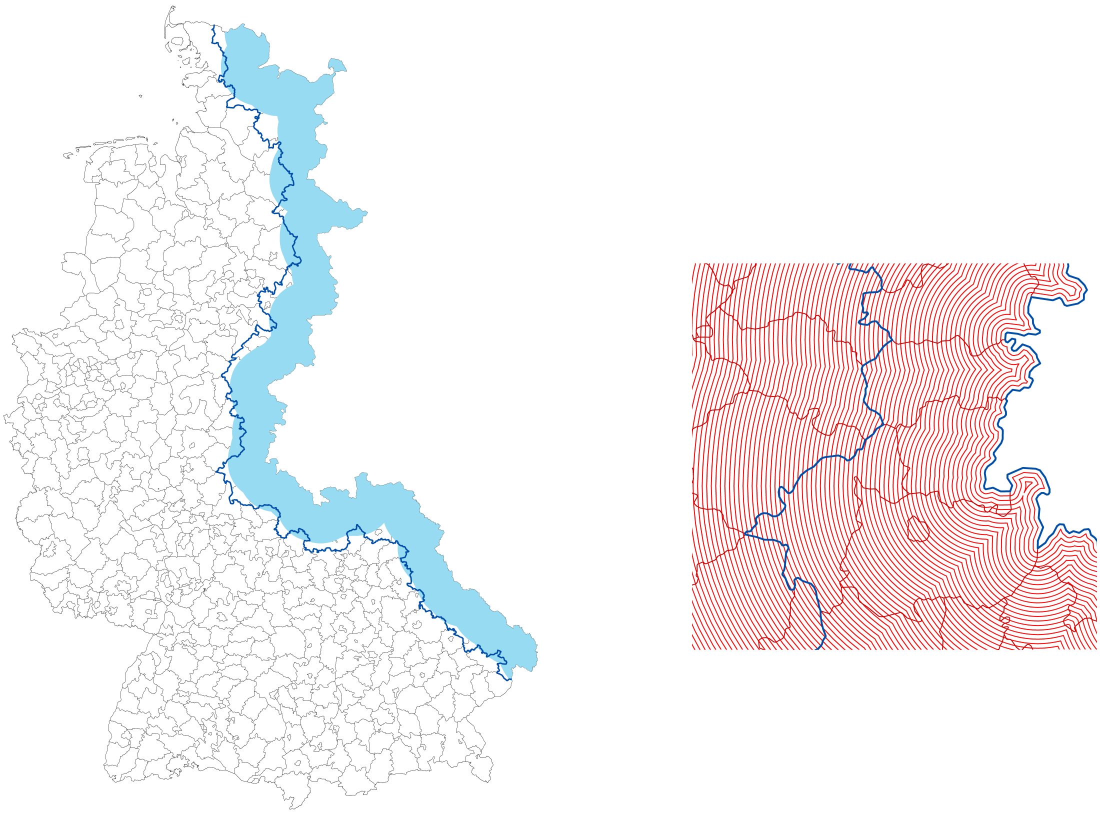
This points to an important advantage of our setting, namely the clear geographic criterion that defined the Zonenrandgebiet. Recall that those districts that accommodated either 50 percent of its area or population within a band of 40 kilometers to the Iron Curtain at the beginning of 1971 became part of the ZRG. The blue-shaded area in Panel A of Figure 2 illustrates the 40-kilometer buffer. It is evident that the ZRG border roughly follows the buffer, but we observe pixel and municipalities at the same distance from the Iron Curtain featuring a different treatment status. The political rule allows us to generate an assignment variable, denoted by M_d, indicating a district's minimum distance from the Iron Curtain that includes the majority share of the district's area. Hence, this assignment criterion does not only depend on a municipality's distance from the Iron Curtain but also on the shape of the superordinate district it belongs to. At $M_0 = 40$, we should expect a discontinuity in the probability of receiving treatment which we can exploit as exogenous variation to identify the causal effect of transfers on economic outcomes. As the 40-kilometer buffer has no natural relevance and does not correspond to administrative borders, it is uncritical to presume that there are no discontinuities in other relevant factors at M_0.

We compute isodistance-curves from the Iron Curtain using GIS software as illustrated in Panel B of Figure 2. This allows us to compute the area share of each district for each distance to the Iron Curtain. Finally, we determine for each district the minimum distance

[24] In this case we split each municipality into $100m \times 100m$ grids, determine longitude, latitude as well as distances from **B** and **G** for each grid cell and take the municipal averages across grid cells to obtain $g(D)$ and $f(\mathbf{L})$.

Figure 2: Assignment variable

Panel A. Panel B.

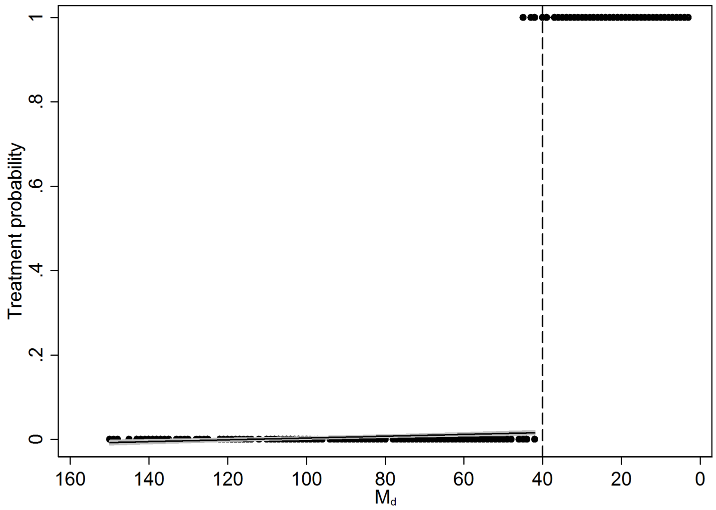


Notes: The above maps show district borders according to the 1971 classification. The light blue area in the left hand map marks the 40-kilometer distance from the Iron Curtain, the dark blue line refers to the ZRG border. The right hand map illustrates the buffer lines (in red) drawn in 1km intervals from the Iron Curtain.

buffer where the area share exceeds 50 percent. Table 3 reports descriptive statistics of M_d for the treatment and control groups.[25] Apparently, none of the control observations was eligible for treatment and all exceptions belong to the treatment group. If these exemptions from the 40-kilometer rule were not too frequent, we should observe a jump

[25] An alternative translation of the treatment rule would be to compute the area share of a district within the 40-kilometer buffer S_d. We did this as a robustness check and find a pronounced discontinuity at $S_d = 0.5$ as suggested by the rule. Yet, this assignment variable has the drawback of clustering at $S_d = 0$ and accordingly is less powerful.

Figure 3: Treatment probability



Notes: The assignment variable is measured at the district level. We consider only districts overlapping with a 150km buffer from the Iron Curtain. All districts further to the west are dropped from the sample.

in the probability of treatment at the threshold $M_0 = 40$:

$$P(T_{id}|\widetilde{M_d}) = \begin{cases} h_1(\widetilde{M_d}) & \text{if } \widetilde{M_d} \leq 0 \\ h_0(\widetilde{M_d}) & \text{if } \widetilde{M_d} > 0, \end{cases} \tag{3}$$

where $\widetilde{M_d} = M_d - M_0$ denotes the centered version of the assignment variable.

Figure 3 depicts the treatment indicator T_{id} against the assignment variable M_d. The discontinuity at 40 kilometers is evident, but the design is fuzzy because a few districts with $M_d > M_0$ still receive ZRG treatment. Overall, non-compliance is not a big issue because only three districts were "mis-assigned". This is most likely driven by the second criterion of the political rule concerning population share. More specifically, the non-compliers are those districts that did not accommodate 50 percent of the area within 40 kilometers to the eastern border, but 50 percent of the population.[26] Although we cannot account for

[26] We lack data about the population distribution within districts such that the second part of the rule may not be considered. Importantly, the rule requires only one of the criteria to be satisfied such that M_d suffices as an assignment variable in the spirit of a fuzzy RDD. Moreover, a precise measure of population distribution within districts was not even available at the time of treatment assignment and it turns out that all but three districts (Schlüchtern, Einbeck, and Peine) were assigned strictly according to the first part of the rule. Hence, we may also drop those three districts and proceed in the spirit of a sharp RDD which yields almost identical results and even smaller standard errors.

this second criterion due to data limitations, we can obtain consistent estimators of the treatment effect by exploiting the discontinuity in the probability. The average treatment effect in this case is given by the ratio between the jump in the outcome and the jump in the treatment probability at M_0 (see Lee and Lemieux, 2010 for details).

We estimate the fuzzy RDD in a parametric as well as in a non-parametric fashion. In the latter approach we estimate the conditional expectations of outcome and treatment probability by means of local linear regressions separately for observations with $\widetilde{M}_d > 0$ and those with $\widetilde{M}_d \leqslant 0$. We employ an edge kernel and follow Imbens and Kalyanaraman (2012) in choosing an optimal bandwidth h^* that minimizes the mean squared error of the average treatment effect.[27] The parametric approach follows a 2SLS approach where the regression equations are given by:

$$\begin{aligned} Y_{id} &= \alpha + f_0(\widetilde{M}_d) + T_{id}[\tau + f_1(\widetilde{M}_d) - f_0(\widetilde{M}_d)] + \varepsilon_{id}, \\ T_{id} &= \gamma + h_0(\widetilde{M}_d) + R_d[\delta + h_1(\widetilde{M}_d) - h_0(\widetilde{M}_d)] + \nu_{id}, \end{aligned} \tag{4}$$

where $R_d = 1[M_d \leqslant M_0]$ indicates eligibility. In what follows, we will generally use linear probability models in the first stage, but the results are very similar to those obtained with a nonlinear probability model in the first stage. Since the political rule is applied at the district level d, we correct the estimated variance-covariance matrix for clustering at the level of districts and for heteroskedasticity of arbitrary form. We limit the sample to observations belonging to districts characterized by $M_d \leq 150$.

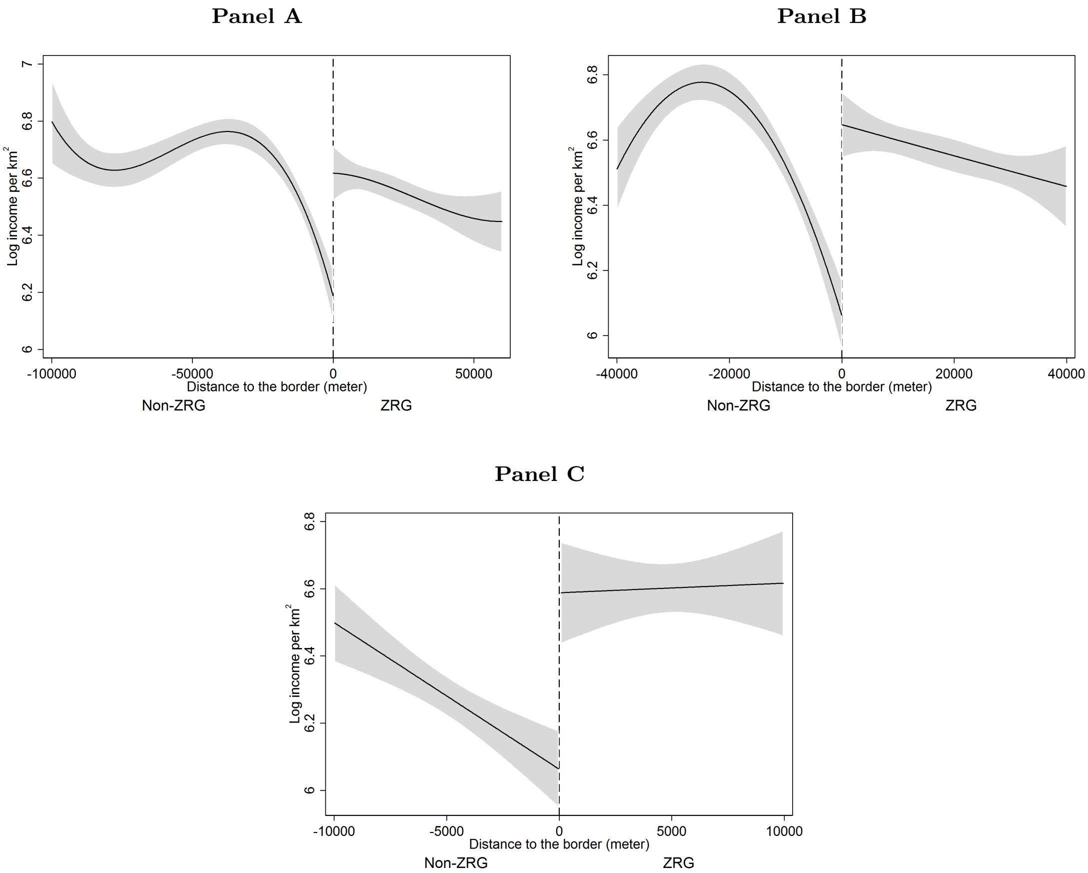
4.2 Results

4.2.1 Income per km^2

Before turning to regressions, a graphical illustration of the data at the ZRG border is instructive. In Figure 4, we plot our main measure of economic activity (log income per km^2) for the year 1986 as a function of distance to the ZRG border. Panels A-C use different windows and different control functions, but all reveal marked discontinuities at the ZRG border. Note that we assign positive and negative distances to the treatment

[27] As noted by Imbens and Kalyanaraman (2012) this procedure often leads to bandwidth choices that are similar to those based on the optimal bandwidth for estimation of only the differences in expected outcomes (and applying the same bandwidth to the expectations of treatment probabilities). This holds also true in our case.

Figure 4: Discontinuities in economic activity - Contemporaneous effect

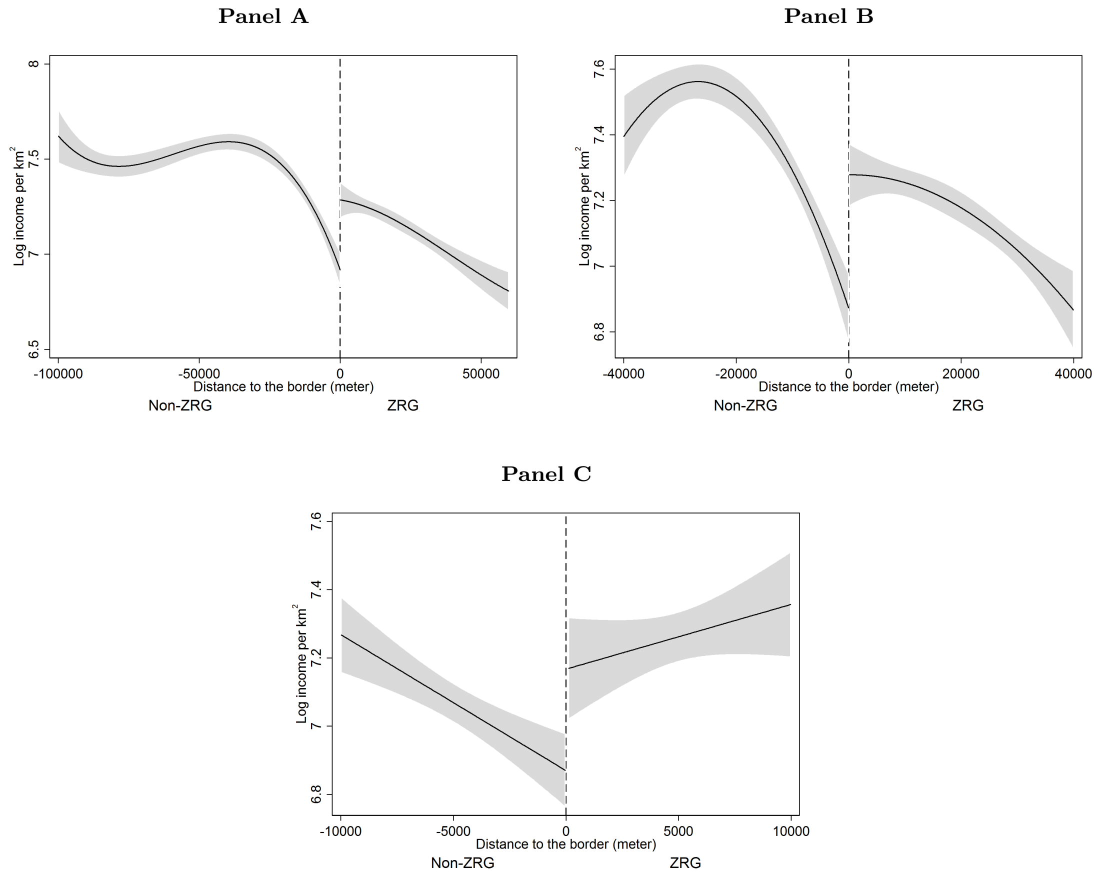


Note: We run separate regressions on each side of the threshold and control for latitude in a linear form. We use a 3rd-order polynomial distance control function for the 100km window (Panel A), a quadratic control function for the 40km window (Panel B), and a linear control function for the 10km window (Panel C). The grey-shaded area represents the 90-percent confidence interval.

and control region, respectively. By collapsing the two-dimensional location to a scalar measure of distance from **B** we cannot ensure that observations to the left and right of the threshold are de facto located in a short distance from each other. As the ZRG border runs more or less straight from the south to the north we can mitigate this shortcoming of the graphical analysis by controlling for a linear trend of latitude.[28] It is evident from Panel A that economic activity declines towards the Iron Curtain. This result is in line

[28] An alternative and qualitatively equivalent way of displaying the discontinuities would be to regress the variable in question on border-segment fixed effects and dummy variables for each bin where the coefficients on these distance dummies correspond to points on the polynomial fit displayed in our figures.

Figure 5: Discontinuities in economic activity - Persistent effect



Note: We run separate regressions on each side of the threshold and control for latitude in a linear form. We use a 3rd-order polynomial distance control function for the 100km window (Panel A), a quadratic control function for the 40km window (Panel B), and a linear control function for the 10km window (Panel C). The grey-shaded area represents the 90-percent confidence interval.

with Redding and Sturm (2008) who have shown that lack of market access causes lower population growth. As cities closer to the Iron Curtain are disproportionately affected by lack of market access, economic activity should be expected to be lower closer to the border. When we zoom in on small windows around **B** (Panels B and C) the general trend to the east gets blurred, but the discontinuity at the border of the transfer program becomes very pronounced. Further, it seems that transfers have shifted economic activity from the western (non-treated) side of the ZRG border to the eastern (treated) side. We will examine this potential externality more closely in section 5 below.

As the ZRG program was eventually stopped in 1994, we inspect whether the jump in economic activity is still observable at the former treatment border in 2010. Figure 5 contains the same panels as the figure on contemporaneous effects. Although the overall level in economic activity has increased over time and the slopes of the curves have changed, the discontinuities are still visible and the 90 percent confidence intervals do not overlap. While such graphical analyses provide a transparent first assessment of whether a discontinuity exists, they provide only limited information about statistical significance and the magnitude of the effects. We thus turn to regression analysis.

Starting with the spatial RDD, Table 4 confirms the first impressions from the plots: Regional transfers to the Zonenrandgebiet exerted a strong and significant effect on economic activity. We run three types of regressions. First, we include a distance control function using asymmetric 3rd- and 5th-order polynomials with segment and state fixed effects. We choose the polynomial orders on the basis of the AIC. Second, we directly control for the location of each municipality by including coordinates in addition to the Euclidean distance. Here we choose 2nd- and 3rd-order polynomials and add state fixed effects.[29] In each case we report robust standard errors as well as standard errors that correct for spatial dependence of unknown form using the method introduced by Conley (1999).Finally, we run non-parametric regressions where the optimal bandwidth h^* is computed according to Imbens and Kalyanaraman (2012) and varied manually for sensitivity analysis in columns (6)-(7).

Among the parametric regressions both the adjusted R^2 and the AIC suggest that the 5th-order and 3rd-order polynomials are preferred in case of the distance and coordinate control functions, respectively. However, the reduction in AIC is only marginal which indicates that there is no further gain to adding higher order terms. Coordinates capture location more precisely than distance from simple segment fixed effects such that we favor the specifications in columns (4) and (5). The latter refers to the non-parametric approach with optimal bandwidth h^* which requires less restrictive functional form assumptions. We find that income per km^2 is predicted to be about 30-50 percent higher than in the counterfactual without regional subsidies in 1986, depending on the specification. Moreover, we can reject the zero for all specifications at a confidence level of

[29]The cubic polynomial of latitude and longitude is defined as $L_{ix} + L_{iy} + L_{ix}^2 + L_{iy}^2 + L_{ix}^3 + L_{iy}^3 + L_{ix}L_{iy} + L_{ix}^2 L_{iy} + L_{ix}L_{iy}^2$. Note that we choose lower order polynomials for $f(.)$ than for $g(.)$ because the bivariate control function requires more parameters to be estimated than the corresponding univariate control function.

Table 4: Spatial RDD: Income per km^2

	Distance control		Coordinate control		Non-parametric		
	3rd	5th	2nd	3rd	h^*	$0.8 \times h^*$	$1.2 \times h^*$
Contemporaneous effect							
ATE	0.484***	0.583***	0.296***	0.528***	0.311***	0.552***	0.239***
	(0.099)	(0.147)	(0.079)	(0.099)	(0.079)	(0.099)	(0.069)
	[0.111]	[0.157]	[0.089]	[0.110]	-	-	-
R^2	0.16	0.17	0.14	0.16	-	-	-
AIC	10,750	10,732	10,869	10,741	-	-	-
Obs.	3,870	3,870	3,870	3,870	3,143	2,297	3,694
Persistent effect							
ATE	0.503***	0.542***	0.296***	0.535***	0.370***	0.518***	0.288***
	(0.095)	(0.142)	(0.076)	(0.095)	(0.077)	(0.097)	(0.067)
	[0.108]	[0.154]	[0.086]	[0.107]	-	-	-
R^2	0.21	0.22	0.20	0.22	-	-	-
AIC	10,454	10,438	10,541	10,404	-	-	-
Obs.	3,881	3,881	3,881	3,881	3,095	2,203	3,652

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis, Conley (1999) standard errors in squared brackets. We drop all observations outside a 100km window of the ZRG border in the parametric specifications. Columns (1)-(4) include state indicators, where (1) and (2) include segment fixed effects in addition. Columns (5)-(7) refer to non-parametric specifications where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008).

99 percent. The lower panel displays the corresponding specifications for the persistent effects of transfers measured in 2010. Notably, all specifications indicate again a positive and highly significant effect. Most importantly, these estimates remain remarkably similar for each type of specification in the two panels.

As we have argued before, we can identify causal effects of regional transfers under even weaker identifying assumptions by exploiting the discontinuity in the probability of receiving transfers at a distance of 40 kilometers from the ZRG-border. It can be virtually ruled out that the 40-kilometer threshold mattered for economic outcomes in the absence of the ZRG program such that this approach is unaffected by potential confounding factors. However, it comes at the cost of lower efficiency as treatment assignment is carried out on the district level. Columns (1) and (2) in Table 5 show regressions that use 2nd- and 3rd-order polynomials of M_d as control functions while columns (3)-(5) report non-parametric regression outcomes with the optimal bandwidth h^* and manual adjustments. Standard errors are generally clustered on the level of districts and we obtain qualitatively

Table 5: Fuzzy RDD: Income per km^2

	Parametric M_d		Non-parametric		
	2nd	3rd	h^*	$0.8 \times h^*$	$1.2 \times h^*$
Contemporaneous effect					
ATE	0.428**	0.482**	0.535***	0.476***	0.564***
	(0.198)	(0.199)	(0.087)	(0.098)	(0.082)
R^2	0.077	0.083	-	-	-
AIC	11,110	11,088	-	-	-
Obs.	3,875	3,875	2,143	1,617	2,581
Persistent effect					
ATE	0.435**	0.485**	0.360***	0.255***	0.424***
	(0.207)	(0.211)	(0.076)	(0.079)	(0.073)
R^2	0.134	0.139	-	-	-
AIC	10,793	10,773	-	-	-
Obs.	3,885	3,885	2,874	2,664	3,041

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors clustered at the district level in parenthesis. Observations with $M_d > 150$ are dropped from the sample. Columns (1) and (2) refer to fuzzy RDD specifications using a two-stage instrumental variables procedure and include state indicators. Note that the instrument is highly relevant in each of the first stages. Specifications (3)-(5) refer to non-parametric specification where we compute the bandwidth h^* according the algorithm introduced by Imbens and Kalyanaraman (2012). Standard errors in columns (3)-(5) are computed according to Imbens and Lemieux (2008).

identical results if we estimate the specifications on a sample collapsed by districts. Note that the non-parametric and contemporaneous estimate increases somewhat compared to the spatial RDD, but the overall picture shows very similar results when comparing the estimates to the corresponding coefficients in the spatial RDD in Table 4. This establishes confidence in the consistent estimation of the treatment effect. Notice that all specifications yield highly significant treatment effects at conventional levels.

Talking about economic magnitude, the effects might appear fairly high at first sight, but need to be qualified in at least two respects. First, the predicted average treatment effect in 1986 is the consequence of subsidies since 1971. As we have documented in section 2, transfers to the Zonenrandgebiet have been quite substantial every year. Second, it is quite plausible that these estimates include negative externalities of shifting activity from the control area to the treatment area, so these estimates must not be interpreted as *new* economic activity generated by the place-based policy. However, we argue that the estimates reflect the total causal effect of transfers into the Zonenrandgebiet on the spatial equilibrium. We provide a more detailed discussion of results in subsection 4.2.3 below.

Table 6: Pre-treatment – 1961

Income per km^2	Parametric M_d 2nd	3rd	Non-parametric h^*	$0.8 \times h^*$	$1.2 \times h^*$
ATE	0.023	0.008	-0.186	-0.297	-0.189
	(0.440)	(0.443)	(0.601)	(0.737)	(0.541)
R^2	0.350	0.352	-	-	-
AIC	1,040	1,041	-	-	-
Obs.	309	309	176	141	193

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. Regressions are based on district level data. Observations with $M_d > 150$ are dropped from the sample. Columns (1) and (2) refer to fuzzy RDD specifications using a two-stage instrumental variables procedure and include state indicators. Note that the instrument is highly relevant in each of the first stages. Specifications (3)-(5) refer to non-parametric specification where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008).

Although we have discussed identifying assumptions and their plausibility in this context in detail, a straightforward placebo test is to check whether there was a discontinuity in economic activity prior to treatment. Unfortunately, income data is unavailable at the municipality level before 1975, so we take GDP data at the more aggregated district level. Using estimates for 1961, it is immediate from Table 6 that none of the specifications reveal higher economic activity in the Zonenrandgebiet that was established only ten years later. The point estimates are positive in the parametric and negative in the non-parametric specifications, but all of the estimates are far from being statistically significant. Further, we use pre-treatment information about population density which is available at the municipality level and confirms that there are no discontinuities at the ZRG border prior to the start of the program (see Figure 6).

4.2.2 Economic channels

What are the underlying channels of higher economic activity in the Zonenrandgebiet? We study a number of potential mechanisms through which regional transfers might operate. Most obviously, as transfers were primarily targeted to subsidize firm investments and public infrastructure, we employ data on private and public capital to explore whether discontinuities prevail. The German Statistical Office provides information about the area share covered by plants, residential structures, or roads and railways. We also use the business tax base as an alternative proxy for the private capital stock.

Table 7: Channels I: Capital

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	2nd	3rd	h^*	2nd	3rd	h^*
			Business tax base per km^2			
ATE	0.366***	0.720***	0.652***	0.463***	0.848***	0.800***
	(0.124)	(0.157)	(0.148)	(0.114)	(0.144)	(0.142)
R^2	0.17	0.19	-	0.18	0.20	-
AIC	12,795	12,718	-	13,244	13,161	-
Obs.	3,533	3,533	2,318	3,792	3,792	2,299
			Private capital stock			
ATE	0.197***	0.341***	0.291***	0.193***	0.298***	0.278***
	(0.062)	(0.078)	(0.070)	(0.058)	(0.073)	(0.066)
R^2	0.11	0.13	-	0.07	0.08	-
AIC	8,895	8,830	-	8,420	8,369	-
Obs.	3,845	3,845	2,730	3,851	3,851	2,839
			Industrial/private capital stock			
ATE	0.113	0.346*	0.581**	0.134*	0.179*	0.174**
	(0.139)	(0.181)	(0.230)	(0.076)	(0.098)	(0.079)
R^2	0.10	0.11	-	0.08	0.09	-
AIC	3,074	3,068	-	7,982	7,966	-
Obs.	1,259	1,259	316	3,230	3,230	2,507
			Public capital stock			
ATE	0.147***	0.111***	0.153***	0.172***	0.138***	0.207***
	(0.032)	(0.040)	(0.039)	(0.032)	(0.040)	(0.040)
R^2	0.27	0.27	-	0.25	0.26	-
AIC	3,885	3,851	-	3,760	3,718	-
Obs.	3,855	3,855	2,433	3,865	3,865	2,312

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. We drop all observations outside a 100km window of the ZRG border in the parametric specifications. Columns (1)-(2) and (4)-(5) refer to parametric specifications and include state indicators. Columns (3) and (6) refer to non-parametric specifications where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008). *Business tax base per km^2* and *Business tax base per employee* are measured in logarithmic terms. The three measures of capital stocks are bounded between zero and unity and which renders estimating linear models inappropriate. Thus we apply a logit transformation to *public capital*, *private capital* and *industrial/private capital*.

A second reason for higher economic activity per square kilometer could be changes in population and employment. Investment subsidies may also raise labor demand and labor productivity (arguably through higher capital stock) affecting the migration decision of households. Furthermore, the ZRG program also supported renovation of private homes, social housing, and cultural activities rendering living in the treatment area more appealing. Finally, we explore whether the human capital of the workforce differs systematically between the treatment and the control area.

For the sake of brevity, we show results from the spatial RDD stressing that the findings are robust to using fuzzy RDD.[30] Table 7 summarizes contemporaneous and persistent effects of the transfer program on capital. We only report 2nd- and 3rd- order polynomials of the augmented coordinate control specifications and non-parametric regressions based on the optimal bandwidth h^*. The estimates suggest that the ZRG treatment has led to a markedly higher stock of both private and public capital. For example, the business tax base per square kilometer is predicted to be around 60-70 percent higher in 1986. Looking at persistence in 2010, we still find a highly significant effect at an even higher level of around 80 percent. Taking the area share covered by plants and residential structures, our estimates suggest that transfers have raised the capital stock by about 30 percent both in 1984 and 2010. Distinguishing between industrial and residential struc-tures, we observe that ZRG treatment led to a higher increase in industrial premises. The public capital stock is predicted to be about 10-20 percent higher compared to the counterfactual.

Turning to labor as a potential channel for higher economic activity and using the same specifications as above, Table 8 reveals that population density was raised by about 40 percent with no indication of a decline in the long term. Econometrically speaking, commuting is costless at the ZRG-border so the change in population can only be attributed to subsidies for social housing and renovation of private residences. The discontinuity in employment per square kilometer is even more pronounced at about 60-70 percent indicating substantial commuting into the Zonenrandgebiet. However, we find no evidence that the composition of the workforce with respect to skills was affected by treatment. The share of high-skilled employees in the Zonenrandgebiet does not differ from the counterfactual scenario without transfers.

It is informative to take a closer look at how the magnitude of effects has developed over time. As we have argued in the previous subsection, GDP is only available at the district

[30]Results from the fuzzy RDD can be obtained from the authors upon request.

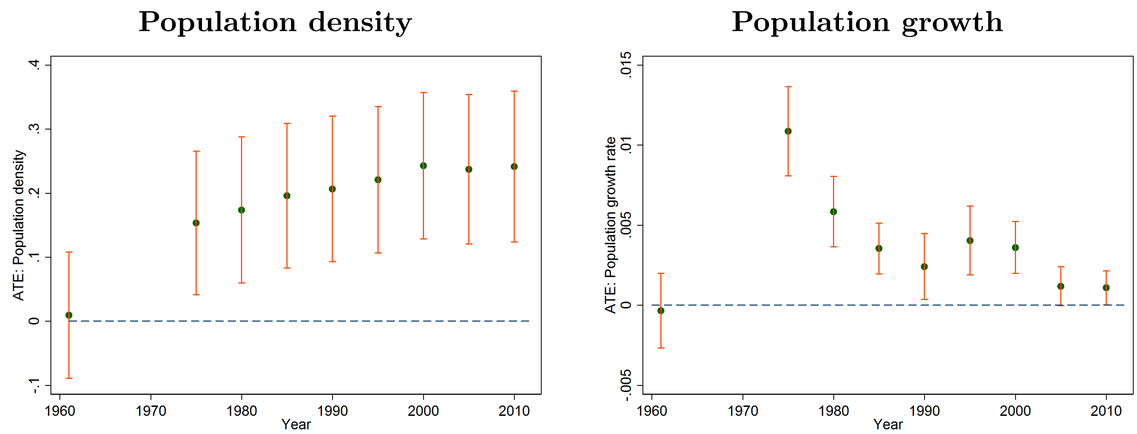
Table 8: Channels II: Labor

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	2nd	3rd	h^*	2nd	3rd	h^*
			Population per km^2			
ATE	0.239***	0.434***	0.372***	0.290***	0.473***	0.425***
	(0.069)	(0.087)	(0.077)	(0.071)	(0.089)	(0.079)
R^2	0.19	0.21	-	0.18	0.21	-
AIC	9,846	9,746	-	9,988	9,876	-
Obs.	3,870	3,870	2,745	3,881	3,881	2,717
			Employment per km^2			
ATE	0.418***	0.658***	0.692***	0.467***	0.723***	0.741***
	(0.108)	(0.137)	(0.124)	(0.110)	(0.140)	(0.133)
R^2	0.18	0.20	-	0.16	0.17	-
AIC	13,120	13,052	-	12,407	12,346	-
Obs.	3,826	3,826	2,601	3,665	3,665	2,269
			Human capital			
ATE	0.016	0.213*	0.113	-0.076	0.116	-0.054
	(0.082)	(0.109)	(0.079)	(0.071)	(0.092)	(0.074)
R^2	0.13	0.14	-	0.12	0.14	-
AIC	3,555	3,541	-	5,372	5,337	-
Obs.	1,782	1,782	1,373	2,576	2,576	1,886

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. We drop all observations outside a 100km window of the ZRG border in the parametric specifications. Columns (1)-(2) and (4)-(5) refer to parametric specifications and include state indicators. Columns (3) and (6) refer to non-parametric specifications where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008). *Population per km^2*, *Employment per km^2*, and *Income per capita* are measured in logarithmic terms. *Human capital* is bounded between zero and unity and which renders estimating linear models inappropriate, thus we apply a logit transformation.

level and at fewer intervals than population data. Since we have found significant and large effects of ZRG transfers on population density, we run the specification with coordinate control functions for a number of years between 1961 and 2010. Panel A in Figure 6 reveals differences in population densities between the Zonenrandgebiet and the control area, while Panel B plots differences in population growth rates for several periods. Note that the points and bars illustrate the point estimates and confidence bands of parametric specifications according to (2). These outcomes shed light on how transfers unfolded their effects over time. Several interesting observations stand out. First, prior to treatment

Figure 6: DYNAMICS



Note: The outcomes are based on spatial RDD with 2nd-order asymmetric coordinate control functions, 100km boundary sample, and – depending on data availability – between 3,523 (in 1950) and 3,881 (in 2010) municipalities per year. The vertical lines mark 90-percent confidence intervals. Average annual growth rates are computed for the intervals 1950-1961, 1961-1975, and from then onwards in five-year intervals. The point estimates suggest a 15-percent higher population density in the treated regions by 1975 which is well in line with the estimated increase in annualized population growth between 1961-1975 of about 1 percent.

there is neither a significant difference in population density nor in population growth. This finding is in line with the insights from Table 6 that there was no discontinuity in GDP per square kilometer in 1961. Second, with the introduction of the transfer scheme the difference in population density developed rather quickly over the first five to ten years while the difference in population growth steadily declined afterwards reaching an almost equal level across treated and control units until the end of the 1980s. Third, there is a surge in relative population growth in the decade after reunification in 1990. This points to an interaction of access to markets in the East with locational advantages due to public investments in the Zonenrandgebiet. We examine this aspect more closely in section 7.2 below.

4.2.3 Discussion: What explains persistence?

We have shown that transfers to the Zonenrandgebiet led to a *persistent* increase in income per square kilometer in the target region, channelled through higher capital stock, population and employment. This outcome can be interpreted as causal if the former ZRG-border does not exhibit any other discontinuity ex post. For example, one can think

Table 9: Other policies

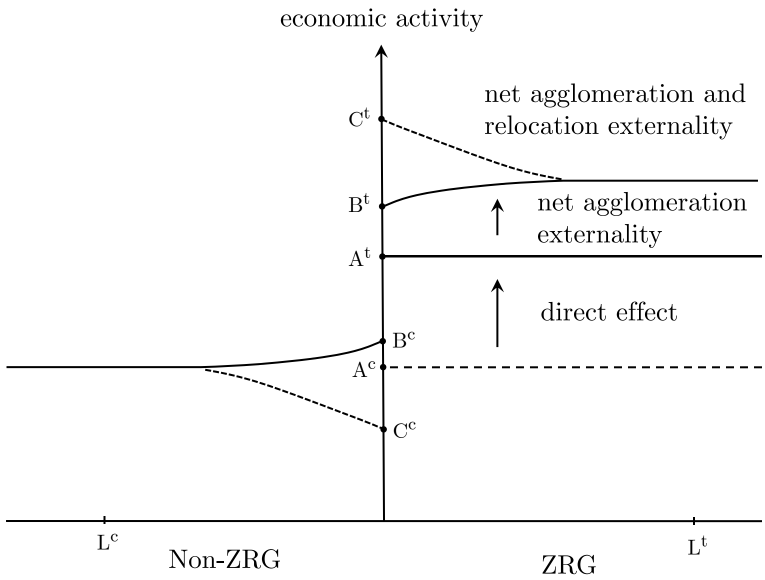
	Local business tax rates (2010)			Federal transfers per capita 1994-2010		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	2nd	3rd	h^*	2nd	3rd	h^*
ATE	0.011*	-0.001	0.012	45.177	163.825	6.219
	(0.007)	(0.008)	(0.007)	(159.865)	(202.927)	(33.509)
R^2	0.30	0.31	-	0.01	0.04	-
AIC	-8,433	-8,450	-	69,799	69,795	-
Obs.	3,881	3,881	2,783	3,870	3,870	1,168

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. We drop all observations outside a 100km window of the ZRG border in the parametric specifications. Columns (1)-(2) and (4)-(5) refer to parametric specifications and include state indicators. Columns (3) and (6) refer to non-parametric specifications where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008). We apply a logit transformation to *business tax rates* which are bounded between zero and unity. Federal transfers are aggregated over the years 1994-2010 and divided by the municipal population in 1994. We use the absolute level of *Federal transfers per capita* (instead of logs) in order to account for zeros. These results are robust to a zero-inflated Poisson model that accounts for the relatively high number of zero transfers.

about policy makers trying to compensate households and firms in the former treatment area in various ways. However, German municipalities do not have control over many policy instruments. Important tax rates like income taxes are chosen at the federal level and have to be approved by the states. And those taxes that municipalities can set themselves are mostly too small to be relevant for location decisions. The business tax rate is an important exception. One could hypothesize that municipalities in the Zonenrandgebiet lowered their business tax rates after 1994 to compensate firms for the loss in subsidies. Based on data from the German Statistical Office, Table 9 shows that there is no discontinuity in business tax rates between 1994 and 2010. Our preferred specifications show insignificant effects. Those that are significant rather point towards higher tax rates in the Zonenrandgebiet.

Further, policy makers could have decided to compensate former ZRG-municipalities by an alternative transfer scheme that substituted the old program, at least to some extent. We use data from the main regional transfer program, the Joint Task, to illustrate that there is no discontinuity in transfer recipience for municipalities at the former ZRG border between 1994 and 2010 (Table 9, columns (4)-(6)). Instead, we observe a discontinuity at the former inner-German border. This result fits into the general picture that regional transfers moved to the new Länder after German reunification (Appendix C, Figure C1).

Figure 7: Interpretation of estimates



So how can persistence in economic activity be explained? There is a lively debate in the literature about the relative importance of agglomeration economies and locational advantage in this context. For example, Bleakley and Lin (2012) or Kline and Moretti (2014) interpret their findings as evidence in favor of agglomeration economies while Davis and Weinstein (2002) stress the importance of locational advantage.[31] One appealing feature of the regression discontinuity approach is that all determinants that are continuous at the relevant threshold are unable to explain the outcome discontinuity. This argument is illustrated in Figure 7. The vertical line represents the geographical border between the treatment area (ZRG) and the control area ($Non - ZRG$) and measures the level of economic activity. Suppose both regions have the initial level of economic activity A^c. Transfers to the ZRG cause an increase in economic activity, for example, because investment subsidies stimulate firms to raise their capital stock and expand production. The direct effect of the transfers is illustrated by the horizontal upwards shift to the level A^t in the ZRG while the level of economic activity remains at A^c in the Non-ZRG.

Increases in economic density may generate agglomeration externalities. These may be positive (e.g. labor-market pooling, knowledge spillovers, etc.) or negative (e.g. crowding).

[31]Redding (2010) provides a recent overview of the empirical literature on new economic geography.

For our example, we assume that the net externality is positive thus shifting the level of economic activity further upwards.[32] If this externality dissipates continuously in space, there is a positive spillover to the control area. Inside the Non-ZRG, agents located directly at the treatment border benefit most from this spillover so economic activity increases the most at this point. The further one moves away from the border, the weaker the spillover effect becomes. It vanishes entirely at the point where the level of economic activity reaches A^c, the pre-treatment level. In the treatment area, however, the strength of the externality is lowest at the ZRG-border because agents are negatively affected by the lower level of economic activity on the other side of the border. The continuity ensures that the difference $B^t - B^c$ equals the direct effect $A^t - A^c$. Even if we observed the former, it is straightforward that this discontinuity does *not* include the spatially-continuous externality. In other words, the regression discontinuity approach perfectly controls for continuous spillovers (see Turner, Haughwout, and van der Klaauw, 2014, for a formal exposition) and thus agglomeration economies.

While this is an important point, our estimate of the average treatment effect is likely to contain a relocation externality that does show a discontinuity at the treatment border. Firms that are located in the Non-ZRG close to the treatment border are *not* indifferent between locating one meter to the left and one meter to the right of the threshold. We should thus expect that ZRG-treatment draws economic activity from the control area to the Zonenrandgebiet. Assuming that migration costs are increasing in distance, we should observe particularly strong relocation activities in a close neighborhood of the ZRG-border. Combining this effect with the net agglomeration externality could result in a function that is represented by the dashed curve in Figure 7. The way we have incorporated it implies that the relocation externality dominates, but we have no priors about the magnitude of this relocation externality. If this shifting of economic activity is relevant in size, our estimates capture both the direct effect and the relocation externality. Note, however, that the continuous agglomeration externalities still cancel. As one observes the discontinuity $C^t - C^c$, it is important to estimate the gradient at the border to obtain the net effect of the place-based policy, that is the sum of the direct effect and the net agglomeration externality. We relegate this exercise to the next section.

Returning to the discussion of what explains persistence, we can be sure that it is not agglomeration economies. But what is it instead? If the discontinuity in capital formation

[32] Of course, the same argument holds with negative net agglomeration externalities.

is the key explanation, why do we not observe a decline in the discontinuity over time? Capital depreciates at least to some extent over 16 years. Firms must have an incentive to replace depreciated equipment such that the difference in capital stock is maintained even in the absence of investment subsidies. The interpretation we suggest is that there must be sunk factors generating a locational advantage in the former treatment area. For example, local governments had to provide public infrastructure for industrial parks to accommodate higher firm activity in the Zonenrandgebiet. Industrial parks themselves needed to be planned and undeveloped land had to be made ready for building. As these structures are still available years after the subsidy program has ended, it is plausible that firms looking for premises are drawn to these industrial parks within ZRG municipalities although no subsidy draws them to that particular area. In this regard, our findings are closely related to Davis and Weinstein (2002, 2008) who show that nuclear bombing in Japan in 1945 only had a temporary effect on population and industrial structure. In interpreting their findings, Redding (2010) argues that "road networks and partially surviving commercial and residential structures may serve as focal points around which reconstruction occurs." Our results are different because, first, it is public policy that changed the economic structure and thus relative locational advantage between municipalities and, second, the policy was expected to be long-lasting thus affecting expectations of households and firms. As a consequence, the temporary policy intervention exhibited long-run implications for the spatial structure of economic activity.

5 Externalities: What is the net effect of the policy?

5.1 Identification

The above results do not allow inference about the net effect of the policy as the treatment may affect outcomes in the neighboring control group.[33] Thus, in order to make statements about the policy's net effect, we need to address the issue of spillovers. In particular, it is likely that place-based investment subsidies have attracted firms from the control region to relocate their businesses into the ZRG. Further, net agglomeration externalities may operate across the treatment border as argued in the previous section. While we cannot disentangle the two types of externalities, we can estimate the overall magnitude to shed

[33] This is also referred to as the stable unit treatment value assumption (SUTVA) (Rubin, 1980) which is often a problematic assumption in natural experiments.

light on the net effect of the policy, described by the sum of the direct effect and the net agglomeration externality in Figure 7.

We follow two alternative approaches to quantify the externality at the border. The first approach is referred to as *spatial exclusion* as it relies on estimating the effect for treatment and control observations that are located sufficiently distant from each other. The idea is that these municipalities are not affected by (local) externalities of the policy (see Neumark and Kolko, 2010). Obviously, this approach contradicts the identification strategy of the spatial RDD which relies on the comparison of outcomes for observations in a close neighborhood. However, we may execute this exercise in the fuzzy RDD. We exploit the fact that each district with $M_d \sim M_0$ accommodates a number of municipalities with varying distances to the treatment border. Thus, by excluding municipalities in the close neighborhood of $\mathbf{B}$ we can remove the part of district outcome that is potentially contaminated by spillovers (see Appendix E, Figure E1 for details). We estimate (4) for a sub-sample that excludes all municipalities bordering $\mathbf{B}$ and additionally establish minimum distances between treatment and control units of 10 kilometers and 20 kilometers in two alternative specifications. Yet, the requirement to maintain a high density of districts at the threshold M_0 limits the minimum distance we can establish between treatment and control observations. It may therefore be impossible to eliminate spillovers completely.

In order to address this limitation, we pursue a complementary approach introduced by Turner, Haughwout, and van der Klaauw (2014) which aims to estimate the outcome gradient in the control area. In particular, this *gradient approach* quantifies the differences in outcomes for control units close to the treatment border and the average value in the control region. In accordance with Figure 7, this approach delivers an estimate of $A^c - C^c$. In the spirit of Turner, Haughwout, and van der Klaauw (2014), we assume that the externality is symmetric around $\mathbf{B}$ such that we cannot reject a zero net effect if $2(A^c - C^c)$ equals the discontinuity $C^t - C^c$ we have identified above. The assumption of symmetric relocation externalities is plausible with moving costs being a continuous function of distance. For instance, households prefer to stay close to their acquaintances and friends or firms face increasing costs if they move further away from the suppliers they have established a relationship with. Defining a binary indicator I_{id} which is unity for municipalities within a certain maximum distance from the treatment border and restricting the sample to districts bordering with **B**, we estimate

$$Y_{id}^{c} = \alpha + \theta_d + \chi I_{id} + f(DG_{id}) + g(DD_{id}) + \varepsilon_i. \tag{5}$$

We add district fixed effects θ_d as well as flexible control functions of distance from the Iron Curtain DG_{id} and distance from the district border DD_{id} in order to control for confounding factors. The latter should account for economic cores being located in the district centers and the former should control for the general declining trend in economic activity towards the Iron Curtain. Note that a large part of this trend is already absorbed by θ_d. Moreover, as **B** follows district borders, it is not highly correlated with **G** such that we observe variation in I_{id} for observations with almost identical distance to the Iron Curtain.[34]

To implement the gradient approach, we do not only exploit municipality-level variation in income per square kilometer, but also proxies for the stock of private capital – which turned out as one of the primary channels in section 4.2.2 – at a very fine spatial scale. We use information from satellite images on land coverage at a $100m \times 100m$ resolution provided by the European Environmental Agencies CORINE project for the year 1990.[35] This allows us to specify proximity to the treatment border I_{id} at very narrow intervals to increase the precision of our estimates.[36]

5.2 Results

The estimation results for the spatial exclusion approach are highlighted in Panel A of Table 10. There are three specifications: (i) "border" excludes all municipalities adjacent to the treatment border, (ii) "5,000m" ignores all jurisdictions whose centroid is located within 5 kilometers to the ZRG border, and (iii) "10,000m" is a similar exercise for the 10-kilometer range. Note that all results are based on the contemporaneous sample.

Starting with the baseline results in columns (2) and (3) of Table 5, we observe from specification "border" that the average treatment effect drops from 0.482 to 0.449 and from

[34] The correlation coefficient of I_{id} and DG_{id} ranges for our municipality- and grid-cell-level samples between 0.18 and 0.28.

[35] Burchfield, Overman, Puga, and Turner (2006) use similar data for the US to study the determinants of urban sprawl.

[36] With the grid-cell data, we add a flexible polynomial of distance to the municipality border to (5) to control for the within-municipality distribution of private capital.

0.535 to 0.465 in the parametric and nonparametric specifications, respectively. Further restricting the sample to minimum distances of up to 10,000 meters yields a reduction of the point estimates of about 30 and 33 percent compared to the benchmark results. Hence, assuming that the externality dissipates linearly implies that relocation activities must occur within 60km to explain the total effect. Note, however, that this would comprise a substantial part of West Germany as the mean distance to **B** across all non-treated municipalities (in all states) is about 113km. With a minimum distance of 10,000m we loose efficiency and the share of observations displaying a level of M_d in the neighborhood of 40 drops considerably.[37] Accordingly, this approach provides evidence for substantial relocation externalities, but we may not completely determine the spatial extent of these negative spillovers.

Panel B of Table 10 reports the findings from the gradient approach where columns (1)-(3) display the results for income per square kilometer with distance bands of 1,500m, 2,500m, and 5,000m from the treatment border to identify observations in the control region that are particularly prone to the relocation externality. Each of the coefficients on these indicators shows a negative sign of about 0.2. Thus, assuming symmetry of the externality and taking our benchmark discontinuity estimates of 0.528 (see column (4) in Table 4), we argue that the negative point estimates of the externalities results in a low net effect. We attribute the negative sign of the sum of externalities mainly to relocation activities while it could in principle also reveal negative net agglomeration externalities. However, given the low density of municipalities in this region, we find the latter interpretation rather implausible. Accounting for the imprecise nature of the estimates, we cannot reject that the discontinuity at the ZRG border is fully driven by a relocation of economic activity from western regions towards the subsidized municipalities.

Columns (4)-(6) refer to the grid-cell data on private capital stock. We estimate linear probability models with the corresponding main discontinuity effects being reported in Table D1 in the Appendix. Exploiting distance bands of 500m, 1,000m, and 1,500m from the treatment border confirms the previous result. We find that cells outside and in the close neighborhood of the treatment area are significantly less likely to be covered by private buildings. Moreover, the direct effect suggests that ZRG treatment increases the probability of a grid-cell being covered by private capital by about 1.6 percentage

[37]While $M_d \in [30, 50]$ holds for about 15 percent of the observations in our benchmark specification, this share drops to about 8 percent with the 10km-exclusion window.

Table 10: EXTERNALITIES

A. Spatial exclusion approach

Log income per km^2	3rd. order polynomial of M_d			Nonparametric h^*		
	border	5,000m	10,000m	border	5,000m	10,000m
ATE	0.449**	0.396*	0.336	0.465**	0.435**	0.358
	(0.220)	(0.209)	(0.239)	(0.217)	(0.218)	(0.259)
R^2	0.09	0.09	0.10	-	-	-
Obs.	3,514	3,408	3,084	1,784	1,678	1,360

B. Gradient approach

	Log income per km^2			$Prob(PrivateCapital = 1)$		
	1,500m	2,500m	5,000m	500m	1,000m	1,500m
χ	-0.183	-0.198*	-0.224**	-0.008**	-0.007*	-0.008**
	(0.154)	(0.117)	(0.103)	(0.003)	(0.004)	(0.004)
				3rd. order distance to municipal border		
	all specifications include district fixed effects and 3rd. order distance to **G**					
R^2	0.33	0.33	0.33	0.03	0.03	0.03
Obs.	466	466	466	2,330,077	2,330,077	2,330,077

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. *Panel A.* excludes all municipalities that are bordering **B** (columns (1), (6)), and are within a distance of 5km and 10km from **B** (columns (2),(3) and (5),(6)). We estimate the fuzzy RDD specifications using a parametric 2SLS approach in columns (1)-(3) and the nonparametric approach in columns (4)-(6). Observations with $M_d > 150$ are dropped from the sample and standard errors are clustered at the district level. *Panel B.* estimates the gradient for observations outside of the ZRG and within a district that borders **B**. The dependent variable in columns (1)-(3) is income per km^2 measured on the municipality level; the dependent variable in columns (4)-(6) refers to a binary indicator which is unity if a $100m \times 100m$ grid is covered by private buildings and zero otherwise. χ is the coefficient on an indicator being unity for observations in a certain distance from **B**: 1,500m-5,000m with municipality data in columns (1)-(3) and 500m-1,500m with grid cell data in columns (4)-(6). Standard errors are clustered on the municipality level in the specifications using grid cell data.

points (see column (3) in Table D1) which is about twice the magnitude of the negative externality.[38] Thus, both approaches and both levels of data variation point to a considerable relocation of economic activity. This is consistent with our finding that a shift of population towards the subsidized regions is among the key drivers of the aggregate effect on income per square kilometer.

[38] Note that the discontinuity estimates for $Prob(PrivateCapital = 1)$ on the grid-cell level are well in line with the estimates for the area share of a municipality covered by private capital as displayed in Table 7. These estimates indicated that the logit-transformed area share, i.e. the odds ratio, increased by 19.7-34.1 percent due to transfers. Given that $Prob(PrivateCapital = 1)$ is about 6 percent in our data, a 1.6 percentage points increase in $Prob(PrivateCapital = 1)$ corresponds to $ln(\frac{0.076}{1-0.076}\frac{1-0.06}{0.06}) \approx 0.253$, i.e. an increase of about 25.3 percent in the odds ratio.

Table 11: Per-capita income and land prices

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	2nd	3rd	h^*	2nd	3rd	h^*
			Income per capita			
ATE	0.028**	0.083***	0.038***	0.006	0.067***	0.044***
	(0.012)	(0.015)	(0.014)	(0.013)	(0.017)	(0.014)
R^2	0.79	0.80	-	0.14	0.17	-
AIC	-3,545	-3,749	-	-2,874	-3,047	-
Obs.	3,870	3,870	3,035	3,881	3,881	2,867
			Land prices			
ATE	0.121	0.392***	0.269**	0.220***	0.353***	0.260***
	(0.111)	(0.150)	(0.117)	(0.049)	(0.060)	(0.063)
R^2	0.23	0.31	-	0.25	0.30	-
AIC	1,759	1,648	-	6,749	6,493	-
Obs.	982	982	410	3,635	3,635	2,350

Notes: Land prices per m^2 and income per capita are in logs. ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. In 1988 we have only data for Lower Saxony. Land prices correspond to so-called 'Bodenrichtwerte' which are expert evaluations of the land value net of the structures value. These values exist for land allocated to different usage types (housing, business and industry) of which we take the average. Note that the results are robust to individual usage types. We drop all observations outside a 100km window of the ZRG border in the parametric specifications. Columns (1)-(2) and (4)-(5) refer to parametric specifications and include state indicators. Columns (3) and (6) refer to non-parametric specifications where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008).

6 Incidence: Who benefitted from transfers?

Policy makers often have low-income households in mind when favoring transfers to lagging regions. However, according to spatial equilibrium theory it is unclear who eventually benefits from the place-based policy. If subsidies raise local investments and wages, it is likely that higher incomes translate into immigration, higher demand, and thus higher prices for land and housing. As a consequence, pre-treatment property owners reap the benefits and higher nominal income is eaten up by higher land rents. This chapter sheds light on this question.

We run the same regressions as in the spatial discontinuity approach with income per capita and land prices as outcome variables. For contemporaneous effects, land prices are only available for Lower-Saxony, while we have information for all relevant states in 2010.

We observe from Table 11 that nominal income per capita has increased by about 4-8 percent both contemporaneously and persistently. However, land prices went up by about 25-35 percent, depending on the specification. As households in Germany spend about 30 percent of their net income on rents,[39] real wages in the target region have not increased. These results stand in contrast with the assessment of the federal empowerment zones program in the US by Busso, Gregory, and Kline (2013). The reason why they do not find a significant response of population and rental rates in the targeted zones may be differences in the time-horizon of the policy. Migration decisions are forward-looking and build on expectations. As ZRG treatment was expected to last for the time of German division, migration incentives could have been stronger in this case.

Note that local subsidies are likely to exert positive externalities on land prices in the neighboring regions as shown by Rossi-Hansberg, Sarte, and Owens (2010) for an urban revitalization program in Richmond (VA). As this externality is continuous in space, our discontinuity estimates again reflect the direct effect on land prices in the treated area which does not include potential externalities (see section 4.2.3).

7 Locational advantage and access to markets

We have argued so far that spatially-continuous determinants of economic activity cancel in the regression discontinuity design and therefore cannot explain our findings of persistence. This does not mean, however, that these factors are unimportant to understand spatial equilibria. In this section, we study interaction effects of policy-induced locational advantage with market access to address two important issues. First, we re-estimate the role of market access for economic development by explicitly accounting for place-based subsidies to the ZRG in the study of Redding and Sturm (2008). Second, we use German reunification and EU Eastern enlargement to examine whether this exogenous variation in market access has caused a head start for ZRG municipalities (due to higher capital stock). These results inform the debate about whether places with higher density exhibit higher returns of regional transfers.

[39]Source: Federal Statistical Office, 2012, Fachserie 15 Reihe 1, Wirtschaftsrechnungen.

7.1 Redding and Sturm (2008) revisited

The importance of regional subsidies for the location decision of firms and households has immediate implications for the findings of Redding and Sturm (2008). In their seminal paper, Redding and Sturm (2008) have used the natural experiment of German division to identify a causal effect of market access for economic development. The authors show that West German cities located closer to the Iron Curtain exhibited lower population growth rates due to a disproportionate loss in market access. Although they mention regional subsidies to cities close to the border and discuss their potential relevance for their estimation, they are unable to control for this influence. As our regression discontinuity approach has identified large positive effects of these transfers for population density and population growth close to the former Iron Curtain, we expect market access to have an even larger impact as suggested by Redding and Sturm (2008) when accounting for the place-based policy. We rerun their suggested difference-in-differences specification with

their data on 119 West German cities covering the period 1919-1988 and add a dummy for being located in the Zonenrandgebiet (ZRG_i) plus an interaction term of this dummy and an indicator variable for ZRG-treatment ($Transfers_t$) in the 1970s and 1980s. Note that ZRG_i is not equal to our cross-sectional treatment indicator T_i in section 4 because location in the Zonenrandgebiet implied treatment only from 1971 onwards. Specifically, we estimate

$$\begin{aligned} Popgrowth_{it} =& \beta_1 Border_i + \beta_2 (Border_i \times Division_t) \\ &+ \gamma_1 ZRG_i + \gamma_2 (ZRG_i \times Transfers_t) + \theta_t + \epsilon_{it}, \end{aligned} \tag{6}$$

where $Popgrowth_{it}$ is the per-year population growth rate of city i in period t. We follow Redding and Sturm (2008) in using the same time periods, namely 1919-1925, 1925-1933, 1933-1939, 1950-1960, 1970-1980, and 1980-1988. $Border_i$ indicates whether a city is located within 75 kilometers to the Iron Curtain and $Division_t$ is a dummy variable equal to one after 1950 and zero otherwise. The time-fixed effect θ_t captures all period-specific effects applying to all cities alike. Columns (1) and (3) of Table 12 report the results in columns (1) and (3) of Table 2 in Redding and Sturm (2008). Columns (2) and (4) show estimates of our specification accounting for place-based policies.

Table 12: The costs of remoteness revisited

Population growth	RS baseline (1)	incl. transfers (2)	RS distance brackets (3)	incl. transfers (4)
Border x Division	-0.746***	-1.084***		
	(0.182)	(0.272)		
Border 0-25km x Division			-0.702***	-1.231***
			(0.257)	(0.297)
Border 25-50km x Division			-0.783***	-1.236***
			(0.189)	(0.253)
Border 50-75km x Division			-0.620*	-0.796*
			(0.374)	(0.444)
Border 75-100km x Division			0.399	0.399
			(0.341)	(0.341)
ZRG		-0.595**		-0.442
		(0.281)		(0.279)
ZRG x Transfers		0.600**		0.704**
		(0.288)		(0.283)
	All specifications include border main effects & period fixed effects			
R^2	0.21	0.22	0.21	0.22
Obs.	833	833	833	833

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. We use the data provided by Redding and Sturm (2008) which contains only cities. The results are robust to using our main dataset comprising all municipalities. The data spans from 1919 to 1988 where the indicators *Division* and $Transfers$ are unity for post 1950 and post 1971, respectively and zero otherwise. The indicator *ZRG* is unity for observations located in the subsidized Zonenrandgebiet and *Border* is unity for observations within 75km from the inner-German border. Further distance brackets at 25-100km from the inner-German border are included. The correlation between *Border* $\times$ *Division* and *ZRG* $\times$ $Transfers$ is about 0.5.

It is immediate from comparing columns (1) and (2) that the role of market access for economic development increases by about 45 percent. Cities within 75 kilometers to the Eastern border exhibit a population growth that is 1.08 percentage points lower than cities located further west. According to the difference-in-differences specification, ZRG treatment raised population growth of cities located in this area by 0.6 percentage points compared to cities outside the ZRG. Note that ZRG and *Border* differ because the border of the Zonenrandgebiet is characterized by a distance of less than 75 kilometers to the Iron Curtain, but some cities fall into both categories (see Table 3). Further, while *Division* receives a value of unity from 1950-1988, $Transfers$ only start in the early 1970s.

Allowing for different distance brackets to the Eastern border reveals an even stronger deviation from the results in Redding and Sturm (2008). Comparing columns (3) and (4) shows that the role of market access is about 75 percent higher within the first 25 kilometers to the Iron Curtain. This effect declines if we move further west. It is 58 percent higher within 25-50 kilometers, and 28 percent higher within 50-75 kilometers distance. As cities located more than 75 kilometers away from the border generally did not receive treatment, the difference in the estimates vanishes completely.

7.2 Did the ZRG benefit more from reunification and EU Enlargement?

Do place-based subsidies exhibit higher returns in lagging regions with lower density? Or should the money rather be invested in highly-agglomerated areas? Glaeser and Gottlieb (2008) argue that "current transportation spending subsidizes low-income, low-density places where agglomeration effects are likely to be weakest." While this statement refers to the US, European regional policy has a similar focus.

To better understand marginal effects of regional transfers, we look at the interaction between policy-generated locational advantage and market access. Better market access implies stronger home-market effects boosting productivity and income. We thus take advantage of German reunification and EU Eastern enlargement in 2004 as sources of exogenous variation in market access to examine whether municipalities in the ZRG benefitted more from market access than jurisdictions outside the treatment area.[40] Based on the findings above, we could attribute heterogeneous effects to policy-induced differences in capital stock or locational advantage.

To identify the effects, we combine the discontinuity approach with the time variation to examine whether discontinuities differ *before* and *after* the events. This design is referred to as "differences-in-discontinuities" in the literature (Grembi, Nannicini, and Troiano, 2014).[41] Comparing municipalities in the close neighborhood of the ZRG-border ensures that municipalities are affected similarly by changes in market access. Moreover, we should expect that municipalities closer to the inner-German border benefitted more from

[40] In a related difference-in-differences approach, Brülhart, Carrère, and Trionfetti (2012) study the employment response in Austrian municipalities due to the Fall of the Iron Curtain.

[41] Typically, regression discontinuity identification is combined with differences over time if the RD assumptions are not satisfied due to confounding discontinuities. As the RD assumptions are valid in our case, we can use the additional variation to exploit treatment effect heterogeneity.

market integration with East Germany than municipalities located further south in the neighborhood of the Czech Republic and Austria (see Figure 1). We thus restrict the sample to those jurisdictions being closer to the inner-German compared to the Czech-German border in the first exercise and vice versa for EU enlargement.

Starting with German reunification, we use the time periods 1980-1985, 1985-1991, 1991-1995, and 1995-2000. Our difference-in-discontinuities specification can be directly derived from (2) where ZRG_i indicates again whether a municipality is located in the Zonenrandgebiet and $Reunification_t$ is a dummy variable equal to one after 1990 and zero otherwise. Hence, we estimate

$$\begin{aligned} Popgrowth_{it} =& \alpha + g_0(D_i) + f(\mathbf{L_i}) + ZRG_i[\tau + \beta Reunification_t + g_1(D_i) - g_0(D_i)] \\ &+ Reunification_t + \epsilon_{it}, \end{aligned} \tag{7}$$

where flexible and asymmetric polynomials of distance to $\mathbf{B}$ and of Cartesian coordinates are included. We focus on population growth due to superior data coverage. The corresponding results are reported in Panel A of Table 13. The first two columns refer to the entire boundary sample while columns (3) and (4) exclude observations that are closer to the border to the Czech Republic than to the inner-German border. Indeed, we observe across all specifications that the interaction $Reunification_t \times ZRG_i$ enters significantly positive. Moreover, the effect tends to be somewhat more pronounced for the sub-sample of municipalities at the inner-German border and we find that the main effect of ZRG_i ceases to be significant with the 3rd-order polynomial. In sum, reunification raised population growth in the ZRG by about 0.13-0.17 percent. Note that the boundary sample focuses on municipalities in the eastern part of West Germany and accordingly, we observe that population growth rates increased due to reunification also for units outside the ZRG.

The second exercise (EU enlargement) focuses on the time periods 1995-2003 and 2004-2010 and substitutes $Reunification_t$ in (7) with an indicator $Integration_t$ being equal to one after the integration of the Czech Republic into the European Union in 2004 and zero otherwise. The corresponding results in Panel B of Table 13 are structured in the same way as in the case of reunification. However, columns (3) and (4) are now confined to municipalities that are located closer to the Czech-German border (stretching about 356km) than to the inner-German border.

Table 13: Reunification & EU integration

A. Reunification: 1980-2000

Population growth	Boundary sample 2nd. order	Boundary sample 3rd. order	Dist(GDR)<Dist(CZ) 2nd. order	Dist(GDR)<Dist(CZ) 3rd. order
ZRG	0.231***	0.136*	0.220***	0.122
	(0.061)	(0.075)	(0.063)	(0.077)
Reunification	0.724***	0.724***	0.744***	0.744***
	(0.029)	(0.029)	(0.030)	(0.030)
Reunification × ZRG	0.139***	0.140***	0.174***	0.174***
	(0.046)	(0.046)	(0.048)	(0.048)
	includes coordinate controls			
R^2	0.11	0.11	0.12	0.12
AIC	51976.69	51926.77	49533.16	49497.36
Obs.	14,948	14,948	14,156	14,156

B. EU integration: 1995-2010

Population growth	Boundary sample 2nd. order	Boundary sample 3rd. order	Dist(CZ)<Dist(GDR) 2nd. order	Dist(CZ)<Dist(GDR) 3rd. order
ZRG	0.257***	0.223***	-0.072	-0.292
	(0.052)	(0.065)	(0.201)	(0.273)
Integration	-0.839***	-0.839***	-0.899***	-0.901***
	(0.025)	(0.025)	(0.073)	(0.072)
Integration × ZRG	-0.114***	-0.114***	0.350***	0.353***
	(0.039)	(0.039)	(0.113)	(0.112)
	includes coordinate controls			
R^2	0.31	0.32	0.56	0.58
AIC	18793.17	18724.79	717.49	705.18
Obs.	7,564	7,564	419	419

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. We drop all observations outside a 100km window of the ZRG border. *ZRG* indicates location in the Zonenrandgebiet, *Reunification* and *Integration* are unity for after 1990 and 2004, respectively and zero otherwise. Panel A uses four periods 1980-1985, 1985-1989, 1990-1995, 1995-2000 whereas Panel B uses the periods 1995-2003, 2004-2010. Columns (3) and (4) in Panel A (Panel B) refer to sub-samples of municipalities that are located closer to the former GDR than to the Czech Republic (closer to the Czech Republic than to the former GDR).

For the full boundary sample the results indicate that population growth was significantly lower after 2004 in the ZRG. However, the majority of ZRG-municipalities is located closer to the inner-German border being affected by EU enlargement only marginally. If we restrict the sample to those jurisdictions being located close to the Czech-German border, we find strong and significantly positive effects. Better market access has raised population growth by 0.35 percent in the ZRG compared to the counterfactual without transfer recipience. This suggests that the formerly subsidized municipalities in the south of West Germany did not experience a higher gain from reunification than the control regions, but experienced such a head start a few years later when markets at their closest border were integrated.

Our findings from both events point to an important interaction between policy-induced locational advantage and market access. The results imply that regional transfers are more effective at places that already exhibit a higher level of market access – like high-density locations. This stands in sharp contrast with the actual allocation of most regional transfers to low density places.

8 Conclusions

We have shown in this paper that temporary regional transfers are able to affect the spatial pattern of economic activity in the long run. As the policy was connected to German division, households and firms expected subsidies to be paid for a longer period and the volume of transfers was substantial. These circumstances were likely influential as migration and location decisions are forward-looking. Uncovering underlying mechanisms of this outcome, we have stressed the importance of capital formation in generating persistence as our identification approach controls for all spatially-continuous agglomeration economies.

While the policy was effective in changing the economic landscape, we are rather pessimistic regarding the net effect of the policy. Estimating local spillovers at the treatment border points to substantial relocation activities. Our results do not allow us to rule out that the place-based policy did not generate new economic activity. Moreover, we have entirely ignored the cost side of the program raising further doubts concerning the efficiency of regional aid. With regard to distributional implications our findings point to large increases in land rents, so transfers primarily benefitted pre-treatment land owners in the Zonenrandgebiet rather than raising real wages.

Finally, we have exploited interaction effects between capital structures and market access to show that municipalities in the Zonenrandgebiet benefitted more from better access to markets in the East than jurisdictions outside the former treatment area. This indicates that places with larger home markets (higher density) respond more to transfers than low-density places.

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Appendix

A Data

Table A1 displays the number of observations by state as well as the area and population shares of the ZRG.

Table A1: Characteristics of the Zonenrandgebiet

	No. districts		No. municipalities		Area ZRG	Pop. ZRG
	Non-ZRG	ZRG	Non-ZRG	ZRG	in %	in %
West Germany	396	106	6,839	1,573	18.6	12.3
Schleswig-Holstein	4	14	414	710	53.3	81.3
Lower Saxony	44	25	711	282	28.6	33.4
North-Rhine Westfalia	86	0	386	0	0	0
Hesse	34	13	323	96	27.8	19.1
Bavaria	131	54	1,533	485	25.8	21.9
Other West-German states	97	54	3,472	0	25.8	21.9

Notes: The states (Bundesländer) Schleswig-Holstein, Lower Saxony, Hesse, and Bavaria belonged to the ZRG. We add North Rhine-Westphalia as it borders with the ZRG, but drop the city states of Hamburg and Bremen. The districts correspond to the 1971 classification while we use data from 1986 for the number of municipalities, and the year 1961 for population shares.

A list of all 1971 districts that belonged to the ZRG is contained in the federal law 'Zonenrandförderungsgesetz', 1971. Data on population by municipality is available from the Federal Statistical Office for the years 1975-2010. For the years 1950-1970 we have acquired the data from the Statistical Offices of the five states we consider. Likewise, information on income, business taxes, and employment was provided by the statistical offices of the individual states. Data on municipal area shares covered by private, public, commercial, industrial, and residential capital was provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development. Depending on the municipality classification of each data source we assign it either to the 1997 or 2010 shape file of municipal boundaries. Thereby, we link 1971 districts (and thus treatment status), coordinates, and distances from the ZRG boundary to the outcome variables. Geospatial data processes have been performed and documented in ArcGIS.

B Non-parametric estimation

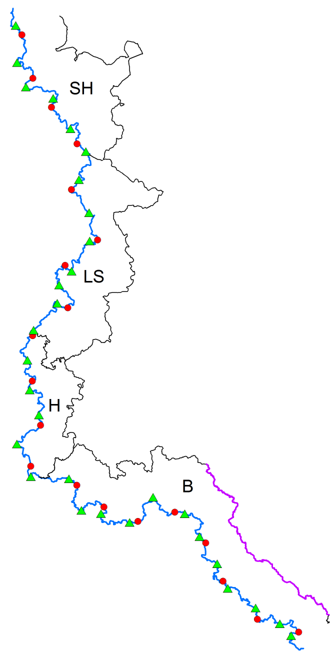
For the nonparametric identification strategy we resort to local linear regressions as these are particular well-suited for inference in the RDD (see Fan and Gijbels, 1996; Imbens and Lemieux, 2008). We employ an edge kernel function and choose different bandwidths according to optimality criteria. We compute the distance of each municipality's centroid to 20 (or 30) border points that are allocated at equal distances along $\mathbf{B}$ as shown in Figure B1. Then, we assign municipalities to the closest border point, add border-point fixed effects, and use the distance to the respective border point in the local linear regressions to estimate $E[Y_{i0}|\mathbf{L}_i = \mathbf{b}]$ and $E[Y_{i1}|\mathbf{L}_i = \mathbf{b}]$. All our results are insensitive to choosing 20 or 30 border points (red dots and blue triangles in Figure B1, respectively).

As a further robustness check, we refrain from allocating border points and estimate bivariate local linear regressions based on Cartesian coordinates. In this approach we use a product kernel $K_{h_x}(L_{ix} - L_{0x})K_{h_y}(L_{iy} - L_{0y})$ and minimize:

$$\sum_{i=1}^{n} \{Y_i - \alpha - (L_{ix} - L_{0x})\,\beta_1 - (L_{iy} - L_{0y})\,\beta_1\}^2\, K_{h_x}(L_{ix} - L_{0x})K_{h_y}(L_{iy} - L_{0y}). \quad \text{(B.1)}$$

Again, this is done separately for units west and east of $\mathbf{B}$ to obtain $\hat{\alpha}$ for both sides. The pair of bandwidths is chosen according to a cross-validation criterion. In practise this approach is less efficient than the univariate approach based on border points because the additional dimension requires disproportionately more observations. Therefore, we present our non-parametric results generally for the border point approach and note that all results are robust to employing bivariate local linear regressions (corresponding tables are available upon request).

Figure B1: Border points

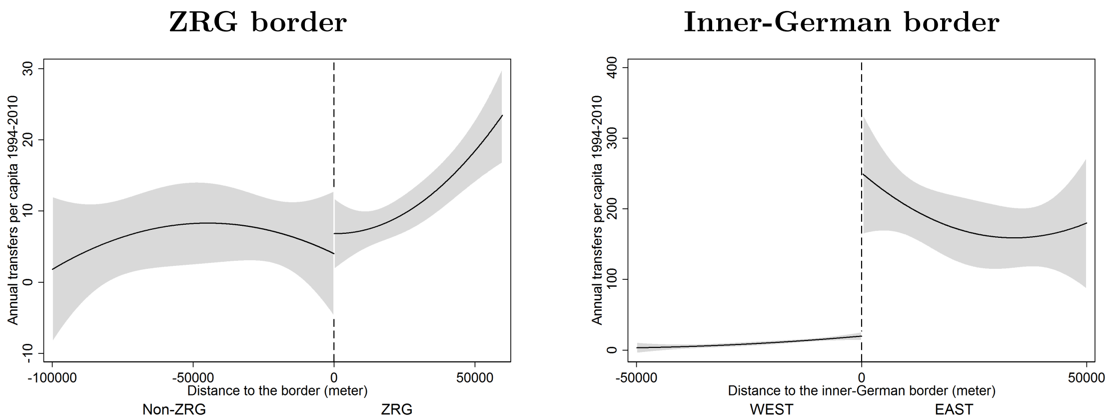


Notes: The red dots (green triangles) mark the 20 (30) border points we employ in our analysis. These are allocated at equal distances along the ZRG border. The black lines mark the inner-German border and the state borders. The treated states were Bavaria (B), Hesse (H), Lower Saxony (LS) and Schleswig-Holstein (SH). The Czechoslovakian border is marked in purple. When splitting up the sample into treated units closer to the former GDR or to Czechoslovakia we use the perpendicular distances of municipal centroids or pixels to the respective borders.

C Regional transfers after 1994

Information on federal regional transfers by recipient municipality, year, and transfer type was provided by the Federal Office for Economic Affairs and Export Control. We aggregate over transfer types (infrastructure and subsidies to the private sector) and over years. While it still holds true after 1994 that transfers are channeled to the east of Germany, we do not observe a discontinuity at the former ZRG border as is shown in Figure C1. The left-hand plot corresponds to the results presented in Table 9. Taking into account that the program was terminated in 1994 and that the former ZRG border does not correspond to district borders while transfer intensities are determined by districts, this is not surprising. Moreover, from the right-hand plot in Figure C1 we observe a pronounced discontinuity of transfer intensity at the former inner-German border. In addition to federal transfers we have accounted for resources redistributed within states according to municipal fiscal equalization schemes (in the case of Bavaria). These within state transfers 'Bedarfszuweisungen' are at much lower scale (about 0.3 percent of the federal transfers) and display no discontinuity at **B**.

Figure C1: TRANSFERS 1994-2010



D Grid-cell data

We use data on $PrivateCapital$ and $Radiance$ for grids of $100m \times 100m$ and 30×30 arc-seconds (about $926m \times 926m$ at the equator), respectively. $Radiance$ of grids is computed according to digital integer numbers reported by satellite data of the Defence Meteorological Satellites Program – Operational Linescan System (DMSP-OLS).[42] These data measure night-time lights in the year 1992 and are widely used in research (see, e.g., Henderson, Storeygard, and Weil, 2012). The information on $PrivateCapital$ is provided by the European Environmental Agencies CORINE project for the year 1990. The data contains a variable that indicates 44 different land cover classes. We set $PrivateCapital = 1$ if a place is covered by 'Continuous urban fabric', 'Discontinuous urban fabric', 'Industrial or commercial units', or 'Construction sites' and zero otherwise. Note that the area-weighted

[42]The satellite data report digital integer numbers ranging from 0 to 63. These may be converted to radiance as a measure of night luminosity by using the formula $radiance = digitalnumber^{1.5}$ for a spatial unit which is denoted in terms of Watts/cm^2/sr/nm (in words: Watts per squared centimeter per steradian per nanometer of wave length).

sum of $PrivateCapital$ is highly correlated with our municipality-level variable for private capital which is described in section 3 (correlation coefficient of 0.84).

Table D1: Spatial RDD: Grid cell data

	Prob(PrivateCapital = 1)			*Log(Radiance)*		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	2nd	3rd	h^*	2nd	3rd	h^*
ATE	0.016***	0.016***	0.013***	0.298***	0.281***	0.252***
	(0.000)	(0.001)	(0.001)	(0.015)	(0.021)	(0.019)
R^2	0.01	0.01	-	0.08	0.10	-
AIC	-210,193	-218,436	-	256,617	254,503	-
Obs.	7,852,560	7,852,560	1,155,429	107,776	107,776	49,304

Notes: ***, **, * denote significance at the 1-, 5-, and 10-percent level, respectively. Robust standard errors in parenthesis. We drop all observations outside a 40km window of the ZRG border. The dependent variable in columns (1)-(3) is a binary indicator which is unity if a $100m \times 100m$ grid is covered by private buildings and zero otherwise. In columns (4)-(6) we use log(Radience) as computed from satellite night-light data for grid cells of 30 arc-seconds (about $926m \times 926m$ at the equator). Information on land coverage and radience refer to the years 1990 and 1992, respectively. Columns (3) and (6) refer to non-parametric specifications where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012) and standard errors are computed according to Imbens and Lemieux (2008).

Table D1 reports the contemporaneous average treatment effects of the ZRG transfers on the probability of a grid cell being covered by private capital in columns (1)-(3) where we report parametric specifications according to (2) as well as a nonparametric estimate based on optimal bandwidth. Likewise, columns (4)-(6) report the average treatment effect for $log(Radiance)$. In each case we find a positive and highly significant effect. Note that the estimates for $Prob(PrivateCapital = 1)$ on the grid-cell level are well in line with the estimates for the area share of a municipality covered by private capital as displayed in Table 7. The latter indicated that the logit-transformed area share, i.e. the odds ratio, increased by 19.7-34.1 percent due to transfers. Given that $Prob(PrivateCapital = 1)$ is about 6 percent in our data, a 1.6 percentage points increase in $Prob(PrivateCapital = 1)$ as displayed in columns (1) and (2) of table D1 corresponds to $ln(\frac{0.076}{1-0.076}\frac{1-0.06}{0.06}) \approx 0.253$, i.e. an increase of about 25.3 percent in the odds ratio. With regard to radiance we computed a conversion factor of income per km^2 and radiance per km^2 based on municipality level information for West Germany. Applying this factor to the estimates in Table D1 shows that the results are well in line with the estimates for our main proxy for economic activity as reported in Table 4.

E Spatial exclusion approach

The spatial exclusion approach is illustrated in Figure E1 where the blue line marks the ZRG border. Blue and yellow shaded areas refer to municipalities in our boundary sample that belong to treated and non-treated areas, respectively. District boundaries (according to the 1971 classification) and municipal boundaries are drawn in red and black, respectively. In the spatial exclusion approach, we drop all treated (non-treated) municipalities within a certain distance from the closest non-treated (treated) municipality. The excluded municipalities are located between the blue and yellow areas (the map corresponds to a 20-kilometer minimum distance between the treatment and the control group). By dropping those municipalities in the close neighborhood of the treatment border, we remove the share of GDP of a district that is potentially affected by positive or negative spillover effects.

Figure E1: Relocation Externalities – Spatial Exclusion Approach