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The persistent effects of regional policy - Evidence from the West-German Zonenrandgebiet

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The persistent effects of regional policy:

Evidence from the West-German Zonenrandgebiet

Maximilian von Ehrlich* and Tobias Seidel†

PRELIMINARY VERSION

Abstract

In this paper, we provide novel evidence on the contemporaneous and persistent effects of regional policy. We apply a quasi-experimental identification strategy exploiting the fact that municipalities in the West-German Zonenrandgebiet (ZRG) were eligible for substantial regional transfers between 1971 and 1994. The ZRG was an approximately 40km-band adjacent to the Iron Curtain during the Cold War in West Germany. Apart from determining transfer eligibility, this pure geographic threshold did not have any institutional, cultural or economic relevance. We use regression discontinuity to estimate the causal effects of regional policy on economic activity. Using disaggregated data on the municipality level and satellite night light data (as a proxy for GDP), we find that the ZRG treatment led to an increase of income per km^2 of about 50 percent in 1986. Importantly, economic density remained high in 2010 although the transfers had phased out in 1994. This speaks against unique equilibria determined by locational advantage, but rather strengthen market externalities as an important explanation for the spatial distribution of economic activity. We also examine several potential channels finding strong contemporaneous and persistent effects of transfers on population density and the business tax base.

Keywords: Regional policy; Economic geography; Persistence; Regression discontinuity; Persistent border effects;

JEL classification: H25; H41; H54; O15; O18; R12

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1 Introduction

Based on the widespread concern that economic development generates unequal living conditions across regions, many governments have installed regional transfer programs to mitigate this effect. For example, the EU dedicates about one third of its overall budget 2014-2020 to regional policy amounting to more than 350 billion euros.[1] The US does not have a unified regional policy, but annual spending on regional development programs is estimated at about 15 billion US dollars per year (Government Accounting Office, 2012). Also China has installed regional policies that resemble those in the EU in terms of instruments and magnitude (EU Commission, 2010). While there is some evidence about the effects of these transfers on growth and employment during transfer programs (see, e.g., Becker, Egger, and von Ehrlich, 2010), little is known about the persistence of these policies after regional redistribution has phased out. Can regional transfers alter the long-run pattern of economic activity and what are potential channels of persistence? From a welfare perspective, a persistent impact on the economy's spatial distribution is likely to dominate any contemporaneous effects on income or employment. Moreover, the central motive of regional policy is to shift the economy to a long-run equilibrium that differs from the market outcome and is more preferred by the electorate. Yet, economic theory casts into doubt whether temporary policy measures can alter the long-run path of regional development.

Identifying a causal effect of regional policy on economic outcome relies on a convincing strategy to deal with the endogeneity of transfers. Federal support does not follow random assignment as poorer regions receive a larger share of the budget. As a consequence, insufficiently controlling for (unobserved) characteristics of targeted and non-targeted jurisdictions implies biased estimates. Further, to make statements about persistent effects, we need to find a case where the transfer program was terminated and we can still isolate potential effects today. We use a quasi-experiment from Germany during the Cold War that allows us to tackle both challenges.

In 1971, the West German government enacted a law that defined a geographical area adjacent to the Iron Curtain to be eligible for substantial regional transfers.[2] All districts (German Kreise) that accommodated either 50 percent of their area or population within

[1] See EU Commission (2011).

[2] This law was called Zonenrandförderungsgesetz.

a distance of 40km to the inner-German and Czechoslovakian border on 1 January 1971 became part of the *Zonenrandgebiet* (ZRG).[3] As shown in Figure 1, it stretched from the Danish border in the North to the Austrian border in the South. A major reason for this privileged treatment was to compensate jurisdictions close to the eastern border for being cut off adjacent markets on the other side of the Iron Curtain. Both psychologically and economically, the remoteness was feared to cause substantial outmigration of households and firms to the western parts of the country.[4] Redding and Sturm (2008) have used the division of Germany to show that less access to markets indeed caused cities close to the border to grow less than cities further west.

Figure 1 about here

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 boundary discontinuity design (BDD) which examines observations in a close neighborhood on either side of the treatment border. Second, we use the political rule which governed the location of the treatment border and features a discontinuity by itself. Knowing the precise political rule that defines the border constitutes a great advantage and enables us to reach beyond previous boundary discontinuity designs.[5] While the first approach allows us to exploit variation on a very fine scale, the latter relaxes the BDD's identifying assumptions and acts as a compelling identification alternative. We find that regional transfers led to a higher income per km^2 in the treatment area by about 50 percent in 1986. Moreover, the effects are still clearly visible in 2010 (16 years after the program phased out) with estimates fluctuating around 50 percent in alternative specifications. In addition to municipal information, we resort to luminosity data recorded by weather satellites as a proxy for GDP (see Henderson, Storeygard, and Weil, 2011, and Chen and Nordhaus, 2011).[6] This increases geographic precision of observations at the border and the corresponding results confirm our finding: Regional policy exhibits persistent effects.

[3]Ziegler (1992). Zonenrandgebiet literally means area adjacent to the (Soviet occupation) zone, that is the German Democratic Republic. It was common in West Germany to refer to the German Democratic Republic as the "Zone".

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

[5]For instance, Michalopoulos and Papaioannou (2014) refer to anecdotal evidence that colonizers drew African borders in an arbitrary manner whereas Bayer, Ferreira, and McMillan (2007) and Black (1999) exclude borders that seem to follow a problematic pattern.

[6]Storeygard (2013), Michalopoulos and Papaioannou (2013, 2014), Henderson, Storeygard, and Weil (2012) and Pinkovskiy (2013) are examples for recent applications of satellite night light data.

To understand more about mechanisms through which persistence emerges, we look into a number of potential channels. We show that income per km^2 rises due to higher population density and employment rates while we find no evidence that educational levels were affected by the policy measure. With respect to private and public capital, we show that ZRG transfers significantly raised the business tax base and infrastructure investments have improved connections to highways, trains, and airports. The transfer effects on local labor and capital are clearly present until today. For example, our results suggest that transfers have raised population density by up to 50 percent both in the mid-1980s and in 2010. The response of the business tax base is even higher as firms were specifically targeted through investment subsidies and tax allowances. In contrast, per-household income was raised only during the times of the program whereas the treatment effect did not prevail in 2010. This suggests that subsidies permitted a contemporaneous rent that vanished over time.

The theoretical regional economics literature provides some guidance on potential implications of transfers. Rather obviously, a permanent income transfer to poorer regions can be expected to reduce income disparity and spatial concentration (Baldwin, Forslid, Martin, Ottaviano, and Robert-Nicoud, 2003). This idea is perfectly in line with our findings.[7] However, it is much less clear whether a temporary shock – such as substantial subsidies to a limited geographic area – can have a *persistent* effect on the spatial distribution of economic activity. In this regard, the theory of location fundamentals suggests that even strong shocks will be reversed as long as they are of temporary nature. Evidence by Davis and Weinstein (2002, 2008) shows that the relative position of Japanese cities stayed constant over a very long time horizon. Even massive destructions in World War II could not alter the previous distribution of population and industries across cities in the long run.[8] This finding conflicts with a large class of economic geography and growth models which yield multiple equilibria that are selected according to initial conditions and expectations (see e.g. Krugman, 1991; Acemoglu, 1995; Nunn 2007). In these models temporary shocks may destabilize the existing equilibrium and shift the economy perma-

[7]However, Martin and Rogers (1995) and Dupont and Martin (2006) argue that the implications of regional policies may be quite complex with potentially adverse local effects. For example, infrastructure investments in the poorer region might lead to more agglomeration in the core region if transport costs *between* both locations decline. Martin (1999) finds similar results in a new growth model. In our context, however, it is noteworthy that regional transfers were more than just infrastructure investments. Most importantly, firms in the Zonenrandgebiet received investment subsidies as well and were allowed substantial tax deductions.

[8]Similar evidence is provided for the effects of U.S. bombings in Vietnam by Miguel and Roland (2011).

nently to a different steady state. Evidence for the presence of multiple spatial equilibria and permanent effects of shocks is provided by Redding, Sturm and Wolf (2011). Their argument rests on the observation that the German division led to a relocation of Germany's leading airport from Berlin to Frankfurt while there is no evidence of a return to Berlin after reunification.[9]

However, these shocks might have to be quite large to move from one equilibrium to another. Economic geography models predict a large degree of path dependence in the spatial distribution of economic activity. A similar characteristic can be derived from big push theories claiming that public investment can only have lasting impacts if they exceed a certain threshold level and have otherwise only temporary effects (see, e.g., Sachs, McArthur, Schmidt-Traub, Kruk, Bahadur, Faye, and McCord, 2004). A recent study by Bleakley and Lin (2012) provides support for path dependence as it demonstrates the long-run effects of historical transport hubs for economic geography. Bleakley and Lin (2012) show that portage sites remain to be population centers even well after the portage-related location advantage became obsolete. According to their findings, transport hubs act as a coordinating device and may govern the selection among multiple equilibria. While portage sites played an important role due to historic transportation technologies, federal governments try to grade up the value of certain places by infrastructure investments or public subsidies. There is very little evidence on whether these federal expenditures are capable of affecting the long-run allocation in space. A recent exception is Kline and Moretti (2014) who assess the long-run benefits of a regional development program in the US. They find a lasting effect on manufacturing employment and estimate the benefits to exceed the overall costs of the program. Our paper fits into this debate by suggesting that regional policy exerts a significant influence on the spatial distribution of economic activity beyond the program period. This supports market externalities as an important explanation for long-run effects of historical events.[10] As we do not observe a tendency towards the pre-treatment equilibrium with respect to population density and the business tax base, our findings do not support the notion of unique equilibria defined by locational advantage.

[9] Duranton and Puga (2004) and Puga (2010) provide reviews of the literature on determinants of agglomeration. See also Rosenthal and Strange (2001) and Burchfield, Overman, Puga, and Turner (2006) for empirical studies on a large set of determinants.

[10] Further evidence on the role of market externalities for location is provided by Ellison, Glaeser and Kerr (2010), Redding and Sturm (2008) and Hanson and Xiang (2004), for example.

Our paper is also closely connected to the recent literature on the impact of historical institutions for economic development. Applications cover the reasons behind productivity differences across countries (Hall and Jones, 1999), the role of colonial heritage and geography (Acemoglu, Johnson, and Robinson, 2001, 2002), legal origins (Glaeser and Shleifer, 2002), taxation systems in British India for present-day outcomes (Banerjee and Iyer, 2005), the impact of African slave trades (Nunn, 2008), the role of the forced mining labor system in colonial Peru (Dell, 2010) or pre-colonial and national institutions for African development (Michalopoulos and Papaioannou, 2013, 2014). Nunn (2009) provides a detailed overview of recent developments in this literature. Our paper adds a further institutional example (regional transfer programs) and is most closely related to Dell (2010) as she applies a boundary discontinuity approach as well and illustrates channels of persistence.

Finally, this paper contributes to empirical research on the impact of regional policy. While earlier studies rebut the influence of regional transfers on convergence (Sala-i-Martin, 1996, Boldrin and Canova, 2001), more recent evidence points to the effectiveness of these subsidies with respect to economic growth (see, e.g., Becker, Egger, and von Ehrlich, 2010). We use a novel identification strategy in this context by exploiting discontinuities at a *geographical* border. Moreover, none of the papers in this literature has looked at the persistent effects of regional policy and potential channels.

The paper is organized as follows. In the next section, we provide more detailed information about the historical and institutional background of the transfer program. In section 3, we discuss the data and descriptive statistics before we lay out the identification strategy in section 4. Our findings are presented in section 5 where we focus on economic activity in subsection 5.1 and channels of persistence in subsection 5.2. We also look into the heterogeneity of treatment effects along the ZRG border in subsection 5.3. We document a number of sensitivity checks in section 6 before we present our concluding remarks.

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. The aim was to create four zones of

roughly equal population size while the zonal boundaries were primarily drawn according to administrative criteria rather than economic ones (Siegler, 1970). After the war, 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 13 August 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.[11] Districts at the inner-German border received support to prevent outmigration of residents and firms. This appeared a serious concern as the Iron Curtain deteriorated the living conditions for both psychological and economic reasons. As policy makers widely regarded the division of Germany as a temporary phenomenon, transfers were justified to preserve the economically strong position of the geographical center of pre-war Germany for the time after reunification.[12] 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)[13] 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 40km to the inner-German or Czechoslovakian border on 1 January

[11] See Karl (2008) for a more detailed review of regional policy in West Germany.
[12] Bundesministerium für innerdeutsche Beziehungen (1987).
[13] See Eckey (2008) for a historical overview of the Joint Task.

1971 became part of the Zonenrandgebiet.[14] 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 shifted to the development of the 'New Länder'. From then onwards, the ZRG was no longer eligible for regional transfers.

Table 1 about here

As Table 1 reveals, the Zonenrandgebiet accounted for 18.6 percent of the West German territory and accommodated 12 percent of the population. It is also evident that there is some degree of heterogeneity between the four States that hosted the ZRG. While population density is above average in Schleswig-Holstein and Lower Saxony, the opposite holds for Hesse and Bavaria. Further, Schleswig-Holstein sticks out as 71 percent of its territory was part of the Zonenrandgebiet while in Hesse, the figure amounts to only 16.9 percent.

What were the instruments of the ZRG-transfer scheme? Firms inside the Zonenrandgebiet could apply for investment subsidies of up to 25 percent and were eligible for special 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. Moreover, companies located in the ZRG were treated with priority in public tendering. A substantial share of subsidies flowed into public infrastructure projects and transfers could also be used for renovation of houses, investments in social housing, day care centers, education and cultural activities.

Based on the Joint Task and the Investment Premium Law, the ZRG received the lion's share of the transfer budget (see Table 2). 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 relative treatment intensity of the ZRG was even higher than stated. Over the period 1972 to 1987, local administrations inside the ZRG received 35-46 percent of all public transfers in West Germany. Looking at these figures within the four States only, the share of received transfers would even range between 60 and 85 percent

[14] According to a statement by state secretary Sauerborn, the 40km-rule also included less needy regions, but was appealing for practical reasons in the first place. This debate already started in the 1950s. See Protocol of the 39th session of the cabinet committee of economics, 1951-1953, available at `www.bundesarchiv.de/cocoon/barch/0001/x/x1951e/kap1_2/kap2_41/para3_2.html`.

in 1984-1987.[15] Relating these numbers to the area and the number of inhabitants, the transfer intensity was 4-6 times higher per capita and about 2-3 times higher per km^2.

Table 2 about here

3 Data

The basis of our empirical work is geographical 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.[16] For municipality data, we use digital maps (shape files) from the Federal Agency for Cartography and Geodesy. 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 0.2 percent of the sample).[17]

Further, we need to match municipality data on (taxable) income, population, employment, business tax base, area, human capital and infrastructure with the digital geographical data. These data are provided by the German Statistical Office (destatis), the Federal Employment Agency (BA), and the Federal Office for Building and Regional Planning (BBR). A potential problem arises as we use the 1997 shape file for municipalities in 1986. Instead of georeferencing a map of German municipalities in that year, we prefer the professionally-generated shape file from 1997 and exclude all jurisdictions where the area in 1986 (as reported by the German Statistical Office) deviates by more than 10 percent from its area in the 1997 shape file. This reduces the sample only by 1.1 percent. As a matter of fact, changes of administrative borders were relatively minor in West Germany since the 1980s. Our sample covers about 8,500 municipalities in 1986 and 2010, respectively.

Night light data are available from weather satellites of the US Defense Meteorological

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

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

[17] 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 a 1971 district.

Program (DMSP) measuring luminosity for 30 arc-second grids (about 0.86km^2 at the equator). Despite issues with the precision of measurement, several papers have documented that luminosity data are a reasonable proxy for economic activity at both the national and local level, if other data is either unreliable or non-existent.[18] For our purpose, satellite data are useful as a robustness for income per area and help focussing on a finer grid around the border. This is a particular advantage in boundary discontinuity designs. Following Chen and Nordhaus (2011), we convert the digital numbers reported in the satellite data to radiance.[19] To allow for a better interpretation, we relate the figure to the West German average and plot the data against the log of income per km^2 in Figure 2. As the correlation coefficient is 0.729, luminosity serves as a decent proxy for spatial income. To better compare the empirical results of income and light intensity, we run a linear regression showing that one unit of radiance translates to about 2.4 units of log income per area. Note that we are not interested in fitting the data well, but rather in obtaining a conversion factor between the two outcome variables to do a back-of-the-envelope comparison later. In the following, we employ radiance data primarily on the pixel level (30 arc-second grids) and report robustness checks using averages of radiance in grid cells of 5km×5km in Appendix XX.

Figure 2 about here

In Figure 3, we illustrate the distribution of economic activity in Germany as captured by radiance. The boundaries of the ZRG are highlighted in blue. The maps display the deciles of economic activity during the ZRG program in 1992 (left) and 16 years after the end of the transfer scheme in 2010 (right). Apparently, the spatial distribution of the economy changed substantially over the 18 years between the two observations. While East Germany displayed predominantly low levels of radiance, the 'New Länder' caught up quickly and the number of pixels in the 7-10th decile increased significantly by 2010. Interestingly, also many regions in the eastern part of West Germany gained in their relative position of radiance. Focusing on the ZRG the theory of location fundamentals would predict that firms and residents migrate towards the former centers of economic activity in the East as soon as Germany is reunified and the ZRG transfers cease to be

[18] See Henderson, Storeygard, and Weil (2011) and Chen and Nordhaus (2011) for details.

[19] We use the terms luminosity and radiance interchangeably. The satellite data reports digital numbers ranging from 0 to 63. This may be converted to radiance using the formula $radiance = digitalnumber^{1.5}$ which is denoted in terms of Watts/cm^2/sr/um.

granted. In contrast, new economic geography models would suggest a head start for regions in the ZRG area due to twenty years of federal aid boosting the home market. According to the latter theory, the benefits from reunification should be decreasing in the distance from the inner-German border and ZRG regions should benefit disproportionately compared to observations west of the ZRG. Hence, it is interesting to examine whether the treatment effect persists in spite of the dramatic turnaround in the German economic landscape and finally to reveal whether the economic gain from reunification was more pronounced in previously treated jurisdictions.

Figure 3 about here

Let us now take a closer look at mean values of variables in both the treatment area (ZRG) and the control area (Non-ZRG) up to a distance of 200km from the ZRG border. Note that radiance data is measured on the pixel level while all other data refer to municipalities. We measure income per area, population density and business tax base per area in logarithmic terms and radiance is scaled by the West German average in the respective year in order to facilitate comparability over time.[20] All monetary variables are measured in 1,000 Euros at current prices. Since not all variables are available for each year, we use the year 1985 for the contemporaneous effects of population density, employment rate, and business taxes whereas the contemporaneous effects on income, human capital and radiance refer to the years 1986, 1987, and 1992, respectively. The persistent effects of transfers are estimated for the year 2010 for all variables except infrastructure where we take 2012. Column (3) of Table 3 reports the standard errors of simple t-tests about the equivalence of the averages in the treatment and control groups. For most variables the differences are significantly different from zero. Moreover, income per km^2, radiance, population density, income per household, employment rate, and business tax base per km^2 are higher in the control group than in the group receiving ZRG transfers. This points to a severe selection issue and implies that an unconditional comparison would lead to the conclusion that regional policy fails to establish equal living conditions across space – neither during the program period nor 16 years after the transfers have phased out. We will show in the sequel of the paper that results look quite different once we properly

[20]Note that satellite types and sensor settings vary so that a direct comparisons over years is principally problematic (see Henderson, Storeygard, and Weil, 2012). We identify treatment effects only from cross-sectional variation and, by scaling with the German average, we put the treatment effects on a comparable basis.

address the endogeneity of transfer recipience.

Table 3 about here

4 Identification

Regional policy is usually targeted at very specific groups of recipients. For instance, these can be regions lagging behind in terms of economic performance, cities being confronted with some kind of social tension, firms demonstrating a notable commercial potential while lacking private funds, or rural areas of remarkable environmental value. Hence, the distribution of public subsidies is certainly not random impeding a causal evaluation of such programs. A major part of regional subsidies in Germany during the times of the Cold War was allocated to a well-defined area according to a precise geographic rule. This unique program gives rise to two types of discontinuities that we can exploit to identify causal effects. 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 subsidies. This approach is referred to as *Boundary Discontinuity Design* (BDD) or Geographic Discontinuity Design.[21]

Standard regression discontinuity approaches build on an exogenously imposed cutoff assigning observations to the treatment and control group. In contrast, boundaries are typically not located at random representing a key concern that BDD has to deal with (Lee and Lemieux, 2010). We address this concern in various ways and, most importantly, we present a second identification strategy building on the discontinuity in the political rule that governed the treatment eligibility of regions. In the following, we will lay out the two identification strategies and discuss the corresponding assumptions.

[21] Recent applications include Bayer, Ferrerira, 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 in Peru, and Michalopoulos and Papaioannou (2013) highlighting the importance of pre-colonial institutions for development in Africa.

4.1 Boundary discontinuity

We denote by Y_{i0} and Y_{i1} the potential outcomes of an observation i (municipality or pixel) in the situations with and without treatment, respectively. Our aim is to identify the effect of a binary treatment T_i which corresponds to $\tau = Y_{i0} - Y_{i1}$. The econometric identification problem is that we do not observe the counterfactual situations for an individual unit and at the same time assignment of treatment is not random. Accordingly, 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 as the location of each municipality is described by both latitude and longitude, $\mathbf{L}_i = (L_{ix}, L_{iy})$.[22] Similarly, the boundary between the treatment area A^t and the control area 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, $T = 1\{\mathbf{L}_i \in A^t\}$, where units east of $\mathbf{B}$ receive treatment while those to the west do not. In the boundary discontinuity design, location acts as the so-called forcing variable and we focus on the discontinuity of the 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 $\mathbf{l}^t \in A^t$ and $\mathbf{l}^c \in A^c$ 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 $\mathbf{b} \in \mathbf{B}$. In most of our analysis, we consider the average treatment effect along the whole border, but we explore heterogeneous treatment effects across locations in section 5.3 and link these differences to observable information.

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,

[22] See Papay, Willett, and Murnane (2011) and Egger and Wamser (2011) for two-dimensional RDDs in a non-geographic context.

2009). Hence, municipalities must not be able to (precisely) manipulate their location relative to the treatment border. We will now discuss in turn, whether these conditions hold in our context.

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. For instance, a border may be set such as to divide mountain areas from the lowlands which would imply a discontinuous change in climate and would influence economic activity. Similarly, a river acting as a border would affect transportation costs discontinuously and could violate the continuity assumption. Common ways to proceed include testing for discontinuities in relevant covariates (see Dell, 2010) and removing border segments from the sample that seem to follow a problematic pattern (see Black, 1999). While we pursue both robustness checks in section 6, 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, a large part of the ZRG border did not coincide with the relevant administrative district borders during the time we study.[23] To further improve confidence in our results, we will also exploit the 40km-rule as an instrument in a fuzzy RDD design (see section 4.2).

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 the treatment area was defined according to a transparent rule and its border was never

[23]Roughly 57 percent of the 1,737km ZRG border ceased to represent a district border between 1971 and the 1990s.

changed (despite changes in jurisdictional boundaries), this assumption seems reasonable. Moreover, municipalities that were located outside the ZRG and merged with municipalities in the treatment area could not become eligible for transfers. The jurisdictional boundaries as of 1 January 1971 remained relevant for treatment throughout the duration of the program.[24] 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 BDD both in a parametric and in a non-parametric way. We state the conditional expectations in (1) as $E[Y_{i0}|\mathbf{L}_i] = \alpha + f(\mathbf{L}_i)$ and $E[Y_{i1}|\mathbf{L}_i] = \alpha + \tau + f(\mathbf{L_i})$ where $f(\mathbf{L_i})$ represents flexible polynomials of geographic location. This yields the following regression model:

$$Y_{is} = \alpha + f(\mathbf{L}_i) + \tau T_i + \theta_s + \varepsilon_{is}, \tag{2}$$

where the average treatment effect is captured by $\hat{\tau}$ and θ_s denotes a border segment fixed effect.[25] The control function may include different measures of geographic location. In addition to using polynomials of latitude and longitude we project the geographic location into a one-dimensional space by computing the Euclidean distance to a border point. For now, we compute for each observation the shortest distance to the border, i.e. the perpendicular which we denote by D_i. The one-dimensional distance allows us to account for asymmetric functional forms on each side of the boundary, $g_0(D_i)$ and $g_1(D_i)$, while the coordinates control for the exact location in space. This may be important as units with the same distance to the ZRG border may in fact be quite different if they are located in different parts (e.g. north versus south or different States). The augmented regression model is given by

$$Y_i = \alpha + g_0(D_i) + f(\mathbf{L_i}) + T_i[\tau + g_1(D_i) - g_1(D_i)] + \varepsilon_i. \tag{3}$$

Since $g_1(D_i) - g_0(D_i)$ converges to zero for observations close to the border, the average treatment effect is still captured by $\hat{\tau}$. Note that this specification with asymmetric

[24] In Appendix XX, we report histograms of the assignment variables and show that there is no evidence of sorting in the data.

[25] Note that all results remain valid without the inclusion of border segment fixed effects. The corresponding specifications are shown in Appendix XX.

polynomials puts less restrictions on the conditional expectations than (2) as it allows the geographic location to enter in a different functional form in A^t and A^c.[26] 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). All specifications are estimates for different windows around the ZRG border and we choose the preferred polynomial order on the basis of the AIC.[27] In addition to robust standard errors, we report standard errors that correct for spatial dependence of unknown form using the method introduced by Conley (1999).

Table 4 reports descriptive statistics on the distance of observations from the ZRG border (D_i). Although the treated area corresponds mostly to a narrow band of 40km there are treated observations in the north-east (in particular on the island Fehmarn) located at a distance of up to 100km from the ZRG border. The closest municipal centroid lies at about 87 meters from the ZRG border whereas pixel data reach up to a distance of only one meter. Note that all observations with $D_i > 200$km are dropped from the sample.

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 the treated and control areas, 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 follow Keele and Titiunik (2013) and estimate univariate local linear regressions for a set of 20 border points $\mathbf{b}^1, ..., \mathbf{b}^{20}$. This provides further information about heterogeneity of treatment effects that we explore in section 5.3. For now, we focus on the average across all treatment effects $\tau(\mathbf{b}^1), ..., \tau(\mathbf{b}^{20})$. The alternative approach follows Papay, Willett, and Murnane (2011) and uses 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.[28] The corresponding results depend

[26] This is evident from the corresponding counterfactual expectations which can be stated as $E[Y_{i0}|D_i] = \alpha + g_0(D_i) + f(\mathbf{L}_i)$ and $E[Y_{i1}|D_i] = \alpha + \tau + g_1(D_i) + f(\mathbf{L}_i)$.

[27] Limiting the sample to small windows around the threshold can substitute for including a higher order control function but requires a sufficient density of observations.

[28] See Appendix X.X for a more detailed description of the non-parametric specification and the results of the bivariate approach.

crucially 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).[29]

Table 4 about here

4.2 Fuzzy political discontinuity

One important advantage of our setting is 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 40km to the Iron Curtain at the beginning of 1971 became part of the ZRG. The blue-shaded area in Panel A of Figure 4 illustrates the 40km-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. 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 40km-buffer has no natural relevance and does not correspond to administrative borders, we believe this to be a strong argument that there are no discontinuities in any other covariates at M_0.

Figure 4 about here

We compute isodistance-curves from the Iron Curtain with ArcGIS as illustrated in Panel B of Figure 4. 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 buffer where the area share exceeds 50 percent. Table 4 reports descriptive statistics of M_d for the treatment and control groups.[30] Apparently, none of the control observations was eligible for treatment and all exceptions belong to the treatment group. If these

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

[30] An alternative translation of the treatment rule would be to compute the area share of a district within the 40km-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.

exemptions from the 40km-rule were not too frequent, we should observe a jump in the probability of treatment at the threshold $M_0 = 40$:

$$P(T_d|\widetilde{M}_d) = \begin{cases} h_1(\widetilde{M}_d) & \text{if } \widetilde{M}_d \leq 0 \\ h_0(\widetilde{M}_d) & \text{if } \widetilde{M}_d > 0, \end{cases} \tag{4}$$

where $\widetilde{M}_d = M_d - M_0$ denotes the centered version of the assignment variable. Figure 5 depicts the treatment indicator T_i against the assignment variable M_d. The discontinuity at 40km 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 criteria 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 40km to the eastern border, but 50 percent of the population.[31] Although we cannot account for 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 (Lee and Lemieux, 2010).

Figure 5 about here

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.[32] The parametric approach follows a 2SLS where the second-

[31] 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.

[32] 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. This holds also true in our case.

stage model is given by:

$$Y_{id} = \alpha + f_0(\widetilde{M}_d) + T_i[\tau + f_1(\widetilde{M}_d) - f_0(\widetilde{M}_d)] + \varepsilon_{id}. \tag{5}$$

In the fuzzy RDD, the treatment indicator is instrumented by the political rule which yields a parametric first-stage of the form:

$$T_{id} = \gamma + h_0(\widetilde{M}_d) + R_d[\delta + h_1(\widetilde{M}_d) - h_0(\widetilde{M}_d)] + \nu_{id}, \tag{6}$$

where $R_d = 1[M_d \leqslant M_0]$. Notice that irrespective of whether we use R_d in a linear first-stage or the prediction of a nonlinear first-stage as an identifying instrument for T_{id} in the second-stage, the fuzzy design model is just identified. 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 on the district level d we correct the estimated variance-covariance matrix for clustering at the level of districts and for heteroskedasticity of arbitrary form.

5 Results

We structure the presentation of results in three parts. First, we lay out the effect of regional transfers on income per km^2 (and proxied by radiance) for the two identification approaches described above. In a second step, we study potential channels which may have contributed to the contemporaneous and persistent effects of transfers. Finally, we unveil heterogeneity of the policy measures' effectiveness across time and space.

5.1 Economic Activity

Before turning to regressions, a graphical illustration of the data at the ZRG border is instructive. Figure 6 depicts our measures of economic activity – log income per km^2 in 1986 and radiance in 1992 – against the distance from the ZRG border. With respect to income, we plot predictions and 90 percent confidence intervals for specifications including a 4th order polynomial of distance D_i (left) as well as linear regressions (right). We use intervals of 10km and 50km from the ZRG border. These regressions are fitted separately on both sides of the border and we add a scatter plot of the averages of the outcomes in

equally sized bins. Due to the fine distribution of satellite data, we depict radiance within small windows of 10km and 5km for quadratic and linear fits. All four plots reveal a clear discontinuity at the ZRG border. Note that economic activity tends to decline towards the East, that is towards the Iron Curtain, but jumps to a higher level once observations belong to the treated area. This points to a contemporaneous effect of the subsidies.

Figure 6 about here

We do a similar exercise for 2010 to get a first impression about potential persistent effects of subsidies (Figure 7). It is evident that all graphs show a clear discontinuity of economic activity at the ZRG border even 16 years after the program was stopped. Again, the 90 percent confidence intervals do not overlap. While such graphical analysis provides a transparent first assessment of whether a discontinuity exists, it provides only limited information about statistical significance and the magnitude of the effects. We thus turn to regression analysis.

Figure 7 about here

Regression results confirm the first impressions from the plots: Regional transfers to the Zonenrandgebiet exerted a strong and significant effect on economic activity. Income per km^2 is predicted to be about 50 percent higher than in the counterfactual without regional subsidies in 1986. Moreover, the effects are still clearly visible in 2010 with estimates fluctuating around 50 percent in alternative specifications. We report results for the boundary discontinuity design in Table 5 while the fuzzy RDD is captured in Table 6. The first two specifications of the BDD include only 3rd and 5th order distance control functions and segment fixed effects. Using the above-mentioned windows, these polynomials turn out optimal according to the AIC. Columns (3) and (4) correspond to the augmented regressions in (3) and include coordinate controls in addition to the Euclidean distance.[33] Again we choose the polynomial orders on the basis of the AIC.[34] Note that we choose lower order polynomials for distance control than for coordinate

[33]We may either choose a very narrow window and a low-order polynomial of the geographic control function or a wider window and a more flexible control function. For our benchmark specifications with municipal data, we restrict our sample to observations within 100km of the ZRG border. Using pixel level we data employ only observations within a 10km distance from the border.

[34]The cubic polynomial of latitude and longitude is defined as $L_{ix} + L_{iy} + L_{ix}^2 + L_{iy}^2 + L_{ix}^3 + L_{iy}^3 + L_{ix}L_{iy} + L_{ix}^2L_{iy} + L_{ix}L_{iy}^2$.

control because the bivariate control function requires more parameters to be estimated than the corresponding univariate control function. Finally, columns (5)-(7) report the results for non-parametric specifications where the window is determined endogenously. Among these alternative regressions, we focus on two preferred specifications. Among the parametric regressions, column (4) accounts for both distance to the ZRG border and coordinates with a reasonably high order of the polynomial. The non-parametric approach with optimal bandwidth h^* is less restrictive with respect to the functional form of the control function. In the fuzzy RDD (Table 6), columns (1) and (2) use 3rd and 5th order polynomials of M_d as the control function while columns (3)-(5) report non-parametric regression outcomes with the optimal bandwidth h^* and manual adjustments. We prefer the non-parametric estimation with optimal bandwidth h^*. The estimates are very similar across specifications, especially between BDD and fuzzy RDD. The latter establishes confidence in the consistent estimation of the treatment effect. Notice that nearly all specifications are significant at the 1% level. This is true for both robust and Conley standard errors in the BDD and clustered standard errors in the fuzzy RDD.

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 evident from Table 3 that municipalities in the treatment area have a much lower average income per km^2 rendering large effects of transfers more plausible.

Interestingly, we also find evidence for long-run implications of regional transfers. After the program was eventually stopped in 1994, there was no reason to relocate firms nor the place of residence as this would incur fixed costs. Moreover, the former treatment regions may have remained more attractive than the neighboring regions due to superior infrastructure, a higher stock of capital or a larger home market. Thus, it is not implausible to still see a difference in economic activity at the former ZRG border.

Table 5 about here

Table 6 about here

These insights are confirmed by relying on luminosity as a proxy for income per km^2, as shown in Tables 5 and 6. According to our preferred specifications, transfers raised

light intensity by 10-14 percentage points in 1992. Moreover, the impact was still visible in 2010 at a difference of 8-16 percentage points. Based on the precision of the estimates, we cannot rule out that the contemporaneous effects are identical to the persistent effects. In the fuzzy RDD, the effect is estimated at about 10 percent in 1992 and 15 percent in 2010. According to the conversion factor of 2.4 from Figure 2, a 15 percentage point increase in radiance would be associated with an increase of income per km^2 of about 35 percent. Note, however, that light intensity is only a proxy rendering a sound comparison with income data difficult for a number of reasons (see Henderson, Storeygard, and Weil, 2010). Nevertheless, the benefit of having more disaggregated data at hand allows precise and more convincing estimation of treatment effects at the border as required by the asymptotic properties of the BDD.

5.2 Channels of persistence

What are the underlying channels of higher economic activity in the Zonenrandgebiet? We can conclude from the persistent effect of regional transfers that there must be something beyond a pure consumption effect. We study a number of potential mechanisms through which regional transfers might operate. On the labor side, it is natural to check whether migration led to higher population density which in turn translates to higher income per km^2. In addition, transfers might have stimulated higher employment rates. Finally, we check whether the share of employees with secondary or tertiary education differs systematically across treatment and control regions. Equivalently to labor, capital might be the channel through which economic activity increased. Unfortunately, we do not have data on the capital stock, but information about the business tax base at the municipality level. As a substantial amount of subsidies was dedicated towards infrastructure investments, we use an index that describes how well a municipality is connected with highways, long-distance train connections or airports. We only report the first of these indices as results are very similar. The index is based on the commuting time by car and the major source to describe the quality of infrastructure as an underlying criterion for the eligibility of regional funds in Germany today. As a final point, we report results for income per household as a tentative measure for productivity.

To start, we plot the data in a similar fashion as before to get a first idea about discontinuities at the ZRG border. We observe very pronounced jumps at the ZRG border

for population density and the business tax base such that 90-percent confidence intervals do not overlap. Discontinuities are also visible for employment share and infrastructure (time to next highway) while education varies rather smoothly at the border. As we pointed out in the previous subsection, we have to rely on regression analysis for statistical inference.

Figures 8 and 9 about here

Table 7 summarizes contemporaneous and persistent effects applying the boundary discontinuity design for the channels we study. For the sake of brevity, we only report 2nd and 3rd order polynomials of the augmented coordinate control specifications 3 and non-parametric regressions based on the optimal bandwidth h^*. A large set of alternative specifications is shown in Appendix X.X. Table 8 follows the same structure and reports results for the fuzzy regression discontinuity design.

We find strong evidence that the regional transfer program raised population density in the Zonenrandgebiet. Relying on our preferred specifications (3rd order coordinate control and non-parametric h^*), population density is estimated to be about 40 percent higher in 1985 than in the counterfactual without transfers. The effect is even higher in 2010 implying a strong persistence. In the fuzzy RDD, estimates are higher for 3rd order coordinate control, but only half the size in the non-parametric specifications. Again, we find a significant persistent effect of transfers on population density.

We also find evidence for a higher employment rate. In the BDD, transfers have led to a 5.4-percentage-point higher employment rate in 1985, further increasing in 2010. Similar to population density, estimates are only about half the size in the non-parametric fuzzy RDD (2.4 percent versus 2.6 percent). A further reason for higher income per km^2 might be a higher educational degree of employees in the ZRG. The composition of the workforce may differ because individuals make a different educational choice or because it was mainly high-skilled workers who migrated into the ZRG. Although we cannot differentiate between both alternatives, we find no evidence of transfers throughout all specifications.

Turning to the capital side, regional subsidies have raised the business tax base per km^2 by around 80 percent in 1985 (BDD). In the fuzzy RDD, the point estimates vary between 70 percent and even more than 100 percent. However, we cannot rule out that the estimates are statistically different from each other. Looking at persistence in 2010,

we cannot confirm that the business tax base has decreased which implies that firms attracted to the ZRG do not leave once subsidies phase out. Estimates vary between 66 and 94 percent. Overall, the business tax base seemed to have responded more to transfers than households. This may confirm the common view that capital is more mobile than labor. Another explanation roots in the institutional setting of the transfer program. Firms were directly targeted through investment subsidies, tax allowances and privileged treatment in public tendering.

A second target of investment subsidies was public infrastructure. Unfortunately, appropriate data for the 1980s is not available. Instead, we rely on a current index of the quality of infrastructure based on the commuting time to the next highway in 2012. The argument is that a better road network reduces estimated driving time by car. Our regression results point to a significant effect of infrastructure investments in the Zonenrandgebiet. In the BDD, commuting to the next highway is estimated to take about 20 percent less time inside the ZRG than in the absence of infrastructure subsidies. In the fuzzy RDD, effects even range between -30 and -63 percent.

Given that our findings do not indicate that transfers to the Zonenrandgebiet have affected educational levels of workers, we may cautiously interpret income per household as a proxy for productivity. We indeed find significant contemporaneous effects of about 3.6-6.9 percent (BDD) and up to 15.9 percent in the fuzzy RDD. However, relying on the fuzzy discontinuity approach, we do not observe any persistence in 2010.

Tables 7 and 8 about here

Summing up, our results confirm the effectiveness of the regional transfer program. We find persistent effects on income per km^2 16 years after the end of the transfer regime and have shown that labor migration and the relocation of firms (business tax base) are important channels behind this aggregate effect. These insights have relevant implications for the debate on whether locational advantage or market externalities explain economic density better. If the latter played no role at all, we should observe a reversion back to the pre-treatment equilibrium. Our results are not in accordance with this hypothesis and provide evidence for the explanatory power of economic geography models.

5.3 Heterogeneous treatment effects

In this section, we examine treatment effects over time and across space. Further, we study whether the fall of the Iron Curtain and German reunification exerted heterogeneous effects on the ZRG, depending on the location of municipalities.

6 Sensitivity

TO BE WRITTEN.

7 Concluding remarks

TO BE WRITTEN.

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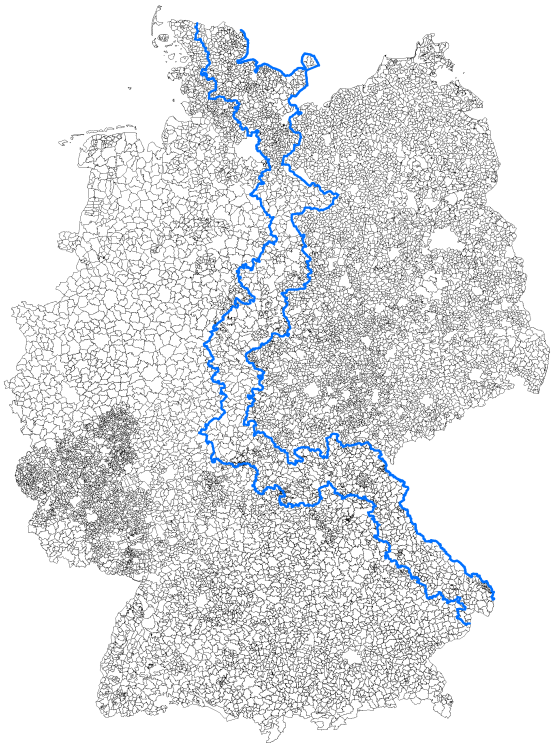
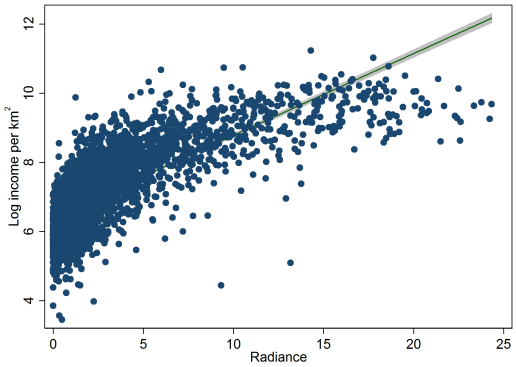
Figures

Figure 1: The German Zonenrandgebiet, 1971-1994



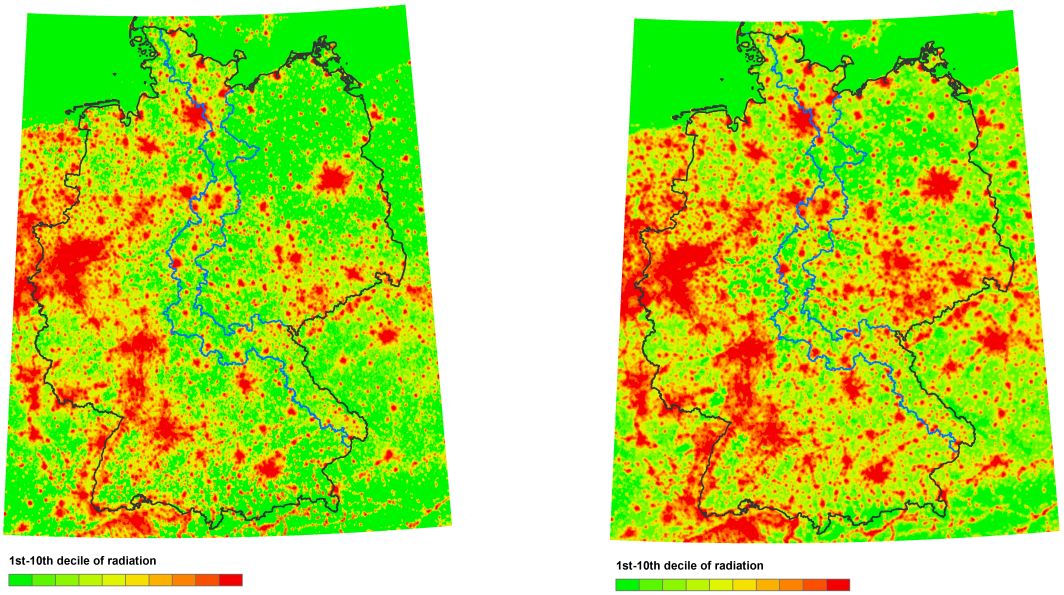
Note: The blue lines mark the western border of the ZRG and the Iron Curtain, respectively. The black lines represent the municipalities according to the 1997 classification. The border of the ZRG follows the administrative districts according to the 1971 classifications which was modified substantially in the mid-1970s.

Figure 2: Night light and income per Area



Note: This plot illustrates log(income per area) and radiance per area for West German municipalities in the year 1992. Radiance is measured in terms of the German average in 1992. The estimated coefficient of the linear fit is about 2.4.

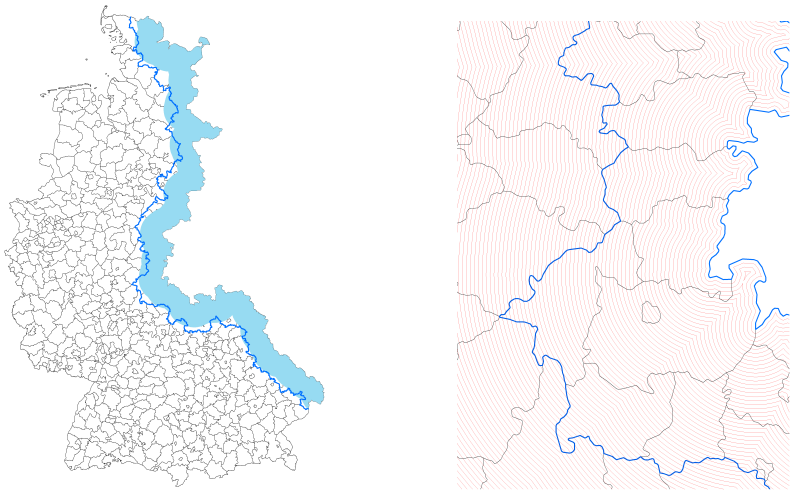
Figure 3: Night-Light Germany, 1992 vs. 2010



Note: The blue lines mark the border of the ZRG and the Iron Curtain

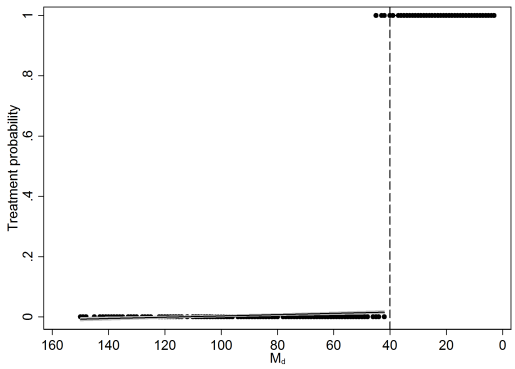
Figure 4: Assignment variable

Panel A. Panel B.



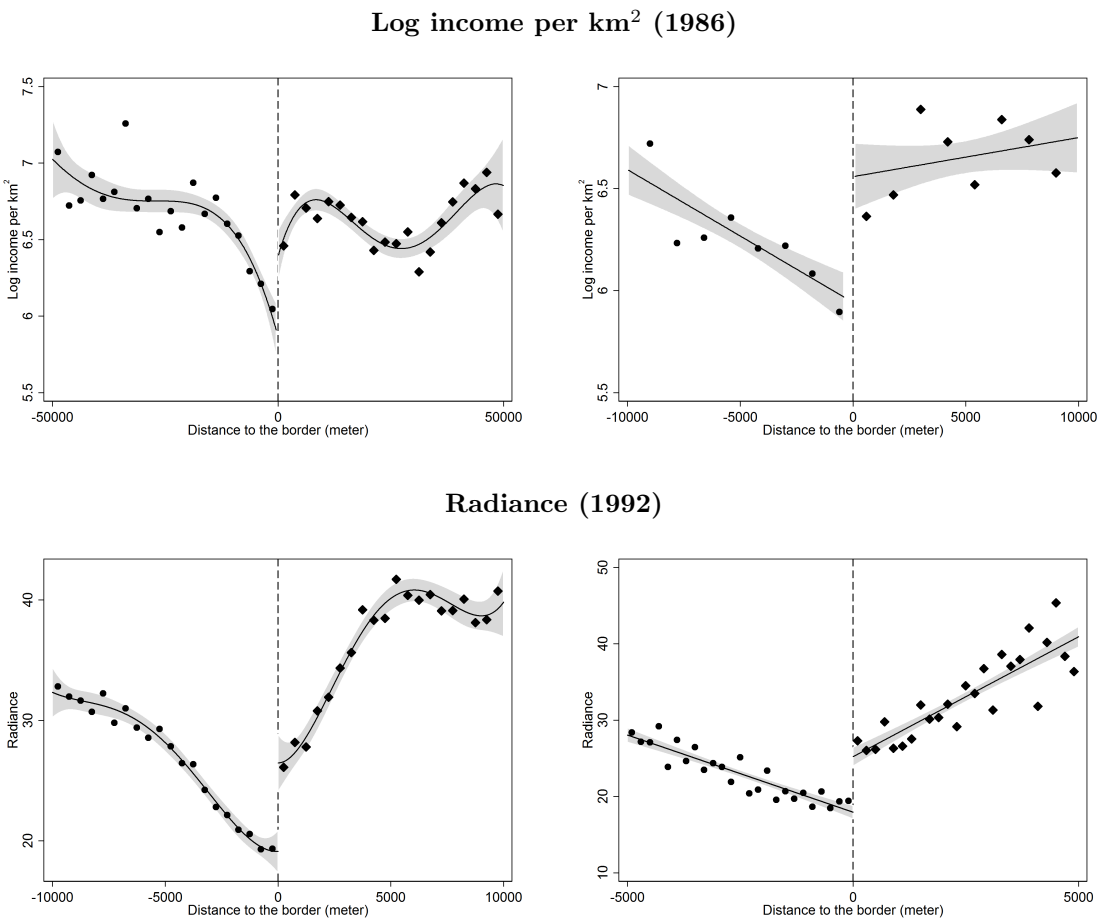
Notes: The above maps show district borders according to the 1971 classification. The light blue area in the left hand map marks the 40km distance from the Iron Curtain. The right hand map illustrates the buffer lines (in red) drawn in 1km intervals from the Iron Curtain.

Figure 5: Treatment probability



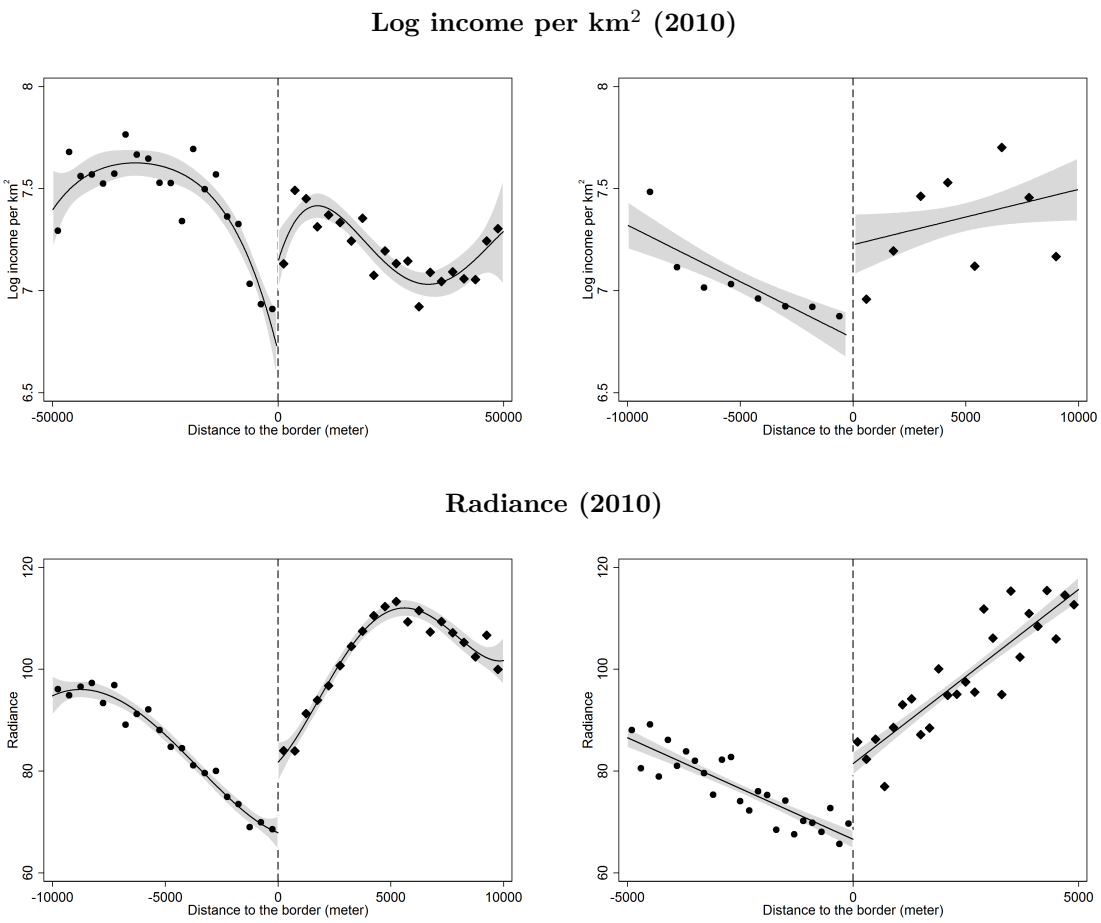
Notes: The assignment variable is measured on the district level. We consider only districts overlapping with a 150km buffer from the Iron Curtain, all districts further to the West are dropped from the sample.

Figure 6: Discontinuities in Economic Activity – Contemporaneous Effect



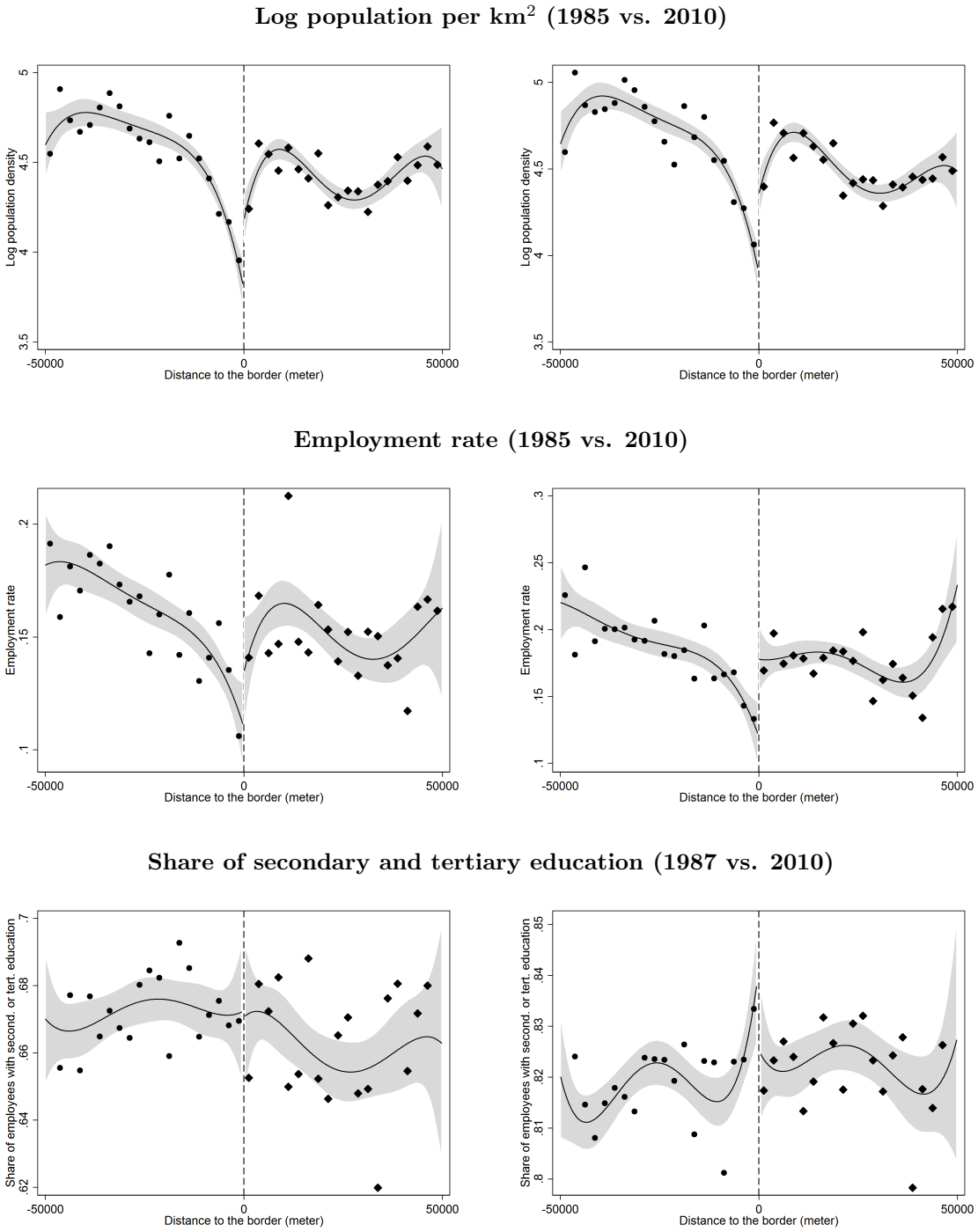
Notes: They grey-shaded area marks the 90-percent confidence interval. The graphs illustrate asymmetric 4th order polynomials (left) as well as linear fits (right) for both outcomes. The binsize is 200 and 500 meters in radiance plots with 4km and 10km window. respectively. With regard to income per km^2 we chose a binsize of 1,000 and 2,000 meters for the plots with 10km and 50km windows, respectively. The data on income per km^2 and radiance refer to 1986 and 1992, respectively.

Figure 7: Discontinuities in Economic Activity – Persistence



Notes: They grey-shaded area marks the 90-percent confidence interval. The graphs illustrate asymmetric 4th order polynomials (left) as well as linear fits (right) for both outcomes. The binsize is 200 and 500 meters in radiance plots with 4km and 10km window. respectively. With regard to income per km^2 we chose a binsize of 1,000 and 2,000 meters for the plots with 10km and 50km windows, respectively. The data on income per km^2 and radiance refer to 2010.

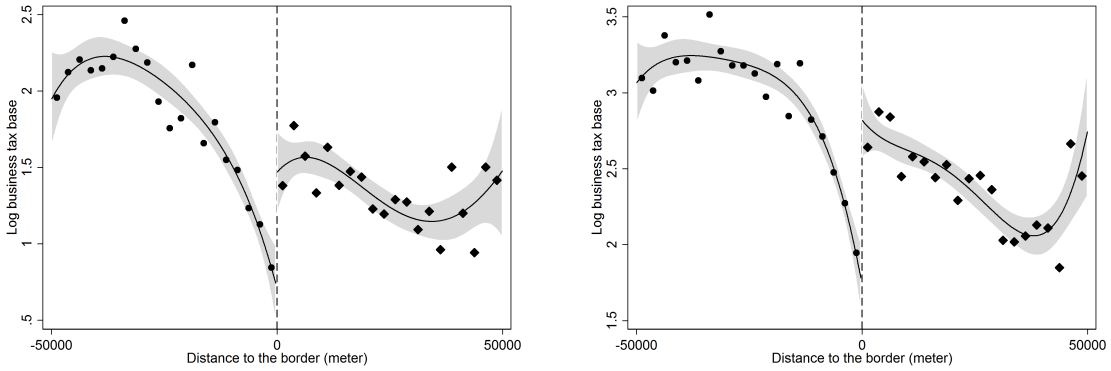
Figure 8: Discontinuities in channels I



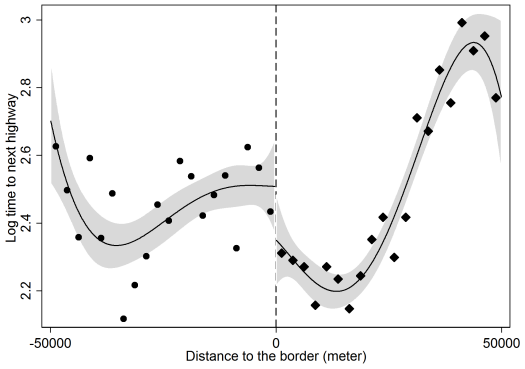
Notes: The graphs illustrate asymmetric 4th order polynomials and scatter plots with binsize of 2,000 meters for all outcomes.

Figure 9: Discontinuities in channels II

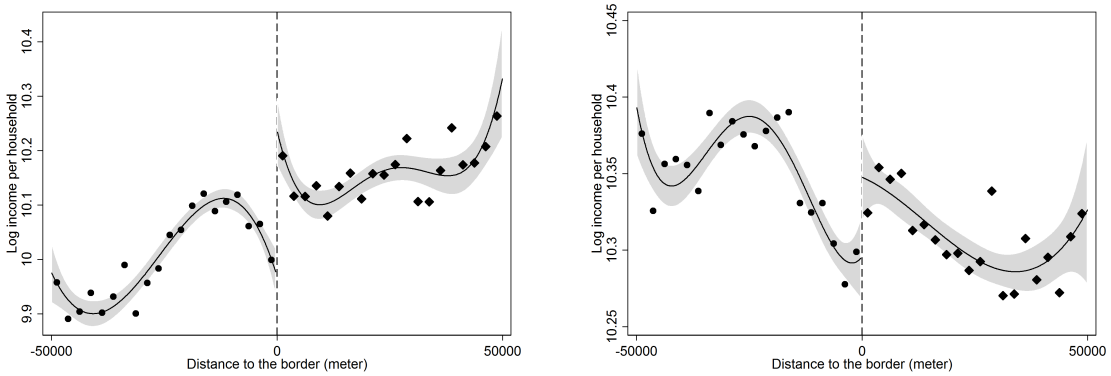
Log business tax base (1985 vs. 2010)



Log time to next highway (2012)



Log income per household (1986 vs. 2010)



Notes: The graphs illustrate asymmetric 4th order polynomials and scatter plots with binsize of 2,000 meters for all outcomes.

Tables

Table 1: Characteristics of the Zonenrandgebiet

	No. districts		No. municipalities		Area ZRG	Pop. ZRG
	Non-ZRG	ZRG	Non-ZRG	ZRG	in %	in %
West Germany	411	94	6,910	1,626	18.6	12.0
Schleswig-Holstein	12	15	417	714	66.5	71.4
Lower Saxony	55	25	743	309	26.7	29.2
Hesse	33	12	327	103	27.8	16.9
Bavaria	114	42	1,556	500	24.7	18.7

Notes: The States (Länder) Schleswig-Holstein, Lower Saxony, Hesse, and Bavaria are ordered according to their geographical location from north to south. The districts correspond to the 1971 classification while we use the 1997 classification for municipalities. Population shares are measured in 1980.

Table 2: Treatment intensity

	Transfers (in million Euros)			Transfers/capita (Euro)		Transfers/km^2 (Euro)	
	Non-ZRG	ZRG	% ZRG	Non-ZRG	ZRG	Non-ZRG	ZRG
1972-1975	1,057.1	710.6	40.2	20.7	101.9	232.3	683.7
1976-1979	1,715.0	919.4	34.9	33.6	131.9	376.8	884.5
1980-1983	2,211.0	1,304.1	37.1	43.3	187.1	485.8	1,254.6
1984-1987	2,260.7	1,933.5	46.1	44.2	277.3	496.7	1,860.2

Notes: These figures are based only on the subsidies from the Joint Task and the Investment Premium Law. Population levels in 1980 are used for all time periods. Based on investment premia and subsidies only. Source: Own calculations based on the Joint Task's Rahmenpläne No. 1, 3, 9, and 13. Available at www.bundestag.de.

Table 3: Descriptive statistics of outcomes

	Mean					Observations	
	Non-ZRG	ZRG	All	S.E.		Non-ZRG	ZRG
Log income per km^2							
1986	6.726	6.582	6.680	.034	***	3,284	1,518
2010	7.614	7.200	7.486	.032	***	3,219	1,437
Radiance							
1992	57.224	33.852	51.262	.331	***	255,242	87,407
2010	130.359	92.456	120.694	.473	***	255,244	87,364
Log population per km^2							
1985	4.726	4.407	4.625	.030	***	3,351	1,542
2010	4.865	4.502	4.749	.030	***	3,308	1,549
Employment rate							
1985	.187	.151	.175	.010	***	3,265	1,515
2010	.209	.176	.199	.005	***	3,211	1,433
Share of employees with secondary or tertiary education							
1987	.668	.664	.667	.003		1,976	609
2010	.814	.822	.816	.002	***	2,587	910
Log business tax base per km^2							
1985	2.010	1.351	1.787	.052	***	2,847	1,461
2010	3.314	2.425	3.041	.048	***	3,195	1,416
Log time to next highway							
2012	2.402	2.433	2.412	.028		3,367	1,569
Log income per household							
1986	9.954	10.153	10.017	.009	***	3,284	1,518
2010	10.370	10.306	10.351	.006	***	3,219	1,437

Notes: We dropped all observations with a distance of more than 200km to the ZRG border. All monetary measures are denoted in current 1,000 Euros. Income per household refers to the ratio of total taxable income and the number of taxable households. Time to highway is in minutes.

Table 4: Distances & assignment variable

	ZRG				Non-ZRG			
	Mean	Std.	Min	Max	Mean	Std.	Min	Max
Distance (municipality)	22,973	16,105	87	102,543	101,292	56,674	269	199,958
Distance (pixel)	22,144	16,517	1	103,790	92,698	57,635	1	199,999
M_d	19.511	11.759	3	45	94.778	29.616	42	150

Notes: Distances are in meter. The assignment variable minimum distance (in km) from the Iron Curtain that includes majority share of district area (M_d) is determined on the district level according to the 1971 classification. Each municipality and each pixel is uniquely assigned to a district. Three districts (Schlüchtern, Einbeck, and Peine) were misassigned as they received treatment though not eligible according to the rule. Of those districts being eligible all received treatment. We dropped all observations with a distance of more than 200km to the ZRG border.

Table 5: Economic activity – Boundary discontinuity

	Distance Control		Coordinate Control		Non-parametric		
	3rd	5th	2nd	3rd	h^*	$0.8 \times h^*$	$1.2 \times h^*$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Log income per km^2			
1986							
ATE	0.629***	0.574***	0.268***	0.487***	0.421***	0.575***	0.293***
	(0.122)	(0.148)	(0.079)	(0.099)	(0.085)	(0.109)	(0.073)
	[0.134]	[0.161]	[0.091]	[0.110]	-	-	-
R^2	0.15	0.15	0.11	0.15	-	-	-
AIC	10351	10340	10511	10358	-	-	-
Obs.	3,745	3,745	3,745	3,745	2,780	1,870	3,353
2010							
ATE	0.524***	0.458***	0.219***	0.484***	0.498***	0.614***	0.337***
	(0.124)	(0.151)	(0.078)	(0.099)	(0.090)	(0.121)	(0.076)
	[0.139]	[0.168]	[0.089]	[0.110]	-	-	-
R^2	0.19	0.19	0.16	0.20	-	-	-
AIC	9539	9525	9669	9510	-	-	-
Obs.	3,590	3,590	3,590	3,590	2,367	1,445	2,983
				Radiance			
1992							
ATE	0.105***	0.150***	0.124***	0.101***	0.164***	0.326***	0.193***
	(0.036)	(0.054)	(0.028)	(0.036)	(0.034)	(0.046)	(0.027)
	[0.051]	[0.051]	[0.057]	[0.060]	-	-	-
R^2	0.05	0.05	0.04	0.06	-	-	-
AIC	146707	146700	146965	145764	-	-	-
Obs.	48,391	48,391	48,391	48,391	26,687	13,438	41,151
2010							
ATE	0.083***	0.116***	0.111***	0.081***	0.155***	0.020	0.223***
	(0.026)	(0.039)	(0.020)	(0.026)	(0.020)	(0.027)	(0.018)
	[0.040]	[0.037]	[0.044]	[0.045]	-	-	-
R^2	0.06	0.06	0.04	0.09	-	-	-
AIC	115970	115964	116607	114319	-	-	-
Obs.	48,391	48,391	48,391	48,391	40,484	22,935	45,202

Notes: ***, **, * denote significance at the 1, 5, and 10 level, respectively. Robust standard errors in parenthesis, Conley (1999) standard errors in squared brackets. Standard errors in (5)-(7) are computed according to Imbens and Lemieux (2008). Income per km^2 and radiance are measured on the municipality and pixel level, respectively. With municipality data we drop all observations outside a 100km window of the ZRG border. With pixel data we focus on observations within a 10km window of the ZRG border. Columns (1) and (2) refer to specifications including a 4th and 5th order polynomial of Euclidean distance plus segment fixed effects (segments have a side length of 100km). Specifications (3)-(4) include 2st-3rd order polynomials of Euclidean distance plus quadratic up to a cubic polynomials of longitude and latitude. All specifications use asymmetric polynomials of distance which allow for different functional forms within and outside of the ZRG. Columns (5)-(7) refer to non-parametric specifications where the bandwith h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012).

Table 6: Economic Activity – Fuzzy Political Discontinuity

	Parametric, M_d		Non-parametric		
	3rd	5th	h^*	$0.8 \times h^*$	$1.2 \times h^*$
	(1)	(2)	(3)	(4)	(5)
Log income per km^2					
1986					
ATE	0.505***	0.645***	0.556***	0.482***	0.568***
	(0.180)	(0.191)	(0.087)	(0.099)	(0.082)
R^2	0.020	0.021	-	-	-
AIC	10867	10863	-	-	-
Obs.	3,747	3,747	2,040	1,528	2,454
2010					
ATE	0.428**	0.519**	0.351***	0.332***	0.372***
	(0.202)	(0.225)	(0.081)	(0.090)	(0.077)
R^2	0.051	0.053	-	-	-
AIC	10074	10070	-	-	-
Obs.	3,592	3,592	2,171	1,683	2,436
Radiance					
1992					
ATE	0.333*	0.417*	0.095***	0.223***	0.288***
	(0.181)	(0.247)	(0.021)	(0.018)	(0.016)
R^2	0.023	0.024	-	-	-
AIC	689676	689478	-	-	-
Obs.	194,090	194,090	71,680	95,939	116,533
2010					
ATE	0.260**	0.331*	0.151***	0.210***	0.224***
	(0.129)	(0.174)	(0.012)	(0.010)	(0.010)
R^2	0.020	0.022	-	-	-
AIC	601049	600581	-	-	-
Obs.	229,822	229,822	94,666	120,960	140,019

Notes: ***, **, and * denote significance at the 1, 5, and 10% level, respectively.

Robust standard errors clustered at the district level in parenthesis. Radiance is measured in terms of the West-German average in the respective year. We use municipality and pixel level data from income per km^2 and radiance, respectively. Observations with $M_d > 150$ are dropped from the sample. Columns (1),(2) and (6),(7) refer to fuzzy RDD specifications using a two-stage instrumental variables procedure where we include a 3th and 5th order polynomial of the instrument. Note that the instrument is highly relevant in each of the first-stages. Specifications (3)-(5), and (8)-(10) refer to non-parametric specification where the bandwith h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012).

Table 7: Channels – Boundary Discontinuity

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	2nd	3rd	h^*	2nd	3rd	h^*
Log population per km^2 (1985 vs. 2010)						
ATE	0.240***	0.447***	0.446***	0.298***	0.490***	0.520***
	(0.068)	(0.085)	(0.082)	(0.071)	(0.089)	(0.086)
	[0.077]	[0.093]	-	[0.079]	[0.096]	-
R^2	0.18	0.21	-	0.18	0.20	-
AIC	9707	9572	-	9745	9614	-
Obs.	3,824	3,824	2,521	3,791	3,791	2,431
Employment rate (1985 vs. 2010)						
ATE	0.037***	0.042***	0.046***	0.043***	0.058***	0.061***
	(0.011)	(0.014)	(0.012)	(0.012)	(0.016)	(0.014)
	[0.011]	[0.012]	-	[0.012]	[0.015]	-
R^2	0.07	0.08	-	0.06	0.07	-
AIC	-4452	-4470	-	-3559	-3588	-
Obs.	3,726	3,726	2,979	3,580	3,580	2,392
Share of employees with secondary or tertiary education (1987 vs. 2010)						
ATE	-0.003	-0.005	-0.004	-0.000	-0.004	0.003
	(0.009)	(0.011)	(0.011)	(0.006)	(0.008)	(0.006)
	[0.006]	[0.007]	-	[0.004]	[0.004]	-
R^2	0.14	0.19	-	0.04	0.06	-
AIC	-4306	-4421	-	-7108	-7167	-
Obs.	1,791	1,791	1,059	2,515	2,515	1,680
Log business tax base per km^2 (1985 vs. 2010)						
ATE	0.512***	0.744***	1.061***	0.466***	0.806***	0.867***
	(0.124)	(0.156)	(0.166)	(0.120)	(0.152)	(0.150)
	[0.133]	[0.165]	-	[0.133]	[0.170]	-
R^2	0.19	0.21	-	0.16	0.18	-
AIC	12894	12792	-	12528	12445	-
Obs.	3,525	3,525	1,988	3,553	3,553	2,139
Log time to next highway (2012)						
ATE				-0.348***	-0.218**	-0.259***
				(0.072)	(0.090)	(0.069)
				[0.107]	[0.118]	-
R^2				0.08	0.13	-
AIC				10000	9795	-
Obs.				3,818	3,818	3,101
Log income per household (1986 vs. 2010)						
ATE	-0.007	0.052***	0.029*	-0.003	0.069***	0.045***
	(0.014)	(0.018)	(0.016)	(0.013)	(0.017)	(0.016)
	[0.018]	[0.023]	-	[0.015]	[0.019]	-
R^2	0.72	0.74	-	0.15	0.20	-
AIC	-2323	-2595	-	-3003	-3251	-
Obs.	3,745	3,745	2,647	3,590	3,590	2,123

Notes: Robust standard errors in parenthesis, Conley (1999) standard errors in squared brackets. We drop all observations outside a 100km window of the ZRG border. Columns (1) and (2) refer to specifications including a 2nd and 3rd order polynomial of longitude and latitude plus asymmetric 2nd and 3rd order polynomials of Euclidean distance from the ZRG border. Columns (3)-(5) and (8)-(10) refer to nonparametric specifications where the bandwith h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012). The share of employees with secondary or tertiary education is measured in 1987 instead of 1985. Data on infrastructure is not available for the 1980s. The infrastructure data refer to 2012.

Table 8: Channels – Fuzzy Political Discontinuity

	Contemporaneous effects			Persistent effects		
	Coordinate control		Non-parametric	Coordinate control		Non-parametric
	2nd	3rd	h^*	2nd	3rd	h^*
Log population per km^2 (1985 vs. 2010)						
ATE	0.340**	0.574***	0.202***	0.394**	0.544***	0.222***
	(0.140)	(0.171)	(0.074)	(0.162)	(0.199)	(0.076)
R^2	0.151	0.155	-	0.149	0.151	-
AIC	10409	10393	-	10455	10446	-
Obs.	4,098	4,098	2,574	4,065	4,065	2,477
Employment rate (1985 vs. 2010)						
ATE	0.023*	0.038*	0.024*	0.021*	0.045**	0.026*
	(0.013)	(0.020)	(0.013)	(0.012)	(0.018)	(0.013)
R^2	0.071	0.071	-	0.067	0.069	-
AIC	-3339	-3340	-	-3810	-3814	-
Obs.	3,014	3,014	2,310	3,832	3,832	2,270
Share of employees with secondary or tertiary education (1987 vs. 2010)						
ATE	-0.006	-0.006	-0.002	-0.001	-0.008	-0.003
	(0.012)	(0.016)	(0.009)	(0.007)	(0.010)	(0.007)
R^2	0.143	0.144	-	0.047	0.048	-
AIC	-4557	-4557	-	-7509	-7508	-
Obs.	1,885	1,885	1,194	2,671	2,671	1,047
Log business tax base per km^2 (1985 vs. 2010)						
ATE	0.650**	1.033***	0.708***	0.306	0.656**	0.675***
	(0.256)	(0.345)	(0.138)	(0.205)	(0.272)	(0.152)
R^2	0.130	0.133	-	0.148	0.151	-
AIC	6258	6254	-	13285	13274	-
Obs.	1,815	1,815	1,212	3,794	3,794	1,749
Log time to next highway (2012)						
ATE				-0.538**	-0.632*	-0.299***
				(0.253)	(0.344)	(0.083)
R^2				0.061	0.062	-
AIC				10669	10667	-
Obs.				4,092	4,092	2,389
Log income per household (1986 vs. 2010)						
ATE	0.113**	0.159***	0.096***	0.011	-0.011	0.020
	(0.047)	(0.054)	(0.026)	(0.045)	(0.066)	(0.013)
R^2	0.808	0.808	-	0.068	0.074	-
AIC	-2439	-2444	-	-3035	-3057	-
Obs.	3,012	3,012	1,175	3,833	3,833	2,632

Notes: Robust standard errors in parenthesis, Conley (1999) standard errors in squared brackets. We drop all observations outside a 100km window of the ZRG border. Columns (1) and (2) refer to specifications including a 2nd and 3rd order polynomial of longitude and latitude plus asymmetric 2nd and 3rd order polynomials of Euclidean distance from the ZRG border. Columns (3)-(5) and (8)-(10) refer to non-parametric specifications where the bandwith h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012). The share of employees with secondary or tertiary education is measured in 1987 instead of 1985. Data on infrastructure is not available for the 1980s. The infrastructure data refer to 2012.