American Economic Journal: Economic Policy 2018, 10(4): 344–374
https://doi.org/10.1257/pol.20160395

The Persistent Effects of Place-Based Policy: Evidence from the West-German Zonenrandgebiet[†]

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Using a natural experiment, we show that temporary place-based subsidies generate persistent effects on economic density. The spatial regression discontinuity design controls for continuous local agglomeration externalities, so we attribute an important role to capital formation in explaining persistent spatial patterns of economic activity. This persistence is driven by higher local public investment levels, which local governments could maintain after the end of the program because of a persistently higher tax base. We also find evidence for significant local relocation of economic activity, which raises doubts that the net effect of the policy is positive. Finally, we show that transfers have capitalized in land rents such that pretreatment landowners have benefited from the program. (*JEL* H71, H76, O18, R11, R12, R51, R58)

When supporting underdeveloped regions, policymakers often hope that temporary transfers establish self-sustaining long-run economic development. The effort is substantial. For example, the EU dedicated approximately one-third of its overall 2014–2020 budget to regional policy, amounting to more than €350 billion (European Commission 2011a). The US does not have a unified regional policy, but annual spending on regional development programs is estimated at 95 billion US dollars per year (Durbin, Emerson, and Cleaver 2012). China has also installed regional policies that resemble those in the EU in terms of instruments and magnitude (European Commission 2011b).

Despite these efforts, little is known about the long-term consequences of these programs and their underlying mechanisms (Neumark and Simpson 2015).[1] The use

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[†] Go to https://doi.org/10.1257/pol.20160395 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1] The literature on place-based policies has mostly investigated the effects of transfers *during* programs, e.g., Busso, Gregory, and Kline (2013) evaluates the federal empowerment zones program in the United States; Glaeser

of a natural experiment from Germany allows us to make progress in this direction. 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 with either 50 percent of their area or population within a distance of 40 kilometers (km) from 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 from adjacent markets on the other side of the Iron Curtain. Policymakers were afraid that the remoteness could cause substantial out-migration 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 toward East Germany after 1990, the place-based policy was phased out until 1994. We are therefore able to study both the contemporaneous and persistent effects of the policy.

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 (RDD) 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 further support our results' validity, we also exploit the political rule that governed the location of the treatment border. As the treatment probability of districts jumps at a distance of 40 km from the Iron Curtain, we apply a classical regression discontinuity design. The advantage is that the 40-km rule does not coincide with any administrative boundary or with geographic features that may cause discontinuities in relevant determinants for outcome. Depending on parametric or nonparametric estimation and the choice of the control function, we find that regional transfers led to higher income per square kilometer (km^2) in the treatment area by approximately 30–50 percent in 1986. Undertaking the same exercise for 2010, that is, 16 years after the program was eventually stopped, there is no indication that the estimated effects have diminished. A similar pattern emerges for capital stock and employment.[4]

However, the estimated increase in income of up to 50 percent cannot be interpreted as *new* economic activity, as we find evidence for substantial local relocation

and Gottlieb (2008) examines the place-based policy of the Appalachian Regional Commission; Gobillon, Magnac, and Selod (2012) studies the French enterprise zone program; and Becker, Egger, and von Ehrlich (2010) focuses on income and employment effects of EU Structural Funds.

[2] See Bundesministerium der Justiz (1971) and Ziegler (1992, 9). Zonenrandgebiet 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."

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

[4] Kline and Moretti (2014) finds persistent effects of a place-based policy in the United States, while Ahlfeldt, Maennig, and Richter (2017) finds no persistence of urban renewal policies in Berlin.

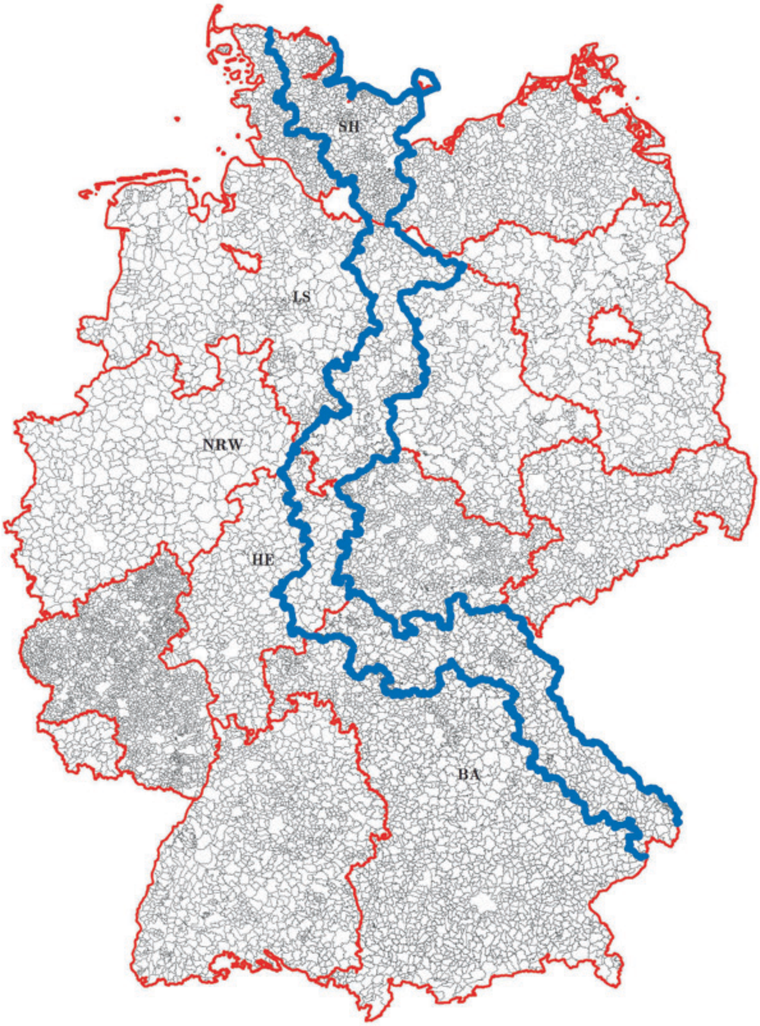

Figure 1. The German Zonenrandgebiet (ZRG), 1971–1994

Notes: The two bold lines mark the western border of the ZRG and the Iron Curtain. The other lines represent the municipalities according to the 1997 classification and the state borders. The border of the ZRG follows the administrative districts according to the 1971 classification, which was modified substantially in the mid-1970s. In most of our analysis, we consider the states Schleswig-Holstein (SH), Lower Saxony (LS), North Rhine-Westphalia (NRW), Hesse (HE), and Bavaria (BA).

within 20–30 km on both sides of the treatment border. As transfer eligibility jumps sharply at the treatment border, firms have an incentive to relocate their activity to the treatment area to take advantage of subsidies. Based on the estimates from two complementary analyses, we cannot rule out that the entire effect is driven by a local shift of economic activity.

To better understand why discontinuities in outcomes do not decline after transfer payments have been stopped, we provide and discuss evidence for several potential explanations. For example, agglomeration economies (e.g., labor market pooling, technology spillovers, or home-market effects) can turn responses to temporary shocks into a long-run outcome.[5] Bleakley and Lin (2012) and Kline and Moretti (2014) show that a temporary natural advantage or a temporary place-based policy has long-run implications for the spatial allocation of economic activity. As they either do not find substantial differences in the capital stock or capital intensity between treated and non-treated locations or argue that the initial capital investments would have depreciated several decades later, the authors interpret their findings as evidence in favor of agglomeration economies. Berger and Enflo (2017) study the short- and long-run effects of the introduction of the Swedish railroad network. Consistent with our results, they find that places that were connected to the railroad early have benefited from relocation from less accessible places, and this temporary advantage persists even when differences in accessibility are reversed.[6]

As we apply a spatial regression discontinuity approach, externalities are only able to explain discontinuities at a geographical border if they do not dissipate continuously with distance but show a discontinuity at the treatment border (see Turner, Haughwout, and van der Klaauw 2014). Our results are based on municipality data that are geographically assigned to the jurisdictions' centroids that can be quite distant from each other. We therefore exploit information from satellite data (capital structures and radiance) at a very fine scale of up to 100m $\times$ 100m and study subsamples of municipality pairs that are well-connected by transport networks and not separated by undeveloped land (e.g., forests) or characterized by a polycentric structure to verify the plausibility of this assumption. As discontinuities remain prevalent in these exercises, there is no strong support for this explanation.

An alternative explanation relates to some form of (policy-induced) locational advantage that does not spill across municipal borders continuously. For example, structures are likely to generate a persistent effect because the associated planning process has a long-term value. It is easier to maintain established structures than plan new ones on the green field. However, local governments would need to reinvest higher amounts to maintain higher capital stocks. The life expectancy of roads and buildings is approximately 30 years (Baldwin et al. 2005), so we should otherwise obtain smaller discontinuities in 2010. A channel we can study empirically is whether local governments use initially higher tax revenues to reinvest in the local capital stock. Data on local public budgets reveal that municipalities in the former Zonenrandgebiet still generate higher tax revenues and spend approximately 15–25 percent more on new and reinvestments. A third explanation for persistence that we explore is potential interactions of shocks with economic density. Studying German

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

[6] Schumann (2014) documents the persistent effects of different levels of the 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) regards the persistent relocation of the main German airport from Berlin to Frankfurt (initiated during the Cold War) as evidence for multiple spatial equilibria. Michaels and Rauch (2018) studies the impact of the Roman Empire on the evolution of urban structures in France and the United Kingdom.

reunification in 1990 and EU enlargement in 2004 shows, however, that these arguably important shocks contribute only 2 to 6 percent to the overall treatment effect.

As a final contribution, we study the distributional implications of the place-based policy. Policymakers often initiate place-based policies to raise wages and employment, in particular of poor households (European Commission 2014). However, there is 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. If land supply is not infinitely elastic, an increase in economic activity eventually leads to higher land prices. Our results confirm these concerns. We find that ZRG transfers raised land rents by approximately 30 percent, which offset the nominal per capita income gain in the recipient regions in the long run. In a Rosen-Roback framework, this points to local persistent production amenities that—according to our empirical approach—have to be discontinuous at the treatment border (cf. Rosen 1979 and Roback 1982).

The paper is organized as follows. In the next section, we provide an overview of the historical and institutional background of the transfer program. Sections III and IV introduce the data and identification strategies we use. We present results in Section V, including the effects on economic activity, local relocation, and distributional implications, and discuss reasons for persistence. Section VI concludes.

I. Historical Background

As Germany's surrender in the Second World War became more likely, the Allied Forces started negotiations about the borders of postwar Germany and the division among the United States, the United Kingdom, 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, which finally closed this last loophole for nearly 30 years.

While regional transfers in the 1950s primarily targeted former industrial centers that were heavily bombed during the war, politicians in West Germany also responded to the new situation of a divided state.[7] Districts at the inner-German border received support to prevent out-migration 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 policymakers 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 prewar Germany for the time after reunification.[8] Hence, politicians recognized the potentially long-lasting consequence of an event that was then still considered

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

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. While a politically established committee determined 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 km of the inner-German or Czechoslovakian border on January 1, 1971 became part of the Zonenrandgebiet (Zonenrandförderungsgesetz 1971). Its area accounted for 18.6 percent of the West German territory and 12.3 percent of the population (see Table A1 in the online Appendix). It is remarkable that the ZRG boundaries were never modified despite substantial changes in district and municipality borders, particularly 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 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 50 percent of the investment volume. 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. Moreover, companies located in the ZRG were treated with priority in public tendering. Beyond firm subsidies, a substantial share of the budget was dedicated to 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 (e.g., subsidies from the Investment Premium Law). This allows us to document that the ZRG received the lion's share of the transfer budget. From 1984 to 1987, between 60 and 85 percent of all public transfers in the states we consider were directed to the ZRG.[9] 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 obtain an idea of the overall size of the program, estimates range from €1.3–2.5 billion (at 2010 prices) per year in the 1980s, which amounts to approximately €194–373 per capita (Ziegler 1992). This makes it comparable to the size of current EU Structural Funds amounting to annual transfers of approximately

[9]Documentation of the Joint Task, Rahmenplan No. 13, is available at https://archive.org/details/ger-bt-drucksache-10-1279.

€230 per capita in regions with the highest transfer intensity (Becker, Egger, and von Ehrlich 2010).

II. 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.

This georeferencing provides us with relevant distance measures for each municipality and coordinates that we use as controls in several econometric specifications. We compile a unique dataset on municipality characteristics between 1984 and 2012 and merge it with the information on location and district affiliation in 1971. In most cases and depending on data availability, we refer to the year 1986 for *contemporaneous* effects of the policy and estimate the *persistent* effects in 2010. We use (taxable) nominal income per km^2 as our main proxy of overall economic activity. We further use data broadly categorized into measures of local labor, capital intensity, public investment, and real income, which we introduce below. Details about the data and data sources are provided in the online Appendix.

We use two different samples based on municipalities and districts (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 populated municipalities in 1986 and 2010, respectively. The *boundary sample* of municipalities used in most estimations contains all jurisdictions with a distance to the ZRG border of less than 100 km. This includes all municipalities in the treated region and approximately 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 approximately 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.

III. Identification

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. This holds also true for the regional subsidies we analyze. Simple t-tests about the equivalence of the averages in the groups of transfer recipients and controls suggest significant differences for many variables across groups.

Table 1—Observational Units

	Number of municipalities				Number of districts	
	Total		Boundary sample			
	1986	2010	1986	2010	Total	Boundary sample
Non-ZRG	3,367	3,391	2,298	2,305	285	165
ZRG	1,573	1,576	1,572	1,576	90	90
Total	4,940	4,967	3,870	3,881	375	255

Notes: We consider the 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 online Appendix A for details). The boundary sample on the municipality level contains all municipalities with a distance to the ZRG border of less than 100 km; the boundary sample on the district level includes all districts with $M_d \leq 150$. Districts are based on the 1969 classification, municipalities on the 1997 and 2010 classifications.

For instance, income per km^2 and population density are higher by about 10 and 27 percent in the group of nonsubsidized municipalities 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.

Yet, the transfer program we study gives rise to two types of discontinuities that generate quasi-random variation and 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. Second, we exploit a discontinuity in the political rule that governed the treatment eligibility of regions and allows for local randomization of transfer recipience. In the remainder of this section, we describe the two strategies in a nonformal way relegating technical details to online Appendix B.

A. *Spatial RDD*

We identify the local average treatment effect of the place-based policy by estimating the discontinuity of outcomes at the treatment border (see Figure 1). Our outset represents a special case of a two-dimensional RDD where the location of each observation i (municipality) relative to the threshold is described by latitude and longitude $\mathbf{L}_i = \left(L_{ix}, L_{iy}\right)$. Similarly, the boundary between the treatment area and the control area consists of an infinite number of border points $\mathbf{b} = \left(b_x, b_y\right) \in \mathbf{B}$. Due to the geographic nature of the policy measure, assignment to treatment is a discontinuous function of location, where units east of $\mathbf{B}$ receive treatment while those to the west do not. Thus, location acts as the so-called forcing variable, and we focus on the discontinuity of expected outcome at the boundary. We implement the spatial RDD in both a parametric and a nonparametric way. In the parametric approach, we include flexible functions of distance from the boundary

D_i as well as flexible functions of $\mathbf{L}_i$. Controlling for location may be important as units with the same distance to the boundary 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). In the nonparametric approach, we address this issue by assigning each observation to the nearest border point out of a set of 20 border points $\mathbf{b}^1, \ldots, \mathbf{b}^{20}$ which are allocated at equal distances along the border. This allows us to condition on border-point fixed effects and estimate univariate local linear regressions with the distance from the respective border point acting as the forcing variable. 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).[10] Table B1 in the online Appendix reports descriptive statistics on the distance of observations from the treatment border.

The identification strategy of a regression discontinuity rests on two comparably weak assumptions (see Hahn, Todd, and van der Klaauw 2001). First, counterfactual outcomes 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 also pursue the analysis using information on capital structures and luminosity that vary at a very fine spatial scale (e.g., grid cells of 100m $\times$ 100m) around the border.

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 that 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 km. 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: roughly 57 percent of the 1,737 km ZRG border ceased to represent a district border already between 1971 and 1978. To further improve confidence in our results, we will contrast the discontinuity at the threshold prior to the start of the program with

[10] We check the sensitivity of our results with 10 and 30 border points and alternative bandwidths in the online Appendix F.

the contemporaneous effects such that time invariant confounding discontinuities will cancel. Finally and most important, we will exploit the 40-km rule that determined the actual treatment border, but did 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 the treatment area was never changed (despite changes in jurisdictional boundaries), this assumption is justified. Note, however, that individuals and firms may choose their place of residence and thus sort across the border. This potential change in the spatial equilibrium is 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 outcomes we study.

B. *Fuzzy RDD: Exploiting the Political Treatment Rule*

Recall that districts accommodating either 50 percent of their area or population within a band of 40 km to the Iron Curtain at the beginning of 1971 became part of the ZRG. Due to the shape of districts, the treatment border does not exactly follow a 40-km buffer, so we observe locations 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 (see Figure 2 and online Appendix B). 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-km 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 the treatment border (M_0).

We generate iso-distance 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 buffer where the area share exceeds 50 percent. Table B1 in the online Appendix reports descriptive statistics of M_d for the treatment and control groups.[11] Apparently, none of the control observations are eligible for treatment, and all exceptions belong to the treatment group. If these exemptions from the 40-km rule were not too frequent, we should observe a jump in the probability of treatment at the threshold $M_0 = 40$.

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

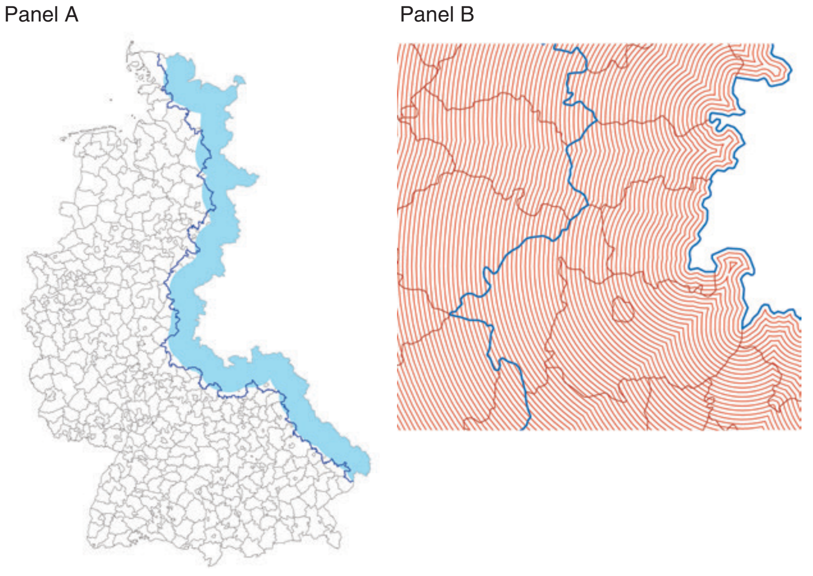

FIGURE 2. ASSIGNMENT VARIABLE M_d

Notes: The above maps show district borders according to the 1971 classification. The shaded area in the left-hand map marks the 40 km distance from the Iron Curtain; the dark line refers to the ZRG border. The right-hand map illustrates the buffer lines drawn in 1 km intervals from the Iron Curtain. In order to obtain M_d, we determine for each district the minimum iso-distance curve from the Iron Curtain where the districts' area share exceeds 50 percent. If the minimum distance is less than 40 km, the district is eligible for treatment.

Figure 3 depicts the treatment indicator against the assignment variable. The discontinuity at 40 km is evident, but the design is fuzzy because a few districts with $M_d > M_0$ still receive ZRG treatment. Overall, noncompliance 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, that is the non-compliers are those districts that did not accommodate 50 percent of the area within 40 km to the eastern border, but 50 percent of the population.[12] 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). As in the spatial RDD approach, we estimate the fuzzy RDD both parameterically and nonparametrically.

[12] 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. A precise measure of population distribution within districts was not even available at the time of treatment assignment and 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.

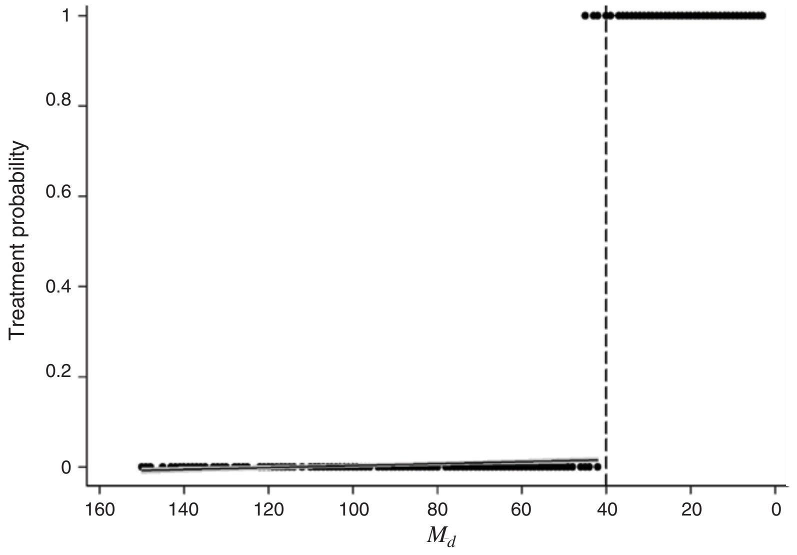

FIGURE 3. TREATMENT PROBABILITY

Notes: The assignment variable M_d indicates a district's minimum distance from the Iron Curtain that includes the majority share of the district's area. We consider only districts overlapping with a 150 km buffer from the Iron Curtain. All districts farther to the west are dropped from the sample.

IV. Results

We start this section by presenting the policy's effect on income per km^2 and several outcome variables relating to capital and labor. We proceed by examining the importance of local relocation at the treatment border, as the policy is likely to draw activity from the control region to subsidized municipalities. Section VC discusses reasons and associated evidence for the persistence in the spatial allocation of economic activity. Based on theoretical insights that shocks capitalize in the fixed factor, we study in Section VD whether transfers have raised land rents in the treatment region. This gives rise to a discussion about the distributional implications of the place-based policy.

A. *Effects on Economic Activity*

Income per km^2.—Before turning to regressions, we plot our main measure of economic activity (log income per km^2) for the years 1986 and 2010 as a function of distance to the ZRG border in Figure 4. Panels A and C use different windows and different control functions from panels B and D, but both reveal marked discontinuities at the ZRG border, both contemporaneously and persistently. Note that negative distances refer to the control region, while positive numbers indicate treated regions (ZRG).[13]

[13] Analogous plots for other outcomes considered are presented in online Appendix A.

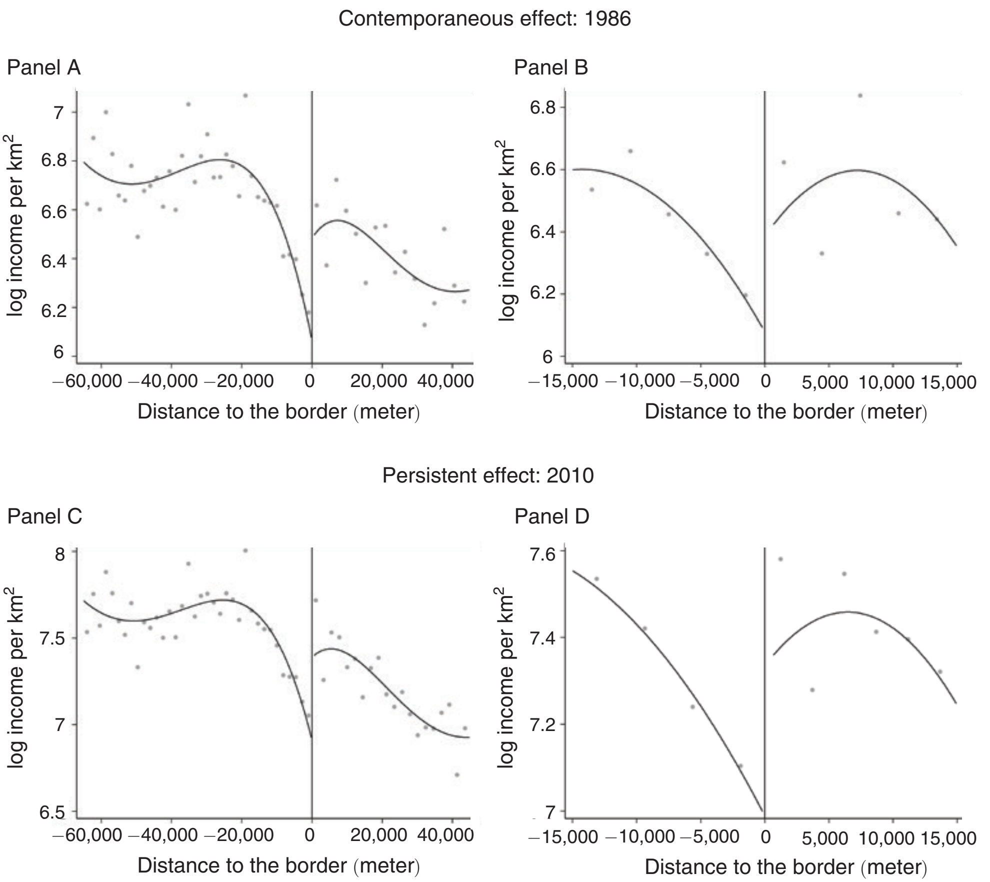

Figure 4. Discontinuities in Economic Activity

Notes: We run separate regressions on each side of the threshold. The plots represent local sample means using nonoverlapping evenly spaced bins on each side of the threshold following the data-driven method for optimal choice of the number of bins described in Calonico, Cattaneo, and Titiunik (2015). The lines represent a fourth-order polynomial distance control function for the 60 km window (panels A and C) and a quadratic control function for the 10 km window (panels B and D).

A shortcoming of the graphical analysis is that by collapsing the two-dimensional location to a scalar measure of distance from the treatment border we cannot ensure that observations to the left and right of the threshold are de facto located a short distance from each other. We follow Calonico, Cattaneo, and Titiunik (2015) and apply an optimal data-driven choice of the number of equally sized bins. As a second observation, 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 VB. 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 2 confirms the first impressions from the plots: regional transfers to the Zonenrandgebiet exerted a strong and significant effect on economic activity (log

Table 2—Spatial RDD: log Income per KM^2

	Contemporaneous effect			Persistent effect		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
log income per km^2	Second (1)	Third (2)	h^* (3)	Second (4)	Third (5)	h^* (6)
ZRG transfers	0.296	0.528	0.412	0.296	0.535	0.404
	(0.080)	(0.099)	(0.090)	(0.077)	(0.096)	(0.091)
Adjusted R^2	0.19	0.22	–	0.19	0.22	–
AIC	10,869	10,741	–	10,541	10,404	–
Observations	3,870	3,870	1,402	3,881	3,881	1,325

Notes: The dependent variable is the logarithm of taxable income at the municipality level. Robust standard errors are in parentheses, all results are robust to using Conley (1999) standard errors that correct for spatial dependence of unknown form. We drop all observations outside a 100 km window of the ZRG border in the parametric specifications. Columns 1–2 and 4–5 include state indicators and, respectively, second- and third-order coordinate control functions. Columns 3 and 6 refer to nonparametric specifications, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). For the nonparametric specifications, we also compute the Calonico, Cattaneo, and Titiunik (2014) robust bias-corrected confidence bounds, which confirm the conventional estimates: the corresponding p-values are 0.021 and 0.031 for the contemporaneous and persistent effects, respectively. Robustness checks using alternative bandwidths as well as parametric, univariate distance control functions are shown in Table F1 in the online Appendix.

income per km^2). Columns 1–2 and 4–5 report results from parametric regressions controlling for the location of each municipality by including coordinates in addition to the Euclidean distance from the treatment boundary and state fixed effects. We choose second- and third-order polynomials based on the adjusted R^2 and the AIC.[14] Estimates in columns 3 and 6 are based on nonparametric regressions where the optimal bandwidth h^* is computed according to Imbens and Kalyanaraman (2012). Columns 1–3 display the contemporaneous effects in 1986, and columns 4–6 display the corresponding specifications for the persistent effects of transfers measured in 2010.

We find that income per km^2 is predicted to be approximately 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 99 percent. Notably, all specifications indicate again a positive and highly significant effect. Most important, these estimates are remarkably similar for each type of specification across time.[15] 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 km from the ZRG border. It can be virtually ruled out that the 40-km 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 1–2 and 4–5 of Table 3 relate to parametric regressions with, respectively, second- and third-order polynomials of M_d as control functions, where standard errors

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

[15] The results are robust to alternative bandwidths and control functions; see online Appendix F.

Table 3—Fuzzy RDD: log Income per KM^2

	Contemporaneous effect			Persistent effect		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
log income per km^2	Second (1)	Third (2)	h^* (3)	Second (4)	Third (5)	h^* (6)
ZRG transfers	0.428	0.482	0.545	0.435	0.485	0.404
	(0.198)	(0.199)	(0.143)	(0.207)	(0.211)	(0.132)
Adjusted R^2	0.14	0.14	–	0.13	0.14	–
AIC	11,110	11,088	–	10,793	10,773	–
Observations	3,875	3,875	997	3,885	3,885	1,037

Notes: The dependent variable is the logarithm of taxable income at the municipality level. Robust standard errors are clustered at the district level in parentheses. Observations with $M_d > 150$ are dropped from the sample. Columns 1–2 and 4–5 refer to parametric fuzzy RDD specifications using a two-stage instrumental variables procedure and include state indicators and second- and third-order coordinate control functions. Note that the instrument is highly relevant in each of the first stages. Specifications 3 and 6 refer to nonparametric fuzzy RDD specifications, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). For the nonparametric specifications, we also compute the Calonico, Cattaneo, and Titiunik (2014) robust bias-corrected confidence bounds, which confirm the conventional estimates: the corresponding p-values are 0.003 and 0.093 for the contemporaneous and persistent effects, respectively. Robustness checks using alternative bandwidths are shown in Table F1 in the online Appendix.

are clustered on the level of districts. Columns 3 and 6 are based on nonparametric estimations with the optimal bandwidth. The overall picture remains the same as in the spatial RDD as the average treatment effect is substantial and remarkably stable after the end of the place-based policy. All specifications yield highly significant effects and the point estimates are similar to the ones in Table 2. This establishes confidence in the consistent estimation of the treatment effect.

Concerning economic magnitude, the effects might appear fairly high at first sight, but they 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 II, transfers to the Zonenrandgebiet were quite substantial every year. Second, it is 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 relegate a more detailed analysis of local relocation to Section VB.

Although we have discussed in detail the identifying assumptions and their plausibility in this context, a straightforward placebo test is used to check whether there was a discontinuity in economic activity prior to treatment. Unfortunately, income data are unavailable at the municipality level before 1975, so we take GDP data at the more aggregated district level. Using estimates for 1961, it is apparent from Table 4 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 nonparametric specifications, but all of the estimates are far from being statistically significant. Further, we use pretreatment information about population density in the years 1951 and 1961, which is available at the municipality level and confirms that there are no discontinuities at

Table 4—Pretreatment—1961

log income per km^2	Parametric M_d Second (1)	Parametric M_d Third (2)	Nonparametric h^* (3)
ZRG transfers	0.394	0.412	−0.054
	(0.589)	(0.591)	(0.638)
Adjusted R^2	0.012	0.018	–
AIC	1,151	1,152	–
Observations	309	309	176

Notes: The dependent variable is the logarithm of taxable income at the municipality level. Robust standard errors are in parentheses. Regressions are based on district-level data. Observations with $M_d > 150$ are dropped from the sample. Columns 1–2 refer to fuzzy RDD specifications using a two-stage instrumental variables procedure and include state indicators and second- and third-order coordinate control functions. Note that the instrument is highly relevant in each of the first stages. Specification 3 refers to a nonparametric fuzzy RDD estimate, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). We also compute the Calonico, Cattaneo, and Titiunik (2014) robust bias-corrected confidence bounds, which confirm the conventional estimate and yield a p-value of 0.613.

the ZRG border prior to the start of the program (see Figure 5 and Figure A1 in the online Appendix), whereas significant differences quickly develop after the start of the program. Figures about the balancing of pretreatment outcomes are presented in online Appendix A.

Capital.—To better understand the underlying drivers of higher economic activity in the Zonenrandgebiet, we first explore the role of capital. As transfers were primarily targeted to subsidize firm investments and public infrastructure, we search for discontinuities in private and public capital stocks. The German Statistical Office provides detailed information about municipalities' land coverage, which we use as proxies for the capital stock. *Public capital* measures the area share of a municipality covered by public infrastructure such as 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. Focusing on commercial capital (*industrial capital*) allows for insights about the relative importance of business activity versus residences. We also use the *business tax base* as an alternative proxy for the private capital stock.

For the sake of brevity, we show results from the spatial RDD, emphasizing that the findings are generally robust to using fuzzy RDD. Table 5 summarizes contemporaneous and persistent effects of the transfer program on capital. We only report second- and third-order polynomials of the augmented coordinate control specifications and nonparametric 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 km^2 is predicted to be approximately 70 percent higher in 1986. Looking at persistence in 2010, we still find a highly significant effect at an even higher level of approximately 80 percent. Taking the area share covered by plants and residential structures, our estimates suggest that transfers have raised the capital stock by

Table 5—Capital

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	Second (1)	Third (2)	h^* (3)	Second (4)	Third (5)	h^* (6)
log business tax base per km^2						
ZRG transfers	0.366	0.720	0.990	0.463	0.848	0.954
	(0.120)	(0.151)	(0.276)	(0.116)	(0.149)	(0.223)
Adjusted R^2	0.17	0.19	–	0.18	0.20	–
AIC	12,795	12,718	–	13,244	13,161	–
Observations	3,533	3,533	1,263	3,792	3,792	1,573
Private capital stock						
ZRG transfers	0.197	0.341	0.327	0.193	0.298	0.372
	(0.064)	(0.078)	(0.081)	(0.063)	(0.078)	(0.081)
Adjusted R^2	0.11	0.12	–	0.06	0.07	–
AIC	8,895	8,830	–	8,420	8,369	–
Observations	3,845	3,845	2,714	3,851	3,851	2,597
Industrial capital stock						
ZRG transfers	0.410	0.658	0.505	0.344	0.407	0.467
	(0.192)	(0.245)	(0.227)	(0.178)	(0.236)	(0.217)
Adjusted R^2	0.10	0.11	–	0.11	0.11	–
AIC	3,877	3,862	–	3,668	3,656	–
Observations	1,260	1,260	782	1,234	1,234	772
Public capital stock						
ZRG transfers	0.147	0.111	0.123	0.172	0.138	0.225
	(0.032)	(0.039)	(0.061)	(0.032)	(0.040)	(0.051)
Adjusted R^2	0.26	0.27	–	0.25	0.25	–
AIC	3,885	3,851	–	3,760	3,718	–
Observations	3,855	3,855	1,479	3,865	3,865	2,059

Notes: All results are based on spatial RDD. Robust standard errors are in parentheses. We drop all observations outside a 100 km window of the ZRG border in the parametric specifications. Columns 1–2 and 4–5 refer to parametric specifications and include state indicators and second- and third-order coordinate control functions, respectively. Columns 3 and 6 refer to nonparametric specifications, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). Business tax base per km^2 is measured in logarithmic terms. The three measures of capital stock are bounded between zero and unity (as they refer to area shares of private, industrial, and public structures), which renders estimating linear models inappropriate. Thus, we apply a logit transformation to public capital, private capital, and industrial capital. Note that information about industrial capital is available for 1988 in only three states. Therefore, we restrict the contemporaneous and persistent estimates to these states. Private capital is the sum of residential capital and industrial capital. Using data about all states in 2010 yields similar results.

approximately 30 percent in both 1984 and 2010. Distinguishing between industrial and residential structures, we observe that ZRG treatment led to a higher increase in industrial premises, as the effect on *industrial capital* is higher than the effect on aggregate private capital. The *public capital* stock is predicted to be approximately 10–20 percent higher compared to the counterfactual.

Labor.—A second reason for higher economic activity per km^2 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 that made living

Table 6—Labor

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	Second	Third	h^*	Second	Third	h^*
	(1)	(2)	(3)	(4)	(5)	(6)
log population per km²						
ZRG transfers	0.239	0.434	0.516	0.290	0.473	0.564
	(0.070)	(0.086)	(0.119)	(0.071)	(0.088)	(0.126)
Adjusted R^2	0.18	0.20	–	0.18	0.20	–
AIC	9,846	9,746	–	9,988	9,876	–
Observations	3,870	3,870	1,844	3,881	3,881	1,547
log employment per km²						
ZRG transfers	0.418	0.658	1.082	0.467	0.723	0.784
	(0.108)	(0.135)	(0.218)	(0.111)	(0.140)	(0.149)
Adjusted R^2	0.18	0.19	–	0.16	0.17	–
AIC	13,120	13,052	–	12,407	12,346	–
Observations	3,826	3,826	1,354	3,665	3,665	2,459
Human capital						
ZRG transfers	0.016	0.213	0.213	−0.076	0.116	−0.066
	(0.075)	(0.099)	(0.138)	(0.068)	(0.089)	(0.101)
Adjusted R^2	0.12	0.13	–	0.12	0.13	–
AIC	3,555	3,541	–	5,372	5,337	–
Observations	1,782	1,782	810	2,576	2,576	1,492

Notes: All results are based on spatial RDD. Robust standard errors are in parentheses. We drop all observations outside a 100 km window of the ZRG border in the parametric specifications. Columns 1–2 and 4–5 refer to parametric specifications and include state indicators and second- and third-order coordinate control functions, respectively. Columns 3 and 6 refer to nonparametric specifications, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). *Population per km²* and *employment per km²* are measured in logarithmic terms. Our measure of human capital refers to the logit transformation of the share of residents with tertiary education.

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. We proxy human capital by the share of residents with a tertiary degree.

Table 6 reveals that the place-based policy raised population density by approximately 40–50 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 km^2 is even more pronounced, indicating substantial commuting into the Zonenrandgebiet. However, we find no evidence that the composition of the workforce with respect to education was affected by the treatment.[16]

It is informative to take a closer look at how the magnitude of effects has developed over time. As we argued in the previous subsection, GDP data are only available at the district level and at fewer intervals than population data. As we have found significant and large effects of ZRG transfers on population density, we run the specification with coordinate control functions for several years between

[16] We have also studied employment by sector, searching for heterogeneous treatment effects with respect to the industry's capital intensity. Data are only available for 1987 but do not indicate significant differences.

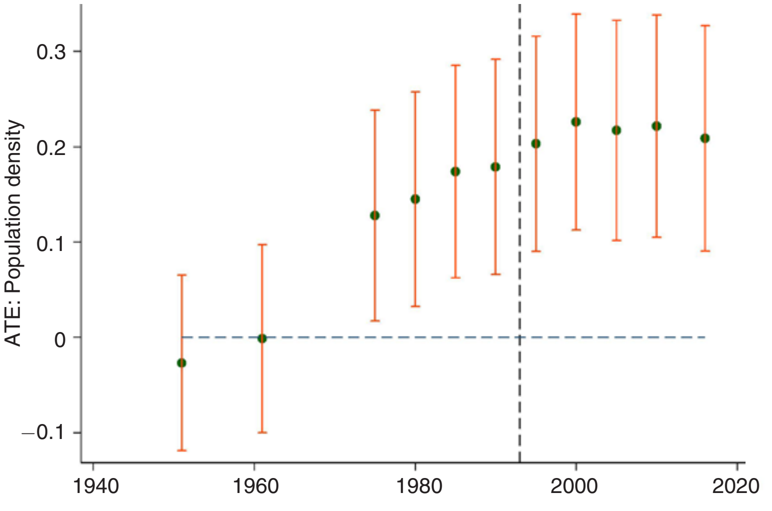

Figure 5. Dynamics in Population Density

Notes: The average treatment effects (ATE) are based on spatial RDD with log population density as the dependent variable, second-order asymmetric coordinate control functions, a 100 km boundary sample, and—depending on data availability—between 3,671 (in 1951) and 3,838 (in 2016) municipalities per year. The vertical lines mark 90 percent confidence intervals. The dashed lines mark the end of the transfer program and the zero level of the treatment effect.

1951 and 2016. Figure 5 reveals differences in population densities between the Zonenrandgebiet and the control area. Note that the points and bars illustrate point estimates and 90 percent confidence bands. It is evident that the difference in population density developed fairly quickly over the first five to ten years after the introduction of the transfer program. We find no evidence for a decline of this difference after the program was stopped. Further, there is no significant difference in population density prior to treatment. This finding is in line with the insights from Table 4 that there was no discontinuity in GDP per km^2 before 1971.

B. *Local Relocation*

While it is striking that the policy has persistently altered the spatial allocation of economic activity, we must not interpret the local average treatment effect as the *net* effect of the policy. We have argued in the context of Figure 4 that transfers have potentially lured economic activity from the control region across the treatment border. While this negative externality preserves the causal interpretation of the average treatment effect, the estimates do not indicate how much of the effect is new activity.

To assess the importance of local relocation, we follow two complementary approaches. First, we study local population growth from the time prior to the policy in 1951 to the peak of the program in 1985. As aggregate population in the greater area around the treatment border is unlikely to be affected by the transfers,

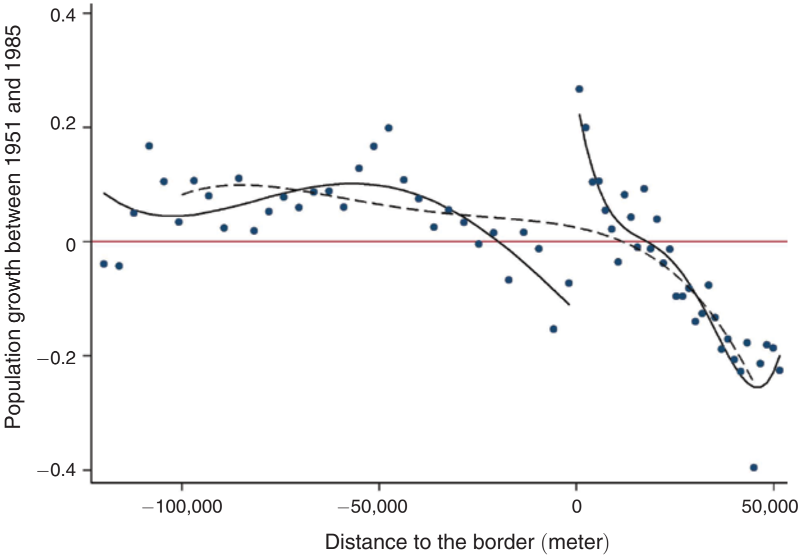

FIGURE 6: BUNCHING APPROACH: POPULATION GROWTH 1951–1986

Notes: Observations are assigned to equally sized bins, where the dots illustrate local averages of population growth rates within bins. The dashed line illustrates the estimated counterfactual distribution. The solid lines represent flexible polynomials fitted separately on both sides of the treatment border. Note that we drop Schleswig-Holstein (the northernmost part of Germany). Because of the state's location between the Baltic and North Seas, there is only a relatively narrow band of municipalities west of the ZRG region. To account for the two-dimensional design, we residualize the dependent variable and the distance from the treatment border on latitude before plotting the local averages by bin and computing the counterfactual distribution. Results are quantitatively similar to an analysis without latitude adjustment.

we compare the observed distribution of population growth across municipalities to a counterfactual distribution and infer whether places just outside the subsidized areas have lost population mass to locations just inside the ZRG. We refer to this exercise as the *bunching approach* because it relates to so-called bunching analyses developed in the tax literature, e.g., by Saez (2010) and Kleven and Waseem (2013). In the second approach, referred to as the *spatial exclusion approach*, we exclude jurisdictions in the proximity of the treatment border, as these are potentially affected most by local relocation, and reestimate the effect on income per km^2 (e.g., Neumark and Kolko 2010).

Bunching.—To identify the degree of relocation, we rely on an estimate of the counterfactual distribution of population growth, i.e., the change in local population between 1951 and 1986 in the absence of the transfer program. We follow the conventional method, compute population growth rates at the municipality level, assign observations to $j \in J$ evenly spaced bins (based on Calonico, Cattaneo, and Titiunik 2015), and depict the local averages by bin against the distance from the treatment threshold. Figure 6 illustrates this idea, where the dots represent the observed population growth rates in bin j, g_j. The dashed curve depicts the counterfactual distribution, $\hat{g}_j$. To obtain $\hat{g}_j$, we fit a flexible polynomial to the observed distribution, excluding observations in a range around the treatment threshold, and extrapolate the fitted distribution to the ZRG boundary. Along the lines of Section VA, we smooth observed population growth rates separately on both sides of the treatment border, which is represented by the solid curves in Figure 6. Note that we use population growth instead of population levels because this gives a smoother distribution and a more precise estimate about the range of relocation.[17]

We learn two things from this exercise. First, the intersections of the solid and dashed curves provide an estimate of the overall relocation area. Inspection of Figure 6 reveals that relocation starts approximately 30 km west of the treatment border and reaches approximately 25 km into the ZRG, implying a total relocation area of approximately 55 km. Second, the integral between the observed and the counterfactual distributions provides us with an estimate of the associated mass of relocated population. We find that the missing mass in the municipalities west of the treatment border amounts to approximately 2.8 percent of the 1951 population in the areas that became the ZRG region 20 years later. The 90 percent confidence intervals for the missing mass range between 1.7 and 3.8 percent. This is in line with the predicted increase of population mass within 25 km east of the treatment border, for which the 90 percent confidence interval overlaps with the one for the missing mass. Thus, we cannot reject the null hypothesis that the additional mass on the eastern side of the treatment border equals the missing mass on the western side of the treatment border. We interpret these results as evidence for significant local relocation. However, given the variability of the data, which is more pronounced than in the traditional, one-dimensional bunching design, the approach does not allow us to provide a precise estimate about the share of local relocation.

Spatial Exclusion.—Having shown that the transfers induced substantial relocation, we next exclude jurisdictions affected by relocation and reestimate the effects on income per km^2. Figure 7 illustrates the idea of this *spatial exclusion approach*. The municipalities to the west of the bold treatment border (shaded in light gray) serve as the remaining control regions, whereas the jurisdictions to the east of the border (shaded in dark gray) are part of the treatment group. Obviously, this approach contradicts the identification strategy of the spatial RDD, which relies on the comparison of outcomes for observations in a close neighborhood.[18] However, we may execute this exercise in the fuzzy RDD. We exploit the fact that each district (boundaries in dark gray) with $M_d \sim M_0$ accommodates municipalities (boundaries in light gray) with varying distances to the treatment border. Thus, by excluding municipalities in the close neighborhood of the ZRG border, we can remove the part of district outcome that is potentially contaminated by spillovers.

Table 7 summarizes the estimation results for the spatial exclusion approach in the year 1986. We examine four specifications: "Border" excludes all municipalities

[17] Specifically, we apply a "differences-in-bunching" approach as we exploit changes in population before and after the introduction of the transfers program, i.e., the introduction of a dominated region. This improves the estimation of the counterfactual compared to an approach without time variation. Moreover, we residualize the dependent variable and the boundary distance on latitude to ensure that we compare units in a close neighborhood.

[18] A recent paper by Einiö and Overman (2016) discusses the trade-off between the conditional independence assumption that requires in the spatial RDD exploiting variation at the boundary and the single unit treatment value assumption (SUTVA), which is more likely to be fulfilled for units further from the boundary.

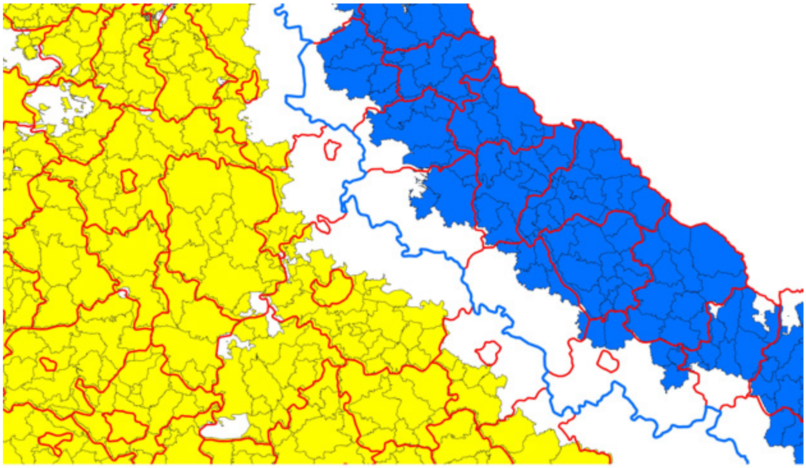

FIGURE 7. SPATIAL-EXCLUSION APPROACH

Notes: The cental, bold line marks the ZRG border. Shaded areas refer to municipalities in our boundary sample that belong to the treated (dark shaded) and non-treated areas (light shaded), respectively. District boundaries (according to the 1971 classification) and municipal boundaries are drawn in non-bold and bold, respectively.

TABLE 7—RELOCATION EXTERNALITIES

	Third-order polynomial of M_d				Nonparametric h^*			
log income per km^2	Border (1)	10 km (2)	20 km (3)	55 km (4)	Border (5)	10 km (6)	20 km (7)	55 km (8)
ZRG transfers	0.449	0.396	0.336	0.044	0.433	0.298	0.298	0.024
	(0.220)	(0.209)	(0.239)	(0.332)	(0.153)	(0.216)	(0.298)	(0.190)
R^2	0.14	0.15	0.15	0.21	–	–	–	–
Observations	3,514	3,408	3,084	1,919	1,065	689	516	1,053

Notes: The dependent variable is the logarithm of taxable income at the municipality level. Estimates refer to the year 1986. We exclude all municipalities contiguous to the treatment border (columns 1 and 5) and within distances of 10 km, 20 km, and 55 km of the treatment border (columns 2–4 and 6–8). We estimate the fuzzy RDD specifications using a parametric 2SLS approach including a third-order coordinate control function in columns 1–4 and a nonparametric approach in columns 5–8, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). Observations with $M_d > 150$ are dropped from the sample, and standard errors are clustered at the district level.

adjacent to the treatment border, "10 km" ignores all jurisdictions whose centroids are located within 5 km of the ZRG border, "20 km" is a similar exercise for a range of 10 km on both sides of the border, and "55 km" excludes the relocation area as identified according to Figure 6. Note that all results are based on the contemporaneous sample. Starting with the baseline results in columns 2 and 3 of Table 3, we observe from the specification "Border" that the average treatment effect drops from 0.482 to 0.449 and from 0.545 to 0.433 in the parametric and nonparametric specifications, respectively. Restricting the sample to a relocation area of 20 km yields a reduction of the point estimates of approximately 20 to 40 percent compared to the benchmark results. Hence, assuming that the externality dissipates linearly implies that relocation activities must occur within 50–100 km to explain the total effect.

With a minimum distance of 10 km from both sides of the border, we lose efficiency, and the share of observations displaying a level of M_d in the neighborhood of 40 drops considerably. Finally, columns 4 and 8 of Table 7 show that the point estimate drops to 3–4 percent and is insignificant when we exclude the full relocation window as identified above.

In summary, both approaches point toward substantial local relocation within a range of approximately 20–30 km on both sides of the treatment boundary. However, none of the specifications provide a precise estimate about the share of local relocation.

C. *What Explains Persistence?*

In this section, we discuss potential mechanisms that could explain the persistence of the policy-induced change in the spatial economic structure. We explore the role of agglomeration externalities; the implications of German reunification in 1990 and EU enlargement in 2004; whether the lifetime of infrastructure investments could be responsible for our findings; and whether the policy-induced increase in economic activity has led to a persistent increase in tax revenues in the former treatment region that helps maintain the higher capital stock.

Agglomeration Externalities.—Economic density generates positive externalities that can explain persistent effects of temporary shocks (e.g., Krugman 1991). Because our identification uses an RD approach at municipal boundaries, agglomeration externalities can only cause persistent discontinuities if these externalities do not dissipate continuously with distance, that is, across the former treatment border. If they did dissipate, any difference in outcome could not be assigned to externalities because locations on either side of the border would be affected by the externality to the same extent. Turner, Haughwout, and van der Klaauw (2014) used this insight to identify the direct effect of land regulation on land prices and welfare. While we relegate a more formal exposition of this idea to online Appendix E, we explore whether there is reason to believe that externalities could be discontinuous at the former treatment border.

So far, we have built our empirical analysis on administrative data at the municipality level such that outcome variables were assigned to the municipalities' centroids. A natural concern would be that there is little or no economic activity at the boundaries of jurisdictions introducing frictions in the diffusion of externalities around the treatment border. The assumption that externalities are spatially continuous would become more credible if we were able to use more disaggregated data around the treatment boundary. Two sources of satellite data prove useful in this context. First, we use information on capital structures at a grid-cell level of 100m $\times$ 100m provided by the European Environmental Agency's CORINE project. The satellite data contain information on numerous land-cover classes, and we set $PrivateCapital = 1$ if a location is covered by private capital structures. Note that the area-weighted sum of *PrivateCapital* is highly correlated with our municipality-level variable for private capital (correlation coefficient of 0.84). Second, we exploit night-light radiance as a proxy for local GDP (see Elvidge et al.

Table 8—Spatial RDD: Grid-Cell Data

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	Second	Third	h^*	Second	Third	h^*
	(1)	(2)	(3)	(4)	(5)	(6)
Pr(*PrivateCapital* = 1)						
ZRG transfers	0.016	0.016	0.007	0.017	0.019	0.014
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Adjusted R^2	0.00	0.00	–	0.00	0.01	–
AIC	−184,345	−192,499	–	1,214,637	1,201,129	–
Observations	7,786,402	7,786,402	3,730,944	7,786,402	7,786,402	692,832
log(*Radiance*)						
ZRG transfers	0.298	0.281	0.291	0.285	0.206	0.291
	(0.014)	(0.019)	(0.025)	(0.013)	(0.017)	(0.024)
Adjusted R^2	0.08	0.10	–	0.09	0.12	–
AIC	256,617	254,504	–	290,388	286,205	–
Observations	107,776	107,776	29,839	125,527	125,527	25,862

Notes: Robust standard errors are in parentheses. We drop all observations outside a 40 km window of the ZRG border. The dependent variable in the upper panel is a binary indicator that is unity if a 100m × 100m grid is covered by private buildings and zero otherwise. In the lower panel, we use log(Radiance) as computed from satellite night-light data for grid cells of 30 arc seconds (approximately 900m × 600m in the center of Germany). Information on land coverage and radiance refers to the years 1990 and 1992, respectively. Columns 1–2 and 4–5 refer to parametric regressions with, respectively, second- or third-order polynomial coordinate control functions. Columns 3 and 6 refer to nonparametric specifications, where the bandwidth h^* is computed according the algorithm introduced by Imbens and Kalyanaraman (2012).

1997 and Henderson, Storeygard, and Weil 2012) at grid cells of 30 × 30 arc seconds (900m × 600m in the center of Germany).

Table 8 shows that both data sources are associated with positive and highly significant average treatment effects. Note that the estimates for $\Pr(PrivateCapital = 1)$ 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 5. The latter indicated that the logit-transformed area share of private capital, i.e., the odds ratio, increased by approximately 30 percent because of transfers. Given that $\Pr(PrivateCapital = 1)$ is approximately 6 percent in our data, a 1.6 percentage point increase in $\Pr(PrivateCapital = 1)$, as displayed in columns 1 and 2 of Table 8, corresponds to $\ln\left(\frac{0.076}{1-0.076}\frac{1-0.06}{0.06}\right) \approx 0.253$, i.e., an increase of approximately 25.3 percent in the odds ratio. To compare the finding relating to radiance, we first compute a conversion factor of income per km^2 and radiance per km^2 of 1.045 using West German municipality data. Thus, the radiance estimates suggest an increase in GDP per km^2 of 21–31 percent. Most important, the effects estimated from grid-cell data again show a high degree of persistence. Hence, the analysis of grid-cell data suggests that there must be something beyond agglomeration externalities that drives persistence.

As a further exercise, we exploit heterogeneity along the ZRG border to estimate the treatment effects for radiance and private capital structure based on grid cells that belong to municipalities where potential frictions are presumably less pronounced. First, we restrict our sample to the 10 percent of municipalities that feature the highest (area-weighted) number of roads crossing the treatment border. Second, we compute the amount of undeveloped land within a 1 km buffer on both sides of the treatment border. Using this information, we restrict the sample to the 10 percent of municipalities featuring the lowest share of undeveloped land in the neighborhood of the boundary. Third, suspecting that unobservable social networks may display frictions at the treatment border, we estimate the effects only for polycentric municipalities, arguing that ties within such scattered municipalities are weaker (relative to cross-border networks) than in monocentric municipalities. If frictions play a role, we would expect lower estimates in these exercises compared to those displayed in Table 8. As shown in online Appendix D, this is not unambiguously the case. While some point estimates go up, others are somewhat lower than those in the full sample. Importantly, these estimates based on small subsamples are not different from the corresponding ones in Table 8 at conventional significance levels. In conclusion, it is impossible to entirely rule out agglomeration economies as an explanation for the estimated discontinuities on the basis of observable variation along the border, but the evidence clearly points toward a further important channel to explain spatial persistence—a determinant of economic activity that does not dissipate across municipality borders.

German Reunification and EU Enlargement.—The place-based policy ended in the early 1990s because German reunification shifted the focus of regional development to the new Länder. Could better access to markets in the east, either through reunification in 1990 or EU enlargement in 2004, explain persistence? For these events to confound the point estimates, the positive shocks need to exert a discontinuous impact at the former treatment border. This appears unlikely. Instead, it is more plausible that the advantage of market access dissipates continuously when moving westward.

However, both events could influence our RD outcome for 2010 if market access interacts with the level of economic activity. As the place-based policy has raised employment and output in the former ZRG, it is possible that municipalities with higher economic activity take more advantage of improved market access. To shed light on this potential explanation, we combine the discontinuity approach with time variation to examine whether discontinuities differ *before* and *after* the events. Comparing municipalities in the close neighborhood of the ZRG border ensures that municipalities are affected similarly by changes in market access. We focus on population density because of superior data coverage. Our difference-in-discontinuities specification follows the benchmark specification in the spatial RDD (c.f. Section IIIA) but is estimated on a pooled dataset and includes an interaction between the ZRG treatment indicator and a dummy variable that is equal to one after the shock has occurred (post-1990 and post-2004, respectively) and zero otherwise. Moreover, based on the insights from Redding and Sturm (2008) that the benefits of market access decline with distance, we include an interaction term between shock and location, $f(\mathbf{L}_i S_t)$. Note that the data are pooled over the years 1989 and 1995 in the case of German reunification and over the years 2003 and 2010 in the case of EU enlargement. The estimation specification is explained in more detail in online Appendix B.

Table 9 reveals for the pooled data that the main effects of the treatment, denoted by *ZRG*, are similar to the benchmark specifications in Table 6. The coefficient on

Table 9—Reunification and EU Integration

log population per km^2	Reunification		EU integration	
	Second	Third	Second	Third
	(1)	(2)	(3)	(4)
ZRG	0.240	0.464	0.305	0.504
	(0.071)	(0.089)	(0.072)	(0.090)
$S \times ZRG$	0.016	0.008	0.009	0.011
	(0.005)	(0.007)	(0.005)	(0.006)
Adjusted R^2	0.19	0.22	0.18	0.21
AIC	18,969	18,705	18,974	18,714
Observations	7,502	7,502	7,456	7,456

Notes: The dependent variable is the logarithm of population per square kilometer. The specifications include second- or third-order polynomial coordinate control functions. *ZRG* indicates whether a municipality is located in the Zonenrandgebiet; *S* is a shock that refers to *Reunification* in columns 1 and 2 and to EU integration in columns 3 and 4. In the former case, the years refer to 1989 and 1995, whereas the years are 2003 and 2010 in the case of EU integration. Standard errors are clustered at the municipality level in parentheses. We drop all observations outside a 100 km window of the ZRG border.

the interaction term is denoted by $S \times ZRG$ and tends to be positive and significant in most specifications. We are interested in the contribution of the respective shocks for the overall discontinuity, that is, $S \times ZRG/(S \times ZRG + ZRG)$. We find that the interaction effects of German reunification and EU enlargement with ZRG treatment explain between only 2 and 6 percent of the total discontinuity.

Endurance of Infrastructure Investments.—A further location-specific reason for persistence could be the longevity of investments in roads and buildings. Although we analyze persistence "only" 16 years after the policy ended, we need to take into account that some investments were already undertaken in the 1970s. This raises the time frame to up to 40 years. Moreover, roads and buildings also depreciate, albeit at lower rates than machinery, so we should at least observe a decline in the estimated discontinuity over time. According to the OECD, the economic lifetime of buildings and roads is calculated to be approximately 30 years; therefore, it is quite remarkable that our RD results do not indicate any decline in estimated discontinuities (Baldwin et al. 2005).

Local Public Investment.—As the place-based policy has raised economic activity in the treatment area, local governments could take advantage of the higher tax base and persistently maintain higher local public investment levels and thus a higher public capital stock. As these local expenditures primarily benefit households and firms *within* a jurisdiction, this channel could contribute to the persistence of the average treatment effect after the program.

Table 10 puts numbers to this story. We observe that the place-based policy has indeed raised the tax income of municipalities in the Zonenrandgebiet. We estimate tax revenues to be approximately 16–17 percent higher both in 1985 and in 2010. Note that local tax rates have not changed between the former treatment and control groups; therefore, the result has to be based on differences in the tax base (see Table C1 in the online Appendix). As expected, federal investment subsidies are

Table 10—Local Public Investment

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	Second (1)	Third (2)	h^* (3)	Second (4)	Third (5)	h^* (6)
log tax revenues per capita						
ZRG transfers	0.059	0.169	0.121	0.094	0.164	0.087
	(0.036)	(0.044)	(0.038)	(0.030)	(0.038)	(0.037)
Adjusted R^2	0.08	0.12	–	0.13	0.14	–
AIC	1,895	1,772	–	2,319	2,259	–
Observations	2,650	2,650	999	3,169	3,169	1,471
log federal investment subsidies per capita						
ZRG transfers	0.652	0.384	0.484	0.085	0.136	0.083
	(0.141)	(0.179)	(0.126)	(0.115)	(0.148)	(0.142)
Adjusted R^2	0.09	0.10	–	0.17	0.18	–
AIC	8,779	8,741	–	8,662	8,626	–
Observations	2,551	2,551	1,861	2,698	2,698	1,657
log public investment per capita						
ZRG transfers	0.251	0.204	0.148	0.213	0.217	0.146
	(0.064)	(0.082)	(0.075)	(0.049)	(0.061)	(0.056)
Adjusted R^2	0.42	0.44	–	0.29	0.30	–
AIC	4,670	4,573	–	5,393	5,342	–
Observations	2,614	2,614	918	3,171	3,171	2,189

Notes: Robust standard errors are in parentheses. Columns 1–2 and 4–5 refer to parametric specifications and include, respectively, second- or third-order polynomial coordinate control functions and state indicators. Columns 3 and 6 refer to nonparametric specifications, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012). We drop all observations outside a 100 km window of the ZRG border in the parametric specifications. *Tax revenues per capita*, *federal investment subsidies per capita*, and *public investment per capita* are measured in logarithmic terms and refer to the years 1985 and 2010. Public investment per capita includes both expenditure for new investment and reinvestments.

significant during the program but become insignificant in 2010. Moreover, even the point estimates are close to zero for the 2010 sample. Hence, transfers that might have substituted subsidies of the ZRG program cannot explain the persistent difference in economic activity. Turning to the expenditure side reveals that the place-based policy has enabled local governments to persistently spend approximately 15–25 percent more on new investment and replacements of depreciated capital. This evidence establishes one possible explanation for why a temporary place-based policy generates long-run effects.

D. *Incidence: Who Benefited from Transfers?*

Policymakers often have low-income households in mind when favoring transfers to lagging regions (e.g., European Commission 2014). 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, pretreatment property owners reap the benefits, and higher nominal income is eaten up by higher land rents.

We run the same regressions as in the spatial discontinuity approach with income per capita and land prices as outcome variables to shed light on this question.

Table 11—Per Capita Income and Land Prices

	Contemporaneous effects			Persistent effects		
	Coordinate control		Nonparametric	Coordinate control		Nonparametric
	Second	Third	h^*	Second	Third	h^*
	(1)	(2)	(3)	(4)	(5)	(6)
log income per capita						
ZRG transfers	0.028	0.083	0.049	0.006	0.067	0.059
	(0.012)	(0.015)	(0.013)	(0.013)	(0.017)	(0.019)
Adjusted R^2	0.29	0.32	–	0.13	0.17	–
AIC	−3,545	−3,749	–	−2,874	−3,047	–
Observations	3,870	3,870	3,565	3,881	3,881	2,233
log land prices						
ZRG transfers	−0.072	0.354	0.075	0.242	0.321	0.297
	(0.151)	(0.199)	(0.109)	(0.057)	(0.073)	(0.059)
Adjusted R^2	0.20	0.26	–	0.19	0.22	–
AIC	1,062	1,015	–	5,343	5,217	–
Observations	564	564	216	2,983	2,983	1,917
log real income						
ZRG transfers	0.074	0.041	0.073	−0.017	0.006	−0.002
	(0.028)	(0.037)	(0.023)	(0.014)	(0.018)	(0.022)
Adjusted R^2	0.10	0.13	–	0.09	0.11	–
AIC	−847	−870	–	−3,497	−3,560	–
Observations	556	556	292	2,748	2,748	1,341

Notes: Land prices per square meter, income per capita, and real income are measured in logarithmic terms. We compute log real income as $\log(income) - 0.25 \times \log(land\ prices)$. Robust standard errors are in parentheses. In 1988, we only have data on land prices for Lower Saxony; in 2010, we have such information for Lower Saxony, North Rhine-Westphalia, Hesse, and Bavaria. 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 100 km window of the ZRG border in the parametric specifications. Columns 1–2 and 4–5 refer to parametric specifications and include, respectively, second- and third-order polynomial coordinate control functions and state indicators. Columns 3 and 6 refer to nonparametric specifications, where h^* denotes the optimal bandwidth computed according to Imbens and Kalyanaraman (2012).

For contemporaneous effects, land prices are only available for a subsample of municipalities (Lower Saxony), while we have information for Lower Saxony, North-Rhine-Westphalia, Hesse, and Bavaria in 2010. We observe from Table 11 that nominal income per capita has increased by approximately 5–8 percent both contemporaneously and persistently. However, land prices increased by approximately 25–35 percent, depending on the specification. As households in Germany spend approximately 20–30 percent of their net income on rents (Statistisches Bundesamt 2017), real wages in the Zonenrandgebiet have not increased.

According to the framework established by Rosen (1979) and Roback (1982), increases in consumption amenities should be associated with lower real wages in spatial equilibrium. Differences in nominal wages may prevail, however, as a result of differences in production amenities. This is consistent with persistent location-specific productivity advantages in the formerly subsidized regions. We use land prices and nominal income to compute a proxy for local real income as $\log(income) - 0.25 \times \log(land\ prices)$. While we find higher real income in the treatment area in 1986, the effects on nominal income and land prices cancel in 2010. This points to temporary real income benefits that have canceled in the long-run equilibrium.

These capitalization effects have rarely been documented in such a pronounced way in the context of place-based policies (see Neumark and Simpson 2015). An exception is the assessment of the federal empowerment zones program in the US by Busso, Gregory, and Kline (2013). They find capitalization effects for owner occupied housing, whereas the evidence for rental rates is less clear. One reason why we do find evidence for such pronounced effects could be the long time horizon of the policy. Transfers were granted for the time of German division, whose end in 1990 was unforeseeable. As migration decisions are forward looking, the indefinite time horizon of the policy could have substantially supported the effectiveness of the program.

V. 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 for the effectiveness of the policy, as migration and location decisions are forward looking. However, we also find evidence for substantial relocation of economic activity, leaving doubts about the efficiency of the policy.

As we use a regression discontinuity design to identify causal effects of the place-based policy, agglomeration economies could only explain the persistent spatial pattern if these externalities were discontinuous at the treatment border. Using data at a fine spatial scale and executing several robustness checks lend little support to this explanation. Instead, we find strong evidence that the temporary place-based policy has generated a location-specific advantage that local governments exploit. Higher economic activity implied higher tax revenues that municipalities have used to maintain the higher capital stock. This channel is still prevalent many years after the policy ended.

A second main conclusion relates to distributional implications of place-based policies. We have identified substantial capitalization effects of transfers such that higher nominal incomes in the Zonenrandgebiet were derogated by higher land rents. As a consequence, transfers primarily benefited pretreatment landowners in the Zonenrandgebiet, rather than raising real wages.

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