

Do Active Labor Market Policies Help Workers?

Evidence from a Spatial Regression Discontinuity Design*

Daniel F. Heuermann[†]
University of Regensburg

Johannes F Schmieder[§]
Boston University,
NBER, IZA

Philipp vom Berge[‡]
Institute for Employment
Research (IAB)

PRELIMINARY

Abstract

Many countries use a wide range of Active Labor Market Policies (ALMP) to help unemployed workers find back into employment. In this paper we study the effectiveness of ALMPs for the long-term unemployed in Germany using a border discontinuity design. We exploit the fact that long-term unemployed are assigned strictly geographically to local unemployment agencies (Job Centers), which provide job search support in the form of ALMP. While many papers have studied the effectiveness of such programs on the individual level, existing research designs often do not allow for estimates that encompass spillovers between workers or the possibility of “threat effects”, where the mere availability of ALMPs encourages workers to search for jobs. A key advantage of our design is that the treatment measure is on the agency level, thus allowing for such spillover and threat effects, while also using an individual-level discontinuity design that allows for very clean identification of the key parameters. Our results indicate that, over a horizon of 48 months, participating in typical ALMP programs does not raise the number of days in employment but instead leads to a small but statistically significant decline in employment and earnings. One exception are wage subsidies that lead to a moderate positive employment effect and sanctions that do not seem to have any effect.

*We would like to thank Isaiah Andrews, Stefan Bender, David Card, Andrew Garin, Larry Katz, Kevin Lang, Jeremy Magruder, Joachim Moeller, Daniele Paserman, and seminar participants at Boston University, University of Goettingen, Harvard University, IAB, University of Regensburg, Yale University, SOLE 2016, ESPE 2016 and ERSAs 2016 for helpful comments. Michael Rindler provided excellent research assistance. All errors are our own.

[†] daniel.heuermann@ur.de

[§] johannes@bu.edu

[‡] philipp.berge@iab.de

1 Introduction

Persistently high unemployment rates after the financial crisis in 2008 have led to renewed interest in policies to support the unemployed. Extensions of unemployment insurance (UI) benefits have played a major role as a strategy, in particular in the US and in several Southern European countries (see [Schmieder and von Wachter, 2016](#), for a recent survey). One concern with extending UI benefits as a response to economics downturns is the potential for moral hazard that leads recipients to reduce their search effort and stay unemployed longer. This may be particularly problematic if long unemployment spells are associated with human capital depreciation that makes it difficult for workers to get back into jobs (e.g. [Schmieder et al., 2016](#); [Kroft et al., 2016](#)). In the presence of such skill depreciation, generous UI systems may lead to a large stock of long-term unemployed with low job finding probabilities ([Ljungqvist and Sargent, 2008](#)). Many countries thus complement their UI system with a wide range of active labor market policies (ALMPs), i.e. programs designed to aid unemployed workers to find jobs either by helping with job search efforts, by building human capital, lower entry barriers into the labor market (via wage subsidies or employment programs), or by creating job search incentives (via monitoring or sanctions). Today, all OECD countries implement a differentiated set of instruments and programs and spend significant resources on ALMPs. E.g. on average, the OECD countries spend 0.6 per cent of their GDP on ALMPs (OECD 2013). In Germany, direct spending on ALMPs for long-term unemployed persons amounts to 3.5 billion Euro annually (BIAJ, 2013). When additionally taking into account administrative costs for staff and rent, this number rises to 8 billion Euro, equalling one per cent of overall public expenditure.

In principle ALMPs may play an important role in reducing the moral hazard of UI benefits and helping the unemployed find back into the labor market.¹ While the potential benefits are clear, there is also concern that many forms of ALMPs may be ineffective, for example because the unemployed acquire skills that are not helpful in their job search, and

¹See [Pavoni and Violante \(2007\)](#) for a theoretical analysis of how such programs may interact with unemployment benefits.

may even be counterproductive, for example because the unemployed spend times in ALMP programs rather than searching for jobs. A key question is therefore whether and which ALMPs are effective and justify their high costs.

A key challenge in evaluating ALMPs is that there are both the direct effects on the program participants as well as indirect effects stemming from threat and spillover effects. The direct effect describes how the job finding rate of an individual who participates in a ALMP program or is sanctioned by a caseworker is affected. The indirect effects on the other hand can affect both program participants as well as individuals who are never selected into a program or personally sanctioned by a caseworker. For example, the mere possibility that an individual could be sanctioned may increase her search effort. Similarly unemployed workers may take up jobs in order to avoid participating an ALMP program, thus making the program effective for individuals who do not participate as in [Black et al. \(2003\)](#). In addition to such threat effects, there may be important spillover effects. One possibility for such spillover effects is that unemployed workers may crowd each other out in the labor market, thus if one worker finds a job due to an ALMP this might make it harder for another worker to do so, as highlighted for example in [Crépon et al. \(2013\)](#) and [Lalive et al. \(2015\)](#). Another possibility for spillover effects would be crowd out of resources within an unemployment agency or perhaps positive learnings spillovers between workers, where non-participants may benefit from participants. While the literature on ALMP evaluations is quite voluminous at this point (for recent surveys see [Card et al., 2010](#), [Card et al., 2018](#), and [Crépon and van den Berg, 2016](#)) and often features high quality research designs such as randomized controlled trials, the vast majority of this research focuses only on the direct effects of ALMPs.² Threat effects and in particular spillover effects are harder to study. For example the commonly used individual level matching approach to construct a control group by design can only capture the spillover effect and similarly for regression discontinuity designs. While RCTs can in principle be randomized at the geographic level (as for example in [Crépon et al., 2013](#)), this is very expensive and difficult to replicate and even if this can

²For a survey on the research on ALMPs in Germany see [Heyer et al. \(2012\)](#).

be achieved is unlikely to allow comparisons of several different types of ALMPs.³

In this paper we study the effectiveness of a wide range of ALMPs in the context of longterm unemployed in Germany using a spatial regression discontinuity design. This design allows us to evaluate a wide range of ALMPs in clean research design that does allow for capturing both threat effects and, at least partially, spillover effects. Our approach exploits the fact that in Germany the longterm unemployed, typically after exhausting regular UI benefits, are assigned strictly geographically to local unemployment agencies, called “Job Centers”, which have significant independence with respect to the intensity and types of ALMPs they use. As a result of this autonomy, unemployed persons assigned to different Job Center districts have different ex-ante probabilities of participating in ALMP programs or being sanctioned. Using exact addresses of almost 3 million long term unemployed, we can compare individuals living very close to the border of Job Center districts. We show that sorting around district borders is unlikely to bias the result and that this can indeed be used for a border discontinuity design to estimate the causal effect of ALMP strategies by different Job Centers. In addition to providing clean causal estimates of a wide range of ALMP instruments, the key contribution relative to the prior literature is that our approach estimates the effects of Job Center level treatments, thus taking into account threat and spillover effects at least to the extent that they occur within Job Centers.⁴

Border discontinuity designs have played an increasingly prominent role in economics. First pioneered in the education literature by [Black, 1999](#) the method has made its way into many context such as the study of historical institutions ([Dell, 2010](#)), minimum wages ([Dube et al., 2010](#)), collective bargaining ([Magruder, 2012](#)), cultural values ([Eugster et al., 2017](#)) or the effect of unemployment insurance extensions ([Dieterle et al., 2020](#); [Boone et al., 2021](#)). But apart from the research on house prices and school quality (see [Black and Machin, 2011](#), for an overview), these papers do not typically use individual-level geographic identifiers and instead rely instead on comparing counties ([Dube et al., 2010](#);

³Threat effects can be studied on the individual level if a person is explicitly assigned to a program, as in [Black et al. \(2003\)](#), or warned of a sanction, but it cannot be studied if threat effects extend to people who merely observe other peoples assignments and feel pressured by that.

⁴We will discuss some of the limitations regarding this below.

Boone et al., 2021) or municipalities (Dell, 2010; Magruder, 2012; Eugster et al., 2017) around borders. A downside of using such cruder geographic information is that the typical regression discontinuity tests (continuity of density and predetermined characteristics around the cutoff) of the identification assumptions are often not implemented and that there are limits to how close to the discontinuity comparisons can be made, thus negating some of the appeal of RD designs.⁵ A methodological contribution in this paper is that it is to our knowledge the first to use geocoded information on individual workers’ addresses to study the impact of labor market policies. This has the advantage of allowing for the typical RD checks. It also is important in our context as it allows us to compare individuals very close to the border (and each other) who, as we show, have access to the same labor market.

However using individual level geocoded data also raises a number of new challenges for identification and concerns whether this allows for a valid design and what kind of treatment effects can be plausibly identified.

The first concern is that individuals may be sorting along Job Center district borders invalidating comparisons across borders. Sorting could be in anticipation of or response to ALMP policies, though given the low saliency of them (the nature of the policy mix is unlikely to be known by the public or to make it into the press) this seems unlikely. More plausible is that sorting happens in response to other policies. Indeed, Job Center districts largely (though not totally) coincide with German counties (“Kreise”) which have some fiscal autonomy, such as setting local business taxes and property taxes. While significant sorting along such local borders has been observed in countries like the US (Black and Machin, 2011) or Switzerland (Basten et al., 2017), there is reason to believe that the relatively small variation in local institutions and a much more centralized education system limits this sorting in Germany. We will address this concern in detail below and will provide extensive evidence on observed sorting around borders.

A second concern is that even if observable and unobservable characteristics of workers vary continuously through space, the spatial density of workers may not. In particular border

⁵Eugster et al. (2017) is a nice exception that does provide a plethora of checks for continuity of other characteristics.

may run along city boundaries, with dense populations on one side of the border and vary sparse or no population on the other side. This creates unique challenges when estimating the effect in a unified regression discontinuity framework and can easily lead to biased estimates. We discuss this issue in Section 3 below and show that this can be resolved through carefully reweighting of observations to smooth out the density.

The third concern is that even if borders give rise to a valid regression discontinuity design, this only estimates the effect of being assigned to one Job Center as opposed to a different Job Center, rather than directly estimating the effect of specific ALMPs. Fortunately for our research design, Job Centers do not vary in terms of passive labor market policies, such as UI benefits and we view essentially everything a Job Center does beyond UI benefit payments as a form of ALMP. To disentangle the effectiveness of individual ALMPs we employ a 2 step procedure in our main analysis where we first use the border discontinuity design to estimate a Job Center Effect (JCE) for each Job Center.⁶ In the second step we then project the JCE on a vector of observable characteristics of the Job Centers such as the amount spent on ALMP programs, the type of ALMPs used or the sanction rate. Thus we obtain cleanly identified JCEs in the first step while using on a regression approach in the second step which relies on observing the relevant features of Job Centers to avoid omitted variable bias. This is similar to [Autor and Houseman \(2010\)](#) who use the quasi-random assignment to welfare-to-work contractors to essentially get contractor effects and then use those to estimate the effects of different placement strategies of these contractors.

Since district borders have very irregular shapes in a two dimensional space (latitude and longitude), there are a number of possibilities to implement a border discontinuity design. We present two different approaches that have different comparative advantages. The first approach is that we use a Distance-to-Border discontinuity design, where we essentially collapse the two dimensional space by calculating for each individual the driving distance to the nearest Job Center district border. This gives rise to many standard one dimensional RD designs with one experiment per border which we can then pool to get overall estimates. The

⁶This approach of essentially using many small binary comparisons to back out a vector of effects for a large number of units is similar to [Avery et al. \(2013\)](#) or [Chetty and Hendren \(2018\)](#).

advantage of this approach is that it is very intuitive and it readily lends itself to graphical analysis akin to class RD designs. Our second approach is to employ a spatial fixed effects RD design, inspired by (Magruder, 2012), which offers a non-parametric approach to directly estimate the spatial discontinuity design in the two dimensional setting. This provides efficient regression estimates for our treatment effects and in particular of the JCEs.

We employ the spatial RD approach to separately evaluate the effectiveness of training measures, wage subsidies, public employment schemes, individual counselling, mobility support and sanctions. In order to take account for potential lock-in effects and to measure medium-term effects, we provide evidence over 48 months after entry into long-term unemployment.⁷ Overall, our results are sobering.⁸ When pooling ALMP policies together, we find evidence for a significantly negative effect of program participation on employment probabilities. This negative effect rises in size over the course of 48 months after participation. At the same time, participation in ALMP programs significantly raises the number of days individuals continue to receive benefit payments over the next four years. The estimates are very similar when using ALMP spending per unemployed as the treatment variable. For sanctions we find essentially a zero effect on long term labor market outcomes. Finally when breaking up ALMPs into different types of programs the only program type with somewhat positive effects that we identify are wage subsidies.

The paper is structured as follows. The next section explains the institutional background, including the role of Job Centers in Germany, and describes the data. In section three we discuss the Distance to Border design with a particular focus on the challenges arising from having individual-level data and non-smooth densities of individuals around borders. We also provide a plethora of graphical evidence both on the identification assumptions and the effects of ALMPs. Section 4 presents the spatial fixed effects approach and provides regression estimates of the effects of ALMPs. Section 5 concludes.

⁷Looking over such a long time horizon is crucial given the conclusion in Card et al. (2018)’s meta-analysis that the average impacts of ALMPs are close to zero in the short run, but rise over longer intervals.

⁸This is broadly consistent with prior research from Germany, such as Lechner and Wunsch (2009); Lechner et al. (2011), though note that this research is all based on matching approaches that rely on a random assignment conditional of observables assumption and naturally cannot take threat or spillover effects into account.

2 Institutional Background, Data and Sample

2.1 Institutional Background

The implementation of ALMPs in Germany is organized as a two-tier system, which follows the systematic split into a regular UI branch, also called unemployment benefits 1 (UI 1) and second tier called unemployment benefits 2 (UI 2), which is a means tested form of benefits with features both of classical UI systems and welfare systems. While the UI 2 system is the fall back option after UI 1 benefits are exhausted, it is not exclusively targeted towards long-term unemployed but also individuals who are not eligible for UI 1 benefits for other reasons, such as lack of labor market attachment.⁹ For simplicity we refer to UI 2 recipients as “long-term unemployed” even though this might include people with only short prior unemployment spells but limited work histories. Since a series of labor market reforms in the early 2000s (‘Hartz reforms’), legal responsibility for the second tier rests with the so-called Job Centers. Active and passive labor market policies in the UI 1 branch are implemented by the local employment agencies, which are organized in larger geographic units (covering, on average, three Job Center districts) and relatively closely tied to the Federal Employment Agency.

In this paper, we are concerned with the effectiveness of ALPMs with respect to UI 2 recipients. For this group, 408 Job Centers in Germany are responsible for implementing both passive and active labor market policies. Job Centers were created in 2005 with the Hartz-reforms and are legally independent public entities. They are provided with global budgets and, as a result, have substantial degrees of freedom of how to actively support UI 2 recipients. While the legal instruments the Job Centers can use are defined in the Social Security Codes II and III, no pre-defined budget exists for the use of specific instruments. In addition, according to §16f in the Social Security Code II, Job Centers are legally allowed to spend part of their budget freely, i.e., on any type of support for the unemployed they deem helpful. Finally, Job Centers have access to various federally funded programs, which

⁹To be eligible for UI 1, a worker must have worked for at least 1 out of the last 3 years.

further increase their room for maneuver. Job Center policies are in most cases steered by the 'Traegerversammlung', which is effectively a governing board consisting of members of the Local Labor Agency and the county administration. This board defines the overall strategy and the priorities to be set within each fiscal year. Given the flexible budgets, case workers can decide freely and on a case-by-case basis which instruments to be implemented for each person, according to her personal needs.

Three features of this overall setting are of key importance for our identification approach. First, the borders of the Job Center districts in most cases coincide with the borders of the 402 counties ('Kreise und kreisfreie Städte') in Germany.¹⁰ The counties themselves have limited authority on labor market or economic policies, but are mainly responsible for administrative issues. In addition, while in the United States access to schools is an important local amenity that leads to sorting along school district borders, in Germany education is funded and administered through the German states. There are differences across states in terms of educational quality, but there is very little variation across counties within states. There are differences in local taxes across municipalities, which, since county borders also run along the municipal borders, gives rise to different tax rates across county borders. The local taxes are business taxes and property taxes. Business taxes may well lead to sorting of business across municipalities but to the extent that individuals close to borders have access to the labor market on either side of the border this should not affect our estimates of the effectiveness of ALMPs. A bigger concern is the presence of local property taxes. That sorting in response to taxes is potentially important is highlighted by [Basten et al. \(2017\)](#) for income taxes in Switzerland, but compared to those tax differences the variation in local property taxes is quite small in Germany. We are not aware of research studying the importance of these taxes for sorting of individuals in Germany, so instead a crucial part of this paper is to show empirically that it does not seem to be the case that individuals sort around Job District borders at least among our sample of long-term unemployed.

¹⁰A notable exception is Berlin which is divided into 8 Job Center districts. Apart from this there are a few cases where counties are divided into two Job Center districts or where two counties constitute a single Job Center district.

The second important institutional feature is that due to the degrees of freedom Job Centers are endowed with, the resources available for ALMP policies and the relative emphasis on different types of ALMPs vary substantially at the Job Center district borders. Figure 1 shows average spending per UI 2 recipient by Job Center district for the years 2007 to 2010. The figure reveals substantial heterogeneity, with spending on ALMP programs ranging from 1000 all the way to around 5000 Euro per unemployed. Furthermore while ALMP spending shows some spatial clustering (such as higher spending in Eastern Germany), there is substantial geographic heterogeneity and there are many borders between Job Centers that show sharp contrasts in spending levels. There is also substantial variation in spending levels over time on the district level

The third geographic feature is that, being effectively NUTS III regions with an average population of 200,000 persons, Job Center districts are relatively small. Especially in densely populated areas their borders cut through local labor markets and commuting zones. For example in Figure 2a) shows the Job Center district borders in Berlin which is divided into 8 Job Centers in total and there are many other similar examples (See Online Appendix Figure A1).

2.2 Data

For our analysis we use the social security data, the so called IEBS, provided by the German Federal Employment Agency. The data contains daily information on employment and unemployment histories on the individual level. The employment data is collected from employment notifications to the social security administration, which are required by law for all jobs liable to social security contributions. These cover roughly 85 percent of employment in Germany. Self-employed and public servants are not contained in the data. The data on unemployment are taken from the German unemployment insurance system. It contains information on benefit receipt, registration as unemployed, and participation in ALMPs. The full data set covers daily information on employment status, wages, benefit receipt, participation in ALMPs, as well as numerous covariates on individual level including precise

information on home and employer addresses. From these data, we have generated summary statistics to gain first insights into the relative importance of different types of instruments and policies. Table 1 contains numbers of participants in the five most frequent types of measures. During the period between 2006 to 2008, 38 per cent of all participants in ALMPs were taking part in measures of public employment, while 31 per cent have undergone training measures. The third row in the table shows that 16 percent of participants were supported under the free funding opportunity contained in §16f (SGB II). Finally, 11 per cent of participants were placed into public employment schemes, while with close to two per cent, wage subsidies play only a minor role.

We complement these information taken from individual level data by aggregate measures on ALMP activities for each Job Center. The first data source we use encompasses annual information on the activities of each Job Center, which is provided by the statistical branch of the Federal Employment Agency on its website. Important in the present context, this data contains information on the absolute and relative numbers of individuals receiving ALG II, as well as on ALMP expenditure by Job Center from 2005 on. In addition to this data, we have obtained the annual budgets provided to each Job Center. These budgets are listed in a reply of the German government to an inquiry from the leftist party 'Die Linke', which is publicly available in the collection of electronic documents in the online library of the German parliament. In Table 2, we have merged this budgetary information for the years 2005 to 2008 with information on unemployment by Job Center region. The upper part of the table contains the budget available for each unemployed person for integration measures ('Eingliederungstitel') like training measures, public employment schemes, and wage subsidies. The lower part of the table shows the annual sums spent for administrative costs ('Verwaltungskosten'), which mainly cover staff costs and rent.

The table yields two notable facts. First, it shows that the Job Centers differ pronouncedly with respect to the budgets available for each unemployed. In 2005, the smallest amount available for each unemployed was 221 Euro (Job Center Munich Land), while the largest amount was 2,823 Euro (Job Center Landau). Secondly, with a rise from 583 to 1,055 Euro,

the sum of integration funds per unemployed has more than doubled over the period of four years. During the same period, administrative funds have risen from 6,7 to 8,5 billion Euros. The more pronounced growth of integration budgets in contrast to administrative budgets provides further evidence for the notion that Job Centers have been able to diversify their policies over time.

2.3 Analysis Sample

Our analysis sample consists of all individuals that entered UI 2 between 2007 and 2010. We start in 2007, since the data on ALMP variation for the first 2 years after the Hartz reforms is very unreliable with many gaps. We focus on the entry cohorts up to 2010, so that we can observe individual labor market histories for a minimum of 48 months, since our data horizon goes up to 2014. Given the German UI system, an UI 2 entrant may either come from receiving UI 1, after she has exhausted her benefits, or directly from employment, e.g., if she did not qualify for UI 1 due to a very short employment spell. An individual may also enter UI 2 after any other spell of non-participation in the labor market (e.g. education, staying at home, living abroad, etc). For our analysis sample we observe the full labor market histories from 2000 to 2014. Furthermore for the years 2007 to 2010 we obtained exact individual home addresses, which we then geocoded using ArcGIS to obtain precise latitude and longitude information. Our final sample size is just under 3 million observations over the 4 UI 2 entry years. Using these geographic coordinates, we also determined the closest Job Center district border for each all individuals. Furthermore, we calculated the shortest distance both as a direct line ('as the crow flies') and the shortest driving distance to this border in kilometers.

3 Graphical Evidence based on Distance-to-Border Design

3.1 Distance-to-Border Regression Discontinuity Design and Balancing Weights

We are interested in estimating the effect of active labor market programs on individual employment outcomes. For every individual i , let c indicate the Job Center district of residence and n the closest neighboring district. We denote a border between the two counties as b , and a subscript b indicates the closest county border for that observation. Let the T_c indicate a treatment such as average expenditure on active labor market programs, or expected days spend in active labor market programs in county c . To the extent that Job Centers treat all employees in their respective county identically, this would translate directly into the same treatment on the individual level. In practice there is of course substantial heterogeneity, both because individuals have a wide variety of characteristics that local caseworkers take into account, but also because different realizations of the job search process for an individual worker will lead to different treatment. For example a worker who finds a job right after entering UI 2 will not be sent to ALMPs by his/her caseworker. Furthermore the ALMP treatment a worker is exposed to goes beyond the individual level assignment into ALMP programs if there are threat or spillover effects. We view T_c as a proxy for the underlying Job Center level treatment variable that captures the propensity of a Job Center to use ALMP programs and will capture both the direct treatment effect as well as spillover and threat effects.

Consider the following model how ALMP treatment may affect employment outcomes:

$$y_{ic} = \beta T_c + \varepsilon_{ic} \tag{1}$$

Since T_{ic} is likely correlated with the state of the labor market in county c , as well as with other county and worker characteristics, estimating this equation using OLS likely leads to significant omitted variable bias.

The problem is that $E[\varepsilon_{ic}|T_c] \neq 0$, since treatment is correlated with many other factors

that vary through space. However, suppose that we can write:

$$\varepsilon_{ic} = f(lat_{ic}, lon_{ic}) + u_{ic}$$

where $E[u_{ic}|lat_{ic}, lon_{ic}] = 0$ and $f(.)$ is a continuous function in space (that is of longitude lon_{ic} and latitude lat_{ic}).

To overcome the omitted variable bias we first employ a Distance-to-Border discontinuity design, where we collapse the twodimensional space into a single “distance” dimension and then use the discontinuity at the boundary as an instrument for the treatment variable. Let $dist_{ib}$ be the distance from the closest border. For each possible treatment variable, we multiply this distance with -1 if individual i is in a district that has a lower value of the treatment T_c then the district on the other side of the border, so that:

$$Dist_{ib} = \begin{cases} -dist_{ib} & \text{if } T_c < T_n \\ dist_{ib} & \text{if } T_c \geq T_n \end{cases}$$

We can then estimate the following equation which represents a pooled regression discontinuity design:

$$y_{ic} = \alpha + \delta T_c + f(Dist_{ib}) + \varepsilon_{ic} \quad (2)$$

Since we have many borders, but the identification strategy derives it’s plausibility from comparing individuals within a specific border, it is natural to also control for border fixed effects in this, so that the identification comes only from individuals living close to either side of a specific border:

$$y_{ic} = \alpha + \delta T_c + f(Dist_{ib}) + \theta_b + \varepsilon_{ic} \quad (3)$$

Estimating equations (2) or (3) using the treatment variables T_c as an outcome as well as employment outcomes, yields a Wald estimator of the treatment effects in equation (1),

by dividing the reduced form by the first stage coefficient.

While this approach appears natural and has been used before in the literature there are several potential pitfalls. A first concern is that even if individuals are close to the same border b , they may be located along very different parts of that specific border and thus not actually that close to each other in practice. Another problem is that borders may run along geographic barriers such as rivers or interstates, such that even individuals very close to each other in terms of direct distance may not be that close in terms of actual travel time and have different access to the local labor market. Figure 2 in the Online Appendix shows the boundary that runs between the cities of Ulm and Neu-Ulm which also follows a river. To deal with these concerns we compute driving distance to the next Job Center district and use this as our distance measure. Furthermore, we identify the border crossing points (that is intersections between streets and the Job Center district borders) that are associated with the shortest driving distance. We can thus estimate equation (3) controlling for border crossing points, rather than just borders. When doing so we guarantee that we are indeed comparing individuals living within close physical and travel distance to each other and thus having similar access to the local labor market.

A second potential pitfall is that density may not be continuous in space. While many borders run through densely populated areas, Figure 2b) shows an example of borders in the outskirts of Stuttgart. While some of the borders run through populated areas, others are along municipal boundaries with populated areas only on one side of the border. In the case of a border where on one side (say A) there are many observations but only very few on the other (B), the border between A and B is not very informative for the effect of interest. In the extreme case of no observations on side B, the border is totally uninformative. Furthermore, if we pool observations from many borders with sharp imbalances in the density of observations along the border, this will also lead to jumps in variables that in reality are actually smoothly distributed at the borders. Online Appendix Figure A4 shows how these jumps can arise in practice. The figure is based on simulated data and just two borders, where the first one exhibits sharp jump in the density at the border while the second border

features a smooth density. In both cases the simulated “education” variable is smooth at the border. When we pool the two borders to generate a typical RD figure in the bottom left, this yields a spurious upward jump at the threshold since on the right side of the threshold the observations from border 2 are much more influential (due to the larger density) thus pushing the average education up. Interestingly, controlling for border fixed effects (bottom right) does not fully solve this. The reason is that the fixed effects themselves are going to pick up the mean value in the border region (that is the full bandwidth used to generate the figure) not necessarily the value right at the border, thus even after pooling there is still a jump, although it is now a downward jump. To the extent that education would be a factor directly affecting outcome variables, this would then bias all results for such outcomes.

To get around this problem we propose to reweight observations around each border to essentially smooth out the discontinuous density. Intuitively at every border the right and the left side should get the same weight when pooling across borders. We break up the sample into distance d bins interacted with borders b . For example observations between 1 and 1.5 km away from border 1 on the left side are one bin with a corresponding bin on the right side. We denote the number of observations in the corresponding bins as $n_{b,d,left}$ and $n_{b,d,right}$ respectively. One simple possibility would be to use the calculate the minimum number of observations on each side of the border and use this as the weight $\omega = \min(n_{b,d,left}, n_{b,d,right})$. In the appendix we show that under the assumption that the variable of interests is distributed homoskedastic and i.i.d, then the weights that provide the most efficient estimate of the difference between the left and the right hand side for bin b, d are given as:

$$\omega_{b,d} = \frac{n_{b,d,right}n_{b,d,left}}{n_{b,d,right} + n_{b,d,left}}$$

. We call these the optimal balancing weights. Note that for large imbalances this is approximately equivalent to $\omega \propto \min(n_{b,d,right}, n_{b,d,left})$, but it is more efficient for smaller differences between the two sides.

Using the simulated data from before, Online Appendix Figure A5 shows that using these balancing weights does indeed solve the problem. The top right panel shows the

pooled RD plot for simulated education with balancing weights (but not border FE) and the distribution is now clearly smooth around the cutoff, though relatively noisy. The bottom right figure shows the pooled RD figure using the optimal balancing weights and controlling for border FE. Note that the education distribution is now smooth as well but also significantly less noisy, suggesting that the main advantage of controlling for border FE after using the balancing weights is not to avoid bias but to increase precision in case of large differences across different borders.

Online Appendix Figure A6 is also based on the same simulated data but now shows results for a simulated outcome variable which is a function of education and a “treatment effect” in form of a jump of 5 units at the border. The left hand side figures clearly show that the pooled RD does not generate consistent estimates of the true treatment effect either with or without controlling for border FE. The right hand side on the other hand shows that using the optimal balancing weights generates consistent point estimates for the treatment effect of around 5 units and the jump is more precise when also including border FE.

3.2 Validity of Design

We next present results on the density and smoothness of predetermined characteristics around Job Center district borders. This requires to take a stance on how to allocate districts for each border to the left or right hand side of the figures (that is whether or not to multiply the distance measure with -1). We focus first on using the average number of days UI 2 entrants spend in ALMP programs in each district as the treatment variable that determines assignment to the left or right. Using our analysis sample, we first calculate for each district and each year the average time individuals who have entered UI 2 in that year spend in an ALMP. If ALMP in the district where the UI 2 recipient lived is smaller than in the next closest district, we multiplied this distance with -1. We restrict the data to individuals who live within 10 kilometers of an inner-German district border. We drop individuals where the closest border is to a neighboring country (e.g., France, Austria, Poland). Furthermore, we drop all borders between counties where the two neighboring counties are not in the same

state in order to avoid confounding effects stemming from differences in state level policies (in practice this restriction makes little difference).

Figure 3 shows the density of observations around the district borders. Each dot consists of all individuals living in a 0.2 kilometer (= 0.12 miles) distance bin to the border. A first noteworthy feature is that the density increases towards the border from both sides. This is due to a simple geometric feature: For example consider a district that is a perfect circle with a 10 km radius. The area of a band that is between 0 and 1 km from the border is given as: $A_2 = \pi(10km)^2 - \pi(9km)^2 = 59.7km^2$, which is more than twice as large as for example a band that is between 5 and 4 km: $A_2 = \pi(5km)^2 - \pi(4km)^2 = 28.3km^2$. Furthermore counties that are small enough can not have anyone living for example 10 km from the closest border. A second noteworthy feature is that the density increases up to the last 2 kilometers from the border and then decreases towards the border. This is because in practice many district borders do not run through cities or towns, but in between them (See Online Appendix Figure A3 for some examples). While they are often very close, this means that fewer people live right next to a border than for example 1 kilometer away from it. And finally, a third feature of this plot is that it is asymmetric, with somewhat more mass to the right, especially in the 2 to 5 km distance range. The reason for this excess mass appears to be that several large cities in Germany, such as Munich or Hamburg, are also their own counties. Since these cities often have higher spending on ALMPs per unemployed, they tend to end up on the right of the cutoff in these graphs and due to their large populations create the hump on the right. While the shape looks unusual for an RD density plot, there is no obvious discontinuity in the density at the cutoff.

An important test for the plausibility of the identification assumptions in the RD design is that predetermined characteristics evolve smoothly at the cutoff. Figure 4 shows how average characteristics of UI 2 entrants in our sample evolve around the border now using 0.5 km bin sizes. Importantly these figures now use the optimal balancing weights derived above and also control for border fixed effects.¹¹ Panel (a) shows the average years of schooling

¹¹The figures are generated by regressing the dependent variable on the distance bins as well as border FE and then plotting the estimated bin effects. The optimal balancing weights are used as weights in this

of UI 2 recipients. The relationship is essentially flat with no jump at the discontinuity. The same holds for a number of other predetermined characteristics such as experience, number of days spend in UI 1 in the previous 3 years or previous employment (Panels b-d). While not shown here, several of these variables would show small but significant jumps at the boundary in the absence of using the balancing weights, highlighting the practical importance of dealing with the non-discontinuous density. Importantly when using the balancing weights, all characteristics evolve very smoothly around the cutoff and do not suggest that there is any sorting at the cutoffs but instead that individuals on both sides are quite comparable to each other.

Another important validity check is whether workers do indeed have access to the same labor market. To investigate this we calculate the probability that a worker’s last pre-unemployment job was in the county on the “left side” for each local border. Figure 4e) shows that apart from a very small jump this is also very smooth around the border. The small jump may stem from the fact that there are actually a sizable number of workers that have very short commutes (0.2 km), perhaps because they live in the building where they work. Nevertheless, the figure clearly shows that workers are quite fluid across the Job District borders. This is also highlighted by Figure 4f), which shows the share of workers whose post-unemployment spell is on the left hand side of the border. Again, this figure only shows a tiny jump at the cutoff highlighting that workers clearly have access to the same labor market on both sides of the cutoff.

While not shown here, the results of these validity checks are very similar if we generate the same figures for other treatments such as the ones we use below to look at outcomes.

3.3 The Effect of ALMP on Labor Market Outcomes

We now turn to estimates of the effect of ALMPs using the Distance-to-Border RD Design outlined in section 3.1. As a first step, we document that ALMP intensity does indeed increase significantly at district borders. Figure 5 (a) shows the average number a new entrant into regression.

UI 2 spends in ALMP over the subsequent 4 year period. As can be seen, to the left of the border, UI 2 entrants spend about 95 days in various active labor market programs. Right at the border there is a sharp jump to around 115 days, suggesting that individuals who live just on the other side of the border spend on average around 20 days more in ALMPs than individuals who are entering UI 2 in counties with more restrictive ALMP policies than their neighbors. Figure 5 (b) shows the same type of figure but using the average spending on ALMP programs as the outcome variable. Interestingly using this treatment assignment by ALMP days, there is no corresponding jump in ALMP spending, which suggests that the larger number of days must be associated with cheaper programs.

Turning to outcomes, Figure 5 (c) shows the number of days individuals receive UI 2 benefits over the next 4 years. Quite strikingly, time spent in UI 2 increases discontinuously at the border by almost 20 days, suggesting that more intense ALMP policies by Job Centers keep UI 2 recipients longer in UI 2, rather than helping them to exit unemployment.

This is consistent with panel (d), which shows no increase in employment at the cutoff and if anything a small decline in employment over the next four years. While in the short run small or negative employment effects could be due to the lock-in effect (program participants not working while on the program), these results suggest that even over a 4 year horizon a higher intensity of ALMPs does not improve labor market outcomes, despite their relatively large costs.

Figure 6 shows results when using average ALMP spending per unemployed as the treatment variable, that is as the variable to decide which district at a particular border to put on the right hand side of the figure. Figure 6 (b) shows the first stage, where ALMP spending (in 1000 Euro per person) is the dependent variable. There is a large jump at the border of almost 500 Euro per person. Figure 6 (a) shows that when ordering districts along this dimension there is also a sizable jump in the number of days spend in ALMP at the border. Given that ALMP spending increases by about 20% at the border and number of ALMP programs increase by about 10 percent it appears that the cost per program day is slightly higher on the right hand side as well, though note that we only use spending over a

single year as the outcome variable, while days in ALMP is measured over 4 years. Despite the fact that ALMP intensity thus increases by every measure at the discontinuity, Figure 6 (c) again shows a sobering result: Days spend in UI 2 benefits increase substantially at the border and Figure 6 (d) shows that this is also associated with a small decrease in days spend in employment. Again it appears that ALMP programs do not help workers find into jobs and at least in the aggregate are counterproductive.

Figure 7 shows results when assigning Job Center districts according to their average sanction rate. The rate captures the average share of UI 2 recipients who are sanctioned (that is experience a cut in their benefits) in any given month. On the left side of the border in Panel (a) there are on average 2 percent of UI 2 recipients sanctioned in a given month, while this increases to almost 3 percent on the right side. Given that most sanctions are relatively short this still means that a sizable number of people are affected by sanctions at some point. Panel (b) shows that the increase in sanction intensity is only very weakly associated with an increase in ALMP spending and (not shown) days in ALMP. Figure 7 (c) and (d) however suggest again that sanctions are not particularly effective in getting people to leave UI 2 or to increase their employment.

3.4 Dynamic Estimates

The previous literature has suggested that the time horizon is important for evaluating the effects of ALMPs, since in the short run lock-in effects can dominate any positive employment effects which are more likely to show up in the long run. To develop a deeper picture of the dynamic effects, we estimate how the treatments and outcomes of UI 2 recipients vary at the district border using a dynamic regression discontinuity design:

$$y_{ict} = \alpha_t + \delta_t D(Dist_{ib} \geq 0) + f_t(Dist_{ib}) + \varepsilon_{ict} \text{ for } t = -12, \dots, 48 \quad (4)$$

where the subscript t indexes the month since entry into UI 2. We estimate this equation separately for each month relative to the entry into UI 2. The estimates for α_t trace out how the dependent variable changes over the months before and after entry into UI 2 for

individuals just next to the border on the side with less ALMPs, while the estimate of δ_t provide an estimate of how much the profile shifts, relative to individuals on the other side. We then plot the resulting estimates for α_t and δ_t for the individuals to the left and to the right side of the district border.

Figure 8 shows these dynamic results for some of the main outcomes. Panel (a) shows days spent in ALMP. Since many UI 2 entrants received UI 1 before (and exhausted UI 1), many were also in ALMP prior to UI 2 entry. However, the levels were essentially identical, giving us further confidence in the border RD design since it shows that there are no differences in terms of UI 1 policies that could confound our results. After UI 2 entry the gap opens up and individuals in the high intensity ALMP counties spend about 50 percent more time in ALMPs than in the low intensity counties just on the other side of the border. Interestingly the difference appears immediately and remains pretty constant from around 6 months onwards and it does not appear that it is concentrated at a particular time of the UI 2 spell.

Panel (b) shows dynamic results for days in UI 2 as an outcome variable. By construction these are 0 before $t = 0$ and then jump up to almost 30 days (i.e. almost all UI 2 entrants initially spend at least a full month in UI 2). As people exit UI 2 this decreases slowly but noticable faster so in the districts with lower ALMP intensity.

In Panel (c) we look at days employed as an outcome variable. Prior to entering UI 2, individuals spent on average about 12 days per month in employment (or a bit more than 1/3 of individuals were employed), which drops in the months before entering UI 2. Again reassuringly the pre-trends and levels are almost identical and furthermore over the first 4-5 months there is no difference after entering UI 2. Interestingly there thus does not appear to be a lock-in effect early on. After about 8 months there is a small gap with the group on the low ALMP intensity side of the border returning quicker to work with about 0.5 additional days per month in employment. This difference remains constant throughout the next 24 months or so and only slowly narrows towards the end. This is consistent with lock-in effects, since ALMP levels are also higher throughout the entire 24 months period, but there is little

sign of catch up even over this very long time horizon and it seems unlikely that individuals in the high ALMP intensity districts will ever catch up the lost time out of work.

Panel (d) of Figure 8 shows that the negative employment effects of ALMP also translate into lower earnings of around 10 to 15 Euro per month from month 5 onwards and then increasing to almost 50 Euro (or 10 percent) towards the end of the time horizon. Unlike for employment, there does not appear to be any catch up and ALMP seems to be associated with very sizable decreases in long-term earnings. Thus it is not the case that ALMP programs compensate for the time out of work by getting people into better jobs conditional on working.

Overall, these results paint a bleak picture of the effectiveness of ALMPs. While this graphical analysis is very intuitive, it is less well suited to disentangle the effectiveness of individual programs or to obtain point estimates of the treatment effects of these programs. We therefore turn to the regression analysis next.

4 Regression Estimates based on Spatial FE Design

4.1 Spatial Fixed Effects Estimator

In order to obtain regression estimates for a number of treatment effects we follow the spatial fixed effects approach used by Magruder (2012), which in turn is based on Conley and Udry (2010) and Goldstein and Udry (2008).

Consider the data generating process from equation (1) above again:

$$y_{ic} = \beta T_c + \varepsilon_{ic} \tag{5}$$

where we still assume that the error term can be written as: $\varepsilon_i = f(lat_i, lon_i) + u_i$ where $E[u_i|lat, lon] = 0$ and $f(\cdot)$ is a continuous function in space. Let $\mathcal{R}(i)$ be the set of all individuals within a radius of r of individual i . Consider all individuals i' within a radius r .

By continuity of $f()$ we get that

$$\lim_{r \rightarrow 0} \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} f(lat_{i'}, lon_{i'}) = f(lat_i, lon_i)$$

This yields

$$\begin{aligned} \lim_{r \rightarrow 0} \left\{ y_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} y_{i'} \right\} &= \lim_{r \rightarrow 0} \left\{ \beta \left[T_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} T_{i'c'} \right] + \varepsilon_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} \varepsilon_{i'} \right\} \\ &= \beta \lim_{r \rightarrow 0} \left\{ \left[T_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} T_{i'c'} \right] \right\} + \lim_{r \rightarrow 0} \left\{ u_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} u_{i'} \right\} \end{aligned}$$

Let $\tilde{T}_i = \lim_{r \rightarrow 0} \left\{ \left[T_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} T_{i'c'} \right] \right\}$ and $\tilde{y}_i = \lim_{r \rightarrow 0} \left\{ y_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} y_{i'} \right\}$ and we get that the estimator

$$\hat{\beta} = \frac{Cov(\tilde{y}_i, \tilde{T})}{Var(\tilde{T})}$$

is a consistent estimator for β , since $\lim_{r \rightarrow 0} \left\{ u_{ic} - \frac{1}{n_{\mathcal{R}(i)}} \sum_{i' \in \mathcal{R}(i)} u_{i'} \right\} = 0$. This suggests a straightforward estimator, we simply transform the treatment and outcome variables of interest, by subtracting for each person the average of treatment / outcome variable in a radius r from the treatment / outcome of that person. Once we have the transformed variables $\tilde{T}_i$ and $\tilde{y}_i$, we regress the transformed outcome variable on the transformed treatment variable to obtain an unbiased estimate of β .

$$\tilde{y}_{ic} = \beta \tilde{T}_c + \varepsilon_{ic} \tag{6}$$

The intuition behind this transformation is straightforward. Since factors ε_{ic} influencing the outcome apart from the treatment variable vary smoothly through space, subtracting the average of individuals in a small neighborhood will difference out the influence of these factors, but also differences in the treatment. Only for individuals at the border where T_c varies discontinuously is not all the variation differenced out and thus the observations close to the cutoff provide the identification.

It is also straightforward to estimate the effect of multiple treatment variables using this approach by transforming each of the treatment variables and then estimating:

$$\tilde{y}_{ic} = \beta_1 \tilde{T}_c^{EmpProg} + \beta_2 \tilde{T}_c^{TrainingProg} + \beta_3 \tilde{T}_c^{WageSub} + \beta_4 \tilde{T}_c^{SanctionRate} + \varepsilon_{ic} \quad (7)$$

Conceptually, the border RD design cleanly identifies the role of assigned to specific Job Centers, while then assigning the impact of Job Centers to a set ALMP programs relies on a conditional independence assumption that other unobserved differences between Job Centers do not drive the correlations. We can make this explicit by estimating the effects of ALMPs in a 2-step procedure, where we first estimate Job Center Effects (JCE), denoted by κ_c using the Spatial FE method:

$$\tilde{y}_{ic} = \sum_{j \in J} \tilde{I}_{j=c} \kappa_j + \varepsilon_{ic} \quad (8)$$

The estimated vector of JCE, $\hat{\kappa}_c$, can then be projected onto the full set of observable characteristics we have on Job Centers:

$$\hat{\kappa}_c = \beta_1 T_c^{EmpProg} + \beta_2 T_c^{TrainingProg} + \beta_3 T_c^{WageSub} + \beta_4 T_c^{SanctionRate} + \varepsilon_c \quad (9)$$

In principle, this two-step approach is equivalent to the single-step estimating of equation (7). An important practical difference however is that for a significant number of Job Centers we do not have valid information on ALMP programs. If a district has missing data then all of its borders with adjacent districts cannot be used in the single-step approach and we lose a lot of power. A key advantage of the two-step estimator is that the first step does not rely on information on ALMP programs, thus we can always use all districts and thus all borders to identify the JCE in equation (8). Then in the estimation of equation (9) we can reduce the observations to districts with non-missing data. This is clearly more efficient than the single step approach. To see this very intuitively imagine three districts A, B, and C, in a row such that A borders B and B borders C without any other borders. Suppose that ALMP information is missing for B. In that case there are no valid border comparisons for estimating (7). However the two step method will still recover the JCE of A, B and C

and thus allow comparing the relative effectiveness of A and C and how this is correlated with different ALMP strategies.

4.2 Estimates based on Spatial FE Design

Note that this section is work in progress and the reported results here should be viewed as preliminary. In particular note that the spatial FE transformation described above is conceptually simple but computationally somewhat challenging as it requires finding for all 3 million observations all neighboring observations among this 3 million observations that are within the radius r . A naive way of computing this would thus require 9 trillion distance comparisons and would take a very long time.¹² At this stage we have results for estimations of equation (7) and we are still working on the implementing the two step method which has the additional challenge of being very memory hungry due to the large number of Job Center dummies that have to be transformed.

The spatial FE estimator does not suffer from the issue of non-smooth densities around borders. Given a radius r for the spatial FE transformation, each individual is only compared to other individuals who are at most r kilometers away from them. By choosing a smaller and smaller r we are getting arbitrarily close to the ideal of only comparing individuals who live right next to each other but on different sides of a district border.

We apply the spatial FE transformation to our data and present results here for a radius of 1 km (0.62 miles). This means that only observations that are within 1km of Job Center district borders contribute to the identification of the results. This short distance guarantees that we are comparing individuals who are very close to each other and within the same labor market.

Table 3 shows spatial FE estimates, that is estimates of equation (6). Each panel corresponds to a different treatment variable and a separate regression. The first panel shows our baseline treatment of average days spent in ALMP programs in a Job Center. The first column shows actual days spent in ALMP programs. Similar to the graphical evidence there

¹²Using highly optimized Mata code this runs in a few hours, but took significantly longer without careful optimization. The code is available on request.

is a clear increase in days spent in ALMP at the district border if individuals are assigned to higher ALMP intensive districts. The effect is 0.66 and highly statistically significant but also smaller than 1, which suggests that average differences between Job Centers are larger than the jumps at the border which is likely because averages are driven both by different compositions of the unemployed and different labor market conditions as well as different propensities to use ALMP in a Job Center. Nevertheless, the Job Center strategies clearly matter for individual ALMP assignments. Column (2) shows the effect on days spent in UI 1 over the next 4 years after UI 2 entry. There is a small negative effect, that is individuals are less likely to spend time in UI 1 if they are in districts with higher ALMP intensity. Most UI 2 recipients in our sample can only get back into the UI 1 system after a long employment spell (at least 1 year) that qualifies them for regular UI 1 benefits. Thus the negative coefficient foreshadows the negative employment effects of ALMP. Column (3) shows that for each day in ALMP workers spend on average an additional 1.4 days in UI 2 and similarly, column (4) shows that they spend an additional 1.1 days registered as job searchers. Thus ALMP seems to significantly increase benefit receipt and prolong unemployment spells. Finally, column (5) shows that time in employment is also reduced by 0.7 days for each day in ALMP.

Several things are striking about these magnitudes: first, the effect of ALMP on days in UI 2 is larger than 1 and therefore larger than a pure lock-in effect without any positive effect of ALMP would explain. It seems like 1 day in ALMP not only keeps a worker 1 more day in UI 2 but more than that. There are several possible explanations for this: Workers may already reduce job search efforts in anticipation of going to an ALMP program. Thus if a worker is assigned to a program that starts in two weeks, he may not search for a job over those two weeks in addition to the program duration. Another possibility would be that the ALMP programs are reducing job-finding probabilities after program completion. For example, a training program may raise false or unrealistic expectations about what kind of job a worker can find thus reducing job-finding probabilities. The fact that days employed are reduced by less than the increase of days in UI2 may in part be due to ALMP programs such as employment programs and wage subsidies that mechanically increase employment.

The second panel of Table 3 shows the results for ALMP spending per person as a treatment variable. The right-hand side variable is measured in 1000 Euro per UI recipient and year. The results are quite consistent with the prior Panel: An additional 1000 Euro in ALMP spending per person increases days in ALMP, leading to a small reduction in time in UI 1 benefits, while significantly increasing time in UI 2 and as searching for a job. Finally, there is a large (42 days) reduction in employment.

The third panel of Table 3 shows the results for the sanction rate on the Job District level. Consistent with the graphical evidence this has only small effects on most outcomes, though it does lead to a small reduction in Days in UI 2, but this may be mechanical since a sanction is a forced reduction in UI 2. Consistent with the interpretation that this is mechanical, we do not see an effect on days registered for job search or days employed.

A particularly attractive feature of the border discontinuity design is that, unlike in most other RD designs, there are a very large number of discontinuities, namely one at each between-district border. Since the mix of different ALMPs varies across Job Centers, each border constitutes a slightly different experiment since the mix of ALMPs changes differently at each one. This allows us to separately identify the effects of different ALMPs, which would not be possible with a single experiment or a single border. For that purpose, we estimate the following equation,

$$xxx$$

where we allow for differential effects of 4 major groups of ALMP policies.

Table 4 shows estimates of equation (7). The results are consistent with the results that analyze one treatment at a time, but shed some additional light on this. Employment programs and training programs look particularly bad in this Table. They significantly increase time in UI 2, while reducing days in employment. This is particularly striking for employment programs since such programs have a mechanical positive effect on employment, but it appears that over this long time horizon, this positive effect is cancelled out and leads to a long term negative impact. Similarly an increase in the average days in training programs by one day leads to a 2.5 days decline in days employed over the next 4 years. The one

variable with a clear positive effect is wage subsidies which lead to a decline in time spent in UI 2 and an increase in employment. Here an important question is a more careful benefit-cost analysis of this since of course a direct subsidy to employment should indeed increase employment, but it is noteworthy that it is at least not counterproductive like the others. Finally the sanction rate is still not strongly correlated with outcomes and does not appear to be a particularly effective tool to get workers back to employment.

As stated before this section is preliminary with much work that still needs to be done, such as comparing different bandwidth, looking at additional outcomes, controlling for more Job Center characteristics, implementing the 2-step approach and more.

5 Conclusion

Active labor market programs are a major policy tool with many governments and UI agencies putting high hopes in these programs to help job seekers find back into work faster. The spatial regression discontinuity design proposed in this paper provides a powerful method to identify the causal effects of such ALMP programs on labor market outcomes.

While the results presented here are preliminary, they appear somewhat sobering at this stage. Over a 4 year horizon, individuals who are assigned to Job Centers that invest significantly higher resources into a variety of ALMP policies do not appear to benefit from these programs but rather spend more time on unemployment benefits 2, spend less time in employment and have lower monthly earnings. The spatial FE analysis that breaks up ALMP programs into their individual components find that employment programs and training programs appear to be particularly ineffective. The one positive effect are wage subsidies which have a positive employment effect and reduce dependency on UI 2.

REFERENCES

- Autor, David H. and Susan N. Houseman**, “Do Temporary-Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from "Work First",” *American Economic Journal: Applied Economics*, July 2010, 2 (3), 96–128.
- Avery, Christopher N., Mark E. Glickman, Caroline M. Hoxby, and Andrew Metrick**, “A Revealed Preference Ranking of U.S. Colleges and Universities *,” *The Quarterly Journal of Economics*, February 2013, 128 (1), 425–467.
- Basten, Christoph, Maximilian von Ehrlich, and Andrea Lassmann**, “Income Taxes, Sorting and the Costs of Housing: Evidence from Municipal Boundaries in Switzerland,” *The Economic Journal*, 2017, 127 (601), 653–687. _eprint: <https://onlinelibrary.wiley.com/doi/pdf/10.1111/eoj.12489>.
- Black, Dan A., Jeffrey A. Smith, Mark C. Berger, and Brett J. Noel**, “Is the Threat of Reemployment Services More Effective Than the Services Themselves? Evidence from Random Assignment in the UI System,” *American Economic Review*, September 2003, 93 (4), 1313–1327.
- Black, Sandra E.**, “Do Better Schools Matter? Parental Valuation of Elementary Education*,” *The Quarterly Journal of Economics*, May 1999, 114 (2), 577–599.
- **and Stephen Machin**, “Chapter 10 - Housing Valuations of School Performance,” in Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, eds., *Handbook of the Economics of Education*, Vol. 3, Elsevier, January 2011, pp. 485–519.
- Boone, Christopher, Arindrajit Dube, Lucas Goodman, and Ethan Kaplan**, “Unemployment Insurance Generosity and Aggregate Employment,” *American Economic Journal: Economic Policy*, May 2021, 13 (2), 58–99.
- Card, David, Jochen Kluve, and Andrea Weber**, “Active Labour Market Policy Evaluations: A Meta-Analysis*,” *The Economic Journal*, 2010, 120 (548), F452–F477. _eprint: <https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1468-0297.2010.02387.x>.
- , — , **and** — , “What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations,” *Journal of the European Economic Association*, June 2018, 16 (3), 894–931.
- Chetty, Raj and Nathaniel Hendren**, “The Impacts of Neighborhoods on Intergenerational Mobility II: County-Level Estimates*,” *The Quarterly Journal of Economics*, August 2018, 133 (3), 1163–1228.
- Conley, Timothy G. and Christopher R. Udry**, “Learning about a New Technology: Pineapple in Ghana,” *American Economic Review*, March 2010, 100 (1), 35–69.
- Crépon, Bruno and Gerard J. van den Berg**, “Active Labor Market Policies,” *Annual Review of Economics*, 2016, 8 (1), 521–546. _eprint: <https://doi.org/10.1146/annurev-economics-080614-115738>.

- , **Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora**, “Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment *,” *The Quarterly Journal of Economics*, May 2013, *128* (2), 531–580.
- Dell, Melissa**, “The Persistent Effects of Peru’s Mining Mita,” *Econometrica*, 2010, *78* (6), 1863–1903. _eprint: <https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA8121>.
- Dieterle, Steven, Otávio Bartalotti, and Quentin Brummet**, “Revisiting the Effects of Unemployment Insurance Extensions on Unemployment: A Measurement-Error-Corrected Regression Discontinuity Approach,” *American Economic Journal: Economic Policy*, May 2020, *12* (2), 84–114.
- Dube, Arindrajit, T. William Lester, and Michael Reich**, “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties,” *The Review of Economics and Statistics*, November 2010, *92* (4), 945–964.
- Eugster, Beatrix, Rafael Lalive, Andreas Steinhauer, and Josef Zweimüller**, “Culture, Work Attitudes, and Job Search: Evidence from the Swiss Language Border,” *Journal of the European Economic Association*, October 2017, *15* (5), 1056–1100.
- Goldstein, Markus and Christopher Udry**, “The Profits of Power: Land Rights and Agricultural Investment in Ghana,” *Journal of Political Economy*, December 2008, *116* (6), 981–1022. Publisher: The University of Chicago Press.
- Heyer, Gerd, Susanne Koch, Gesine Stephan, and Joachim Wolff**, “Evaluation der aktiven Arbeitsmarktpolitik: Ein Sachstandsbericht für die Instrumentenreform 2011,” *Journal for Labour Market Research*, March 2012, *45* (1), 41–62. Number: 1 Publisher: SpringerOpen.
- Kroft, Kory, Fabian Lange, Matthew J. Notowidigdo, and Lawrence F. Katz**, “Long-Term Unemployment and the Great Recession: The Role of Composition, Duration Dependence, and Nonparticipation,” *Journal of Labor Economics*, January 2016, *34* (S1), S7–S54. Publisher: The University of Chicago Press.
- Lalive, Rafael, Camille Landais, and Josef Zweimüller**, “Market Externalities of Large Unemployment Insurance Extension Programs,” *American Economic Review*, December 2015, *105* (12), 3564–3596.
- Lechner, Michael and Conny Wunsch**, “Are Training Programs More Effective When Unemployment Is High?,” *Journal of Labor Economics*, October 2009, *27* (4), 653–692. Publisher: The University of Chicago Press.
- , **Ruth Miquel, and Conny Wunsch**, “Long-Run Effects of Public Sector Sponsored Training in West Germany,” *Journal of the European Economic Association*, August 2011, *9* (4), 742–784.
- Ljungqvist, Lars and Thomas J. Sargent**, “Two Questions about European Unemployment,” *Econometrica*, 2008, *76* (1), 1–29. _eprint: <https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.0012-9682.2008.00816.x>.

- Magruder, Jeremy R.**, “High Unemployment Yet Few Small Firms: The Role of Centralized Bargaining in South Africa,” *American Economic Journal: Applied Economics*, July 2012, 4 (3), 138–166.
- Pavoni, Nicola and G. L. Violante**, “Optimal Welfare-to-Work Programs,” *The Review of Economic Studies*, January 2007, 74 (1), 283–318.
- Schmieder, Johannes F. and Till von Wachter**, “The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation,” *Annual Review of Economics*, 2016, 8 (1), 547–581. _eprint: <https://doi.org/10.1146/annurev-economics-080614-115758>.
- , – , and **Stefan Bender**, “The Effect of Unemployment Benefits and Nonemployment Durations on Wages,” *American Economic Review*, March 2016, 106 (3), 739–777.

Table 1: Share of ALMPs by Type

	2006	2007	2008	All Years
Education and Training	31.5	30.0	32.1	31.2
Public Employment Schemes	37.7	39.5	39.3	34.1
Wage Subsidies	3.2	3.4	3.3	3.3
External Placement Service	11.1	8.2	13.9	11.0
Flexible Funding	14.8	19.1	16.6	16.8

Table 2: Descriptive Statistics of Job Center Budgets

Year	Mean	SD	Min	Max
Integration funds per unemployed by jobcenter				
2005	1,061	583	221	2,823
2006	1,586	751	417	4,700
2007	1,852	852	722	4,919
2008	2,388	1,055	594	6,884
Administrative funds by job center (in Million Euro)				
2005	6,715	116,999	206,781	814
2006	7,688	13,472	239,716	1,076
2007	8,032	13,644	233,596	1,080
2008	8,549	16,359	267,963	1,099

Table 3: The Effect of Average Days in ALMP on Labor Market Outcomes - Border Discontinuity Design using Spatial Fixed Effects

	(1) Days in ALMP	(2) Days UI 1 Benefits	(3) Days UI 2 Benefits	(4) Days registered job search	(5) Days employed
Avg. Days in ALMP	0.66 [0.051]**	-0.098 [0.048]*	1.43 [0.21]**	1.11 [0.19]**	-0.69 [0.21]**
Observations	2402485	2402485	2402485	2402485	2402485
ALMP Expenditure (per person)	18.5 [2.79]**	-3.01 [1.77]	66.4 [7.70]**	73.8 [7.18]**	-42.7 [7.99]**
Observations	2716303	2716303	2716303	2716303	2716303
Sanction Rate (pp)	2.48 [2.33]	0.47 [1.47]	-15.8 [6.40]*	4.12 [5.98]	-3.65 [6.64]
Observations	2680245	2680245	2680245	2680245	2680245

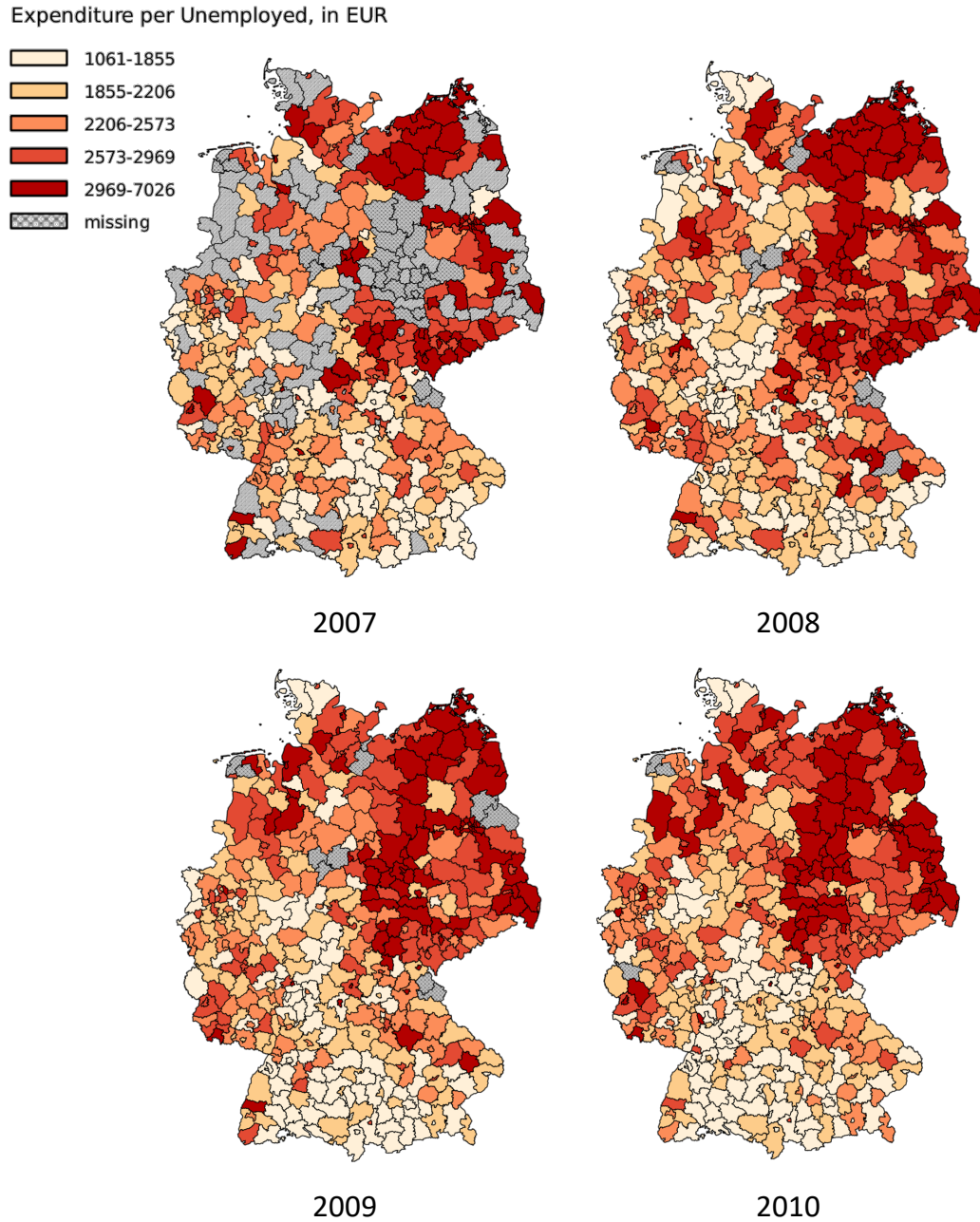
Notes: Coefficients from Border Discontinuity - Spatial FE Regressions with 1km radius. Outcomes are the total number of days over next 4 years after entering UI 2. Treatment variables are the average number of days (sanction share) of all workers at the jobcenter that a UI 2 recipient is assigned to over the next 4 years. Standard errors clustered on job center district level (* P<.05, ** P<.01)).

Table 4: Comparing the Effects of Different Types of ALMP on Labor Market Outcomes - Border Discontinuity Design using Spatial Fixed Effects

	(1) Days in ALMP	(2) Days UI 1 Benefits	(3) Days UI 2 Benefits	(4) Days registered job search	(5) Days employed
Days in Employment Program	0.95 [0.26]**	-0.15 [0.090]	4.72 [0.40]**	4.26 [0.37]**	-1.63 [0.41]**
Days in Training Program	0.87 [0.21]**	-0.23 [0.069]**	3.23 [0.30]**	3.02 [0.28]**	-2.47 [0.31]**
Days in Wage Subsidy	0.11 [0.43]	0.65 [0.15]**	-5.24 [0.67]**	-2.03 [0.62]**	3.24 [0.69]**
Sanction Rate (pp)	1.85 [2.68]	0.23 [0.86]	-0.48 [3.76]	10.5 [3.50]**	-4.82 [3.86]
Observations	2326169	2326169	2326169	2326169	2326169

Notes: Coefficients from Border Discontinuity - Spatial FE Regressions with 1km radius. Outcomes are the total number of days over next 4 years after entering UI 2. Treatment variables are the average number of days (sanction share) of all workers at the jobcenter that a UI 2 recipient is assigned to over the next 4 years. Standard errors clustered on job center district level (* P<.05, ** P<.01)).

Figure 1: Spending on ALMP per Recipient of Unemployment Insurance 2 by Job Center District

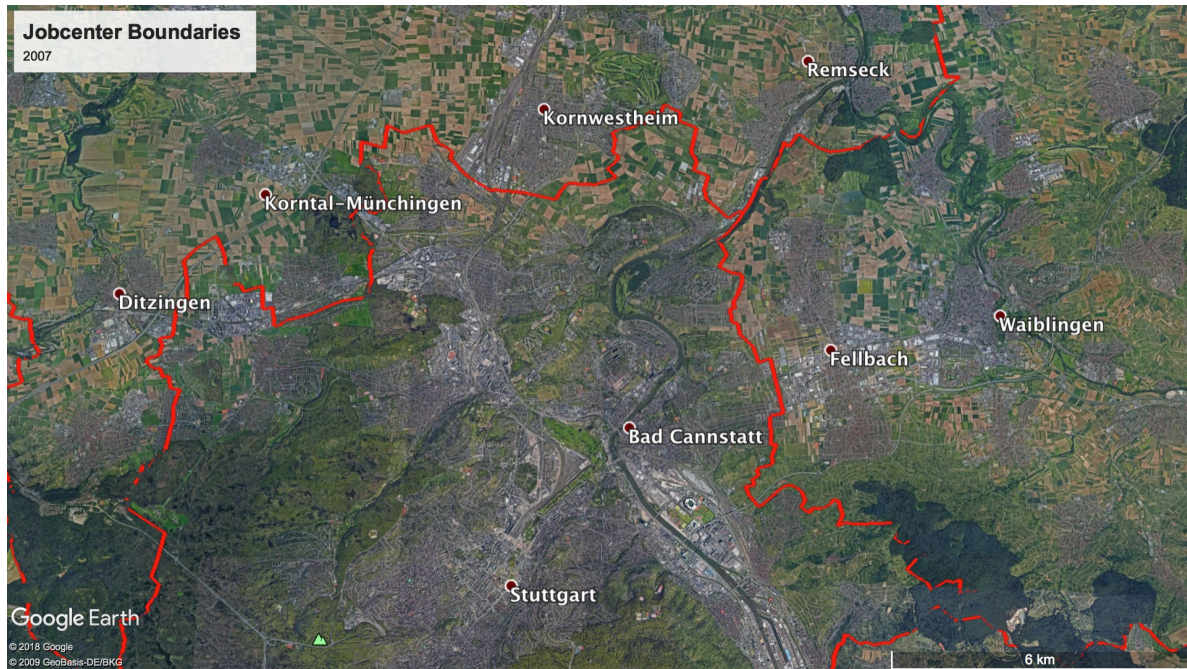


Notes: The maps shows the average spending on ALMP per UI2 recipient by job district. Spending is measured in Euro.

Figure 2: Examples of Job Center District Borders



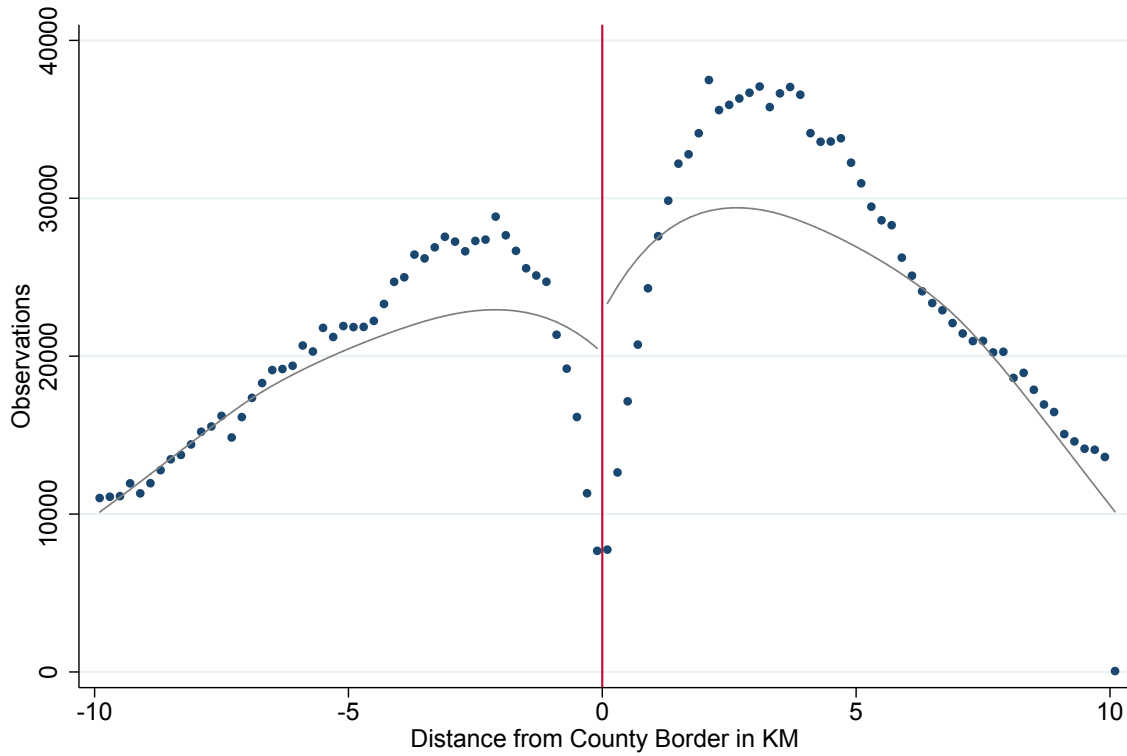
(a) Berlin



(b) Stuttgart

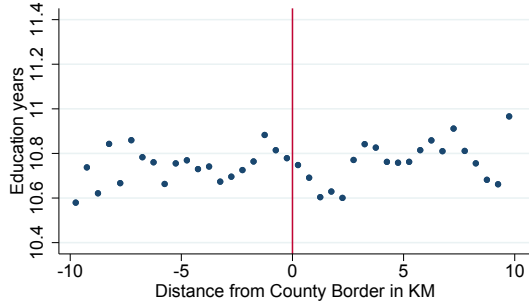
Notes: The maps show boundaries of Job Center districts as of 2007 for select areas of Germany. The maps are generated using Google Earth.

Figure 3: Number of UI2 Recipients Around County Borders - Low ALMP Density on the Left

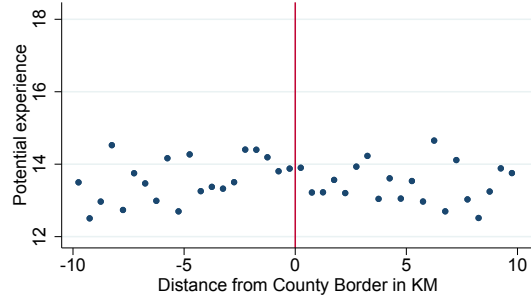


Notes: Each bin contains the number of individuals within a 200 meter bandwidth of distance from the nearest county border. For each individual the distance is measured as the nearest driving distance to the nearest county border. If the adjacent border has a higher average intensity of ALMP, the distance is multiplied with -1, so that the observations to the left correspond to individuals who are on the side with a lower propensity to send the long term unemployed into active labor market programs.

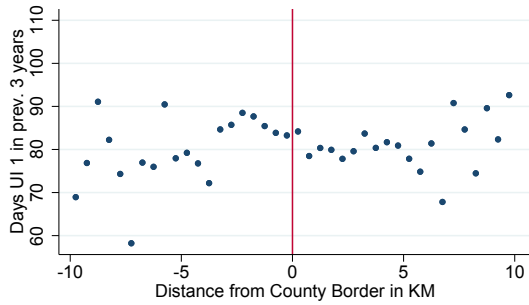
Figure 4: Validity of Distance to Border RD Design - Low ALMP Density District on the Left



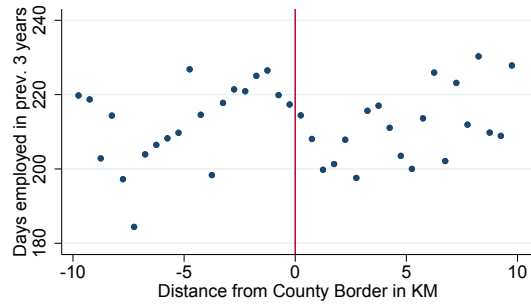
(a) Average years of schooling



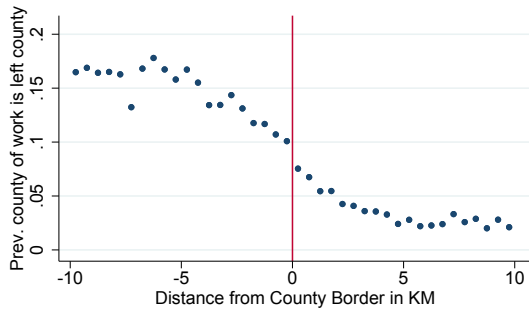
(b) Experience



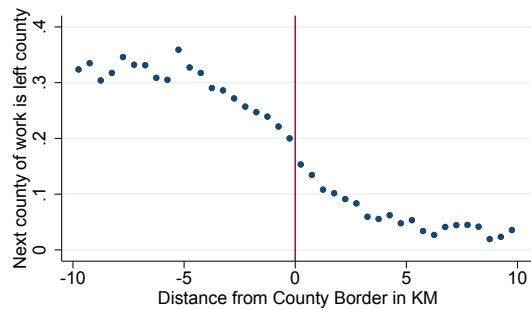
(c) Days on UI1 in prev. 3 years



(d) Days worked in prev. 3 years



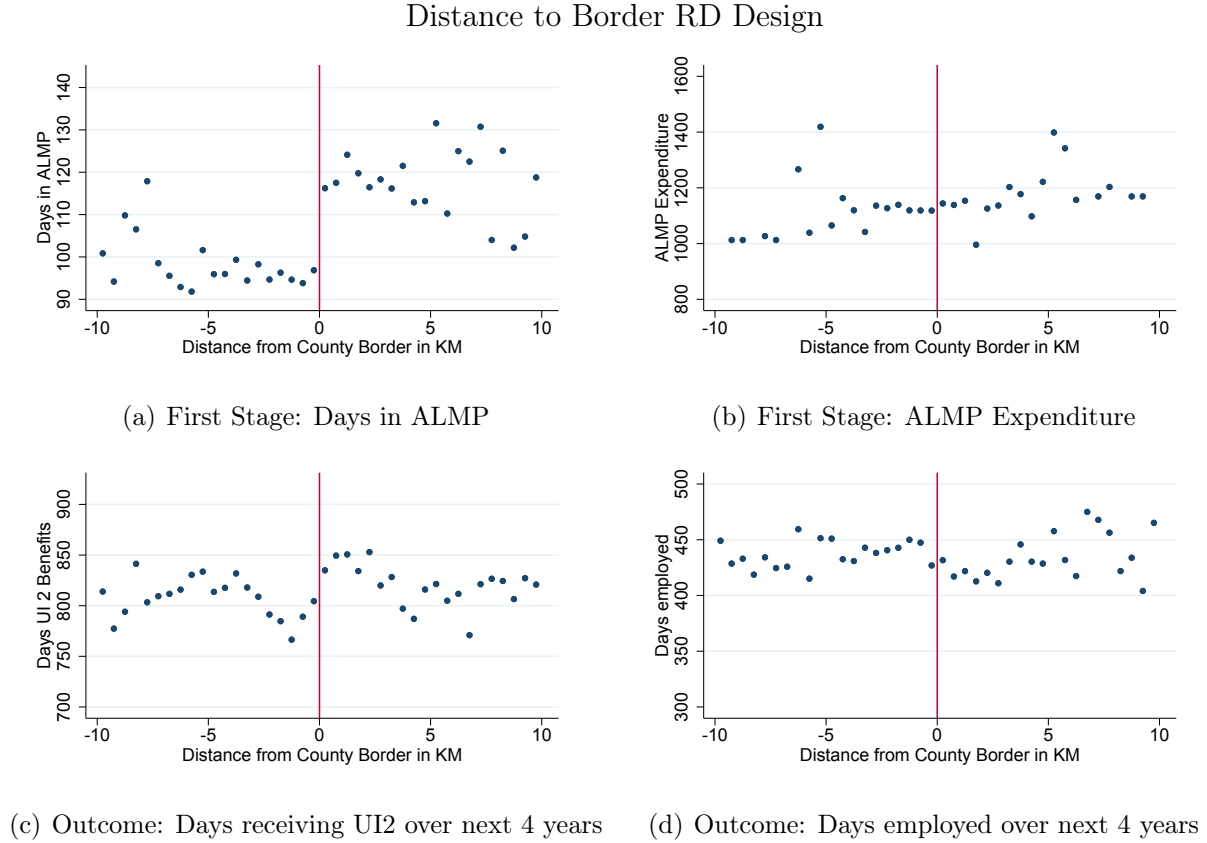
(e) Last job on left side of border



(f) Next job after UI2 on left side of border

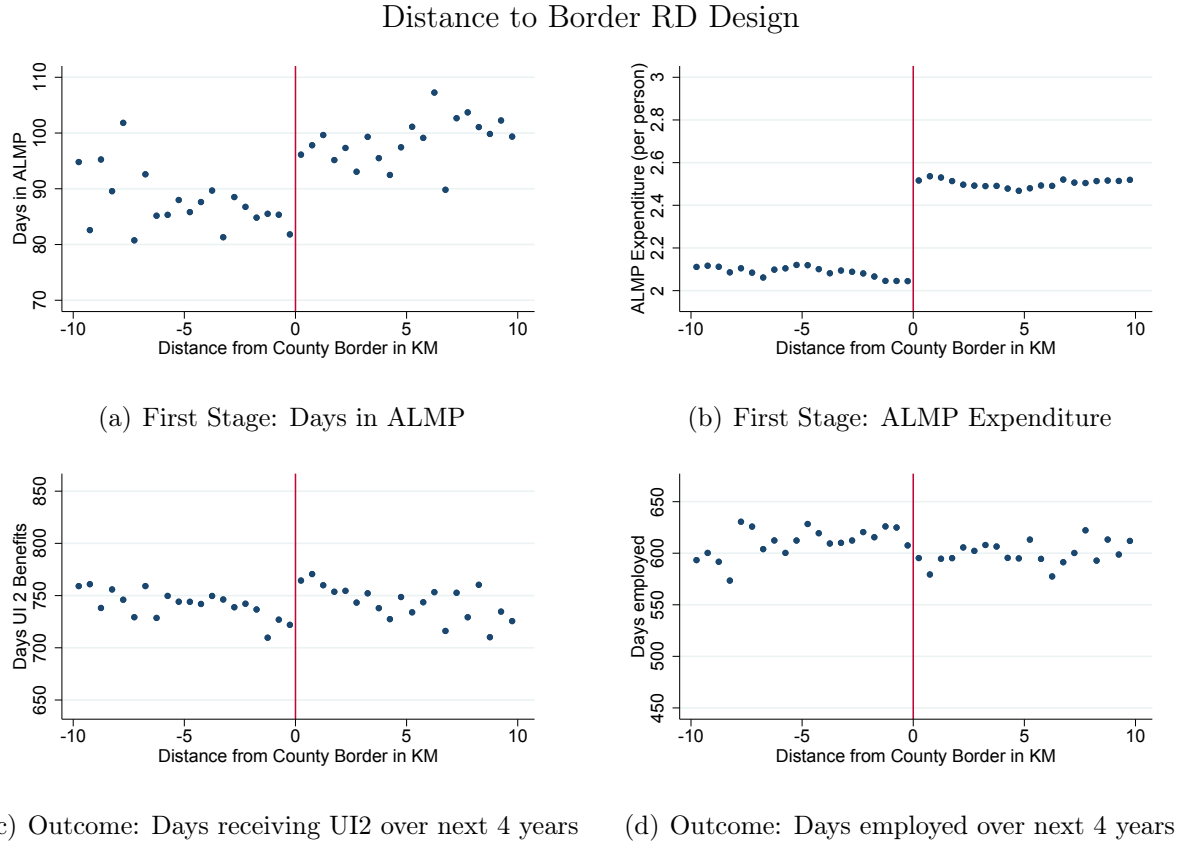
Notes: Each bin contains all individuals within a 0.5 kilometer bandwidth of distance from the nearest job district border. Panel (a) shows the average years of schooling in each bin. Panel (b) the years of experience observed in the data. Panel (c) the share of individuals who are female, and Panel (d) the number of days individuals have received regular unemployment benefits (UI1) in the 3 years prior to entering unemployment insurance 2 (UI2). For each individual the distance is measured as the distance to the nearest JC district border. If the adjacent border has a higher average intensity of ALMP, the distance is multiplied with -1, so that the observations to the left correspond to individuals who are on the side with a lower propensity to send the long term unemployed into active labor market programs.

Figure 5: The Effect of Days of ALMP on Labor Market Outcomes



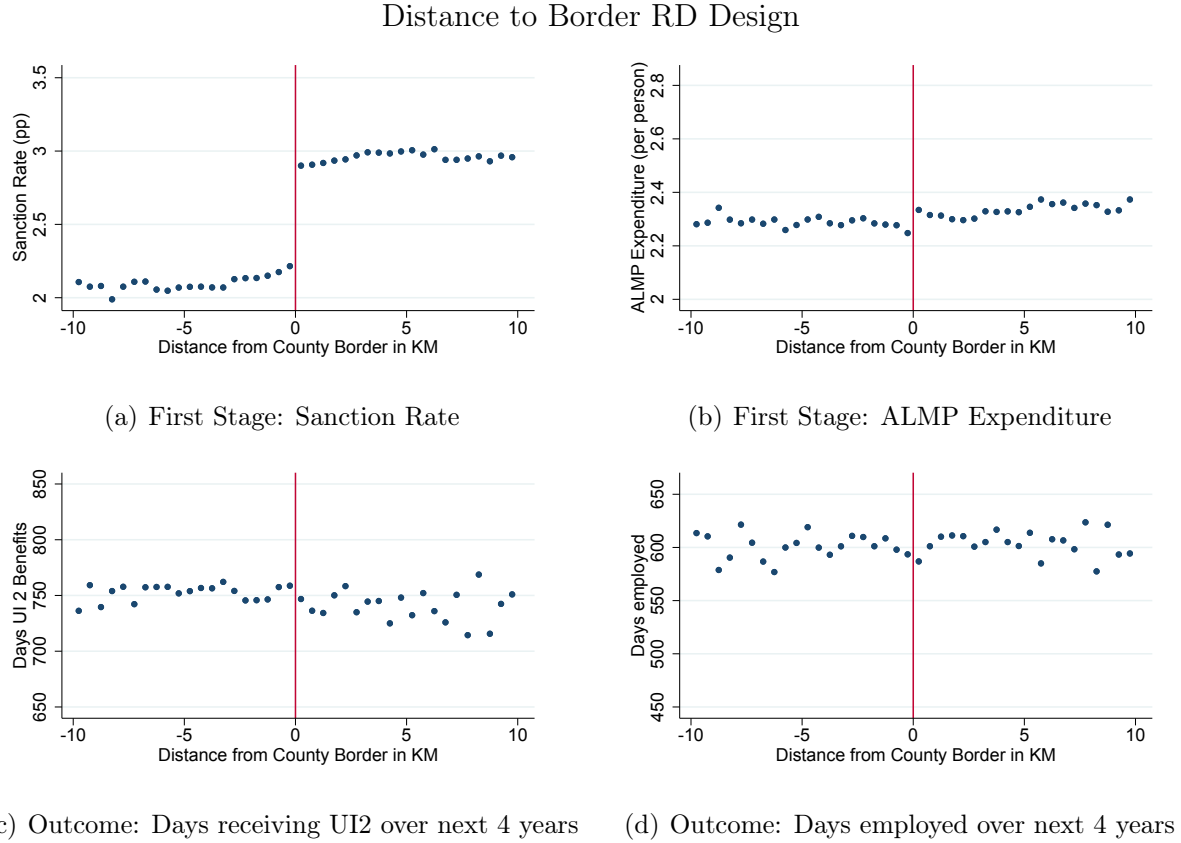
Notes: The binsize is 0.5 kilometers of distance to the closest JC border. For each individual the distance is measured as the driving distance to the nearest JC district border. Bins are weighted using the optimal balancing weight described in text. The treatment variable is the average number of days on ALMP in the job center district. If the adjacent district has a higher average number of ALMP days than the district where a person lives, then the distance is multiplied with -1, so that the observations to the left correspond to individuals who are on the side with a lower ALMP intensity.

Figure 6: The Effect of Spending on ALMP on Labor Market Outcomes



Notes: The binsize is 0.5 kilometers of distance to the closest JC border. For each individual the distance is measured as the driving distance to the nearest JC district border. Bins are weighted using the optimal balancing weight described in text. The treatment variable is the average per person spending on ALMP in the job center district. If the adjacent district has a higher per person spending than the district where a person lives, then the distance is multiplied with -1, so that the observations to the left correspond to individuals who are on the side with a lower ALMP spending.

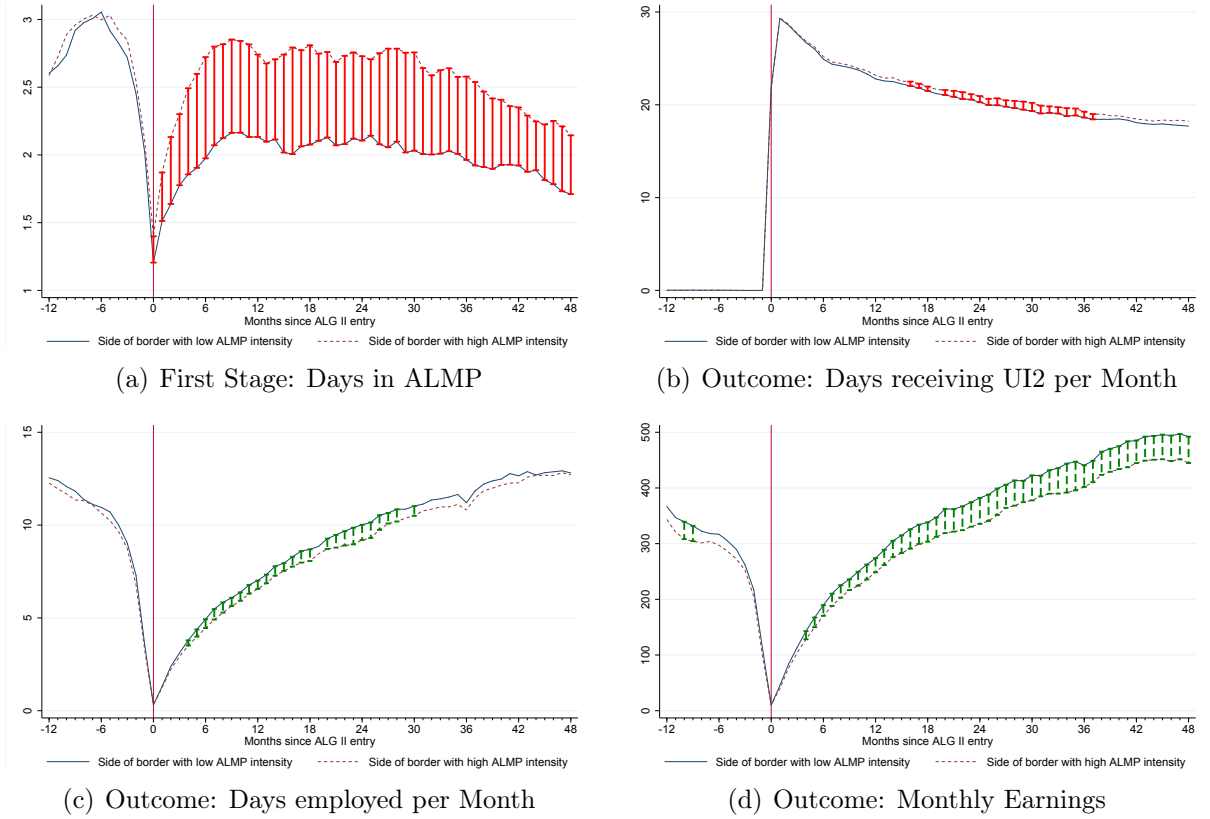
Figure 7: The Effect of UI2 Sanctions on Labor Market Outcomes



Notes: The binsize is 0.5 kilometers of distance to the closest JC border. For each individual the distance is measured as the driving distance to the nearest JC district border. Bins are weighted using the optimal balancing weight described in text. The treatment variable is the average share of individuals in the job center district who are sanctioned in a given month. If the adjacent district has a higher sanction rate than the district where a person lives, then the distance is multiplied with -1, so that the observations to the left correspond to individuals who are on the side with a lower sanction rate.

Figure 8: The Effect of Days in ALMP on Labor Market Outcomes

Dynamic Distance to Border RD Design



Notes: The binsize is 0.5 kilometers of distance to the closest JC border. For each individual the distance is measured as the driving distance to the nearest JC district border. Bins are weighted using the optimal balancing weight described in text. The treatment variable is the average share of individuals in the job center district who are sanctioned in a given month. If the adjacent district has a higher sanction rate than the district where a person lives, then the distance is multiplied with -1, so that the observations to the left correspond to individuals who are on the side with a lower sanction rate.