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Public employment services under decentralization: Evidence from a natural experiment

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**PUBLIC EMPLOYMENT SERVICES UNDER
DECENTRALIZATION:
EVIDENCE FROM A NATURAL EXPERIMENT**

LUKAS MERGELE AND MICHAEL WEBER

ABSTRACT. This paper studies whether the decentralization of public employment services (PES) increases job placements among the unemployed. Decentralizing PES has been a widely applied reform used by governments aiming to enhance their efficacy. However, economic theory is ambiguous about its effects, and empirical evidence has been scarce. Using a difference-in-differences design, we exploit unique within-country variation in decentralization provided by the partial devolution of German job centers in 2012. We find that decentralization reduces job placements by approximately 10%. Decentralized providers expand the use of active labor market programs and monitoring strategies which diminish job seekers' reemployment prospects but shift costs to higher levels of government.

JEL-Classification: H11, H75, I38, J48

Keywords: Decentralization, public employment services, job placements

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1. INTRODUCTION

Governments commonly maintain public employment services that match job seekers with employers to increase reemployment rates. To improve the efficacy of these services, several countries, including Canada, Denmark, Germany, Italy and Sweden, initiated reforms decentralizing responsibilities for public employment services to sub-national levels of government. These initiatives follow the classic theoretical argument that decentralized public employment offices are better informed about local economic conditions and preferences compared to a central agency. Hence, providing local governments with the autonomy to tailor labor market policies to these needs should result in superior policy outcomes (Oates, 1972; Faguet, 2004).

However, economic theory suggests at least three arguments why local policymakers could utilize their additional power for other objectives than reducing unemployment. First, they may aim to maximize their constituency's tax base by strictly focusing on job placements within their own region. This strategy would come at the cost of lower labor market mobility across regions and lead to fiscal externalities by creating a geographical lock-in of job seekers (Wildasin, 1991; Lundin and Skedinger, 2006). Second, local policymakers could strive to shift fiscal costs to other levels of government (Weingast et al., 1981; Besley and Coate, 2003). Thus, they might favor certain active labor market policies (ALMP) or monitoring strategies even if these policies are less effective in facilitating reemployment as long as they result in fiscal gains for the local constituency, for example because costs are covered by the national budget. Third, local policymakers seeking reelection may pressure decentralized employment services to ease welfare recipients' job search obligations (Brollo et al., 2015). This could also reduce the job-finding rate if public employment services at the local level are more susceptible to political influences than at the national level.

As economic theory is ambiguous, it is an empirical question whether centralized or decentralized regimes produce better employment services. This question has remained unanswered due to empirical constraints, most importantly a lack of suitable control groups as the degree of decentralization usually varies between countries but not within them. If control groups were available, short program durations or simultaneous reforms obstructed the identification of causal effects (see Lundin and Skedinger, 2006; Boockmann et al., 2015).

In this paper, we address these challenges by exploiting a large-scale German policy experiment. This policy induced permanent within-country variation in the centralization of public employment services unimpacted by simultaneous reforms. The setting enables us to make two major contributions. For one, we provide clean evidence on the effect of decentralization on job finding. We thereby uncover important transition dynamics while tracking the decentralization effect over a period of four years. For another, we examine channels for this finding by analyzing changes in the main underlying activities of employment offices. These are providing job seekers and firms with placement services, managing active labor market programs (ALMPs) and monitoring job search efforts. In so doing, we provide an exploratory analysis to determine whether our findings are compatible with local governments following other idiosyncratic incentives that are not beneficial to job seekers.

Implemented in 2012, the German policy reform involved the devolution of public employment offices – referred to as ‘job centers’ hereafter – to the district level within 41 of Germany’s 402 districts.¹ Job centers typically serve the long-term unemployed or people with very low earnings. For these groups, job centers play a crucial role in matching job seekers with potential vacancies (Pissarides, 1979; Graversen and van Ours, 2008; Fougère et al., 2009). Before the 2012 reform, individual job center policies were determined under the guidelines, directives and supervision of the Federal Employment Agency (FEA), in cooperation with local authorities. After 2012, authorities of the 41 treated districts were free to independently manage and stipulate these policies. The financing of job centers remained unaffected by the reform. For all job centers, the federal government covered welfare benefits and costs for active labor market programs while local authorities funded accommodation costs.

We use this German policy reform to identify the causal effect of decentralizing job centers in a difference-in-differences framework. We implement the approach by estimating an aggregate stock-flow matching function using job centers that remained centralized as a control group (see Coles and Smith, 1998; Ebrahimi and Shimer, 2010). Our analysis employs an aggregate administrative dataset comprising the monthly stocks and gross flows of unemployed welfare recipients and vacancies in German districts from 2009 to 2015. The data further provides detailed information on ALMP inflows, enforcement of job-search requirements, and job quality indicators. We find that decentralization decreases the number of new job matches by roughly 17% in the first year and up to 10% during the second to fourth post-reform years. This effect is equivalent to an increased average unemployment duration of three months. We run a battery of robustness checks including placebo tests and triple-difference models that all support our results being driven by decentralization rather than confounding factors.

Having established this robust negative effect on job finding, we explore whether decentralization caused a geographical lock-in of job seekers or other changes in the job centers’ placement, ALMP, and monitoring strategies that could account for these losses. Most importantly, we identify an immediate and permanent shift towards public job creation programs that are ineffective in increasing reemployment rates compared to other measures (see, for instance, Card et al., 2010). Decentralized job centers also temporarily reduced the enforcement of job seeker obligations, possibly reflecting transition processes rather than actual strategic changes. We do not find evidence for geographical lock-in effects. Moreover, we find no evidence that decentralization altered their placement strategies which would have altered placement quality in terms of job stability. Due to legal restrictions, we also rule out job centers having increased their employees’ caseloads or employed caseworkers with different backgrounds and skills.

We conclude that job seekers did not benefit from decentralization. Decentralized job centers adjust labor market policies but in a way that does not improve job seekers’ reemployment prospects as exemplified by the increase in ineffective job creation programs. Decentralized job centers potentially favored these schemes because they

¹German districts (*Kreise und kreisfreie Städte*) are an administrative subdivision similar to counties in the US. Job centers are organized at the district level.

generate local public goods whereas the federal government covers most of the associated costs. Our findings have important consequences for public budgets. Via the lower job finding rate alone, our estimates imply that the 2012 decentralization caused additional fiscal costs of about 500 million euros. Hence, our study emphasizes that decentralization reforms necessitate a careful assessment of potential incentive problems and fiscal externalities to avoid unintended consequences.

This paper speaks to two strands of literature. First, it contributes to fiscal federalism research that has analyzed whether states should provide public goods and services at a centralized or decentralized level (see Geys and Konrad, 2010, for a review). Thus far, this literature has almost exclusively investigated decentralization with respect to public finances, education policies, environmental policies or political institutions.² Little attention has been paid to labor market institutions (Martinez-Vazquez et al., 2017). This gap is surprising given that policymakers worldwide have pressed ahead promoting the decentralization of labor market institutions on a large scale. Second, we address the labor economics literature dealing with individual job matching instruments. This literature has made great progress in credibly identifying causal effects of active labor market policies (e.g. Black et al., 2003; Blundell et al., 2004; Card et al., 2010; Crépon et al., 2013) but remained agnostic about the institutional environment. In particular, it has remained silent on the question under which level of centralization such services should be delivered.³

Two studies have started to address these problems. Lundin and Skedinger (2006) study a Swedish pilot reform that granted municipal authorities a voting majority in the local employment committees, the bodies responsible for designing local labor market policies. The authors find that municipalities subsequently organized more ALMP projects and hard-to-place job seekers more likely enrolled in municipal projects. Remarkably, the official program period lasted only for three months, which was too short for employment outcomes and longer-lasting effects to be studied. Boockmann et al. (2015) examine a partial decentralization of German public employment offices from 2005 (see also Holzner and Munz, 2013) and find a negative effect of decentralization on the job-finding rate of men. Unfortunately, the empirical setting was constrained by a landmark unemployment benefit reform that directly coincided with the decentralization process. In contrast to these papers, our study has the following advantages. We observe the decentralization effect over a period of four years, are able to examine employment as well as local labor market policies, and our setting is not impaired by simultaneous reforms.

The paper proceeds as follows. Section 2 provides details on the German system of public employment services and its 2012 reform. Section 3 describes the data and our empirical strategy. Section 4 presents the estimated effects of decentralization,

²See, for example, Baicker and Gordon (2006); Neyapti (2010); Baicker et al. (2012) for public finance, Barankay and Lockwood (2007); Ahlin and Mörk (2008); Galiani et al. (2008) for education policies, Sigman (2002); Banzhaf and Chupp (2012); Lipscomb and Mobarak (2017) for environmental policies, and Blanchard and Shleifer (2001); Enikolopov and Zhuravskaya (2007); Fan et al. (2009) for political institutions.

³A small number of papers have compared public to private provision regimes (see, for instance Heinze et al., 2006; Bennmarker et al., 2013; Behaghel et al., 2014). In general, contracting out seems to increase the job-finding rate, though contracts need to be properly designed to prevent “creaming” and “parking”.

and section 5 explores underlying channels. Section 6 examines the validity of these results, and Section 7 discusses their implications.

2. POLICY BACKGROUND

2.1. German Job Centers. German job centers are one-stop local employment offices that play a central role in the German welfare system. As of January 2012, they have served 2 million long-term unemployed job seekers and 2.4 million employed workers with very low labor incomes, or 8% of the Germany's working age population.⁴ Their clients' poor labor market prospects give job centers a major role in welfare-to-work transitions (see, for instance, Fougère et al., 2009). Job centers engage in job counseling and assign clients to jobs or ALMP measures. They also monitor their clients' job search efforts and may temporarily impose cuts on unemployment benefits if a job seeker does not comply with their job-seeker obligations. These include actively searching for a new job, meeting with their caseworkers, participating in assigned ALMP measures and accepting appropriate job offers.

Unique to Germany, two types of job centers exist that vary in their degree of local autonomy as portrayed in Table 1. The first column introduces centralized job centers (*gemeinsame Einrichtungen*), which are governed by the Federal Employment Agency (FEA) in cooperation with the respective district authority. In charge of all labor market integration tasks, the FEA supervises the local employment offices using target agreements, directives and technical supervision such that the provision of public employment services is comparatively standardized across centralized job centers. In particular, placement, ALMP, and sanction policies follow nationwide guidelines with limited strategic leeway for local adjustments. The district administration mainly provides social inclusion services, for instance in the case of drug addiction or psychological problems.

The second type of job center is decentralized with district administrations assuming responsibility for all employment services (*zugelassene kommunale Träger*, second column of Table 1). Unlike their centralized counterparts, these job centers operate completely independently of the FEA except for the exchange of unemployment registration data. Decentralized job centers constitute a regular part of the district administration led by the district mayor. There is no general technical supervision by the FEA. District governments only sign target agreements with their respective state governments, their sole de-jure supervisors.

Both job center types share a common legal framework and financing rules. The federal government covers unemployment benefits and expenditures for labor market programs of job-center clients while the local administrations finance their accommodation. The autonomy of decentralized job centers with regard to placement, ALMP and sanction strategies potentially allows for a better adjustment to local labor market conditions which could improve job finding. However, the financing structure could incentivize local decision makers to implement strategies that are primarily beneficial for local budgets rather than job seekers.

⁴Job centers serve the residents of their district. Six job centers serve multiple districts, covering 16 districts in total.

Table 1. Job centers by type of organization

| | Centralized | Decentralized |
|---------------------------|--------------------|------------------------|
| Task responsibilities | | |
| Placement services | FEA | District |
| Social inclusion services | District | District |
| ALMP assignments | FEA | District |
| Monitoring & sanctions | FEA | District |
| Governance | | |
| Affiliation | FEA & district | District |
| Target agreements | With FEA | With state authorities |
| Technical supervision | FEA | Customized |
| Financing | | |
| Unemployment benefits | Federal government | Federal government |
| ALMP measures | Federal government | Federal government |
| Accommodation costs | District | District |

Notes.– FEA: Federal employment agency. ALMP: Active labor market programs.

Sources.– Ruschmeier and Oschmiansky (2010); Boockmann et al. (2015).

2.2. The 2012 Decentralization. Decentralized job centers were established in two waves. The first wave, in 2005, established job centers as one-stop employment offices for the first time into the welfare system whereby decentralized job centers were set up in 67 districts of Germany’s 402 districts. This wave also coincided with a large-scale reform of long-term unemployment benefits (Hartz reform, see Dustmann et al., 2014; Nagl and Weber, 2016).⁵ The second wave, in 2012, devolved job centers in 41 other districts. This decentralization provides a pre-reform period and took place without other simultaneous labor market reforms. For these reasons, we focus our analysis entirely on the second wave of reform.

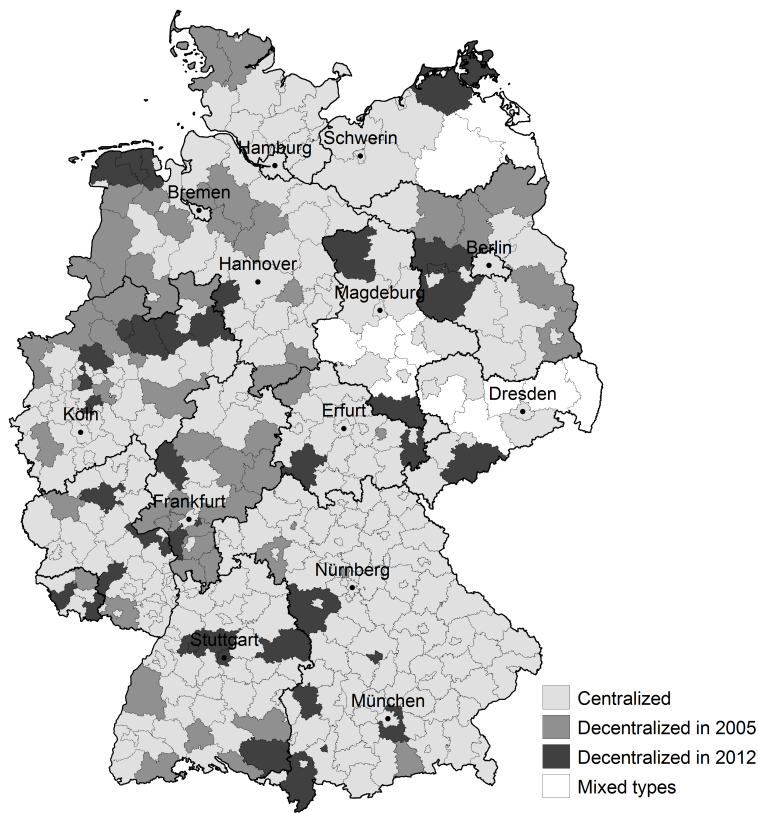
The districts to be reformed in 2012 were determined within a state-quota system. Districts willing to decentralize first had to apply to their respective state governments. The application period started on 3 August 2010 and ended on 31 December 2010. Local councils were required to back the application with a two thirds majority vote. Then, the state governments nominated those applicants allowed to decentralize. The number of nominations was subject to a quota specific to each state, proportional to the state’s number of delegates in the upper house of parliament. The total quota for Germany as a whole was 41 districts. If the number of applying districts fell short of the spot in one state, remaining places were filled by districts from other states. Those districts allowed to decentralize their job centers were officially announced on 14 April 2011. Decentralization took place on 1 January 2012.

Thanks to the state-quota system, job centers were decentralized in districts all across Germany (see Figure 1). They do not cluster in regions with particularly poor or

⁵An official evaluation of this decentralization wave led to inconclusive results (Deutscher Bundestag, 2008; Holzner and Munz, 2013; Boockmann et al., 2015), such that no political consensus was reached about the preferred regime. As a compromise, the co-existence of centralized and decentralized job centers was continued.

strong labor market conditions, nor are they disproportionately located in cities or rural areas. They also resemble one another in more general economic indicators. Table 2 presents major district characteristics by job center type for the pre-application year 2010. As shown in the comparison of means, both groups exhibited on average the same gross domestic product, fiscal situation, population size, sectoral structure and unemployment composition. A difference arises only for the monthly job-finding rate. As we use job finding solely as an outcome variable in our framework, district fixed-effects will account for these differences in our estimations. In sum, these results supply first evidence that the two groups of districts are observationally equivalent.

Figure 1. German districts by job center type



Notes.— Mixed types refer to districts where decentralized and centralized job centers coexist e.g. due to district mergers.

Sources.— Geodata: GeoBasis-DE / BKG 2014.

3. DATA AND EMPIRICAL STRATEGY

3.1. Data. We utilize a rich administrative dataset at the district level to examine the effects of decentralization on job finding and other labor market outcomes. The data stem from the job centers' operational processes and are subsequently compiled into monthly reports (*Arbeitsmarkt in Zahlen*) by the FEA's statistical office. The reports provide monthly observations on unemployment, vacancies, ALMP participation, benefit sanctions, and employment quality indicators. For all variables,

Table 2. Major district characteristics in 2010 by job center type

| Variable | Group means | | P-Value for equality of means |
|-----------------------------------|--------------------------|-------------------------|-------------------------------------|
| | Decentralized in 2012 | Remained centralized | |
| GDP per capita (in 1,000 euros) | 29.670 (14.831) | 29.085 (11.196) | 0.766 |
| Public debt p.c. (in 1,000 euros) | 1.725 (1.403) | 1.587 (1.248) | 0.518 |
| Urban district (dummy) | 0.225 (0.423) | 0.313 (0.464) | 0.257 |
| East Germany (dummy) | 0.200 (0.405) | 0.173 (0.379) | 0.681 |
| Civil labor force (in 1,000) | 154.230 (96.583) | 131.493 (179.278) | 0.432 |
| Employment rate | 0.724 (0.170) | 0.763 (0.216) | 0.269 |
| Share: Agriculture | 0.021 (0.019) | 0.023 (0.021) | 0.527 |
| Share: Mining and energy | 0.014 (0.008) | 0.013 (0.009) | 0.904 |
| Share: Manufacturing | 0.204 (0.086) | 0.195 (0.087) | 0.535 |
| Share: Construction | 0.066 (0.024) | 0.066 (0.025) | 0.926 |
| Share: Trade, transp., comm. | 0.254 (0.042) | 0.251 (0.039) | 0.632 |
| Share: Finance and real estate | 0.142 (0.045) | 0.141 (0.046) | 0.824 |
| Share: Public and priv. services | 0.299 (0.056) | 0.310 (0.065) | 0.285 |
| Job-center unemployment rate | 0.048 (0.025) | 0.047 (0.029) | 0.812 |
| Share: Young (15–24 years) | 0.080 (0.016) | 0.078 (0.020) | 0.426 |
| Share: Old (55–64 years) | 0.123 (0.028) | 0.124 (0.022) | 0.843 |
| Share: Foreign nationals | 0.190 (0.130) | 0.165 (0.093) | 0.139 |
| Monthly job-finding rate | 0.042 (0.011) | 0.047 (0.014) | 0.041** |
| Monthly flow rate into ALMP | 0.148 (0.043) | 0.162 (0.051) | 0.105 |
| Monthly sanctioning rate | 0.018 (0.006) | 0.019 (0.006) | 0.197 |
| Observations | 40 | 294 | |

Notes.– Sample as described in section 3.1. Standard deviations in parentheses. P-values given for t-test of mean equality. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Source.– German Statistical Office and Federal Employment Agency

we readily observe stocks as well as gross flows and thus do not have to deal with time aggregation issues.

The data cover the universe of German districts and effectively consider all relevant unemployed job seekers because unemployment registration is mandatory for receiving unemployment benefits. From this sample, we omit 11 districts in which centralized and decentralized job centers co-exist due to administrative reforms. This also includes one district which was part of the 2012-reform, leaving us with a total of 40 treated districts. Finally, we omit districts that decentralized job centers in 2005 although their inclusion as an additional control group does not alter our results. The sample period ranges from January 2009 to December 2015, i.e. from three years before to four years after the decentralization. Due to partially missing observations, we remove the first three post-reform months from our sample.

The data allow us to study the effect of decentralization on a wide range of labor market outcomes. With job placements being the primary task of job centers, we consider the monthly outflow from unemployment into employment as our main outcome of interest. To investigate channels explaining potential changes in unemployment outflows, we analyze several additional outcomes. These include the monthly flows of unemployed into different types of ALMP measures, the number of sanctions issued in a month on non-compliant job seekers, and the outflow into permanent jobs. The latter refers to jobs lasting for at least three months. Our main covariates are stocks and flows of unemployed and vacancies, respectively. In additional specifications, we also consider the demographic composition of local unemployment, such as the share of foreign nationals, people younger than 25 years, and older than 55. Table 3 presents descriptive statistics of our main variables in the resulting sample.

3.2. Econometric Model. The functional form of our econometric model is motivated by a stock-flow matching model with Cobb-Douglas technology (Coles and Smith, 1998; Ebrahimi and Shimer, 2010).⁶ Analogous to a production function, the stock-flow matching function models the gross flow from unemployment into jobs ('matches') as an output produced by the stocks of vacancies and unemployed and their respective inflows. We interpret the total factor productivity of the matching function as an indicator for the efficiency of the local job center in bringing unemployed back to work. The decentralization status of a job center then constitutes one component of this indicator.

To identify the causal effects of decentralization, we employ a difference-in-differences framework at the district level. Our treatment group comprises 40 districts whose job centers were decentralized in 2012, while our control group contains 294 districts whose job centers remained centralized throughout the sample period. Log-linearizing the stock-flow matching function, our estimation equation then reads

$$M_{it} = \delta D_{it} + \beta_1 U_{it} + \beta_2 V_{it} + \beta_3 \tilde{U}_{it} + \beta_4 \tilde{V}_{it} + \alpha_i + \mu_t + \varepsilon_{it} \quad (1)$$

where M_{it} denotes matches defined as transitions from unemployment into jobs for district (i.e. job center) i and month t , our main outcome. The dummy variable D_{it} indicates whether a job center is decentralized or not. U_{it} and V_{it} denote the stocks

⁶The stock-flow matching function has received empirical support both at the micro and the macro level (Gregg and Petrongolo, 2005; Andrews et al., 2013) with strong evidence for a Cobb-Douglas functional form (see Petrongolo and Pissarides, 2001, for a survey).

Table 3. Descriptive statistics of main regression variables

| Variable | Mean | SD | Min. | Max. |
|----------------------------------|-------|-------|------|-------|
| Monthly job-finding | 4.64 | 0.88 | 1.39 | 8.95 |
| Vacancies, inflow | 5.76 | 0.73 | 3.09 | 9.65 |
| Vacancies, stock | 6.73 | 0.75 | 4.28 | 10.47 |
| Unemployed, inflow | 6.32 | 0.88 | 3.93 | 10.68 |
| Unemployed, stock | 7.98 | 0.99 | 5.48 | 12.19 |
| Share: Unemployed <25 yr | 7.42 | 2.17 | 0.00 | 17.12 |
| Share: Unemployed >55 yr | 15.32 | 3.76 | 6.82 | 33.06 |
| Share: Foreign nationals | 17.31 | 10.34 | 0.34 | 57.88 |
| Flow into ALMP | 5.91 | 1.03 | 2.20 | 10.03 |
| Into short-term training | 5.53 | 1.03 | 0.41 | 9.41 |
| Into subsidized employment | 2.79 | 1.20 | 0.41 | 7.34 |
| Into medium-term training | 2.88 | 1.41 | 0.41 | 8.38 |
| Into public job creation schemes | 3.61 | 1.47 | 0.41 | 9.19 |
| New sanctions | 3.95 | 0.99 | 0.00 | 8.73 |
| Stock of sanctions | 4.80 | 0.98 | 0.41 | 9.48 |
| Stock of benefit sanctions | 4.75 | 0.98 | 0.41 | 9.45 |
| Stock of accommodation sanction | 2.64 | 1.01 | 0.41 | 6.95 |
| Outflow out of welfare | 5.67 | 0.84 | 3.43 | 9.82 |
| Permanent outflow out of welfare | 5.32 | 0.84 | 2.77 | 9.53 |
| Share: Permanent outflow | 0.94 | 0.02 | 0.79 | 0.99 |

Notes.— Monthly district-level data. All level-variables are in logs. $N = 26,998$.

of unemployed and vacancies, whereas \tilde{U}_{it} and \tilde{V}_{it} denote their respective inflows in this month. We include district-specific effects α_i to account for time-invariant differences in matches across districts, and month-fixed effects μ_t to capture business cycle and seasonal fluctuations. Our parameter of interest is δ , which provides the treatment effect of decentralization on the conditional outflow from unemployment to employment. Standard errors are clustered at the district- and month-level to account for unobserved correlation within these dimensions (Bertrand et al., 2004).

Our empirical approach relies on two main identifying assumptions.⁷ First, centralized and decentralized job centers experience the same fundamental labor market trends in the absence of the policy change. Second, decentralization has no effect on job finding in unreformed districts (stable unit treatment value assumption, SUTVA). We find descriptive support for common trends in the following section and more formal support for both assumptions in Section 6.

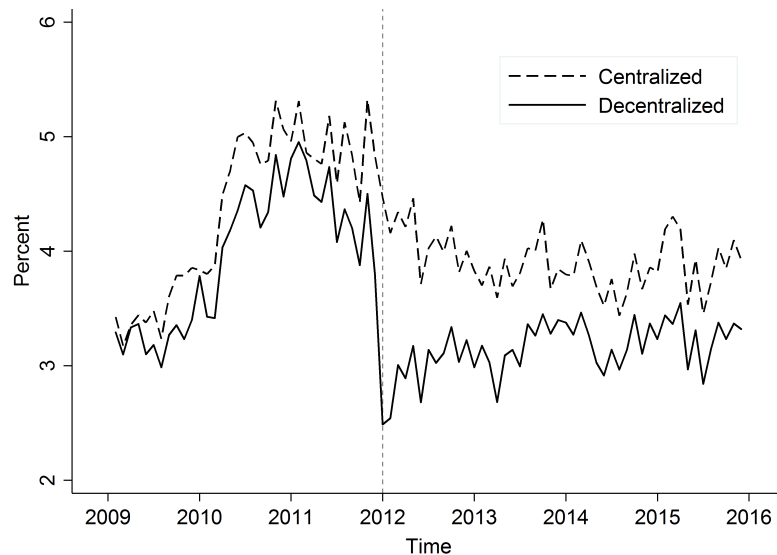
4. THE EFFECTS OF DECENTRALIZATION ON JOB FINDING

This section presents descriptive, static parametric and dynamic parametric results for the effect of decentralization on job finding. Illustrating the effect of decentralization descriptively, Figure 2 shows the evolution of the seasonally adjusted average

⁷We have already discussed in Section 2 that the 2012 decentralization did not coincide with other reforms that could have affected the two groups of districts systematically differently.

aggregate monthly job-finding rates by job center type over time. The job-finding rate is calculated as the outflow out of unemployment over the unemployment stock at the beginning of the month. The figure illustrates that in the three years before the reform, the job-finding rates' evolution was remarkably similar in both groups of job centers. This lends credibility to the common trends assumption and affirms that the reform did not target districts with particularly bright pre-treatment trends. We plot descriptive graphs for further labor market outcomes in Figures A.1 and A.2 in Appendix A, again confirming parallel pre-reform trends. However, after the decentralization in January 2012, the job-finding rate of decentralized job centers sharply dropped relative to centralized job centers. It declined from about 4.5% to roughly 3% in treated districts, and from approximately 5% to around 4% in non-treated districts. The gap slightly narrows during the following years, but does not return to its pre-reform size. This points to permanent negative effects of decentralization on job finding.

Figure 2. Average aggregate monthly job-finding rates by job center type



Notes.— The figure depicts the seasonally adjusted average aggregate monthly job-finding rate. It is calculated as the monthly outflow out of unemployment into employment over the unemployment stock at the beginning of the month. The time-labels (x-axis) refer to January of a given year.

We now investigate whether parametric estimates will support our descriptive findings. Table 4 reports the difference-in-differences estimates from equation (1) for the outflow from unemployment into employment. Each column represents a regression of log transitions into jobs on a decentralization indicator, district and month fixed effects, as well as subsequently introduced covariates. Column 1 gives the average treatment effect of decentralizing job centers while controlling only for fixed effects. The estimate implies that average monthly flows into jobs decreased by roughly 11% due to decentralization. Columns 2 to 4 refine the model's precision by adding a set of local labor market characteristics that remove cross-district differences. In

particular, column 2 adds the monthly stocks of vacancies and unemployed. Building on column 2, column 3 includes the respective inflows, completing the basic stock-flow model. The coefficients of the stock-flow variables are in line with the concept of stock-flow matching. Job finding is more elastic with respect to the inflows of new vacancies rather than its stock, while it is more elastic with respect to the stock of unemployed rather than its inflow. The decentralization effect remains robust and stable. Column 4 additionally controls for shares of three demographic groups that are typically hard to place into jobs, i.e. the share of unemployed below the age of 25, the share of unemployed above the age of 55, and the share of foreign unemployed. As expected, higher shares of these hard-to-place job seekers in the group of unemployed *ceteris paribus* reduce the unemployment outflow into employment. Yet, controlling for these groups does not alter our decentralization estimate. Our finding is also robust to including linear trends into the specification and using alternative sample periods (see Tables B.1 and B.2 in Appendix B).

Hence, we conclude that decentralization reduced the monthly flow into jobs on average by about 10% within four years following the reform. This effect size is equivalent to an increase in the average unemployment duration by about three months.⁸

Next, we investigate whether the negative effect of decentralization is declining over time, as Figure 2 may suggest. We therefore modify the stock-flow matching model from equation (1). Adding a full series of annual leads and lags of the reform, the regression equation now reads

$$M_{it} = \sum_{\substack{\tau=2009 \\ (\tau \neq 2010)}}^{2015} \delta_{\tau} D_{i\tau} + \beta_1 U_{it} + \beta_2 V_{it} + \beta_3 \tilde{U}_{it} + \beta_4 \tilde{V}_{it} + \alpha_i + \mu_t + \varepsilon_{it} \quad (2)$$

where τ denotes years and δ_{τ} are yearly coefficients. As anticipatory treatment effects could have occurred already when the decentralizing districts were announced in April 2011, all treatment effects are estimated relative to the base year 2010, when districts had to apply for decentralization. Estimating quarterly effects leads to qualitatively similar but less precise results.

Figure 3 depicts the resulting evolution of the decentralization effect on job finding from three years before to four years after the reform. During the pre-reform period, all coefficients are statistically insignificant. This finding rules out anticipatory decentralization effects and supports the common trends assumption underlying our identification strategy. In the first year after decentralization, monthly unemployment outflows were strongly reduced by about 17%. During the following three years, this effect weakens over time but still amounts to almost 8% in the fourth year after decentralization. Seven expert interviews with division heads of state and federal ministries as well as job center directors suggest that the first year after the reform was influenced by the organizational transition. Employees had to adapt to new IT systems and practices. Moreover, some functions that used to be centrally provided by the FEA before had to be built up in decentralized job centers. We thus conclude

⁸The average aggregate monthly job-finding rate in centralized districts amounts to 3.8%. Assuming a constant job-finding probability over the duration in unemployment, this implies an average unemployment duration of about 26 months. A 10% decrease of job finding, therefore, implies an increase in average unemployment duration by almost 3 months.

Table 4. Difference-in-differences: Average effect of decentralization on monthly log flows into jobs

| Variable | (1) Fixed Effects | (2) Stocks | (3) Stock- Flow | (4) Controls |
|--------------------|-------------------------|-----------------------|-----------------------|-----------------------|
| Decentralized | -0.112 *** (0.025) | -0.120 *** (0.024) | -0.096 *** (0.022) | -0.101 *** (0.022) |
| Vacancies, stock | | 0.040 *** (0.015) | 0.010 (0.013) | 0.014 (0.012) |
| Unemployed, stock | | 0.805 *** (0.051) | 0.692 *** (0.046) | 0.696 *** (0.047) |
| Vacancies, inflow | | | 0.097 *** (0.012) | 0.097 *** (0.012) |
| Unemployed, inflow | | | 0.280 *** (0.025) | 0.277 *** (0.025) |
| Unemployed <25 yr | | | | -0.004 * (0.002) |
| Unemployed >55 yr | | | | -0.009 *** (0.002) |
| Foreign nationals | | | | -0.008 *** (0.002) |
| R-squared | 0.955 | 0.960 | 0.962 | 0.963 |
| Districts | 334 | 334 | 334 | 334 |
| Observations | 26,998 | 26,998 | 26,998 | 26,988 |

Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. All continuous variables in logs. Regressions include a full set of dummies for districts and months. Standard errors given in parentheses are clustered at the job center and the month level.

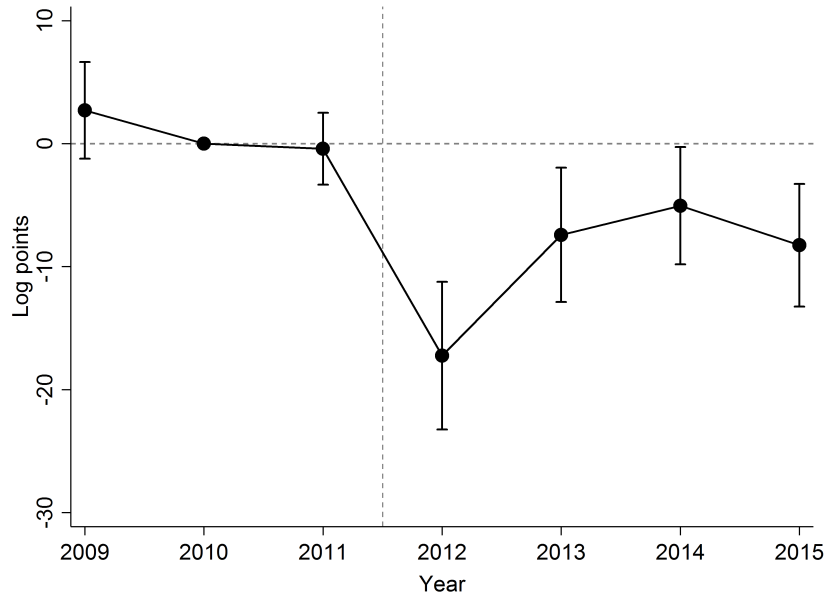
that decentralization initiates a transition phase with a particularly pronounced drop in the job finding rate during the first year but also induces a more permanent and economically relevant negative effect in subsequent years that requires explanation.

5. POLICY ADJUSTMENTS

We now explore channels that might explain the negative effect of job center decentralization on job finding. As argued above, decentralization may lead to (i) a geographical lock-in of job seekers; (ii) changes in the inflows into ALMP measures; (iii) changes in the monitoring and sanctioning of job seekers or (iv) changes in the placement strategy. Finally, we will briefly discuss additional features of public employment services playing a potential role in the decentralization process.

5.1. Geographical Lock-in of Job Seekers. If local decision makers aim to maximize the tax base of their constituency, they have an incentive to match job seekers only with vacancies in their own district. This would lead to a lower mobility

Figure 3. Dynamic treatment effects of decentralization on monthly flows into jobs



Notes.— The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression of the log monthly flow from unemployment into jobs, as given by equation (2). The year 2010 is the baseline category. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

of job seekers across districts and could explain a lower job finding rate under decentralization (Lundin and Skedinger, 2006). This phenomenon has been termed as ‘geographical lock-in’ of job seekers and could create an uncoordinated fiscal externality among districts (Wildasin, 1991).

To examine whether decentralization induces geographical lock-in, we analyse whether the elasticity of job finding with respect to vacancies from surrounding districts has decreased after decentralization. Therefore, we extend our previous model (1) by adding spatial lags of all variables as well as interaction terms of the spatial lags with the decentralization dummy. Hence, we estimate models of the form

$$M_{it} = \delta D_{it} + Q_{it}\beta + WQ_{-it}\gamma + D_{it}WQ_{-it}\theta + \alpha_i + \mu_t + \varepsilon_{it} \quad (3)$$

where D is the decentralization indicator, Q is a vector collecting the stock and flow variables for unemployed as well as vacancies, and W represents a spatial weights matrix based on row-normalized inverse distances. The remaining variables are defined as before. Our coefficient vector of interest now becomes θ , in particular its elements with respect to vacancies. The index i denotes the focal district, whereas $-i$ refers to the ‘neighbors’ of district i . To provide a meaningful interpretation of the decentralization coefficient δ in the face of interaction terms, we center all continuous variables around their mean and standardize them by their standard deviation.

Table 5 presents our results. The first column repeats our baseline estimation using the centered and standardized variables, demonstrating that the decentralization effect remains unaffected by this transformation. Column 2 adds the spatially lagged variables as well as an interaction of the spatially lagged vacancy inflow with the decentralization indicator. Column 3 substitutes this interaction with that of the spatially lagged vacancy stock with decentralization. Column 4 includes a full set of interactions of spatially lagged stock and flow variables with the decentralization indicator. In all models, the resulting mean decentralization effect is very similar to our previous estimates. None of the models confirms any statistically significant decrease of the job finding elasticity with respect to the stock or inflow of vacancies in surrounding areas. Confirming the results by Lundin and Skedinger (2006), we conclude that decentralization did not increase the geographical lock-in of job seekers. Instead, decentralization appears to have reduced the overall efficiency of the job matching process.

Table 5. Difference-in-differences: Effect of decentralization on monthly log flows into jobs accounting for vacancies in surrounding job centers

| Variable | (1) Baseline | (2) Spatial lags I | (3) Spatial lags II | (4) Spatial lags full |
|------------------------|-----------------------|--------------------------|---------------------------|-----------------------------|
| Decentralized (D) | -0.107 *** (0.025) | -0.110 *** (0.024) | -0.107 *** (0.027) | -0.113 *** (0.027) |
| D×W×Vacancies, inflow | | -0.026 (0.068) | | -0.077 (0.074) |
| D×W×Vacancies, stock | | | -0.019 (0.062) | -0.004 (0.065) |
| D×W×Unemployed, stock | | | | 0.149 (0.162) |
| D×W×Unemployed, inflow | | | | -0.015 (0.166) |
| R-squared | 0.962 | 0.962 | 0.962 | 0.963 |
| Districts | 334 | 334 | 334 | 334 |
| Observations | 26,998 | 26,998 | 26,998 | 26,998 |

Notes. – * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a separate estimation of equation 3. *Decentralized (D)* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. *W* represents a spatial weights matrix with row-normalized inverse distances as weights. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs, centered and standardized. Standard errors given in parentheses are clustered at the job center and the month level.

5.2. Active Labor Market Policies. Changes in the assignment of job seekers into ALMP measures constitute a second potential channel explaining the reductions in job finding. Decentralized job centers could use their autonomy to better tailor ALMP strategies to local economic conditions. Yet they may also promote program types that provide additional gains for the local constituency such as local public goods. Similarly, Lundin and Skedinger (2006) point out that decentralized job centers might prefer ALMP measures that help to maximize the local tax base, even

if they came at the cost of higher geographical lock-in. However, an ALMP strategy that does not focus on the most effective programs for increasing reemployment rates will reduce the aggregate job-finding rate.

For German job centers, the four most common ALMP categories are short-term classroom and on-the-job training of up to 3 months (*Aktivierung und berufliche Eingliederung*), medium-term vocational training and re-training (*Berufliche Weiterbildung*), wage subsidies (*Aufnahme einer Erwerbstätigkeit*), and public job creation schemes (*Beschäftigung schaffende Maßnahmen*). For all these measures, the federal government bears the cost of program participation.⁹ Yet only public job-creation schemes offer the additional advantage of participants providing local public goods, such as cleaning streets, gardening parks or supporting local facilities' management. A shift towards public job-creation schemes could therefore reduce the districts' own expenditures for these goods. Unfortunately, public job-creation schemes are also very ineffective in increasing reemployment rates compared to other measures (for large-scale meta-studies, see Heckman et al., 1999; Card et al., 2010; Kluge, 2010). Simple ordinary least squares estimates using our sample indeed confirm that job finding is barely correlated with previous inflows into public job-creation schemes (see Figure C.1 in Appendix C). On the other hand, previous inflows into short-term training and wage subsidy programs exhibit strong positive correlations with job placements.

To assess whether decentralization caused a shift toward less effective ALMP measures, we employ the stock-flow model from equation (1) but use outflows from unemployment into the different ALMP programs as the outcome variables. Table 6 presents the respective results. The first column indicates that decentralized job centers do not assign their clients more or less often to ALMP measures in general compared to centralized job centers. Thus, we can rule out changes in the overall use of ALMP measures accounting for the reductions in job finding. Columns 2, 3 and 4 indicate that the job center types do not differ with respect to their use of short-term training, medium-term training and wage subsidies in a statistically significant way although decentralized job centers tend to use these ALMP measures less intensively. Column 5 reveals that decentralized job centers sent about 30% more unemployed job seekers into job creation schemes. The lower effectiveness of this program type indicates that this policy change contributes to the observed loss in job finding. In fact, decentralization increased the average inflow rate into job creation schemes by almost the same amount as it decreased the average aggregate job-finding rate, namely by about 0.3 percentage points.¹⁰

To ensure we do not overlook any underlying dynamic changes, we employ equation (2) to estimate the year-specific impacts of decentralization on the inflows into the different ALMP programs. Figure 4 presents the results for our four most important program types. For short-term training, medium-term training and wage subsidies, we do not observe temporary or permanent changes that are statistically significant

⁹District authorities mainly pay for accommodation costs of job seekers, see Section 2.

¹⁰In the post-reform period, the average aggregate monthly job-finding rate of decentralized districts was 3.1% and the average monthly inflow rate into job creation schemes was 1.2%. With treatment effects of -10% and $+32\%$ respectively, the job-finding rate changed by $3.1\% \cdot \frac{-0.1}{1-0.1} = -0.34$ percentage points and the job-creation inflow rate increased by $1.2\% \cdot \frac{0.32}{1+0.32} = 0.29$ percentage points.

Table 6. Difference-in-differences: Effect of decentralization on monthly log flows into active labor market policies (ALMPs)

| Variable | (1) All ALMPs | (2) Short- term training | (3) Medium- term training | (4) Wage subsidies | (5) Job creation schemes |
|---------------|---------------------|-----------------------------------|------------------------------------|--------------------------|-----------------------------------|
| Decentralized | 0.037 (0.065) | -0.073 (0.089) | -0.032 (0.072) | -0.009 (0.069) | 0.320 *** (0.079) |
| R-squared | 0.946 | 0.915 | 0.766 | 0.850 | 0.821 |
| Districts | 319 | 319 | 319 | 319 | 319 |
| Observations | 25,839 | 25,838 | 24,404 | 25,350 | 24,783 |

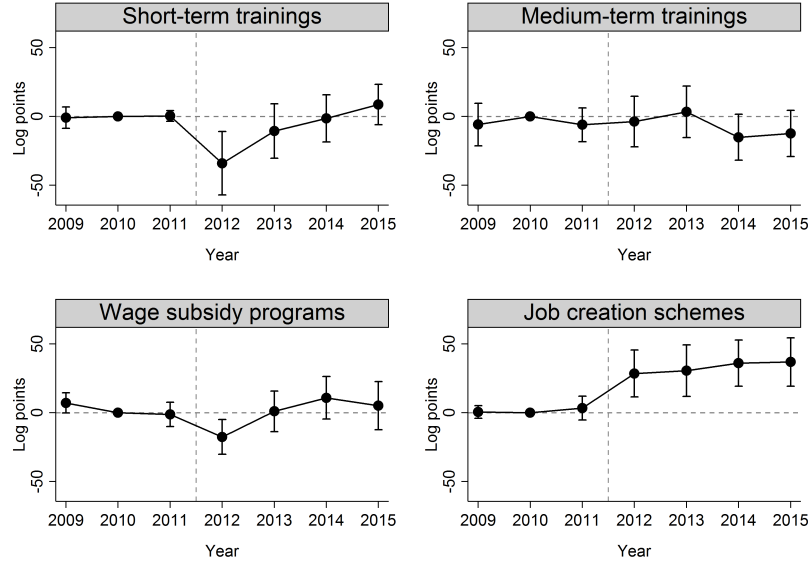
Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. The dependent variables are inflows of unemployed into the respective ALMP categories. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Sample sizes vary due to missing observations. Standard errors given in parenthesis are clustered at the job center and the month level.

at the 95%-confidence level. Inflows into medium-term training appear to be slightly reduced in the long run. For job-creation schemes, in contrast, inflows increase directly after decentralization and remain at a permanently higher level.

Apparently, local authorities exploited the decentralization of their job centers to shift fiscal costs from their own to the federal budget. The incentive to do so is inherent in a system where the national government covers large parts of the costs of program participation and subsequent unemployment while not being able to influence the local job centers' ALMP strategy.

5.3. Monitoring and Sanction Strategies. Changes in the sanction strategy of local job centers constitute another potential channel that might explain lower job finding after decentralization. Sanctions are temporary reductions in unemployment benefits when job seekers do not comply with their job seeker obligations, such as search and meeting duties. Ample empirical evidence confirms that stricter sanction regimes and even the credible threat of being sanctioned increase the job-finding rate (see van den Berg et al., 2004; Abbring et al., 2005; Lalive et al., 2005; Boone et al., 2009) although Arni et al. (2013) imply that sanctions should not be set discouragingly high to achieve the desired outcome. We confirm this notion for our sample within a simple exploratory analysis where we regress contemporaneous job finding on previous sanction activities (see Figure C.2 in Appendix C). The resulting correlations show that job finding is increasing with sanctions issued in previous months. This effect is not driven by high sanctions which also cut accommodation costs but by lower benefit sanctions. In sum, we expect fewer but stricter sanctions to reduce job finding.

Figure 4. Dynamic treatment effects of decentralization on monthly entries into ALMP measures



Notes.— The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression of the monthly inflow into different ALMP measures as given by equation (2). The year 2010 is the baseline category. The regressions include a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

Decentralized job centers could prefer to sanction welfare recipients less intensively as laxer enforcement may affect the job-center clients' voting behavior and increase the re-election prospects of incumbent local politicians (see Brollo et al., 2015). Centralized job centers do not encounter this incentive as the FEA is a federal institution not relying on local constituents. In addition, decentralized job centers may emphasize sanctions that lower local public expenditures due to the financing structure of welfare support in Germany. Minor non-compliance to job seeker duties will first reduce federally financed benefit payments. Severe or repeated failures to comply will lead to higher sanctions that also include reductions of the accommodation costs financed by local governments. Hence, decentralized job centers could reduce local welfare expenses by imposing stricter sanctions affecting accommodation costs more often.

We employ our baseline model from equation (1) to explore whether decentralization leads to changes in the monitoring strategies. Table 7 presents our estimates for the total number of sanctions imposed, sanctions in place and sanction types used. There is a strong indication for a negative decentralization effect: According to column 1, the number of sanctions newly imposed in a given month decreased by about 8%. Column 2 suggests that the monthly stock of sanctions decreased by as much as 14%. Larger effects on the sanctions' stock than on the flow reflects that some sanctions

endure several weeks, exacerbating the effect on the stock variable. We continue with the stock of sanctions because it is only possible to distinguish benefit from accommodation sanctions for this variable. The final two columns then reveal that decentralization lowered the number of benefit sanctions imposed but left the level of reductions in accommodation payments unaltered. This means that decentralized job centers imposed on average fewer sanctions overall but with a relatively higher strength among those that remained, assuming job seeker compliance did not change due to the reform. Fewer but stricter sanctions might be in the interest of localized job centers for the political and fiscal considerations outlined before.

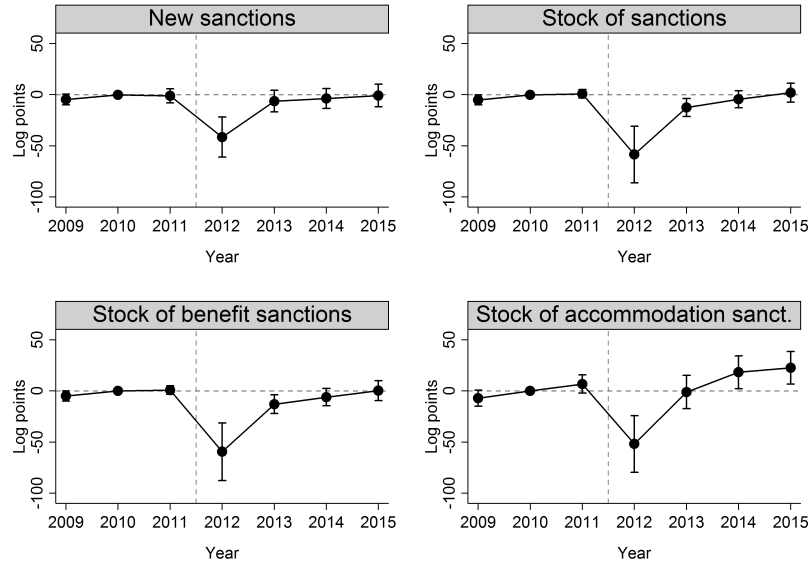
Table 7. Difference-in-differences: Effect of decentralization on monthly sanctions of unemployed

| Variable | (1) New sanctions | (2) Stock of all sanctions | (3) Stock of benefit sanctions | (4) Stock of accommoda- tion sanctions |
|---------------|-------------------------|----------------------------------|---|--|
| Decentralized | -0.080 * (0.048) | -0.141 ** (0.055) | -0.154 *** (0.056) | 0.008 (0.086) |
| R-squared | 0.927 | 0.955 | 0.954 | 0.835 |
| Districts | 334 | 319 | 319 | 319 |
| Observations | 25,497 | 25,773 | 25,773 | 25,555 |

Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

To explore the permanency of these effects, we move again to the dynamic specification of our model as given by equation (2). Figure 5 summarizes the impact of decentralization on our four sanction outcomes. It reveals a reduction in new sanctions during the first year following the reform of up to 40 log points but none in the following years. The stock of all sanctions as well as the stock of benefit sanctions followed a very similar pattern. Accommodation sanctions also experienced drastic decreases in the first post-reform year but were subsequently expanded, leading to statistically significant increases in the third and fourth post-reform years. These results indicate that decentralized job centers do not treat their clients more generously on a permanent basis. Therefore, it is unlikely that the permanently lower job finding is due to a laxer sanctioning regime. However, the time pattern may help to explain the particularly pronounced drop in job finding during 2012. Caseworkers might have been busy coping with new procedures rather than actively monitoring and sanctioning job seekers at that time. Moreover, there is a strong indication that decentralized job centers shifted their sanctions policy to include more cuts in accommodation costs of job seekers. Such a sanction strategy brings relief to local budgets but likely does not lead to additional job placements due to its discouraging effect on job seekers.

Figure 5. Dynamic treatment effects of decentralization on sanctioning of unemployed



Notes.— The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression on the log monthly number of new sanctions as given by equation (2). The year 2010 is the baseline category. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

5.4. Placement Strategies. Decentralizing job centers may provide gains other than higher job finding, such as improved job quality. In particular, job centers may accept a lower placement rate if they emphasize the quality rather than the quantity of their placements. In Germany, decentralized job centers may focus on stable, higher-paying placements because the districts bear the accommodation costs for households on welfare, irrespective of the employment status. In contrast, centralized job centers have an incentive to focus on the number of placements regardless of job quality as any person exiting unemployment reduces FEA expenditures.

We assess the effect of decentralization on the placements' quality using outflows from welfare rather than from unemployment. The welfare data consider all people on welfare of which only about half are registered as unemployed. The remaining welfare recipients are mainly ALMP participants, employed but earning low incomes or unable to work due to familial or health reasons. Total outflows from welfare are about two to three times larger than flows from unemployment into jobs. People will exit welfare if their household income exceeds a subsistence threshold that varies according to household size and local costs of living. They may also exit at the start of retirement. Therefore, our identification strategy rests on the assumption that decentralization affected welfare outflows only via job placements, and that other

components of welfare outflows remained unaffected by this reform or simultaneous unobserved shocks.

Table 8 presents our estimation results. In all columns, we re-estimate equation (1) using vacancies and welfare stocks and inflows as control variables as well as a full set of job center and month fixed effects. In column 1, we focus on the total outflow out of welfare as a rough indicator for reemployment wages. We do not observe a statistically significant effect of decentralization, implying that decentralized job centers do not achieve more high-paying placements than their centralized counterparts. However, they also do not perform significantly worse. Apparently, the lower unemployment outflows observed above do not translate into fewer welfare outflows after decentralization. Two explanations can reconcile these findings. First, unemployment outflows are too small compared to welfare outflows such that the negative effect on the former does not carry over to a negative effect on the latter. Second, decentralized job centers were just reluctant to place their clients into low wage jobs while their placement efficiency for higher paying jobs was not affected by decentralization. Further research using more detailed data will be necessary to disentangle these two explanations.

Table 8. Difference-in-differences: Effect of decentralization on the composition of monthly outflows and unemployment stocks

| Variable | (1) Outflows out of welfare | (2) Permanent outflows out of welfare | (3) Share of permanent outflows |
|---------------|-----------------------------------|--|--|
| Decentralized | -0.019 (0.016) | -0.002 (0.015) | 0.003 * (0.002) |
| R-squared | 0.982 | 0.976 | 0.529 |
| Districts | 334 | 334 | 334 |
| Observations | 26,542 | 26,542 | 26,542 |

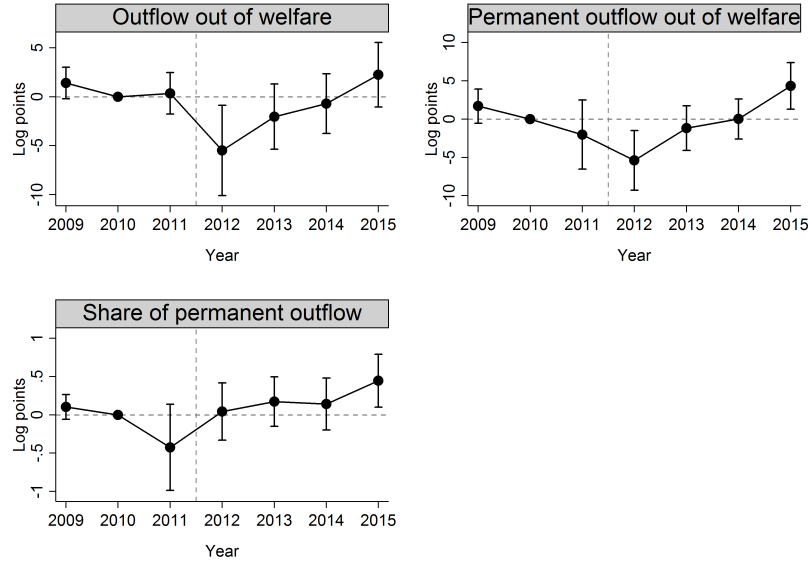
Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Regressions include the stocks and flows of welfare recipients and vacancies as well as a full set of dummies for job centers and months.

In column 2, we narrow our analysis to welfare outflows without returns into welfare during the next three months ('permanent placements'). We consider this as a rough measure of the placements' stability. Again, we do not find a statistically significant impact of decentralization. When using an alternative outcome measure, the share of permanent placements in all outflows from welfare in column 3, we observe a small positive, but only weakly statistically significant impact of decentralization.

Figure 6 presents the respective dynamic treatment effects. These vary over time, with a drop around the decentralization period and a tendency to increasingly stable placements in the longer run. Yet, owing to large standard errors, the treatment effects are almost always statistically insignificant. Altogether, we conclude that the

decentralization of job centers had no positive impact on the placement quality in the four years following the reform.

Figure 6. Dynamic treatment effects of decentralization on monthly welfare outflows



Notes.— The figure depicts coefficients and their 95%-confidence intervals of yearly leads and lags of the decentralization indicator from a stock-flow regression of the monthly outflow out of welfare or the share of permanent outflows as given by equation (2). The year 2010 is the baseline category. The regressions include a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

5.5. Further Considerations. Current literature is increasingly emphasizing the importance of caseworker characteristics in the job matching process (see, for instance, Behncke et al., 2010; Hainmueller et al., 2016). Job placements will suffer from decentralization if decentralized job centers reduce the number of caseworkers or replace experienced ones with less qualified employees. In our example, however, this is not the case. Due to the law regulating decentralization reform, about 95% of the administrative and caseworker staff in the decentralized job centers continued to work for the communal job centers after their reform (Deutscher Bundestag, 2012). The law (§6c SGBII) also prescribed that employees and civil services should retain their prior wages and hierarchy levels. Consequently, changes in the job-center personnel cannot explain permanently reduced job finding.

Finally, differences in the controlling systems possibly contribute to lower job finding through decentralized job centers. As described in Section 2, centralized job centers are under the technical supervision of the FEA while decentralized job centers are not. The FEA imposes a very rigorous target control system on centralized job centers that include target agreements, performance dialogs, ranking comparisons and strict monitoring by a federal institution (Vorstand der Bundesagentur für

Arbeit, 2014). Decentralized job centers only have to report to state ministries but otherwise remain independent. They are members of a voluntary benchmarking program organized by the Federation of German Cities and Communes. Interview partners from ministries and job centers suggest that the FEA controlling system has tighter and more demanding requirements. In addition, its stronger focus on outflow measures may partially explain why centralized job centers generate higher job finding rates.

6. SENSITIVITY ANALYSES

The results presented thus far suggest that decentralization decreased job finding while increasing the inflows into job creation schemes. We now assess the validity of these inferences in detail. There are three major concerns. First, the common trends assumption might be invalid due to the state-quota system inducing a selection problem or due to unobserved labor market shocks. Second, the SUTVA might be violated if labor markets extend beyond district borders and spatial spillovers between treated and non-treated districts arise. Third, our findings might rely on overly restrictive functional form assumptions and other model specifications. In the following paragraphs, we provide a battery of analyses to address each of these concerns. We will focus on our main outcome, the outflow of unemployed into jobs, and provide results for the other main outcomes in Appendix ?? (Tables D.2, D.3 and D.4).

6.1. Selection and Unobserved Shocks. Table 9 summarizes the results of several checks regarding selection and the common trend assumption. The first column analyzes the districts' decision to apply for decentralization. Districts might have based this decision on some time-varying characteristics that are unobserved in our data. If applicants and non-applicants differ significantly from each other with respect to such characteristics, our decentralization estimates are biased. We control for this bias using two alternative specifications. First, we restrict our control group to the non-successful applicants and re-estimate equation (1). If this restriction drives our decentralization estimate down to zero, our main specification has estimated an application rather than a decentralization effect. However, column 1 of Table 9 demonstrates that our estimated decentralization effect on job finding is still -9% using the restricted control group. As this result is very similar to our initial estimate, we take this analysis as initial evidence that applicants and non-applicants do not differ systematically from each other.

As an alternative control for self-selection at the district level, we define the unsuccessful applicants as a placebo treatment group and compare their outcomes to the districts that did not apply for decentralization, i.e. we estimate the effect of being interested but not actually being decentralized. If this estimate is statistically significant, applicants likely differ from non-applicants. As column 2 of Table 9 presents, the applicant status has no such effect on job finding. Thus, we conclude that applicants and non-applicants do not differ with respect to relevant unobserved, time-varying characteristics.

Table 9. Assessing the common trend assumption: Effect of decentralization on log monthly flows into jobs for different control and treatment groups

| Variable | (1) | (2) | (3) | (4) | (5) |
|---------------|------------------------------------|------------------------------|-----------------------------|-----------------------|-----------------------|
| | Denied applicants as only controls | Denied applicants as treated | Over-subscription subsample | Conditional DiD | Triple differences |
| Decentralized | -0.090 *** (0.027) | -0.005 (0.017) | -0.090 ** (0.035) | -0.099 *** (0.021) | -0.095 *** (0.024) |
| R-squared | 0.948 | 0.965 | 0.947 | 0.955 | 0.957 |
| Districts | 76 | 294 | 167 | 330 | 334 |
| Observations | 6,100 | 23,814 | 13,508 | 26,674 | 54,052 |

Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. The outcome variable is the monthly log outflow out of unemployment into jobs. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 4 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. All regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

Successful and non-successful applicants will differ from each other if state governments in the state-quota process successfully chose those applicants for decentralization that were most likely to reap the greatest benefit from decentralization. Our estimated main decentralization effect would then be upwardly biased and even more negative. We assess this kind of selection by restricting our sample to states where the number of applicants exceeded the state quota (‘oversubscription’) and governments had an actual choice among applicants. Selection would be an issue if estimating equation (1) results in less drastic reductions using the ‘oversubscription’ subsample than when using the baseline sample. Column 3 of Table 9 shows that the decentralization effect for the ‘oversubscription’ subsample is incredibly similar to our baseline estimate. Hence, selection into decentralization at the state-level is also unlikely.

We now ask whether job centers of the treatment and the control group have experienced different labor market trends for reasons unrelated to the formal selection process. If observable characteristics influence the unobserved trends, reweighing our observations with regard to these characteristics should reinforce the validity of common trends assumption and should affect our baseline decentralization estimates significantly. Therefore, we employ a variant of the conditional difference-in-differences estimator (see Heckman et al., 1997, 1998, and Appendix D.1 for details). As column 4 of Table 9 indicates, our estimates of the decentralization effect on job finding hardly change due to the balancing. This implies that labor market

trends of decentralized and centralized job centers did not depend on observable characteristics.

Finally, districts from the treatment and the control group could have experienced systematically different unobserved labor market shocks that affect our estimates. The widespread geographical distribution of treated districts makes such an event unlikely. Here it is important to note that the particular German institutional setup allows for an explicit assessment. We exploit data available due to job seekers usually receiving public employment services from local employment offices and not from the job centers during their first 12 months of unemployment.¹¹ These local but federal employment offices are centrally organized throughout Germany, and they were neither directly nor indirectly affected by the decentralization of job centers. However, district-specific labor market shocks and trends should affect the unemployed registered at local employment offices and job seekers registered at job centers alike. We use the unemployed registered at the local employment offices in the same district as an additional comparison group to control for time-varying district-specific shocks in a triple differences estimation (see, for instance, Gruber, 1994). As shown by column 5 in Table 9, the decentralization effect on unemployment outflows from job centers is again estimated to be about -10% . This result affirms that our preferred specification is not biased by unobserved district-specific shocks, and that the common trend assumption is likely to hold.

6.2. Spatial Spillovers. We now address the second major concern to validity, potential spillovers among districts. In particular, we worry about indirect treatment effects on non-decentralized job centers and labor market regions extending beyond district borders. Table 10 summarizes the results for this analysis.

Table 10. Assessing SUTVA and spatial effects: Effect of decentralization on monthly log flows into jobs for different model specifications

| Variable | (1) Controls without non-treated neighbors | (2) Spatial lag in X | (3) X measured at commuting zone level |
|---------------|---|-------------------------|---|
| Decentralized | -0.093 *** (0.023) | -0.099 *** (0.022) | -0.120 *** (0.024) |
| R-squared | 0.959 | 0.962 | 0.958 |
| Districts | 222 | 334 | 334 |
| Observations | 17,926 | 26,998 | 26,998 |

Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. The outcome variable is the monthly log outflow out of unemployment into jobs. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

¹¹This is because unemployed receive unemployment insurance benefits during this period.

Indirect treatment effects arise if job finding in decentralized job centers declines and if centralized job centers in neighboring districts advise their clients on the ‘additional’ unmatched vacancies. This would increase job finding in centralized districts bordering a treated region. Such spillovers would dilute the control group, violate the SUTVA, and exaggerate our estimate of the true reduction in job finding. To examine this problem, we drop all units from the control group that border districts with decentralized job centers. If any spillover effects dilute our baseline specification, this change to the control group should reduce the size of the estimated treatment effects. However, column 1 of Table 10 confirms our baseline estimate. Hence, we conclude that decentralization did not generate spillover effects on non-decentralized districts.

In spite of this finding, there might be more complex spatial patterns with spillovers reaching beyond direct neighbors. For instance, job search competition is larger if unemployment is large and vacancies are scarce in nearby districts. To capture such effects, we estimate a standard spatial lag in X model (see LeSage and Pace, 2009). We add spatial lags for each explanatory variable in our baseline regression using row-normalized inverse distances between districts as respective spatial weights. As column 2 of Table 10 shows, the inclusion of spatially lagged covariates does not alter our results. To confirm this finding, we aggregate the stocks and inflows of unemployed and vacancies on the commuting-zone level based on the commuting zones definition by Kropp and Schwengler (2016). Column 3 presents our estimation of equation (1) employing the commuting-zone variables. The result reveals that our decentralization effect remains very similar to previous estimates, albeit with a slightly higher magnitude of about -12% . In summary, none of the three spatial approaches used suggests that geographic spillovers invalidate our main findings.

6.3. Model Misspecification. Finally, we analyze whether our model imposes improper functional form assumptions and whether standard errors are calculated correctly. To relax the functional form assumption, we run a synthetic control approach following Abadie and Gardeazabal (2003) and Abadie et al. (2010). This method is purely data-driven and non-parametric. Nevertheless, its results, described in Appendix D.2, are highly similar to those derived from our stock-flow model in equation (1). We conclude that our model does not impose improper functional form assumptions.

Next, we examine whether our standard errors are correctly sized and do not overstate the significance of our findings. Serial correlation in particular, which we deal with by two-way clustering standard errors at the district and month-level, is a frequent concern in difference-in-differences studies (Bertrand et al., 2004). Following Huber et al. (2013), we run an empirical Monte-Carlo simulation on our subsample of non-treated districts. In each replication, we randomly assign a placebo treatment status to 41 districts and then estimate the effect of the placebo treatment as in our main model. With 5,000 replications, we find significant pseudo-decentralization effects at the 5% level in less than 5.9% of all cases. Furthermore, we inspect the distribution of the resulting t-statistics for the decentralization coefficient to confirm it follows a normal distribution (Figure D.2 in Appendix D). Both checks yield adequate results and ensure that the size of our standard errors is correct.

7. CONCLUSION

Few studies have examined the impact of decentralizing public employment services although numerous countries have implemented such reforms. In this paper, we provide the first comprehensive analysis of public employment services under decentralization and their effect on job finding and labor market policies. Exploiting a unique German policy experiment that transferred 41 federally-managed job centers to the district level, we estimate that job-center decentralization reduced job finding by approximately 10% within four years. Estimates from dynamic models point to the existence of a reform transition period lasting for about one year. Nevertheless, efficiency losses are still sizable even four years after decentralization. We uncover that decentralization leads to a significant increase of inflows into job creation schemes while leaving overall ALMP participation unchanged. Moreover, decentralization temporarily reduced benefit sanctions in the first year after decentralization, likely reflecting a transitional process. In contrast, we do not find evidence for higher quality placements or increased geographical lock-in of job seekers. Finally, we can rule out a difference between the two provider types driven by caseworker quality or quantity.

The persistent drop in job finding combined with the increased use of job creation schemes indicates that local politicians utilized decentralization to shift fiscal costs from their own to the national budget. Local administrations have tangible financial benefits from job creation scheme participants in the form of public goods, while the federal government primarily bears the costs of program participation and subsequent unemployment.

The decentralization reform had substantial impacts on public budgets. Assuming outflows into non- or self-employment remained unaffected, average unemployment duration in decentralizing job centers increased by about 3 months. Since benefit and accommodation payments amount to about 820 euros per unemployed person per month (see Weber et al., 2014, p. 4), these figures imply additional costs of about 2,500 euros per unemployed. On average, 27,000 persons register as new unemployed at the 41 decentralized job centers each year, not accounting for re-entries after ALMP measures or very short employment spells (see Hofmann and Stephan, 2016). Thus, a conservative estimate is that the 2012 decentralization caused additional annual costs of at least 66 million euros. Further fiscal burdens arise from prolonged job counseling, additional ALMP participations and foregone tax revenues.

Our findings are informative for policy makers considering to reform and decentralize public employment services. Canada, Denmark, Italy and other countries have undergone significant decentralizations in the past but cannot evaluate the impact of their reforms because they lack a proper treatment-control-group design. Other countries, including Germany, have been discussing whether to (further) decentralize their public employment services. Our findings imply that decentralized job centers may fail to internalize the effects of their strategies on total public budgets and individual reemployment rates. More generally, they strongly suggest the importance of carefully studying the incentive effects arising from decentralization, as ill-designed institutional structures may significantly reduce the job centers' matching efficiency.

Therefore, this analysis should serve as a starting point for further research distinguishing the impacts of decentralization under alternative financing rules and division of competences. Additional research is also necessary to study the internal structures and strategies adopted by centralized and decentralized job centers in more detail. Moreover, the interaction of localized provision modes with the political sphere is clearly under-explored. Finally, long-term effects extending beyond the temporal constraints of this paper, as well as alternative outcome measures, such as re-employment wages and match durations, will help to understand the consequences of decentralization. The decentralization of public employment services remains a crucial topic for future research.

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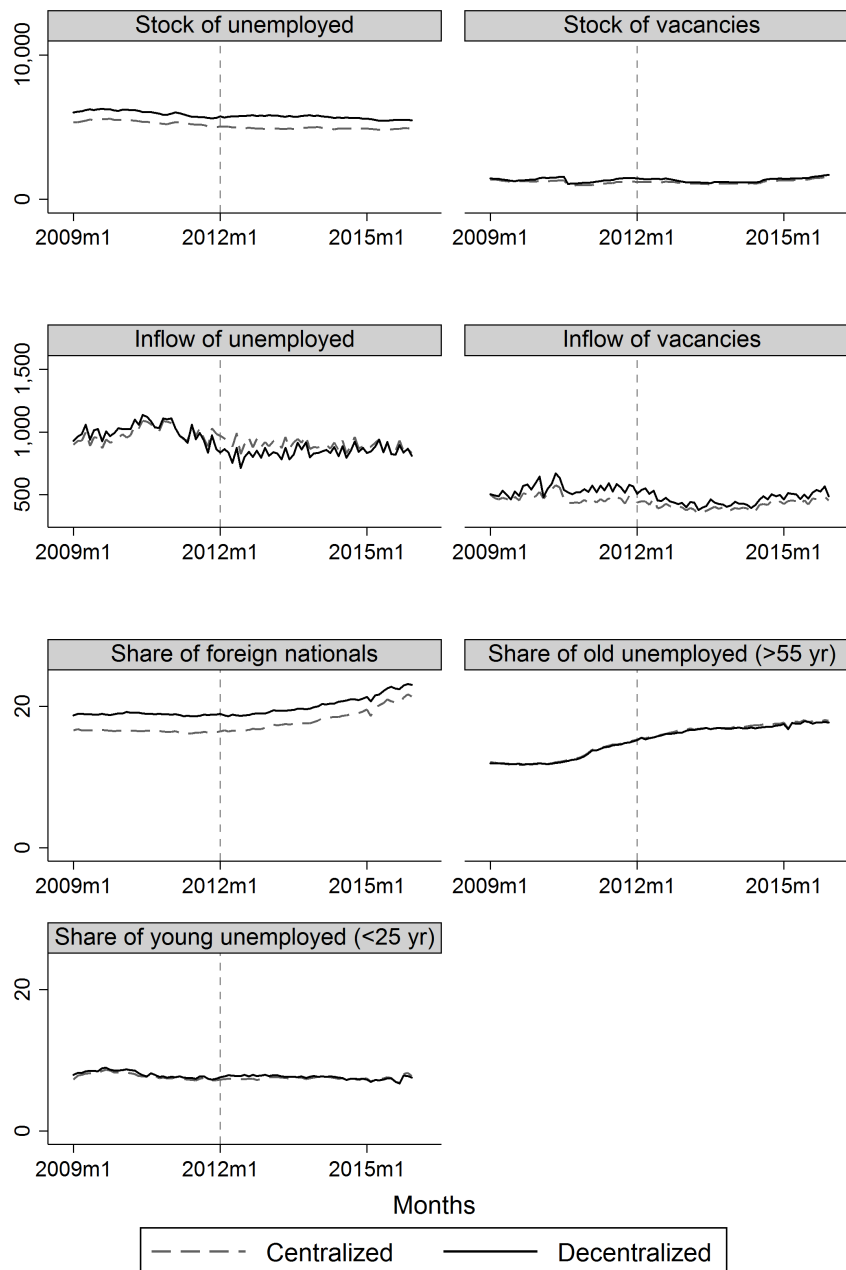
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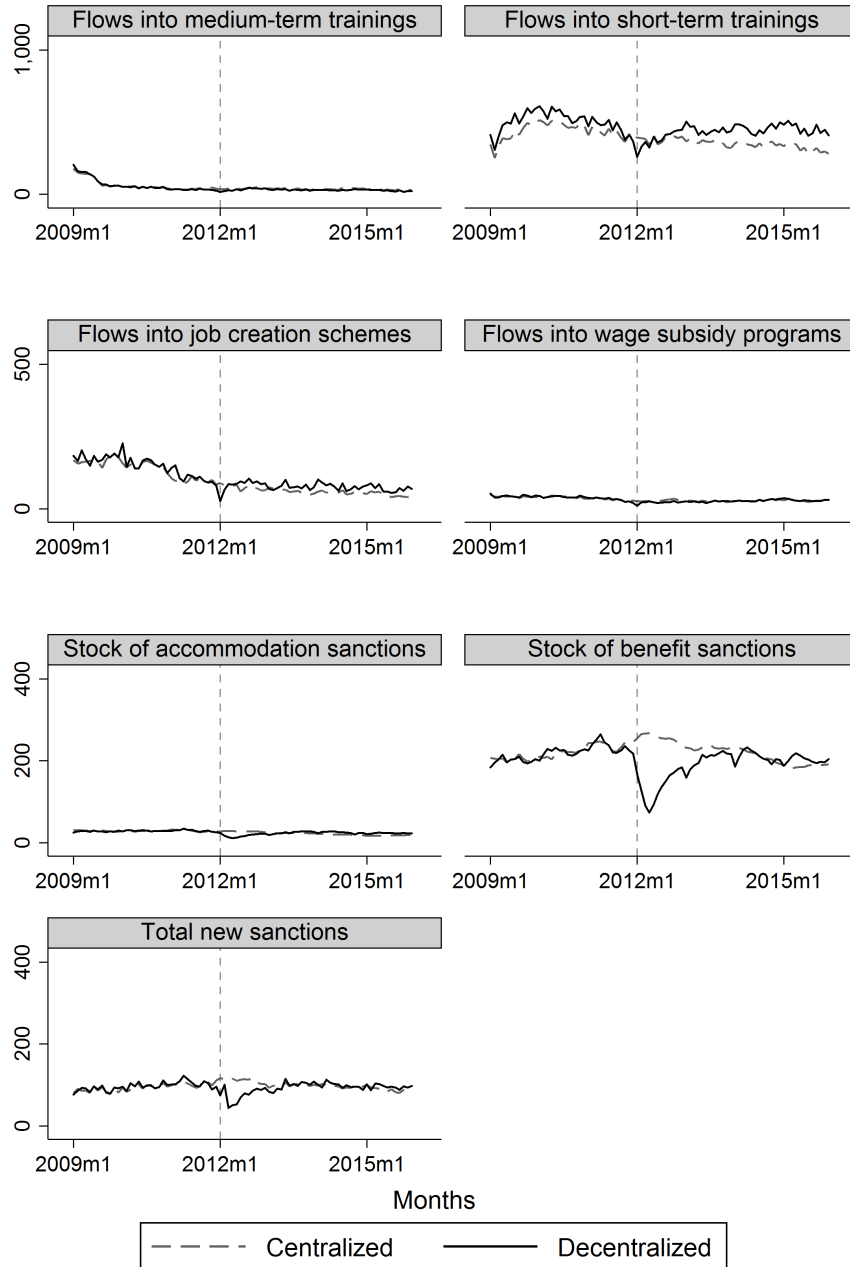
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APPENDIX

APPENDIX A. ADDITIONAL DESCRIPTIVE GRAPHS

Figure A.1. District characteristics over time by job center type (part I)

Notes.— The upper four panels depict monthly stocks and inflows of vacancies and unemployed for districts in our sample. The lower three panels depict the demographic composition of the unemployed.

Figure A.2. District characteristics over time by job center type (part II)

Notes.— The upper four panels depict monthly inflows into almp measures for districts in our sample. The lower three panels depict the stocks of sanctions in place by sanction type and the total inflow of new sanctions per month.

APPENDIX B. TIME TRENDS AND ALTERNATIVE SAMPLE PERIODS

Table B.1 adds linear time trends to the baseline model specified in equation 1 while Table B.2 varies the sample period to ensure our results are not driven by the transition period around the reform's implementation.

Table B.1. Difference-in-differences: Adding linear time trends to the baseline model

| | (1) East trend | (2) State trends | (3) District trends |
|---------------|----------------------|----------------------|------------------------|
| Decentralized | -0.096*** (0.022) | -0.106*** (0.022) | -0.145*** (0.046) |
| R-squared | 0.999 | 0.999 | 0.999 |
| Districts | 334 | 319 | 334 |
| Observations | 26,998 | 25,783 | 26,998 |

Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. All continuous variables in logs. Regressions include a full set of dummies for districts and months. Standard errors given in parentheses are clustered at the job center and month level.

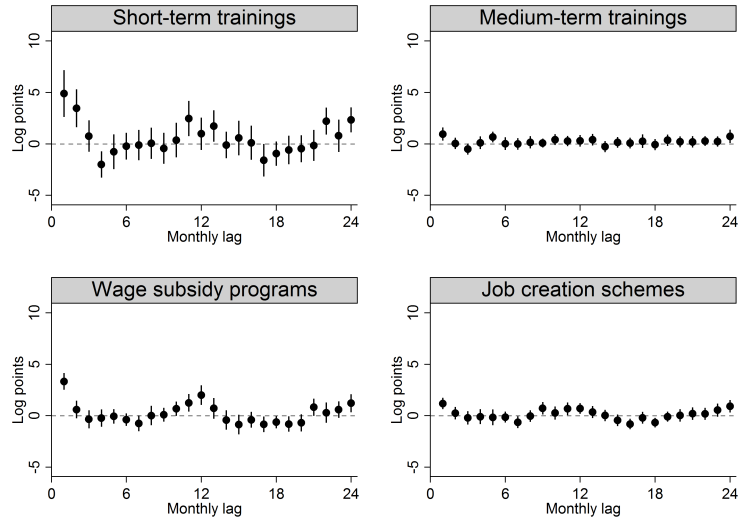
Table B.2. Difference-in-differences: Alternative sample periods

| | (1) W/o 2011 | (2) W/o 2012 | (3) W/o 2011 & 2012 |
|---------------|----------------------|----------------------|---------------------------|
| Decentralized | -0.102*** (0.023) | -0.077*** (0.022) | -0.083*** (0.022) |
| R-squared | 0.962 | 0.963 | 0.962 |
| Districts | 334 | 334 | 334 |
| Observations | 22,990 | 24,023 | 20,015 |

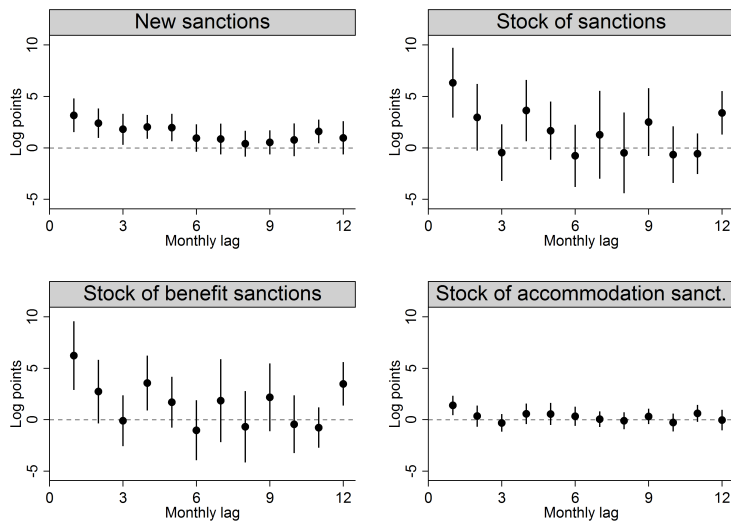
Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 1. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. All continuous variables in logs. Regressions include a full set of dummies for districts and months. Standard errors given in parentheses are clustered at the job center and month level.

APPENDIX C. CORRELATION ANALYSES OF ALMP AND SANCTION
EFFECTIVENESS

Figure C.1. OLS: Correlations of lagged entries into ALMP measures with job finding



Notes.— The figures depict coefficients and their 95% confidence intervals from a simple regression of job finding on the lags of monthly inflow into job-creation schemes. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

Figure C.2. OLS: Correlations of lagged sanctions with job finding

Notes.— The figures depict coefficients and their 95% confidence intervals from a simple regression of job finding on the lags of monthly sanctions. The regression includes a full set of dummies for job centers and months. Standard errors are clustered at the job center and the month level.

APPENDIX D. FURTHER SENSITIVITY ANALYSES

D.1. Conditional Difference-in-Differences. If observable characteristics influence the unobserved labor market trends of centralized and decentralized job centers, reweighing our observations with regard to these characteristics should reinforce the validity of the common trends assumption. Therefore, we employ a variant of the conditional difference-in-differences estimator (see Heckman et al., 1997, 1998). This estimator balances the treatment and the control observations with regard to their fundamental characteristics before running the difference-in-differences regression. Usually, balancing is performed on the propensity score which requires estimating potentially restrictive probit or logit models in the first place. In contrast, we use entropy balancing which is a non-parametric method. Entropy balancing assigns each control unit a non-negative weight such that the reweighted control group and the treatment group match exactly in terms of pre-specified sample moments of their covariate distributions (Hainmueller, 2012).

We balance the growth rates of major population and labor market groups. Table D.1 presents these mean growth rates and the statistical significance of their differences across subsamples before and after matching. It turns out that the mean growth rates were quite similar already before applying entropy balancing. Yet, entropy balancing further reduces any differences.

Table D.1. Balancing of mean growth rates for 2000–2010 (GDP, population, employment) or 2007–2010 (unemployment, welfare)

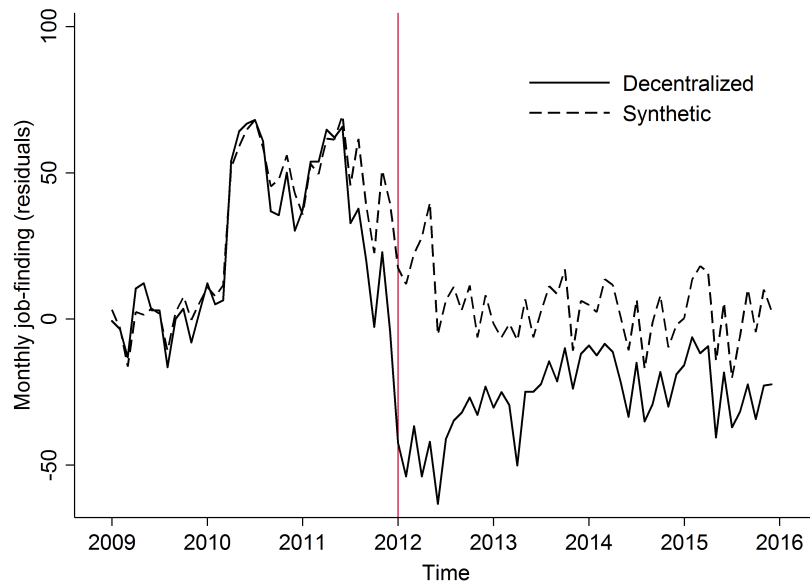
| Variable | Treated | Unbalanced Control | | Balanced Control | |
|---------------------------|---------|--------------------|----------|------------------|---------|
| | Mean | Mean | P-Value | Mean | P-Value |
| GDP per capita | 22.970 | 24.099 | 0.578 | 22.970 | 1.000 |
| Civil labor force | −4.053 | −3.337 | 0.517 | −4.052 | 1.000 |
| Young (15–24 yr) | −2.231 | 0.245 | 0.382 | −2.231 | 1.000 |
| Prime-aged (25–54 yr) | −4.611 | −4.361 | 0.808 | −4.610 | 0.999 |
| Old (55–64 yr) | −2.702 | −1.763 | 0.530 | −2.702 | 1.000 |
| Foreign nationals | 1.079 | 6.711 | 0.017 ** | 1.088 | 0.997 |
| Employment | 1.125 | 2.862 | 0.138 | 1.127 | 0.999 |
| Agriculture | −14.563 | −13.323 | 0.696 | −14.561 | 1.000 |
| Mining and energy | −2.042 | 1.540 | 0.490 | −2.040 | 1.000 |
| Manufacturing | −7.174 | −6.951 | 0.931 | −7.172 | 1.000 |
| Construction | −15.049 | −16.063 | 0.667 | −15.052 | 0.999 |
| Trade, transp., comm. | 0.974 | 3.123 | 0.217 | 0.975 | 0.999 |
| Finance and real estate | 21.660 | 25.296 | 0.170 | 21.663 | 0.999 |
| Public and priv. services | 9.043 | 9.737 | 0.669 | 9.043 | 1.000 |
| Job-center unemployment | −11.523 | −13.497 | 0.309 | −11.525 | 0.999 |
| Young (15–24 yr) | −14.847 | −19.351 | 0.282 | −14.851 | 0.999 |
| Prime-aged (25–54 yr) | −13.299 | −15.246 | 0.312 | −13.302 | 0.999 |
| Old (55–64 yr) | 7.777 | 9.107 | 0.708 | 7.779 | 1.000 |
| Foreign nationals | −10.500 | −12.917 | 0.301 | −10.501 | 1.000 |
| Population on welfare | −7.451 | −8.826 | 0.322 | −7.453 | 0.999 |
| Young (15–24 yr) | −13.762 | −15.478 | 0.421 | −13.765 | 0.999 |
| Prime-aged (25–54 yr) | −10.174 | −11.706 | 0.290 | −10.176 | 1.000 |
| Old (50–64 yr) | 5.320 | 4.634 | 0.559 | 5.319 | 1.000 |
| Foreign nationals | −4.323 | −7.023 | 0.119 | −4.325 | 0.999 |
| | 40 | 290 | | 290 | |

Notes.— P-values given for t-test of mean equality. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

D.2. Synthetic Control. To relax the functional form assumption from our main model, we run a synthetic control approach following Abadie and Gardeazabal (2003) and Abadie et al. (2010). The synthetic control approach is purely data driven and makes no functional form assumption. For each treated district, we construct a synthetic counterfactual as a linear combination of the control group districts. The resulting synthetic control unit is then used to extrapolate the counterfactual evolution of job finding of the treated unit for the post-treatment period.

The linear combination is chosen such that the synthetic control unit resembles the treated unit's job-finding flow during the first half of the pre-intervention period as closely as possible. We use the second half of the pre-treatment interval as a validation period to confirm the model's validity. 'Closeness' is measured as the Mean Squared Prediction Error (MSPE). Predictions are based on observed stocks and inflows of unemployed and vacancies, as well as the shares of young, old and foreign individuals among the total stock of unemployed. All data are demeaned and seasonally adjusted.

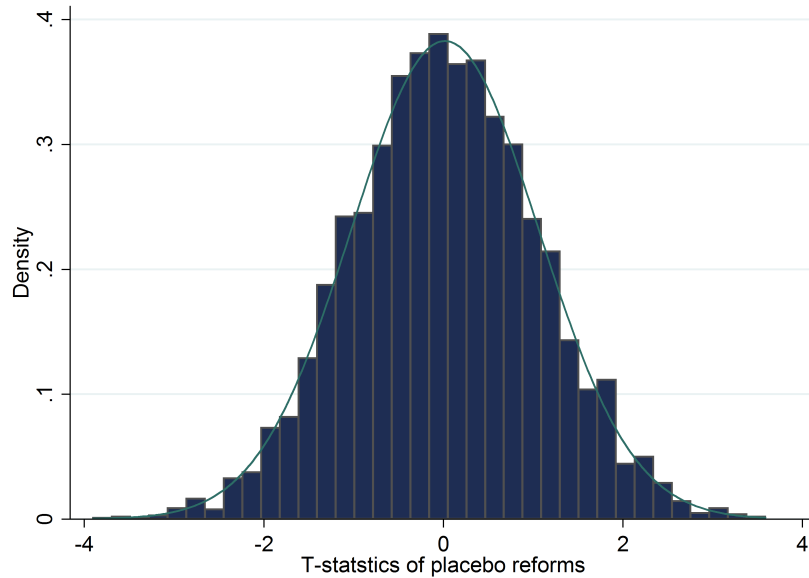
Figure D.1 presents the resulting evolution of the average job-finding flow of treated and synthetic control units. Across the entire pre-treatment period, the job-finding flows in both groups are almost identical, suggesting the synthetic control group successfully replicates the evolution of decentralized districts. Yet there seems to be a beginning divergence starting around the announcement date of districts to be decentralized in April 2011 which supports the use of the year 2010 as the baseline category for our parametric dynamic models. After the decentralization in 2012, job finding in affected districts declined significantly relative to the synthetic control observations. After about one year, the job finding in treated districts slowly converges to the synthetic control group again but stabilizes at a lower level. On average, job finding in decentralized districts is around 10% below synthetic levels, consistent with our DiD estimates.

Figure D.1. Synthetic control approach

Notes.— Time-labels (x-axis) refer to January of a given year. Synthetic control approach with seasonally adjusted job-finding as the outcome variable, i.e residuals from a regression of monthly job finding levels per district on eleven month dummies and an intercept. Donor pools for synthetic control units include all districts not decentralizing in 2012. Predictor variables include all covariates from the baseline regression as well as the shares of old, young and foreign individuals among the total stock of unemployed. The second half of the pre-treatment interval is used as a validation period. Synthetic control was computed for each treated district individually and then averaged across all decentralizing districts.

D.3. Empirical Monte-Carlo simulation. Figure D.2 refers to an empirical Monte-Carlo simulation following Huber et al. (2013), where randomly chosen non-reforming districts receive placebo treatments to confirm that our standard errors are correctly sized.

Figure D.2. Histogram of t-statistics for decentralization coefficient from placebo treatments (N=5,000 replications)



Notes.— T-statistics computed from 5,000 estimations of equation (1) where *Decentralized* is a dummy equaling 1 for 41 randomly chosen districts with centralized job centers and 0 otherwise. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months. Standard errors are two-way clustered on the job center and the month level.

D.4. Sensitivity Analyses for Inflows into ALMPs and Flows of Sanctions.
(subsequent pages)

Table D.2. Difference-in-differences: Effect of decentralization on log monthly inflows into all ALMP measures under different specifications

| Variable | (1) Base speci- fication | (2) Denied applicants as only controls | (3) Denied applicants as treated | (4) Over- subscription subsample | (5) Conditional DiD | (6) Controls without non- treated neighbors | (7) Spatial lag in X | (8) X measured at com- muting zone level |
|---------------|--------------------------------|--|---|---|---------------------------|--|----------------------------|---|
| Decentralized | 0.037 (0.065) | 0.005 (0.074) | 0.030 (0.037) | -0.156 * (0.084) | 0.011 (0.059) | 0.031 (0.066) | 0.019 (0.063) | -0.006 (0.068) |
| R-squared | 0.946 | 0.919 | 0.952 | 0.929 | 0.927 | 0.940 | 0.947 | 0.943 |
| Districts | 319 | 76 | 279 | 167 | 315 | 208 | 319 | 319 |
| Observations | 25,839 | 6,156 | 22,599 | 13,527 | 25,515 | 16,848 | 25,839 | 25,839 |

Notes. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 5 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

Table D.3. Difference-in-differences: Effect of decentralization on log monthly inflows into job creation schemes under different specifications

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---------------|----------------------|------------------------------------|------------------------------|-----------------------------|----------------------|--|----------------------|------------------------------------|
| | Base specification | Denied applicants as only controls | Denied applicants as treated | Over-subscription subsample | Conditional DiD | Controls without non-treated neighbors | Spatial lag in X | X measured at commuting zone level |
| Decentralized | 0.320 *** (0.079) | 0.125 (0.108) | 0.204 ** (0.087) | 0.302 ** (0.133) | 0.257 *** (0.086) | 0.311 *** (0.082) | 0.322 *** (0.079) | 0.267 *** (0.080) |
| R-squared | 0.821 | 0.801 | 0.822 | 0.798 | 0.810 | 0.819 | 0.822 | 0.819 |
| Districts | 319 | 76 | 279 | 167 | 315 | 208 | 319 | 319 |
| Observations | 24,783 | 5,998 | 21,632 | 12,732 | 24,459 | 16,250 | 24,783 | 24,783 |

Notes. – * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 5 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job centers and months.

Table D.4. Difference-in-differences: Effect of decentralization on log monthly total new sanctions under different specifications

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---------------|---------------------|------------------------------------|------------------------------|-----------------------------|-----------------------|--|---------------------|------------------------------------|
| | Base specification | Denied applicants as only controls | Denied applicants as treated | Over-subscription subsample | Conditional DiD | Controls without non-treated neighbors | Spatial lag in X | X measured at commuting zone level |
| Decentralized | -0.080 * (0.048) | -0.084 (0.053) | -0.005 (0.031) | -0.096 * (0.053) | -0.107 *** (0.039) | -0.070 (0.049) | -0.081 * (0.048) | -0.101 ** (0.049) |
| R-squared | 0.927 | 0.912 | 0.932 | 0.882 | 0.919 | 0.923 | 0.928 | 0.926 |
| Districts | 334 | 76 | 294 | 167 | 330 | 222 | 334 | 334 |
| Observations | 25,497 | 5,755 | 22,538 | 12,893 | 25,185 | 16,797 | 25,497 | 25,497 |

Notes.— * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (1). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Standard errors given in parentheses are clustered at the job center and the month level. Standard errors for column 5 were obtained by 200 bootstrap replications of the combined balancing and estimation procedure. Regressions include the stocks and flows of unemployed and vacancies as well as a full set of dummies for job center and months.