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7957
2019

November 2019

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CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
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Editor: Clemens Fuest
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CESifo *Working Paper No. 7957*
Category 4: Labour Markets

Public Employment Services under Decentralization: Evidence from a Natural Experiment

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 public job creation schemes which diminish job seekers' reemployment prospects but shift costs to higher levels of government.

JEL-Codes: H110, H750, I380, J480, J640.

Keywords: decentralization, public employment services, job placements.

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October 29, 2019
We thank participants of the EALE 2017 conference, the EEA 2016 Annual Conference, the ERSA 2017 conference, the ESPE 2014 Annual Conference, the SES 2014 Annual Conference, the 7th IAB GradAB Workshop, the 2015 Potsdam Workshop in Empirical Economics, the VfS Annual Conference 2017, the 2015 Warwick Economics PhD Conference as well as seminar audiences in Berlin, Dresden, Leipzig, and Nuremberg for helpful comments and suggestions. In particular, we are grateful to Bernd Fitzenberger, Erik Grönqvist, Rafael Lalive, Ben Lockwood, Rajshri Jayaraman, Udo Kreickemeier, Martin Lundin, Eva Mörk, Steve Pischke, Björn Öckert, Alexandra Spitz-Oener, Marcel Thum, Fabian Waldinger, Felix Weinhardt, and Josef Zweimüller for their advice. We also thank the German Federal Ministry of Labour and Social Affairs as well as our contact persons at eleven state ministries for providing invaluable guidance on the institutional setting. Mergele gratefully acknowledges financial support through the DFG grant RTG 1659 as well as excellent research assistance by Alex Graf and Alex Rebmann.

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 unimpaired 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 five years after the reform. 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; Ebrahimy and Shimer, 2010). Our analysis employs an aggregate administrative data set comprising the monthly stocks and gross flows of unemployed welfare recipients and vacancies in German districts from 2007 to 2016. 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 fifth post-reform years. This effect is equivalent to an increased average unemployment duration of three months. We run a battery of robustness checks including individual-level analyses, 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 changes in the job centers placement policies that could account for these losses. We consider a shift toward placements with higher quality, a geographical lock-in of job seekers, ALMP strategies, the monitoring system, and the role of caseworkers. We again employ a difference-in-differences framework, using both aggregate and indivdiual-level data. Most importantly, we identify an immediate and permanent shift towards public job creation programs that are ineffective in increasing reemployment rates compared to other ALMP measures (see, for instance, Card et al., 2017). We do not find evidence for a higher quality of placements, geographical lock-in effects or changes in the monitoring and counseling activities.

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 generate local public goods whereas the federal government covers most of the associated costs. Our findings have important consequences for public budgets. Hence, our study emphasizes that decentralization reforms necessitate a careful assessment of potential incentive problems and fiscal externalities to avoid unintended consequences.

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

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 five 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, and section 5 explores underlying channels. Section 6 examines the validity of these results and Section 7 concludes.

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

[2]See, 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.

[3]A small number of papers have compared public to private provision regimes, finding mixed results for job seeker-outcomes (see, for instance, Heinze et al., 2006; Bennmarker et al., 2013; Behaghel et al., 2014).

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] Job centers serve the residents of their district.[5] 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. According to the social security code, the aim of job centers is to integrate clients into employment and allow them to live a dignified life.

Unique to Germany, two types of job centers exist that differ 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.

	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).

Table 1. Job centers by type of organization

[4]Job centers do not serve short-term unemployed covered by unemployment insurance. These individuals are served in separate local employment offices. We further explain and make use of this institutional feature in section 6.1.
[5]Six job centers serve multiple districts, covering 16 districts in total (as of December 2011).

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.

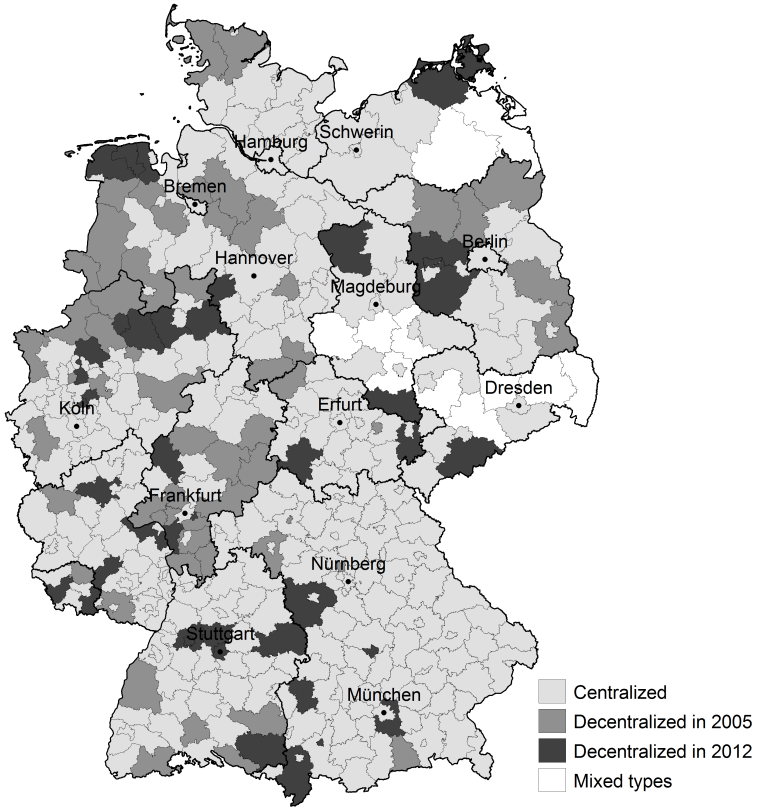
2.2. **The 2012 Decentralization.** Decentralized job centers were established in two waves. The first wave, in 2005, established job centers as one-stop employment offices for the first time into the welfare system whereby decentralized job centers were set up in 67 districts of Germany's 402 districts. 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).[6] 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. 75 districts applied. If the number of applying districts fell short of the available spots 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 strong labor market conditions, nor are they disproportionally located in cities or rural areas. They also resemble one another in more general economic

[6]An 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.

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. A simple F-Test with $F(20, 298) = 1.08$ and $p = 0.37$ also does not reject the hypothesis that the group differences in Table 2 are jointly different from zero. In sum, these results supply first evidence that the two groups of districts are observationally equivalent.



Notes.– Mixed types refer to districts where decentralized and centralized job centers coexist e.g. due to district mergers.
Source.– Geodata: GeoBasis-DE / BKG 2014.

Figure 1. German districts by job center type

3. Data and Empirical Strategy

3.1. **Data.** We utilize three data sets to examine the effects of decentralization on job finding and other labor market outcomes: monthly official aggregate data at the

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

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

district level (OAD), daily administrative data at the individual level (SIAB) and yearly survey data at the individual level (PASS-ADIAB).[7]

Monthly official aggregate data (OAD) are taken from the labor market reports of the FEA's statistical office (*Arbeitsmarkt in Zahlen*). The reports are based on data from the job centers' operational processes. They cover the universe of German job centers and all unemployed job seekers, as unemployment registration is mandatory for receiving welfare benefits. The reports provide monthly observations at the district level on unemployment, vacancies, ALMP participation, and benefit sanctions. For all variables, we readily observe stocks as well as gross flows and thus do not have to deal with time aggregation issues. For the unemployed, we also observe the demographic composition such as the share of foreign nationals, people younger than 25 years, and older than 55. We use the OAD data to study the effect of decentralization on unemployment outflows into jobs and into different ALMP programs. The sample period ranges from 2007 to 2016, i.e. from five years before to five years after the decentralization reform. This long time span allows us to check for short-term and medium-term dynamics of any decentralization effects. We note that all OAD figures are based on a legal definition of unemployment that explicitly distinguishes between 'unemployed job seekers' and 'ALMP participants'. As a consequence, ALMP participants are not included in the stock of unemployed, and direct ALMP-to-job transitions are not counted as job finding. We demonstrate in Section 4.4 that this has little impact on our estimate of the effect of decentralization on aggregate job finding. Appendix A.1 provides further details and descriptive statistics for the aggregate OAD data.

We complement our district-level data with two individual-level data sets. Our first individual-level data set is the weakly anonymous Sample of Integrated Labor Market Biographies (Years 1975–2014, SIAB 7514). The SIAB is a high-quality administrative data set at the individual level which compiles compulsory notifications of employers to the German social security system with process data from the German unemployment and welfare system. The data covers a 2% random sample of all individuals who have been employed, unemployed or – since 2005 – on welfare in Germany. For these persons, we observe a variety of demographic variables, their daily employment biographies including wages, and their places of residence and work. We will use the SIAB data mainly to assess decentralization effects on post-unemployment outcomes. Additionally, the SIAB allows us to build an aggregate data set similar to the OAD data but with economically more meaningful definitions of unemployment. We will use this aggregated data from the SIAB to verify our results from the official labor market reports. The SIAB allows inferring periods of ALMP participation but does not report in which ALMP programs job seekers registered at decentralized job centers take part (Antoni et al., 2016, p. 22). Thereby it is impossible to draw conclusions regarding the effect of decentralization on inflows into particular ALMP programs. Observations based on the SIAB end in December 2014, effectively censoring all ongoing spells at this point in time. Appendix A.2 provides further details and descriptive statistics for the SIAB data.

[7]For the individual-level data sets, data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequent remote data access (projects fdz1537 and fdz1538). See Antoni et al. (2016, 2017) for detailed data descriptions.

Finally, we make use of the weakly anonymous data of the Panel Study 'Labour Market and Social Security' (PASS), which is linked to administrative data of the IAB (PASS-ADIAB, version 7515, waves 1–10). The PASS is a yearly survey particularly designed for households receiving unemployment benefit II, i. e. clients of the German job centers. This data is then linked to the above-mentioned administrative data sources. After applying our initial sample restriction (unemployed household heads receiving unemployment benefit II), the PASS-ADIAB covers about 16,157 person-year observations. Households are drawn from recipient registers of the FEA in 300 of Germany's more than 8,000 postcode areas. PASS is thus more restricted in its geographic scope and overall size but allows to study additional outcomes such as sanction durations as well as the number of contacts and counseling sessions with job center caseworkers. Its limited coverage also does not enable us to compute dynamic effects of decentralization. The PASS sample period ends in 2016. Appendix A.3 provides further details and descriptive statistics for the PASS data.

For all three data sets, we implement the same sample definitions. The sample period starts in 2007 and ends in 2014 (SIAB) or 2016 (OAD, PASS-ADIAB), respectively. We omit aggregate and individual-level observations from districts that decentralized their job centers already in 2005, although their inclusion as an additional control group does not alter our results. From the remaining regions, 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. Our final sample covers observations from 334 out of 401 German districts.

3.2. **Econometric Model.** 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. We estimate econometric models both at the aggregate and the individual level. In either model, standard errors are clustered two-dimensionally by district and by month to account for unobserved correlation within these two dimensions (Bertrand et al., 2004).

For the analysis at the aggregate level, we employ a functional form that is motivated by a stock-flow matching model with Cobb-Douglas technology (Coles and Smith, 1998; Ebrahimy and Shimer, 2010).[8] 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 as well as 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. Log-linearizing the stock-flow matching function, our estimation equation then reads

$$M_{dt} = \delta\, D_{dt} + \beta_1 U_{dt} + \beta_2 V_{dt} + \beta_3 \widetilde{U}_{dt} + \beta_4 \widetilde{V}_{dt} + \alpha_d + \mu_t + \varepsilon_{dt} \tag{1}$$

[8]The 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).

where M_{dt} denotes matches defined as transitions from unemployment into jobs for district (i.e. job center) d and month t, our main outcome. The dummy variable D_{dt} indicates whether a job center is decentralized or not. U_{dt} and V_{dt} denote the stocks of unemployed and vacancies, whereas $\widetilde{U}_{it}$ and $\widetilde{V}_{it}$ denote their respective inflows in this month. We include district-specific effects α_d 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.

At the individual level, we employ a reduced form model of the form

$$Y_{idt} = \delta\, D_{idt} + X'_{it}\beta + \alpha_d + \mu_t + \epsilon_{idt} \tag{2}$$

where Y_{it} denotes the outcome of job seeker i in district d in month t. The dummy variable D_{idt} is 1 for residents of decentralizing districts who register for unemployment on or after January 1, 2012, and 0 otherwise. We control for a vector X_{it} of job seeker characteristics measured at the beginning of the unemployment spell, including age, age squared, gender, foreign nationality, high school degree, professional degree, and occupational group. To capture labor market histories, we add the pre-unemployment wage and five variables to separately measure the number of days in employment in each of the five years prior to registering at the job center.[9] Analogous to our aggregate analysis, we include district-specific effects α_d to account for time-invariant differences in labor market outcomes across districts and month-fixed effects μ_t to absorb business cycle and seasonal fluctuations. If the dependent variable Y_{idt} is a binary indicator, such as the indicator of having found a job, we estimate linear probability models. Our parameter of interest is δ, the average treatment effect of being registered at a decentralized job center. As the control group also includes partially treated unemployment spells that start before a job center reform and continue thereafter, our estimations of δ may be attenuated – a presumption we are going to address in Section 4.4.

Our empirical approach generally relies on two main identifying assumptions.[10] 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.

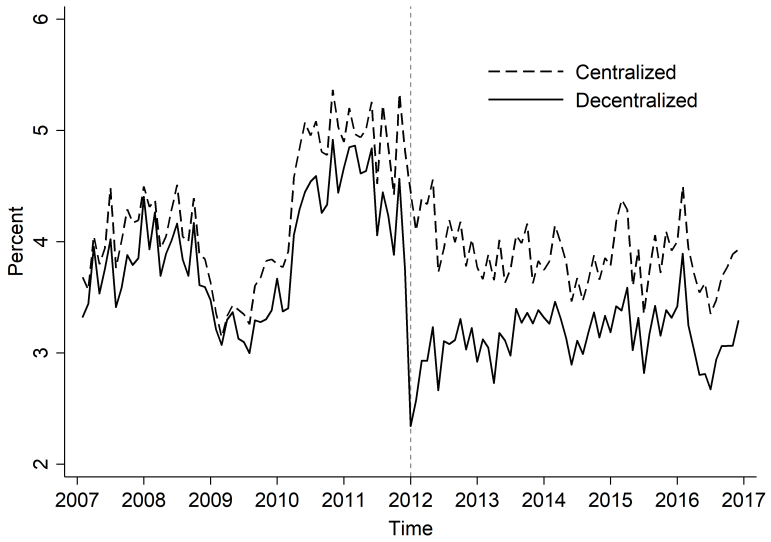
4. The Effects of Decentralization on Job Finding

This section presents results for the effect of decentralization on job finding. We start with a descriptive analysis of the aggregate job-finding rate and then proceed with estimating static and dynamic treatment effects using our aggregate OAD data. In the last part of this section, we address potential shortcomings of the OAD-based estimates using individual-level data from SIAB.

[9]Usually, job seekers register at job centers after one year in unemployment when their entitlement to the short-term unemployment benefit UB I expires.

[10]We 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.

4.1. **Descriptive results.** Illustrating the effect of decentralization descriptively, Figure 2 shows the evolution of the seasonally adjusted average 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 five 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 B.1 and B.2 in Appendix B.1, again confirming parallel pre-reform trends. However, after the decentralization in January 2012, the job-finding rate of decentralized job centers dropped sharply 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 narrows until mid-2015 but widens again thereafter and does not return to its pre-reform size. This points to permanent negative effects of decentralization on job finding.



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 on the x-axis refer to January of a given year.
Source.– OAD data. Sample period 2007–2016.

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

4.2. **Static treatment effects.** We now investigate whether parametric estimates will support our descriptive findings. Table 3 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 district-specific trends into the empirical model, using interacted fixed effects and using alternative sample periods (see Tables S.1, S.2 and S.3 in Appendix S.1).

Hence, we conclude that decentralization reduced the monthly flow into jobs on average by about 10% within five years following the reform. This effect size is equivalent to an increase in the average unemployment duration by about three months.[11]

4.3. **Dynamic treatment effects.** 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

$$M_{it} = \sum_{\substack{\tau=2007 \\ (\tau \neq 2011)}}^{2016} \delta_\tau \, D_{i\tau} + \beta_1 U_{it} + \beta_2 V_{it} + \beta_3 \widetilde{U}_{it} + \beta_4 \widetilde{V}_{it} + \alpha_i + \mu_t + \varepsilon_{it} \tag{3}$$

where τ denotes years and δ_τ are yearly coefficients. All dynamic effects are estimated relative to the pre-treatment base year 2011. 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 five years before to five 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 years, this effect weakens over time but still amounts to almost 10% in the fifth year after decentralization. Seven expert interviews with division heads of state and federal ministries as well as job

[11]The 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.

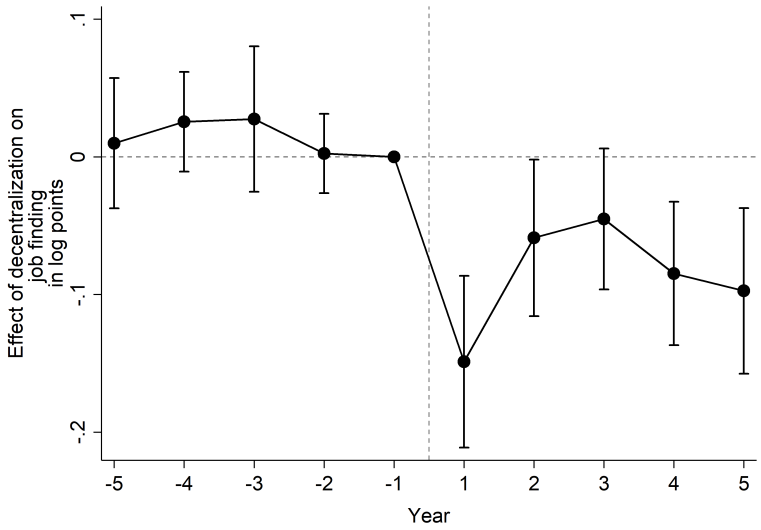
Variable	(1) Fixed Effects	(2) Stocks	(3) Stock-Flow	(4) Controls
Decentralized	−0.119 ***	−0.124 ***	−0.096 ***	−0.100 ***
	(0.028)	(0.025)	(0.021)	(0.021)
Vacancies, stock		0.029 **	−0.027 **	−0.025 **
		(0.013)	(0.011)	(0.011)
Unemployed, stock		0.562 ***	0.372 ***	0.383 ***
		(0.036)	(0.031)	(0.031)
Vacancies, inflow			0.110 ***	0.111 ***
			(0.012)	(0.012)
Unemployed, inflow			0.331 ***	0.327 ***
			(0.021)	(0.021)
Unemployed: <25 ys				−0.004 **
				(0.002)
Unemployed: >50 ys				−0.005 ***
				(0.002)
Unemployed: foreign				−0.004 **
				(0.002)
Outcome Mean	4.667	4.667	4.667	4.667
R-squared	0.951	0.955	0.958	0.958
Districts	334	334	334	334
Observations	39,018	39,018	39,018	39,018

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (1), where the outcome is the log outflow from unemployment into employment. *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 job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016.

Table 3. Difference-in-differences: Average effect of decentralization on monthly log flows into jobs

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 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.

4.4. **Robustness checks using alternative sample definitions.** In the remaining section, we verify that our main result is not driven by the nature of our OAD data. Two issues raise our concern. First, by excluding ALMP participants from the unemployed and the unemployment-to-employment flows, our OAD-based estimates might overstate the negative effect of decentralization on aggregate job



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 (3). The year 2011 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. The outcome mean is 4.667.
Source.– OAD data. Sample period 2007–2016.

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

finding. Second, some of the decentralization effect might be driven by changes in the composition of the inflows into unemployment. Such changes are unobserved in our aggregate figures. To analyze both issues, we draw on the individual-level data from the SIAB.

First, we address the appropriate definition of unemployment. Economic theory considers any non-employed job seeker as unemployed, irrespective of enrollments into ALMP programs, while our OAD data excludes such ALMP participants. Therefore, we use the SIAB to build three OAD-style aggregate data sets with alternative definitions of unemployment. The first definition considers only official unemployment registrations, mirroring the legal unemployment definition underlying our OAD data. The second unemployment definition additionally includes periods of reported ALMP participation. The third unemployment definition further includes observational gaps, such that unemployment lasts from the first official unemployment registration at a job center until the start of the next employment spell. This definition follows a proposition by Fitzenberger and Wilke (2010) that is now widely used in the literature.[12]

[12]Unlike the first two unemployment definitions, this one keeps job seekers in the unemployment pool even in case of long illness, parental leave or similar events. However, in some instances, it may mistakenly also define periods of self-employment, education or other non-unemployment periods unobserved in SIAB as unemployment.

We then re-estimate the aggregate model (1) for each definition of unemployment. Table 4 presents the results. We find strong negative effects of decentralization on aggregate job finding, irrespective of the particular definition of unemployment. Even with our broadest definition, we estimate a treatment effect that is very close to our initial OAD-based results. The same holds true for the respective dynamic estimations provided in the Appendix S, Table S.7. The inclusion of ALMP participants in the pool of unemployed does not affect these findings, because job seekers enrolled in ALMP participants are much less likely to find a job than other unemployed job seekers. This conclusion is also supported by Appendix B.2, which provides a descriptive overview of these transitions. In sum, the evidence implies that our main result does not depend on the exact definition of unemployment.

Unemployment:	(1) Official Unemployment	(2) Official Unemployment + ALMP	(3) Official Unemployment + ALMP + Gaps
Decentralized	−0.101 ***	−0.113 ***	−0.106 ***
	(0.028)	(0.026)	(0.025)
Outcome Mean	1.113	1.263	1.477
R-squared	0.674	0.705	0.727
Districts	334	334	334
Observations	25,257	26,032	24,866

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. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

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

We now turn to our second concern. The reduction of the aggregate job-finding rate could also reflect changes in the inflow into unemployment. In fact, Table B.2 in the Appendix shows that decentralization slightly increased the share of lower qualified job seekers among the inflow into unemployment. Since lower qualified job seekers are harder to be placed into jobs, this compositional effect could add to our estimate of the negative decentralization effect. On the other hand, we also observe a decentralization-related decline in the share of foreign citizens and an increase in the amount of days worked prior to registering for unemployment. Both changes are rather associated with an increasing job-finding probability.

To investigate whether these compositional changes affect our results, we re-estimate the effect of decentralization directly at the individual level, using the specification of equation (2). We use two different outcome measures, the log duration in job-center unemployment and the probability of finding a job out of unemployment, while controlling for a range of job-seeker characteristics as well as job center and months fixed effects. We estimate the models for the raw sample and for a matched sample, where inflows of the control group are reweighted such that their average characteristics match exactly the average characteristics of the inflows of the treatment group. The re-weighing uses the entropy balancing procedure proposed by (Hainmueller, 2012) and is performed separately for the pre-treatment and the post-treatment period. This analysis provides us with two insights. First, it gives an estimate of the decentralization effect on job finding at the individual level. Second, by comparing the results for the raw and the matched sample, we can assess whether changes in the inflow composition affect our results.

Variable	Raw sample		Matched sample	
	(1) Log unemployment duration	(2) P(Exit to Job)	(1) Log unemployment duration	(2) P(Exit to Job)
Decentralized	0.090 ***	−0.032 ***	0.087 ***	−0.030 ***
	(0.029)	(0.009)	(0.029)	(0.009)
Outcome Mean	5.364	0.724	5.364	0.715
R-squared	0.093	0.084	0.094	0.087
Districts	344	344	344	344
Observations	121,675	121,675	121,675	121,675

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table 5. Difference-in-differences: Average effect of decentralization on individual unemployment duration and job finding

Table 5 shows the results when employing the broadest definition of unemployment. The first two columns present the results for the raw sample, the last two columns present corresponding results for the matched sample. In the first column, we observe that decentralization increased the average duration in unemployment. In the second column, we find that decentralization reduces the individual probability of finding a job. Using the matched sample, we obtain quantitatively and qualitatively very similar results. This indicates that the observed changes in the inflow composition

of decentralized job centers do not affect our results. Several additional analyses corroborate this finding. Table C.1 in the Appendix shows that the larger unemployment duration is independent of the exact definition of unemployment. The findings for the probability of finding a job are more diverse. Table S.8 confirms the similarity between the dynamic effects for the individual-level outcomes and the patterns found at the aggregate level. The models in Table S.10 then repeat the baseline analysis for our main outcomes while excluding unemployment spells first registered in the pre-treatment year 2011. The estimates are highly similar to their equivalents using the full sample, suggesting that attenuation bias through including partially treated spells in the control group is not a major concern. Similarly, Table S.11 presents models which only include unemployment spells starting before June 2013 and models where unemployment spells are capped at 365 days. Their results indicate that our main effect is also insensitive to the censoring of the SIAB data in 2014. Altogether, the individual-level analyses support our previous finding of a lower aggregate monthly job-finding rate. They also suggest that this reduction cannot be attributed to changes in the inflow composition of unemployed.

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) higher-quality placements; (ii) a geographical lock-in of job seekers; (iii) changes in the flows into ALMP measures; (iv) changes in the monitoring and sanctioning of job seekers or (v) changes in the job center contact intensity. Finally, we will briefly discuss additional features of PES playing a potential role in the decentralization process.

5.1. **Placement Quality.**

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 individual-level data from SIAB. We consider three different job quality indicators: whether the new job is a regular full-time position, whether the new employment lasts for at least six months, and the log daily wage difference between the new and the previous job (Nekoei and Weber, 2017). For all quality indicators, we re-estimate equation (2) using individual covariates and a full set of job center as well as month fixed effects as control variables.

Table 6 presents our estimation results using the broadest definition of unemployment. For the full-time and job duration indicators in the first two columns, we do not observe an effect of decentralization. Column three suggests that the reform entails a moderate wage change increase of 2.9 log points. Using other definitions of unemployment confirms the results for the first two job quality indicators but

also suggests a null-effect for the change in wages (Table C.2, Appendix C). Similar conclusions are proposed by the respective dynamic estimations in Table S.9 (Appendix S), though findings seem non-reliable for the job duration indicator due to a significant pre-reform change. Tables S.12 and S.13 show that the previous results are insensitive to the right-censoring of spells in 2014. In sum, these results imply that decentralized job centers do not achieve more stable or long-lasting placements than their centralized counterparts. If anything, there is weak evidence that decentralization is associated with moderately higher wages of new job matches, compared to jobs held before unemployment.

Variable	(1) P(Regular full-time job)	(2) P(Empl. lasts $>$ 6 months)	(3) Log wage change (in Euro)
Decentralized	−0.003	−0.005	0.029 *
	(0.013)	(0.011)	(0.015)
Outcome Mean	0.469	0.494	0.126
R-squared	0.210	0.039	0.377
Districts	344	344	344
Observations	88,148	88,148	88,148

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table 6. Difference-in-differences: Average effect of decentralization on job characteristics after unemployment

5.2. **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 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 analyze if decentralization increased the probability of job seekers to be placed more often in jobs within their home district. We again draw on the SIAB data, which provides for each job seeker their place of residence during unemployment as well as their

Variable	(1) Job in district of residence
Decentralized	0.003
	(0.012)
Outcome Mean	0.631
R-squared	0.084
Districts	344
Observations	88,148

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the following control variables: age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table 7. Difference-in-differences: Average effect of decentralization on location of new jobs

place of work during employment. Places can be identified at the district level. We re-estimate the individual-level equation (2) using as dependent variable a dummy that indicates whether the new job is located in the person's place of residence as stated from the unemployment spell. The variable is explicitly set to zero if people move to their new place of work. Table 7 does not indicate any evidence that the likelihood of being placed 'at home' has either increased or decreased after decentralization. This result is independent of the definition of unemployment used, see Appendix C.2. There, we additionally show that geographical lock-in is also not an issue at the aggregate level. Hence, decentralization did not increase the geographical lock-in of job seekers.

5.3. **Active Labor Market Policies.** Changes in the assignment of job seekers into ALMP measures constitute a third potential explanation for 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.[13] 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 very ineffective in increasing reemployment rates compared to other measures (for large-scale meta-studies, see Heckman et al., 1999; Kluve, 2010; Card et al., 2017). Wapler et al. (2018) explicitly show this program type to reduce the regional matching efficiency between job seekers and vacancies in the German context.

To assess whether decentralization caused a shift toward less effective ALMP measures, we employ our OAD data and the stock-flow model from equation (1).[14] This time, we use outflows from unemployment into the different ALMP programs at the district-level as the outcome variables. Table 8 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 aggregate job finding. In fact, decentralization increased the average inflow rate into job creation schemes by a similar magnitude as it decreased the average aggregate job-finding rate.[15]

One could suppose that the 'additionally' assigned partcipiants in job creation schemes found new jobs while enrolled in the program, driving down the observed job finding flows in our OAD data and inducing a bias in our corresponding decentralization effect from Section 4. We argue that this is rather unlikely. First, there is large empirical evidence that job creation schemes are a rather ineffective ALMP measure. Second, this general result seems to hold also for Germany, as a simple descriptive analysis using our SIAB data in Appendix B.2 suggests. Third, our results in Section 4.4 indicate that our main results are robust to including ALMP participants in the unemployment stocks and flows.

[13]District authorities mainly pay for accommodation costs of job seekers, see Section 2.

[14]Individual-level data is not applicable for this purpose as neither SIAB nor PASS provide sufficient information on ALMP participants in decentralized job centers.

[15]In 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 +30% 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 $1.2\% \cdot \frac{0.3}{1+0.3} = 0.23$ percentage points.

Variable	(1) All ALMPs	(2) Short-term trainings	(3) Medium-term trainings	(4) Wage subsidies	(5) Job creation schemes
Decentralized	0.033	−0.068	−0.057	−0.046	0.302 ***
	(0.064)	(0.095)	(0.071)	(0.074)	(0.081)
Outcome Mean	5.911	5.410	3.179	2.863	3.179
R-squared	0.939	0.860	0.806	0.839	0.822
Districts	319	319	319	319	319
Observations	37,323	37,318	35,669	36,773	35,925

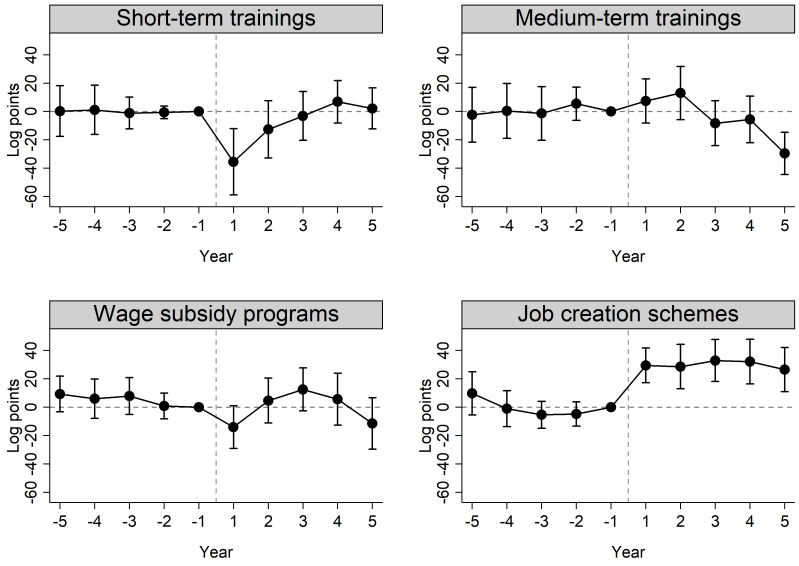
Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents an estimation of equation (1). The dependent variables are inflows of unemployed into the respective ALMP categories. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the stocks and inflows 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.

Source.– OAD data. Sample period 2007–2016.

Table 8. Difference-in-differences: Effect of decentralization on monthly district-level log flows into active labor market policies (ALMPs)

We now investigate the underlying dynamic changes in ALMP assignments. We employ equation (3) 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 systematic or permanent changes that are statistically significant at the 95%-confidence level. Inflows into medium-term training appear to be slightly reduced in year five. For job-creation schemes, in contrast, inflows increase directly after decentralization and remain at a permanently higher level.

Possibly, some local authorities used the decentralization of their job centers to shift fiscal costs from their own to the federal budget. The incentive to do so is inherent to a system where the national government covers the costs of program participation and subsequent unemployment while not being able to influence the local job centers' ALMP strategy. However, decentralization may not only shift financial but also political incentives. As additional job creation schemes lower the official unemployment figures (see Section 3.1 for a discussion of this problem) and allow for a better provision of public services, they may also serve as a tool for incumbent local politicians ahead of local elections. The auxiliary analysis in Appendix C.4 shows that decentralized providers rely on job creation schemes more heavily ahead of communal elections but not ahead of elections at the state level, indicating an electoral component of decentralization.



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 (3). The year 2011 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. Outcome means are 5.410, 3.179, 2.863, and 3.179 respectively.
Source.– OAD data. Sample period 2007–2016.

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

5.4. **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. Decentralized job centers may have a lower tendency to impose sanctions due to the financing structure of welfare support in Germany. The phase-out of welfare benefits is such that federally financed benefit payments will be reduced first. Sanctions large enough to reduce welfare payments beyond the welfare benefit then will also reduce the accommodation costs financed by local governments. In financial terms, spending reductions from sanctions therefore primarily benefit the central government whereas local governments' spending would only be reduced if job seekers severely or repeatedly fail to comply with their job centers. Hence, decentralized job centers face weaker financial incentives to impose sanctions. However, 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; Arni et al., 2013; van den Berg et al., 2014). Hence, we expect fewer sanctions to reduce job finding which would provide an additional explanation for our main finding.

To explore changes in monitoring and sanctions strategies, we apply the baseline model from equation (2) to the sanction outcomes that we obtain from the PASS data. Table 9 presents our estimates for the total number of sanctions received per year as well as their average duration. There is no indication for a negative decentralization effect: According to columns 1 and 2, decentralization did not alter the number of sanctions imposed. As the number of sanctions may hide differences in the duration of the benefit cuts, we next explore changes in sanction lengths. However, columns 3 and 4 suggest that decentralization also did not shift average sanction durations.[16] Hence, the potential conclusion that decentralized job centers imposed fewer or shorter sanctions is not supported by our analysis.

[16]In Table S.14 of Appendix S.7, we repeat the analysis for our job finding outcomes using PASS. This is to show that PASS is generally able to capture relevant effects of decentralization despite of the smaller sample size, e.g. for duration in unemployment.

Variable	(1) Sanctions p.a.	(2) Sanctions p.a.	(3) Log Sanction Duration	(4) Log Sanction Duration
Decentralized	0.006	0.007	0.038	0.012
	(0.014)	(0.014)	(0.148)	(0.143)
Controls	No	Yes	No	Yes
Outcome Mean	0.077	0.077	4.519	4.519
R-squared	0.036	0.044	0.233	0.258
Observations	16,143	16,143	844	844

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, dummies for gender and foreign nationality as well as a full set of dummies for school degrees, professional degrees, job centers and months. Standard errors given in parentheses are clustered at the job center and the survey month level. *Source.*– PASS-ADIAB 7515. Sample period: 2007–2016 (Survey waves 1–10).

Table 9. Difference-in-differences: Average effect of decentralization on sanctions issued and sanction durations

5.5. **Caseworkers and Job Counseling.** Current literature is increasingly emphasizing the importance of caseworkers 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 the 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. The law (§6c SGBII) also prescribed that employees and civil servants should retain their prior wages and hierarchy levels. A report for the German parliament confirmed that districts complied with the provisions of the law. Consequently, changes in the job-center personnel cannot explain permanently reduced job finding.

We back up and extend this argument with an analysis based on the PASS data. In particular, we examine whether decentralization changed the number of personal contacts as well as the number of detailed consultations between job seekers and caseworkers. We then estimate the effect of decentralization on these variables using equation (2). Table 10 presents the results. Columns 1 and 2 show that the number of annual personal contacts was unaffected by decentralization. Columns 3 and 4 point out that the same holds true for detailed consultation sessions between job seekers and caseworkers. These results strongly support the insight from the parliamentary report which concluded that decentralization did not coincide with changes in the availability of caseworkers.

Variable	(1) Personal contacts	(2) Personal contacts	(3) Detailed consulta-tions	(4) Detailed consulta-tions
Decentralized	−0.230	−0.197	0.185	0.137
	(0.666)	(0.615)	(0.481)	(0.426)
Controls	No	Yes	No	Yes
Outcome Mean	9.457	9.457	5.356	5.356
R-squared	0.128	0.180	0.082	0.107
Observations	13,201	13,201	8,050	8,050

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, dummies for gender and foreign nationality as well as a full set of dummies for school degrees, professional degrees, job centers and months. Standard errors given in parentheses are clustered at the job center and the survey month level.

Source.– PASS-ADIAB 7515. Sample period: 2007–2016 (Survey waves 1–10). Wave 7 omitted as it does not include caseworker contact items. Detailed consultation item only available for subset of survey participants.

Table 10. Difference-in-differences: Average effect of decentralization on job seekers' contacts with job centers

5.6. **Further Considerations.** 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 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 requirements with a stronger emphasis on quantitative outflow measures. Hence, it may partially explain why centralized job centers generate more job placements.

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 S.5 (Tables S.5 and S.6).

6.1. **Selection and Unobserved Shocks.** Table 11 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 11 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 main result from Table 3, we take this analysis as initial evidence that applicants and non-applicants do not differ systematically from each other.

Variable	(1) Denied applicants as only controls	(2) Denied applicants as treated	(3) Over-subscription subsample	(4) Conditional DiD	(5) Triple differences
Decentralized	−0.087 ***	−0.006	−0.095 ***	−0.095 ***	−0.071 ***
	(0.025)	(0.017)	(0.021)	(0.020)	(0.019)
Outc. Mean	4.666	4.647	4.594	4.673	5.534
R-squared	0.943	0.960	0.952	0.952	0.975
Districts	75	294	309	330	334
Observations	8,722	34,395	36,093	38,550	78,096

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. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. 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.
Source.– OAD data. Sample period 2007–2016.

Table 11. Effect of decentralization on log monthly flows into jobs for different control and treatment groups

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 11 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.

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 negative main decentralization effect would then be biased upwards and the true effect was 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 11 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 S.2 for details). As column 4 of Table 11 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. Still, we explicitly assess this issue using another particularity of the German unemployment system. We exploit that in each district two different kinds of public employment services are at work: one for the short-term unemployed (local employment offices) and one for the long-term unemployed (job centers).[17] The local employment offices are all governed by the FEA and are therefore centrally organized throughout Germany. 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

[17]This is because unemployed receive unemployment insurance benefits during the first twelve months in unemployment, financed by contributions of employers and employees.

instance, Gruber, 1994). As shown by column 5 in Table 11, 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. Table S.2 in Appendix S provides an alternative safeguard against regional correlated shocks by adding year by region fixed effects to the baseline model. As regions, we use either the 16 German states, or commuting zones as defined by Kropp and Schwengler (2016), as well as the 170 unemployment insurance regions (*Arbeitsagenturbezirke*)[18]. Our finding is also robust to these modifications.

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 12 summarizes the results for this analysis.

Variable	(1) Controls without non-treated neighbors	(2) Spatial lag in X	(3) X measured at commuting zone level
Decentralized	−0.093 ***	−0.100 ***	−0.133 ***
	(0.022)	(0.021)	(0.023)
Outcome Mean	4.675	4.673	4.673
R-squared	0.954	0.958	0.956
Districts	222	334	334
Observations	25,915	39,018	39,018

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. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016.

Table 12. Assessing SUTVA and spatial effects: Effect of decentralization on monthly log flows into jobs for different model specifications

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

[18]Unemployment insurance regions are an administrative unit of the FEA in between the state and district level, typically covering about three districts.

treatment effects. However, column 1 of Table 12 confirms our baseline estimate. Combining these results with our analysis on geographical lock-in in section 5.2, 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 high 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 12 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 S.3, 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 that it follows a normal distribution (Figure S.2 in Appendix S). 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 five years. Efficiency losses are still sizable even five years after the reform's implementation. We uncover that decentralization leads to a significant increase of inflows into job creation schemes while leaving overall ALMP participation unchanged. In contrast, we do not find evidence for higher quality placements or geographical lock-in of job seekers, less strict sanction strategies, or fewer caseworker contacts.

The persistent drop in job finding combined with the increased use of job creation schemes indicates that local politicians possibly 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. As a consequence, extended benefit durations, additional job search assistance, and foregone tax revenues likely sum up to a substantial burden for public budgets.

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 will help to understand the consequences of decentralization more thoroughly. The decentralization of public employment services remains a crucial topic for future research.

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Appendix

Appendix A. Further Data Set Information

A.1. **OAD Data.**

Variable	Mean	SD	Min.	Max.
Job-finding	4.67	0.89	1.61	8.95
Vacancies, inflow	5.75	0.76	3.09	9.65
Vacancies, stock	6.71	0.81	2.94	10.47
Unemployed, inflow	6.30	0.89	3.64	10.68
Unemployed, stock	7.96	1.02	5.48	12.29
Share: Unemployed <25 yr	7.75	2.13	0.06	30.58
Share: Unemployed >55 yr	14.57	4.10	3.44	31.21
Share: Unemployed Foreign	18.36	10.50	0.74	59.43
Flow into ALMP	5.91	1.03	2.20	10.22
Into short-term training	5.41	1.04	0.00	9.47
Into subsidized employment	2.86	1.19	0.00	7.51
Into medium-term training	3.18	1.52	0.00	8.78
Into public job creation s	3.67	1.48	0.00	9.22

Notes.– Monthly aggregate district-level data. All level-variables are given in logs. $N = 39,018$.
Source.– OAD data. Sample period 2007–2016.

Table A.1. Descriptive statistics of district-level variables from OAD data.

A.2. **SIAB Data.** The SIAB is a high-quality administrative data set at the individual level which compiles compulsory notifications of employers to the German social security system with process data from the German unemployment and welfare system. For each person in the SIAB, we observe a variety of demographic variables, their daily employment biographies including wages, and their places of residence and work.

For our analyses, we first assign each person to one of the following labor market states per day: unemployment, employment, and ALMP participation.[19] We then Impose three different definitions of unemployment, that are nested within each other:

(1) Unemployment = registered unemployment. This definition mirrors the definition of unemployment in the official statistics and our OAD data.
(2) Unemployment = registered unemployment + ALMP participation.
(3) Unemployment = registered unemployment + ALMP participation + observational gaps. This definition follows a proposition by Fitzenberger and Wilke (2010) that is now widely used in the literature.

[19]ALMP participation is not directly reported by decentralized job centers, but can be approximated by the registration 'not-unemployed job seeker'. To avoid mis-classifications, we use only 'not-unemployed job seeker' registrations that follow an actual unemployment registration within a certain time interval.

Using the prepared data, we build an aggregate panel data set and a micro data set. The panel data set aggregates the individual observations at the district-month level and has the same structure as the official statistics from our OAD data. For the aggregation, we apply the following definitions that are taken from the official statistics:

- Inflows: Sum of people entering unemployment from other labor market states or after gaps of more than 7 days.
- Stocks: Sum of people being unemployment and having been unemployment in the last 7 days.
- Job finding: Sum of people leaving unemployment and entering employment by the beginning of the next months.

When calculating inflows, stocks, and job-finding flows, we apply each of our three definitions of unemployment. For our regression analyses, we take the log of all variables. Table A.2 presents descriptive statistics of the resulting data sets.

Variable	Mean	SD	Min.	Max.
Panel A: Official Unemployment ($N = 25,534$)				
Log inflow	2.166	0.964	0.000	6.534
Log stock	4.172	0.996	0.693	8.323
Log outflow into jobs ('job-finding')	1.113	0.868	0.000	5.525
Panel B: Official Unemployment + ALMP ($N = 26,311$)				
Log inflow	1.874	0.940	0.000	6.263
Log stock	4.405	0.989	1.099	8.547
Log outflow into jobs ('job-finding')	1.263	0.896	0.000	5.659
Panel C: Official Unemployment + ALMP + Gaps ($N = 25,134$)				
Log inflow	1.277	0.890	0.000	5.649
Log stock	4.734	0.941	1.792	8.778
Log outflow into jobs ('job-finding')	1.477	0.904	0.000	5.775

Notes.– Monthly district-level data based on SIAB.
Source.– SIAB 7514. Sample period 2007–2014.

Table A.2. Descriptive statistics of district-level aggregates from SIAB

Next to the aggregate panel data sets, we construct corresponding individual-level data sets using additional sample restrictions. First, we consider only unemployment spells that start with an unemployment registration at a job center between January 2007 and June 2014. The time limits mitigate the problem of left-censoring and increase the probability that an unemployment spell leads to a new job within our observation period. Second, we only consider the very first unemployment spell after employment, deliberately omitting unemployment re-entries after observational gaps of more than 7 days. We also omit unemployment re-entries within 30 days after the end of the last unemployment spell, even if there was a short employment spell in between. By doing so, we aim to reduce unobserved correlations across

unemployment spells. Finally, we require unemployment spells to last at least a minimum of 15 days to obtain a credible influence of a job center on its clients' unemployment duration, job finding probability and post-unemployment outcomes. We then consider as job finding any transition out of unemployment into employment, if the employment spell starts within 30 days after the end of the unemployment spell, if the employment spell lasts more than 7 days, and if the new job pays a positive wage. Unemployment spells are right-censored at the end of the sample period, if the job seeker migrates to ananother job center or if the job seeker is unobserved for more than 7 days. Table A.3 presents descriptive statistics of the resulting data set for our broadest definition of unemployment.

Variable	Mean	SD	Min.	Max.
Decentralized	0.125	0.331	0	1
Age (in years)	36.732	11.480	15	65
Female	0.418	0.493	0	1
Foreign nationality	0.207	0.405	0	1
High school degree	0.189	0.392	0	1
Professional degree				
None	0.296	0.456	0	1
Vocational degree	0.622	0.484	0	1
University degree	0.081	0.273	0	1
Occupational group				
Agriculture	0.031	0.173	0	1
Production	0.250	0.433	0	1
Consumer Services	0.252	0.434	0	1
Business Services	0.345	0.475	0	1
Scientific Serices	0.100	0.300	0	1
Other	0.019	0.138	0	1
Pre-unemployment histories				
Prior daily wage (in Euro)	30.262	26.165	0.1	810.5
Days in empl., $\tau - 1$	133.493	115.270	0	365
Days in empl., $\tau - 2$	122.531	135.379	0	365
Days in empl., $\tau - 3$	124.669	143.238	0	365
Days in empl., $\tau - 4$	122.508	145.007	0	365
Days in empl., $\tau - 5$	121.547	146.269	0	365
Unemployment characteristics				
Unemployment duration (days)	424.920	518.534	15	2922
Exit to job	0.724	0.446	0	1
Characteristics of new job[a]				
Regular, full-time job	0.469	0.500	0	1
Daily wage (in Euro)	32.699	25.447	0.1	510.2
Empl. lasts > 6 months	0.494	0.499	0	1
Located in district of residence	0.631	0.482	0	1

Notes.– Individual-level data. Statistics for broadest unemployment definition: Unemployment includes registered unemployment, ALMP participation and observational gaps. τ represents the year in which UE was registered at the job center. $N = 121,675$.

a. Only for job seekers who found a job out of unemployment. $N = 88,148$.

Source.– SIAB 7514. Sample period 2007–2014.

Table A.3. Descriptive statistics of individual-level data from SIAB

A.3. **PASS-ADIAB Data.** The Panel Study 'Labour Market and Social Security' (PASS) is a yearly survey particularly designed for households in receipt of unemployment benefit II, i. e. clients of the German job centers. The survey data is linked to administrative data records of the German social security system, yielding the PASS-ADIAB. The administrative records also include the district of current residence which we use to assign the treatment group status. As PASS is a household

survey, i.e. potentially interviewing all household members, we restrict the sample to household heads. Table A.4 presents descriptive statistics of the resulting data.

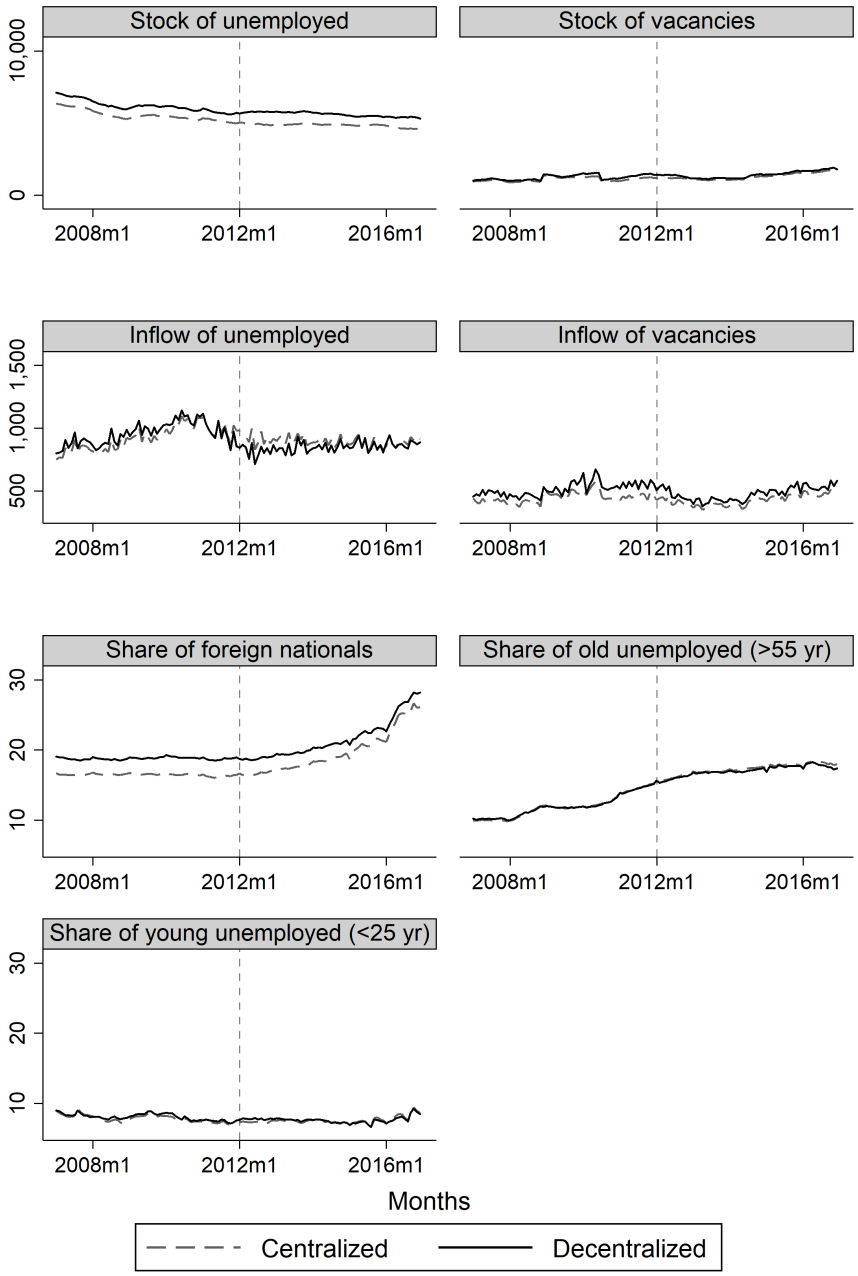
Variable	Mean	SD	Min.	Max.
Decentralized	0.047	0.212	0	1
Age (in years)	44.532	11.502	17	64
Female	0.487	0.499	0	1
Foreign nationality	0.097	0.297	0	1
High school degree	0.196	0.397	0	1
Professional degree				
None	0.309	0.462	0	1
Vocational degree	0.589	0.491	0	1
University degree	0.098	0.298	0	1
Sanctions (per year)				
Yearly sanctions received	0.077	0.293	0	4
Sanctions' average duration, in days[a]	116.694	83.371	1	360
Log sanctions' avg. duration, in days[a]	4.519	0.749	1	5.887
Job center contacts (per year)				
No. of personal contacts[b]	9.457	10.983	0	50
No. of detailed consultations[c]	5.356	6.767	1	50

Notes.– Yearly individual-level data. $N = 16,157$.
a. Only available if at least one sanction has been imposed. $N = 900$.
b. Item not included in 2013. $N = 13,216$.
c. Item not included in 2013. Only available if person had any contact with job center. $N = 8,072$.
Source.– PASS-ADIAB 7515. Sample period: 2007–2016 (Survey waves 1–10).

Table A.4. Descriptive statistics of individual-level variables from PASS

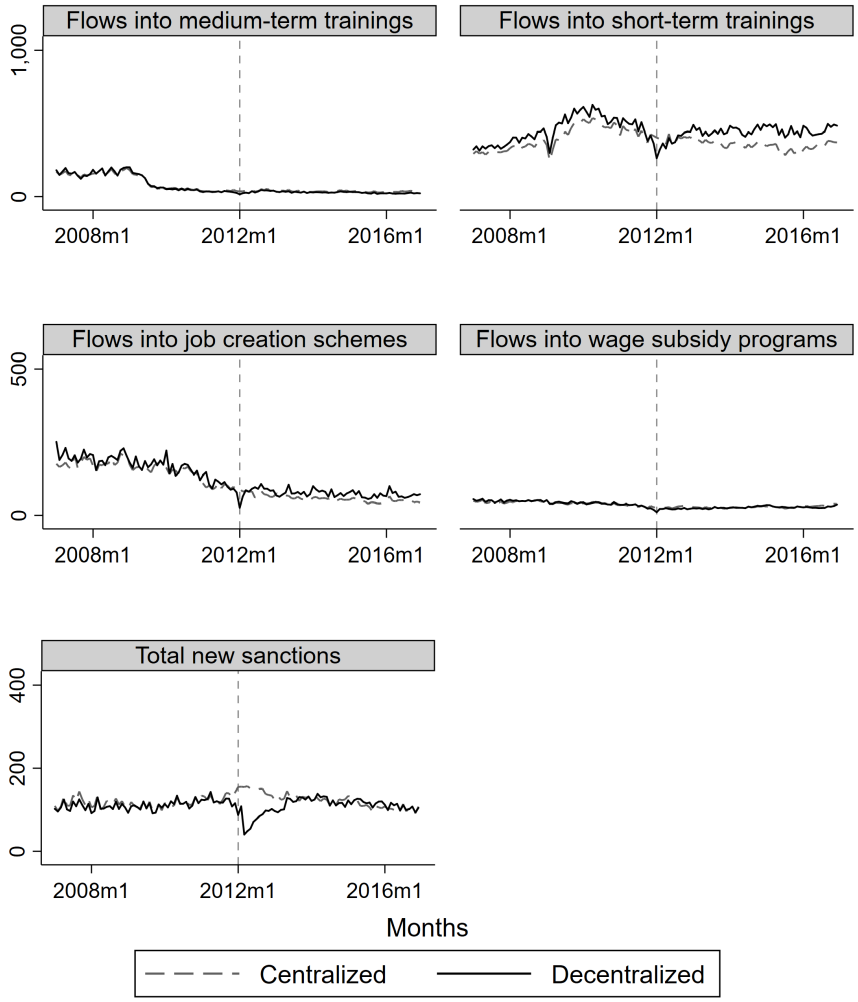
Appendix B. Further Descriptive Results

B.1. **Further Common Trend Graphs.**



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.
Source.– OAD data. Sample period 2007–2016.

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



Notes.– The upper four panels depict monthly inflows into almp measures for districts in our sample. The lower panel depicts the total inflow of new sanctions per month.
Source.– OAD data. Sample period 2007–2016.

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

B.2. **Job Finding out of ALMP Programs.** A drawback of our OAD data is its distinction between 'unemployed job seekers' and 'ALMP participants'. Thus, ALMP participants are excluded in the stock of unemployed, and direct ALMP-to-job transitions do not count as job finding. We estimated a negative effect of decentralization on job finding (unemployment-to-employment flows, see Section 4), but a positive effect on unemployment outflows into job creation schemes (see Section 5.3). We thus may underestimate the true job finding rate of decentralized job centers – combined outflows from unemployment and ALMP into jobs – and, therefore, overestimate the negative effect of decentralization on job finding.

In Section 4 we address this issue using aggregated data from SIAB. In this appendix, we provide additional descriptive analyses of the trajectories of persons entering ALMP programs. Since SIAB does not directly report ALMP participation for job seekers at decentralized job centers, the analysis is restricted to job seekers at centralized job centers. Our focus is on participants in job creation schemes. This is the only program type whose usage intensity systematically differs between centralized and decentralized providers.

Table B.1 shows the distributions of labor market states observed after the end of an ALMP spell by ALMP type. We distinguish employment, unemployment, assignment to another ALMP program, and gaps. The category 'Gap' is assigned if the former ALMP participant was not observed in the SIAB by the beginning of the next month after completing the program. Censored observations are not included. From all job seekers, 32% found a job out of registered unemployment. In contrast, only 10% of job seekers enrolled in job creation schemes found a job. A much larger share, 65%, returned to unemployment after program completion. 4% were immediately assigned to another ALMP program. Similar results can be observed for the other ALMP types. Although their job-finding rates are somewhat higher, they do not reach or even exceed the job-finding rate out of registered unemployment. Altogether, these figures imply that omitting ALMP participants from the pool of job seekers should increase rather than decrease the aggregate job-finding rate in our OAD data. We conclude that we do not overestimate the negative effect of decentralization on job finding, especially in view of our additional results on ALMP inflows.

	Direct transitions into			
ALMP-Type	Empl.	Unempl.	ALMP	Gap
Registered unemployment	32	0	33	24
Wage subsidy	27	17	4	52
Short-term training	17	68	3	12
Long-term training	14	71	2	20
Job creation schemes	10	65	4	33
Other	10	16	47	27

Note.– Only job seekers registered at centralized job centers.
Source.– SIAB 7514. Sample period 2007–2014.

Table B.1. Destinations after transitions out of ALMP programs (in %)

B.3. **Composition of Inflows into Unemployment.**

Variable	Pre-Treatment (2007–2011)		Post-Treatment (2012–2014)		Difference in Differences
	Treatment group	Control group	Treatment group	Control group	
Age (in years)	36.777	36.537	37.480	37.056	0.127
Female (D)	0.426	0.424	0.438	0.428	0.012
Foreigner (D)	0.209	0.196	0.210	0.230	−0.037***
High School degree (D)	0.160	0.182	0.181	0.217	−0.013*
Qualification					
None	0.281	0.286	0.318	0.321	−0.006
Vocational deg.	0.655	0.636	0.620	0.583	0.024***
University deg.	0.063	0.078	0.061	0.096	−0.017***
Occupational group					
Agriculture	0.036	0.032	0.028	0.029	−0.006
Production	0.276	0.255	0.232	0.232	−0.018
Consumer Serv.	0.249	0.248	0.266	0.264	0.003
Business Serv.	0.336	0.345	0.353	0.350	0.006
Scientific Serv.	0.085	0.100	0.099	0.106	0.010*
Other	0.017	0.020	0.022	0.019	0.006**
Prior daily wage	30.327	30.080	31.137	30.532	0.351
Days in empl., τ-1	128.917	132.174	140.342	137.046	6.065**
Days in empl., τ-2	119.476	118.695	135.766	130.394	5.742*
Days in empl., τ-3	125.016	123.125	133.157	126.875	4.947
Days in empl., τ-4	124.383	122.241	126.331	121.963	1.946
Days in empl., τ-5	123.666	122.274	123.903	118.875	4.231
Districts	294	40	294	40	334
Observations	10,770	73,804	4,522	32,579	121,675

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Statistics for broadest unemployment definition: Unemployment includes registered unemployment, subsequent ALMP participation and observational gaps. Treatment group: Job seekers registered at job centers which were decentralized in 2012. Control group: Job seekers registered at job centers that remained centralized. *D* indicates dummy variables. Difference-in-Differences regressions include a full set of dummies for districts (job centers) and months. τ represents the year in which UE was registered at the job center. Standard errors are clustered at the job center and the month level.

Source.– SIAB 7514. Inflows into job center unemployment 2007–06/2014. $N = 121,675$.

Table B.2. Difference-in-differences: Effect of decentralization on composition of unemployment inflows

Appendix C. Further Analysis of Main Effects and Policy Adjustments

C.1. **Individual-level Estimates of the Job-Finding Effect.**

Variable	Raw sample		Matched sample	
	(1) Log unemployment duration	(2) P(Exit to Job)	(1) Log unemployment duration	(2) P(Exit to Job)
Panel A: Official Unemployment				
Decentralized	0.147 ***	−0.014	0.138 ***	−0.014
	(0.039)	(0.011)	(0.035)	(0.011)
Outcome Mean	4.691	0.553	4.718	0.553
R-squared	0.072	0.050	0.076	0.053
Districts	344	344	344	344
Observations	124,706	124,706	124,706	124,706
Panel B: Official Unemployment + ALMP Participation				
Decentralized	0.236 ***	0.006	0.232 ***	0.008
	(0.031)	(0.012)	(0.030)	(0.012)
Outcome Mean	4.980	0.472	5.023	0.472
R-squared	0.081	0.052	0.085	0.053
Districts	344	344	344	344
Observations	123,588	123,588	123,588	123,588
Panel C: Official Unemployment + ALMP Participation + Observational Gaps				
Decentralized	0.090 ***	−0.032 ***	0.087 ***	−0.030 ***
	(0.029)	(0.009)	(0.029)	(0.009)
Outcome Mean	5.364	0.724	5.364	0.715
R-squared	0.093	0.084	0.094	0.087
Districts	344	344	344	344
Observations	121,675	121,675	121,675	121,675

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 20071–2014.

Table C.1. Difference-in-differences: Average effect of decentralization on individual unemployment duration and job finding

C.2. **Placement Quality.**

Variable	(1) P(Regular full-time job)	(2) P(Empl. lasts > 6 months)	(3) Log wage change (in Euro)
Panel A: Official Unemployment			
Decentralized	0.020	−0.006	0.035
	(0.015)	(0.013)	(0.024)
Outcome Mean	0.475	0.510	0.179
R-squared	0.249	0.046	0.351
Districts	344	344	344
Observations	55,248	55,248	55,248
Panel B: Official Unemployment + ALMP Participation			
Decentralized	0.006	−0.017	0.026
	(0.014)	(0.010)	(0.019)
Outcome Mean	0.492	0.509	0.164
R-squared	0.227	0.043	0.369
Districts	344	344	344
Observations	68,430	68,430	68,430
Panel C: Official Unemployment + ALMP Participation + Observational Gaps			
Decentralized	−0.003	−0.005	0.029 *
	(0.013)	(0.011)	(0.015)
Outcome Mean	0.469	0.494	0.126
R-squared	0.210	0.039	0.377
Districts	344	344	344
Observations	88,148	88,148	88,148

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table C.2. Difference-in-differences: Average effect of decentralization on job characteristics after unemployment

C.3. **Geographical Lock-in of Job Seekers.**

In Section 5.2 we examine whether decentralization induces geographical lock-in of job seekers, i. e. whether decentralization increased the probability of job seekers to be placed more often in jobs of their home district. In the paper, we used our SIAB sample and did not find an effect at the individual level. Table C.3 shows that this result is independent of the definition of unemployment.

Sample	(1) Official Unemployment	(2) Official Unemployment + ALMP	(3) Official Unemployment + ALMP + Gaps
Variable			
Decentralized	0.006	0.002	0.003
	(0.015)	(0.011)	(0.012)
Outcome Mean	0.639	0.636	0.631
R-squared	0.095	0.090	0.084
Districts	344	344	344
Observations	55,248	68,430	88,148

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The column presents an estimation of equation (2). The outcome variable is an indicator whether the new job after unemployment is located in the job seeker's district of residence. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level. Different observation numbers stem from the different definitions of unemployment.
Source.– SIAB 7514. Sample period 2007–2014.

Table C.3. Difference-in-differences: Average effect of decentralization on location of new jobs

In the following, we complement these analyses using our OAD data. Adopting a more indirect approach, we investigate whether the elasticity of job finding with respect to vacancies from surrounding districts has decreased after decentralization. Therefore, we extend our aggregate model (1) by adding spatial lags of all variables as well as interaction terms of the spatial lags with the decentralization dummy as covariates. This gives rise to a spatial cross-regressive model (Halleck Vega and Elhorst, 2015). Hence, we estimate models of the form

$$M_{it} = \delta\, D_{it} + Q_{it}\beta + Q_{-it}\gamma + D_{-it}\eta + D_{it}Q_{-it}\theta + \alpha_i + \mu_t + \varepsilon_{it} \tag{4}$$

where D_{it} is the decentralization indicator, Q_{it} is a vector collecting the stock and flow variables for unemployed as well as vacancies in district i at month t, and Q_{-it} is the corresponding vector of spatially lagged covariates of all other districts. All remaining variables are defined as before. Q_{-it} is obtained by multiplying the vector of covariates with a spatial weight matrix W that is based on row-normalized inverse distances. Each element of Q_{-it} provides a weighted sum of unemployed or vacancies, respectively, in surrounding districts, giving lower weight to unemployed and vacancies in greater distance. The coefficient vector γ informs about the importance of these unemployed and vacancies in surrounding districts for job placements by the job center in district i. To examine whether decentralization has affected the elasticity with respect to vacancies in surrounding districts, we interact the spatially lagged covariates Q_{-it} with our decentralization indicator D_{it}. Our coefficient vector of interest, therefore, becomes θ, in particular its elements with respect to the spatially lagged vacancies. To provide a meaningful interpretation of the 'raw' decentralization coefficient δ in the face of interaction terms, we center all continuous independent variables around their mean and standardize them by their standard deviation.

Table C.4 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 main effect of decentralization is very similar to previous estimates (row 1). Job finding increases with additional vacancy inflows in surrounding districts but not with vacancy stocks (rows 2 and 3). None of the models indicates a statistically significant decrease of the job finding elasticity with respect to the neighboring stock or inflow of vacancies after decentralization (rows 4 and 5). 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.

Variable	(1) Baseline	(2) Spatial lags I	(3) Spatial lags II	(4) Spatial lags full
Decentralized (D_{it})	−0.096 ***	−0.107 ***	−0.103 ***	−0.097 ***
	(0.021)	(0.021)	(0.024)	(0.022)
Spatially lagged vacancy inflow		0.074 ***	0.074 ***	0.077 ***
($\tilde{V}_{-it}$)		(0.024)	(0.024)	(0.024)
Spatially lagged vacancy stock		−0.025	−0.025	−0.023
(V_{-it})		(0.031)	(0.031)	(0.030)
$D_{it} \times \tilde{V}_{-it}$		−0.000		−0.014
		(0.013)		(0.017)
$D_{it} \times V_{-it}$			−0.005	−0.004
			(0.015)	(0.020)
Outcome Mean	4.673	4.673	4.673	4.673
R-squared	0.958	0.959	0.959	0.959
Districts	334	334	334	334
Observations	39,018	39,018	39,018	39,018

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a separate estimation of equation 4. The outcome variable is the monthly log outflow out of unemployment into jobs. *Decentralized* (D) is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the stocks and inflows of unemployed, vacancies, spatially lagged unemploymed and spatially lagged vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016.

Table C.4. Difference-in-differences: Effect of decentralization on monthly district-level log flows into jobs accounting for vacancies in surrounding job centers

C.4. **ALMP Policies and Elections.**

Decentralized job centers may expand job creation schemes strategically ahead of local elections to reduce official unemployment figures and ease the provision of local public services. This argument should particularly apply to elections at the district-level which also has the political responsibility for decentralized job center. We thus hand-collected all dates of communal elections within our sample period from the websites of the state's election administrations. In addition, we collect the dates of all state-level elections. We do so to compare the communal election results with an election setting where local governments themselves do not seek reelection and may only play a marginal role. Communal election dates vary by state and are typically held every 6 years. The terms of office for state-governments is five years. Hence, we usually observe two communal and state elections for each district, one before and one after the decentralization.

We augment the main model from equation (1) with an additional election variable. We implement the election variable as a dummy equaling 1 in the month before an

election and 0 otherwise. We add an interaction term of decentralization and the election to check whether decentralized job centers may use ALMPs more strategically ahead of elections than centralized providers. Standard errors are now clustered by state and year. We thereby allow for error correlations across districts within states and across months within years.

Table C.5 presents the results. The first column repeats the specification from Table 8 but applies standard error clustering at a higher level, as described above. The results documents that the higher level clustering does not affect our basic finding, a significant increase in job creation schemes due to decentralization. Column 2 then presents a specification augmented with the communal election variables. A forthcoming communal election per se is not associated with expanded job creation schemes. However, decentralization increases flows into these programs by about 15% ahead of a communal election and in addition to a robust main effect of decentralization. Column 3 then shows that job creation schemes remain unchanged in the month ahead of a state election where district policies should be of lesser importance. In sum, these results are suggestive evidence that decentralization changes the dynamics not only of local policies but also of local politics.

Variable	(1) W/o elections	(2) Communal elections	(3) State elections
Decentralized	0.302 ***	0.300 ***	0.304 ***
	(0.079)	(0.080)	(0.079)
Pre-election		−0.011	−0.026
		(0.060)	(0.052)
Decentralized ×Pre-election		0.148 ***	−0.096
		(0.043)	(0.091)
Outcome Mean	3.668	3.668	3.668
R-squared	0.822	0.822	0.822
Observations	35,925	35,925	35,925

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 job creation schemes. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Election variable is a dummy equaling 1 in the month before an election 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 parentheses are clustered at the state and year level.
Source.– OAD data. Sample period 2007–2016.

Table C.5. Job Creation Schemes Ahead of Elections Under Decentralization

Online Appendix

Appendix S. Further Sensitivity Analyses

S.1. **Time trends, fixed effects and alternative sample periods (OAD).** For our analysis at the aggregate level, specifications in Table S.1 add linear time trends to the baseline model specified in equation 1 and specifications in Table S.2 add region by year fixed effects. Table S.3 varies the sample period to ensure our results are not driven by the transition period around the reform's implementation.

	(1) East trend	(2) State trends	(3) District trends
Decentralized	-0.097***	-0.112***	-0.093***
	(0.021)	(0.019)	(0.033)
Outcome Mean	4.673	4.673	4.673
R-squared	0.999	0.999	0.999
Districts	334	334	334
Observations	39,018	39,018	39,018

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. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016.

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

	(1) Year by State FEs	(2) Year by Comm. zone FEs	(3) Year by UI region FEs
Decentralized	-0.114***	-0.115***	-0.138***
	(0.019)	(0.019)	(0.023)
Outcome mean	4.673	4.673	4.673
R-squared	0.999	0.999	0.999
Districts	334	334	334
Observations	39,018	39,018	39,018

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. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months in addition to a full set of region by year fixed effects. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016.

Table S.2. Difference-in-differences: Adding region by year fixed effects to the baseline model

	(1) W/o 2011	(2) W/o 2012	(3) W/o 2011 & 2012
Decentralized	-0.099***	-0.085***	-0.088***
	(0.021)	(0.021)	(0.022)
Outcome Mean	4.675	4.652	4.653
R-squared	0.958	0.959	0.958
Districts	334	334	334
Observations	35,010	36,043	32,035

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. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016 with gaps as specified in column titles.

Table S.3. Difference-in-differences: Alternative sample periods

S.2. **Conditional Difference-in-Differences.** If observable characteristics influence the unobserved labor market trends of centralized and decentralized job centers, reweighing our observations in the OAD data 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 in the OAD data. Table S.4 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. The result of the decentralization effect based on this balancing is included in Table 11.

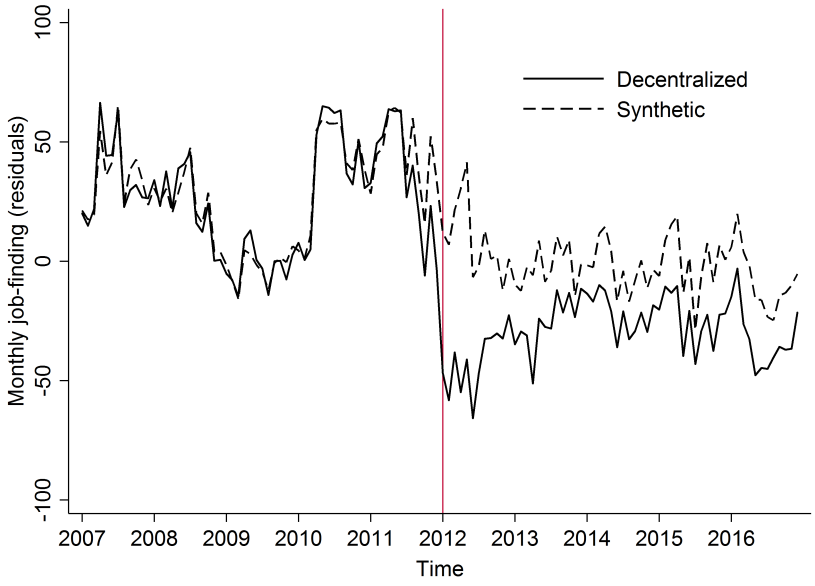
	Treated	Unbalanced Control		Balanced Control	
Variable	Mean	Mean	P-Value	Mean	P-Value
GDP per capita	22.970	24.099	0.578	22.970	1.000
Civil labor force	−4.053	−3.337	0.517	−4.052	1.000
Young (15–24 yr)	−2.231	0.245	0.382	−2.231	1.000
Prime-aged (25–54 yr)	−4.611	−4.361	0.808	−4.610	0.999
Old (55–64 yr)	−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
Districts	40	290		290	

Notes.– 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. Sample period 2000–2010 (GDP, population, employment) or 2007–2010 (unemployment, welfare).

Table S.4. Conditional difference-in-differences: Mean growth rates before and after entropy balancing)

S.3. **Synthetic Control.** We implement the synthetic control method by constructing a synthetic counterfactual as a linear combination of the control group districts for each treated district. 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.



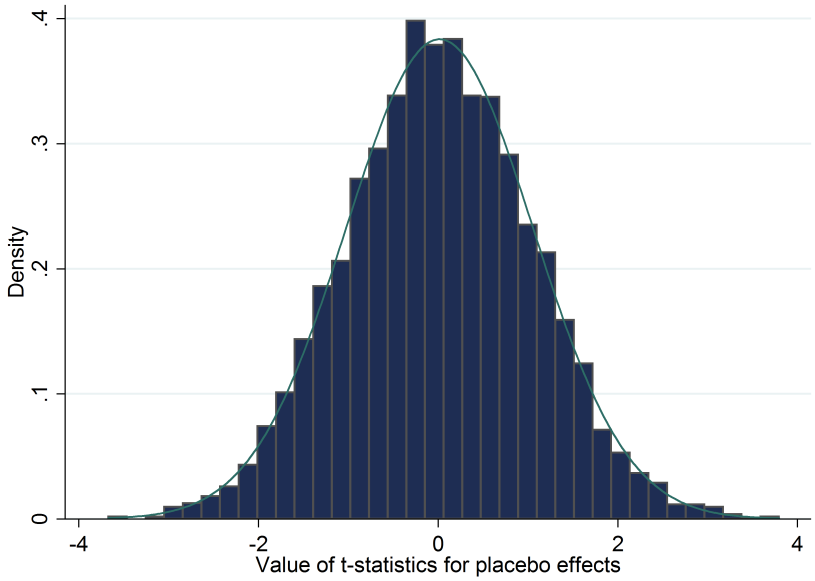
Notes.– Time-labels (x-axis) refer to January of a given year. Synthetic control approach with seasonally adjusted job finding as the outcome variable, i.e residuals from a regression of monthly job finding levels per district on eleven month dummies and an intercept. Donor pools for synthetic control units include all districts not decentralizing in 2012. Predictor variables include all covariates from the baseline regression as well as the shares of young, old, 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.
Source.– OAD data. Sample period 2007–2016.

Figure S.1. Synthetic control approach

Figure S.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. With 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.

S.4. **Empirical Monte-Carlo simulation.** Figure S.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.



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 inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– OAD data. Sample period 2007–2016.

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

S.5. **Sensitivity Analyses for Inflows into ALMPs.** (subsequent pages)

Variable	(1) Base specification	(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 commuting zone level
Decentralized	0.033	−0.037	0.089 **	0.037	0.016	0.026	0.013	−0.021
	(0.064)	(0.073)	(0.039)	(0.065)	(0.055)	(0.065)	(0.063)	(0.067)
R-squared	0.939	0.910	0.944	0.931	0.921	0.932	0.940	0.936
Districts	316	75	276	292	312	206	316	316
Observations	36,972	8,775	32,292	34,164	36,504	24,102	36,972	36,972

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. The outcome mean for the main sample is 4.673.
Source.– OAD data. Sample period 2007–2016.

Table S.5. Difference-in-differences: Effect of decentralization on log monthly inflows into all ALMP measures under different specifications

Variable	(1) Base specification	(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 commuting zone level
Decentralized	0.302 ***	0.095	0.206 **	0.306 ***	0.249 ***	0.286 ***	0.292 ***	0.248 ***
	(0.081)	(0.115)	(0.094)	(0.083)	(0.090)	(0.084)	(0.082)	(0.082)
R-squared	0.822	0.797	0.822	0.811	0.815	0.821	0.822	0.818
Districts	319	75	279	294	315	208	319	319
Observations	35,925	8,570	31,367	33,004	35,457	23,536	35,925	35,925

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. The outcome mean for the main sample is 3.668.
Source.– OAD data. Sample period 2007–2016.

Table S.6. Difference-in-differences: Effect of decentralization on log monthly inflows into job creation schemes under different specifications

S.6. **Dynamic Analysis using Individual-Level Data.**

Unemployment:	(1) Official Unemployment	(2) Official Unemployment + ALMP	(3) Official Unemployment + ALMP + Gaps
δ_{2007}	0.048	0.054	0.028
	(0.052)	(0.052)	(0.040)
δ_{2008}	−0.026	0.026	0.035
	(0.044)	(0.041)	(0.034)
δ_{2009}	0.023	0.028	−0.003
	(0.059)	(0.056)	(0.050)
δ_{2010}	0.041	0.058	0.044
	(0.042)	(0.043)	(0.039)
δ_{2012}	−0.131 ***	−0.112 **	−0.137 ***
	(0.046)	(0.045)	(0.043)
δ_{2013}	−0.068	−0.088 *	−0.064
	(0.048)	(0.048)	(0.043)
δ_{2014}	−0.055	−0.040	−0.056
	(0.045)	(0.047)	(0.038)
Outcome Mean	1.113	1.263	1.477
R-squared	0.674	0.705	0.727
Districts	334	334	334
Observations	25,257	26,032	24,866

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation 3. 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. The baseline year is 2011. Regressions include the stocks and inflows of unemployed and vacancies as well as a full set of dummies for job centers and months. All continuous variables in logs. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table S.7. Difference-in-differences: Dynamic effect of decentralization on monthly log flows into jobs for different definitions of unemployment by year

Variable	(1) Log unemployment duration	(2) P(Exit to Job)
δ_{2007}	−0.015	0.003
	(0.044)	(0.012)
δ_{2008}	−0.022	0.003
	(0.053)	(0.012)
δ_{2009}	−0.011	−0.004
	(0.044)	(0.015)
δ_{2010}	−0.071	−0.010
	(0.044)	(0.009)
δ_{2012}	0.107 **	0.039 ***
	(0.053)	(0.010)
δ_{2013}	0.053	−0.030 **
	(0.052)	(0.012)
δ_{2014}	−0.003	−0.030
	(0.057)	(0.025)
Outcome Mean	5.364	0.724
R-squared	0.093	0.084
Districts	344	344
Observations	121,675	121,675

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation for the individual-level equivalent of equation (3). Unemployment includes registered unemployment, ALMP participation and observational gaps. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. The baseline year is 2011. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table S.8. Difference-in-differences: Dynamic effect of decentralization on individual unemployment duration and job finding

Variable	(1) P(Regular full-time job)	(2) P(Empl. lasts > 6 months)	(3) Log wage change (in Euro)	(4) Job in district of residence
δ_{2007}	−0.026	−0.023	−0.051	0.012
	(0.021)	(0.017)	(0.035)	(0.020)
δ_{2008}	−0.010	−0.040 **	−0.037	0.012
	(0.021)	(0.015)	(0.032)	(0.020)
δ_{2009}	−0.029	−0.029 *	−0.040	0.025
	(0.020)	(0.017)	(0.036)	(0.023)
δ_{2010}	−0.008	−0.033 *	−0.033	0.010
	(0.019)	(0.017)	(0.034)	(0.019)
δ_{2012}	−0.020	−0.033 *	0.001	0.024
	(0.022)	(0.017)	(0.028)	(0.021)
δ_{2013}	−0.018	−0.025	−0.006	0.015
	(0.024)	(0.023)	(0.042)	(0.021)
δ_{2014}	−0.008	−0.037	−0.019	−0.009
	(0.027)	(0.027)	(0.044)	(0.039)
Outcome Mean	0.469	0.494	0.126	0.631
R-squared	0.211	0.039	0.377	0.084
Districts	344	344	344	344
Observations	88,148	88,148	88,148	88,148

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation for the individual-level equivalent of equation (3). Unemployment includes registered unemployment, ALMP participation and observational gaps. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. The baseline year is 2011. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.

Source.– SIAB 7514. Sample period 2007–2014.

Table S.9. Difference-in-differences: Dynamic effect of decentralization on job characteristics after unemployment

S.7. **Further Analyses based on SIAB Data.** For our analysis at the individual level, Table S.10 presents the results for our baseline estimations when excluding unemployment spells with registration date in 2011. We thereby may be able to observe whether partially treated spells included in the baseline estimations attenuate estimated effects.

Variable	(1) Log unemployment duration	(2) P(Exit to Job)	(3) P(Regular full-time job)	(4) P(Empl. lasts > 6 months)	(4) Log wage change (in Euro)
Decentralized	0.096 ***	−0.032 ***	0.000	0.000	0.037 **
	(0.028)	(0.009)	(0.014)	(0.011)	(0.017)
Outcome Mean	5.364	0.724	5.364	0.494	0.715
R-squared	0.096	0.090	0.209	0.041	0.379
Districts	344	344	344	344	344
Observations	105,595	105,595	76,110	76,110	76,110

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. Employment duration is censored at 450 days. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014, excluding unemployment spells starting in 2011.

Table S.10. Difference-in-differences: Average effect of decentralization when excluding unemployment spells starting in 2011

We also study whether the main result at the individual level is sensitive to the right-censoring of unemployment spells in 2014, the end of the SIAB sample period. First, we restrict the sample to unemployment inflows occurring until June 2013. For these unemployment spells, right-censoring should be less binding. Second, we right-censor all unemployment spells at 365 days in unemployment. We thereby measure a more short-run outcome that should also be less affected by right-censoring of the sample.

Variable	Exclude UE spells starting later than 06/2013		Right-censor UE spells at 365 days	
	(1) Log unemployment duration	(2) P(Exit to Job)	(1) Log unemployment duration	(2) P(Exit to Job)
Decentralized	0.115 ***	−0.035 ***	0.081 ***	−0.043 ***
	(0.031)	(0.009)	(0.019)	(0.012)
Outcome Mean	5.415	0.752	5.083	0.522
R-squared	0.086	0.056	0.063	0.059
Districts	344	344	344	344
Observations	107,004	107,004	121,675	121,675

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. Employment duration is censored at 450 days in the first block and at 365 days in the second. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.
Source.– SIAB 7514. Sample period 2007–2014.

Table S.11. Difference-in-differences: Average effect of decentralization on individual unemployment duration and job finding for alternative outcome and sample definitions

Variable	(1) P(Regular full-time job)	(3) P(Empl. lasts > 6 months)	(3) Log wage change (in Euro)
Decentralized	−0.009	−0.003	0.032 **
	(0.016)	(0.011)	(0.016)
Outcome Mean	0.472	0.506	0.119
R-squared	0.209	0.029	0.378
Districts	344	344	344
Observations	80,537	80,537	80,537

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. Employment duration is censored at 450 days. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.

Source.– SIAB 7514. Sample period 2007–2014, where new unemployment inflows are only considered until June 2013.

Table S.12. Difference-in-differences: Average effect of decentralization on job characteristics after unemployment, sample restricted to UE inflows until June 2013

Variable	(1) P(Regular full-time job)	(3) P(Empl. lasts > 6 months)	(3) Log wage change (in Euro)
Decentralized	0.001	−0.004	0.037 *
	(0.017)	(0.014)	(0.022)
Outcome Mean	0.504	0.502	0.168
R-squared	0.236	0.049	0.366
Districts	344	344	344
Observations	63,624	63,624	63,624

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). Unemployment includes registered unemployment, ALMP participation and observational gaps. Employment duration is censored at 365 days. *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, the log previous daily wage, the number of days in employment for each of the five years prior to unemployment, dummies for gender, foreign nationality and high school degree, as well as a full set of dummies for professional degrees, occupational groups, job centers and months. Standard errors given in parentheses are clustered at the job center and the month level.

Source.– SIAB 7514. Sample period 2007–2014.

Table S.13. Difference-in-differences: Average effect of decentralization on job characteristics after unemployment, unemployment spells censored at 365 days

S.8. **Further Analyses based on PASS Data.**

We repeat the analysis of our main job finding outcomes using PASS to demonstrate that the PASS sample size is generally sufficient to replicate the main effect of decentralization. We therefore create a specification that mimicks the SIAB definitions as far as possible.

Variable	(1) Log unemployment duration	(2) P(Exit to Job)
Decentralized	0.233 **	0.057
	(0.107)	(0.041)
Outcome Mean	5.653	0.639
R-squared	0.188	0.147
Observations	4,076	4,076

Notes.– * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Each column presents a different estimation of equation (2). *Decentralized* is a dummy equaling 1 for districts with decentralized job centers and 0 otherwise. Regressions include the age, age squared, dummies for gender and foreign nationality, duration of previous employment, as well as a full set of dummies for school degree, professional degrees, job centers and months. Standard errors given in parentheses are clustered at the job center and the survey month level.

Source.– PASS-ADIAB 7515. Sample period: 2007–2016 (Survey waves 1–10).

Table S.14. Difference-in-differences: Average effect of decentralization on individual unemployment duration and job finding