

The short-run and long-run effects of
decentralizing public employment services

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The short-run and long-run effects of decentralizing public employment services*

Abstract

A German policy experiment from 2012 allows me to identify the short-run and long-run causal effects of decentralization on the placement efficiency of public employment services (PES). I exploit variation over time and across districts with different types of PES in a difference-in-differences framework. Decentralization had significant negative effects on placements in the short run, but not in the long run. This points to a transition process, during which caseworkers acquire relevant placement skills. Decentralization also had a persistent negative effect on sanctions on the welfare recipients, which probably indicates a reluctance of local authorities to reduce the well-being of their potential voters.

JEL Code: C21, E24, J48.

Keywords: Public employment services, job placement, matching efficiency, difference-in-differences.

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1. Introduction

Equilibrium unemployment is determined, among others, by the degree of coordination frictions on the labor market (Petrongolo and Pissarides, 2001). In order to reduce these coordination frictions, and thereby unemployment, countries all over the world maintain public employment services (PES). These services provide registered job seekers with information about job offers and registered vacancies with information about potential applicants. When operating efficiently, PES reduce search costs of both, job seekers and firms, thereby increase the labor market's matching efficiency and positively affect welfare (van Ours, 1994; Yavas, 1994; Fougère *et al.*, 2009).¹ Additionally, PES administer benefit payments, monitor the job seekers' search efforts, and assign job seekers into training measures or jobs. Increasing the efficiency of PES may have even larger effects on unemployment than reducing unemployment benefits (Launov and Wälde, 2015). The European Commission (2010, 2015) therefore assigns PES a central role in its 'Europe 2020 Integrated Guidelines'. It recommends its member states to adopt policies that improve labor market matching and to implement performance measurement systems for their PES.

One major determinant of the efficiency of an organization like the PES is its degree of centralization (see e. g. Prud'homme, 1995; Finn, 2000; Hutchcroft, 2001; Richardson *et al.*, 2002; Besley and Coate, 2003). With regard to PES, both the centralized and the decentralized setting have their respective advantages. In a centralized setting, adopted for example in the UK or in Austria (see Weishaupt, 2011), local PES offices are subject to directives by a central institution. This allows for a more efficient controlling, and it eases the implementation of common standards and new, potentially improved, practices. In a more decentralized setting, implemented for example in Switzerland (see Lalive *et al.*, 2005) or Denmark (see Weishaupt, 2011), local PES offices are more

¹PES are particularly efficient for job seekers with little access to alternative search channels and with poor labor market prospects; but they are not necessarily efficient for workers with higher productivity (see, for instance, Pissarides, 1979; Gregg and Wadsworth, 1996; Boone and van Ours, 2009; Fougère *et al.*, 2009; Cueto *et al.*, 2014; Holzner and Watanabe, 2015).

flexible. They may develop independent strategies by their own to react to the specific conditions of their local labor market and they are usually more involved in local networks.

In this study, I examine whether the efficiency of PES can be enhanced by decentralization. I make use of a policy experiment in Germany, which allows to study centralized and decentralized PES within a common institutional and economic framework. I thus overcome the problem that the degree of centralization of PES usually differs between countries, but not within. Cross-country comparisons, however, reveal little on the effects of decentralization, as any cross-country difference in the aggregate job finding rate does not only reflect the efficiency of the respective PES, but also differences in other labor market institutions like the tax and transfer system, the employment protection legislation, and minimum wages.

The German policy experiment concerns the so-called job centers, local PES at the district level which take care of unemployed and welfare recipients in terms of job placement, counseling, and welfare administration. With regard to job placement, the majority of these job centers is bound to guidelines and directives of the Federal Employment Agency (centralized setting). In 2012, job centers in 41 out of then 402 German districts were decentralized and thus became independent of the Federal Employment Agency. They could then define their own placement strategy, adjust the caseload of their caseworkers, and choose the rigor of their sanction policy. Indeed, decentralized job centers tend to organize themselves differently from centralized job centers (see Deutscher Bundestag, 2008; Boockmann *et al.*, 2015).

The 2012 decentralization provides a natural experiment that allows to identify the causal effect of decentralization on the job centers' placement efficiency. As job centers may have self-selected into decentralization, I apply the difference-in-differences (DD) estimator. I make use of the fact that centralized and decentralized job centers face on average the same fundamental labor market trends and the same labor market institutions. The decentralization of job centers in 2012 also did not coincide with any

other reform regarding the German labor market.

I assess the effect of decentralization on the job centers' placement efficiencies at the macro level. I therefore approximate the coordination process on the labor market by estimating the well-known aggregate stock-flow matching function at the district level, which I augment for the structure of the unemployed and for spatial spillovers. I estimate the causal effect of decentralization on a monthly basis to investigate the effect's evolution over time. This allows me to assess the short-run and long-run effects of decentralization for up to three years after the reform. Furthermore, I elaborate on potential channels explaining any efficiency gains or losses after decentralization.

My results suggest that the decentralization did not improve the job centers' placement efficiency. In contrast, the placement efficiency dropped by 30% immediately after the decentralization of the respective PES. In the following months, the placement efficiency of decentralized job centers increased again such that the efficiency loss was not statistically significant anymore about two years after the reform. When I repeat this analysis for alternative outcomes, the outflow out of welfare and the sustainable outflow out of welfare, I obtain smaller but more permanent negative treatment effects of decentralization. Further inspection of this issue reveals that the permanent effects come along with a permanent effect of decentralization on the sanction rate of the welfare recipients. While sanctions are known to increase the job finding rate (see, for example, Lalive *et al.*, 2005; Arni *et al.*, 2013; van den Berg and van der Klaauw, 2015), the joint negative effect of decentralization on welfare outflows and sanctions has, to the best of my knowledge, not yet been observed.

I conclude that decentralization negatively affects the placement efficiency with regard to unemployed in the short run due to a learning-curve effect. In the long run, however, any efficiency differences between centralized and decentralized PES vanish. Apparently, there is little to gain by switching from one system to the other. However, if local authorities have to decide on the PES' strategies, they may adopt a less rigorous sanction policy than centralized PES, although this will negatively affect the outflow out

of unemployment or welfare, respectively. The local authorities may nevertheless favor a laxer sanction scheme because they prefer not to harm their potential voters among the welfare recipients too much. This consideration points to a political economy of sanctioning welfare recipients.

My analysis is related to two branches of literature. First, I add to the young and small literature that assesses the effect of decentralization on the efficiency of *public* employment services.² For instance, a higher share of centrally released directives seems to have a positive impact on the job-finding rate, although estimates for a sample of German PES are in general not statistically significant (see Boockmann *et al.*, 2014). More generally, decentralization appears to have no statistically significant effect on aggregate matching efficiency in the long run (see Holzner and Munz, 2013; Boockmann *et al.*, 2015). My results confirm these findings, but additionally indicate that there are significant negative treatment effects of decentralization in the short run and the medium run. Furthermore, there are probably negative long-run effects of decentralization on the outflow out of welfare.

Second, I contribute to the broader literature on features of PES that affect the aggregate job finding rate. In general, the job finding rate will be higher if PES follow a stricter monitoring and sanction policy (see, for example, Lalive *et al.*, 2005; Arni *et al.*, 2013; van den Berg and van der Klaauw, 2015), if the PES' caseworkers face lower caseloads (see, for instance, Koning, 2009; Hainmueller *et al.*, 2015), and if PES place more emphasis on quick rather than on stable placements, as the search for better matches usually takes more time (see, for example, Caliendo *et al.*, 2013). I provide evidence that the monitoring and sanction intensity is in turn affected by the degree of decentralization of the respective PES. In contrast, my results do not indicate that decentralized PES place more emphasis on stable placements than centralized PES.

²There is a much larger literature that is concerned with decentralization in terms of contracting out placement services to *private* providers. In general, contracting out is shown to increase the job-finding rate, though contracts need to be properly designed in order to prevent creaming and parking (see e. g. Dockery and Stromback, 2001; Heinze *et al.*, 2006; de Koning, 2007; Bernhard and Wolff, 2008; Heyer *et al.*, 2012; Benmarker *et al.*, 2013).

The remainder of the paper is structured as follows. Section 2 provides some important details on the German job centers and on the decentralization process. In section 3, I derive reduced-form equations that relate the decentralization to various outcome indicators and their respective determinants. In section 4, I discuss how I can identify the effect of decentralization using these reduced form equations within the difference-in-difference framework. In section 5, I present details on my aggregate panel data set. Section 6 provides the estimation results on the short-run and long-run effects of decentralization as well as some sensitivity analyses. Section 7 concludes.

2. The German job centers and the 2012 reform

The German job centers are a new type of public employment services that was introduced in 2005 as part of the German Hartz reforms (Jacobi and Kluge, 2007). Usually, there is one job center per district. The job centers are designed as one stop shops that provide the usual services of a PES and some additional services. Among others, they provide their clients with job offers, assign them into active labor market programs (ALMP) and monitor their clients' job search efforts. The job centers may also temporarily reduce the benefits of clients who do not comply with their job seeker duties.³ The rigor of the monitoring and sanction regime is subject to some discretionary power of the job centers. As a more rigorous sanction scheme usually generates larger unemployment outflows (see, for instance, Abbring *et al.*, 2005; Lalive *et al.*, 2005; van der Klaauw and van Ours, 2013), any difference in the placement efficiencies of different job centers may therefore have its root in different sanction strategies.

The job centers' clients are welfare recipients entitled to the welfare scheme *unemployment benefit II*. A worker qualifies for this scheme if her household income and wealth fall below certain thresholds, irrespective whether she is unemployed or employed. For

³Unemployed job seekers entitled to the welfare scheme *unemployment benefit II* are obliged to search actively for a new job, to frequently meet their caseworkers, to participate in assigned active labor market programs, and to accept reasonable job offers provided by the PES. Employed welfare recipients have to frequently meet their caseworkers as well.

instance, short-term unemployed who are entitled to the unemployment insurance benefit usually do not meet this requirement. Thus, the job center clients can roughly be summarized as (long-term) unemployed job seekers and as employed workers with low labor income. These clients have comparably poor labor market prospects and little access to alternative job search channels. For them, welfare-to-work transitions often result from placements by the PES (see, for instance, Gregg and Wadsworth, 1996). The local aggregate job finding rate of the job center clients is therefore tightly linked to the job centers' placement efficiency. Thus, the German job centers provide an ideal opportunity to study the effects of features of PES on placements.

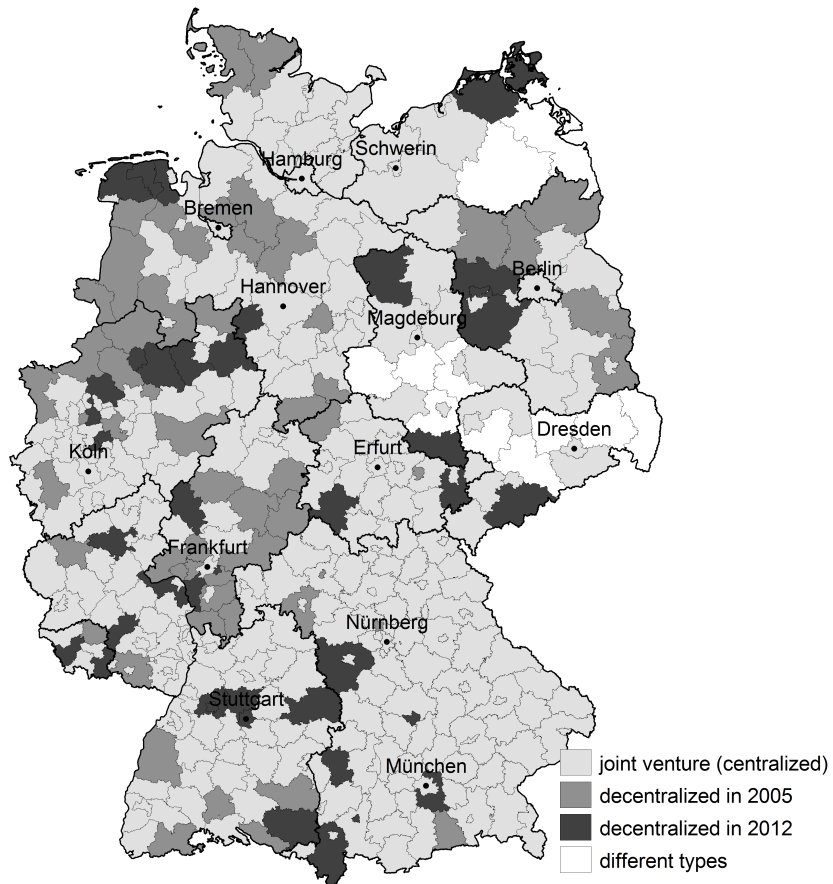
There are two types of job centers which differ in their degree of centralization. The first type is organized as joint ventures (*gemeinsame Einrichtungen*) of the district administration and the respective local employment agency.⁴ Each local employment agency is bound to the Federal Employment Agency by target agreements, directives and technical supervision, so that the provision of employment services within the joint ventures is centrally organized all over Germany. In particular, the caseload, placement and sanction policy in the joint ventures follows quite stringent guidelines, although strategic and organizational decisions regarding the joint venture are made jointly by the district administration and the local employment agency. The district administration itself is mainly in charge for non-PES related tasks like the administration of additional welfare payments and specific counseling, for instance in case of drug addiction.

The second type of job centers is run by the district administration alone (opting-out or *zugelassene kommunale Träger*). The organization of employment services in these job centers is completely independent from the Federal Employment Agency. The districts have just to sign target agreements with their respective state governments, which are their solely supervising institutions. This results in a decentralized organization of PES, though the job centers may coordinate their strategies and adopt best practices

⁴Until 2011, the joint ventures were called *Arbeitsgemeinschaften* or *Träger mit getrennter Aufgabewahrnehmung*.

by choice. A survey conducted in 2007 (see Deutscher Bundestag, 2008; Boockmann *et al.*, 2015) reveals that opting-out job centers and joint ventures adopt indeed quite different structures and strategies. For instance, joint ventures often counsel job seekers depending on their employment prospects while decentralized job centers rather counsel each job seeker with equal intensity. Such structural and strategic differences may cause differences in placement efficiencies between centralized and decentralized job centers.

Figure 1: Spatial distribution of job centers by type at the district level



Geodata: © GeoBasis-DE / BKG 2014.

As of 2012, decentralized job centers were in charge in 108 out of 402 German districts. In 67 districts, job centers were decentralized already in 2005, while in the other 41 districts job centers were decentralized on 1 January 2012. In 11 districts, centralized and decentralized job centers co-exist next to each other because administrative reforms in Mecklenburg-Vorpommern, Saxony and Saxony-Anhalt merged districts but not the corresponding job centers. Figure 1 illustrates the spatial distribution of districts by

their type of job center. The 41 districts whose job centers were decentralized in 2012 will constitute my treatment group and are fairly evenly distributed across Germany. On average, these job centers should therefore face the same fundamental labor market trends as the job centers that remained centralized.

The districts whose job centers were decentralized in 2012 do not constitute a random sample from the population of German districts, but were determined by a two stage selection process. First, districts willing to decentralize their job center had to apply at their respective state government. For the 2012 reform, the application period started effectively on 3 August 2010 and ended on 31 December 2010 (Deutscher Bundestag, 2010).⁵ Second, the state governments chose from all applications those districts that were allowed to decentralize their job centers. The nominations were subject to a cap specific to each state. The total number of nominees in Germany was limited to 41. If in one state the number of applying districts fell short of the cap, the remaining places were filled by districts of other states. The list of districts that were finally allowed to decentralize their job centers in 2012 was officially announced on 14 April 2011. The criteria that determined the state governments' selection processes were not published. Likewise, it remains unknown why districts applied for decentralization in the first place. Thus, when estimating the causal effect of decentralization on a job center's placement efficiency, I have to account for a potentially severe selection bias.

3. Theoretical considerations

3.1. Reasons for decentralization

Before I discuss the relationship between decentralization and the placement efficiency, it is useful to reflect why certain districts applied for the decentralization of their job centers in the first place. A potential explanation arises from the allocation of costs

⁵Districts that had already opted out did not need to apply again.

and tasks within the job centers.

The Federal Employment Agency covers the expenditures for its employees in the joint ventures and for the *unemployment benefit II*. Above a threshold, the *unemployment benefit II* will be reduced almost one-by-one for every Euro a beneficiary earns. Thus, the Federal Employment Agency has an interest in quick placements, although these may come at the cost of the quality of subsequent matches (see, for instance, Caliendo *et al.*, 2013).

The districts have to cover the labor costs of their employees in the job center as well as parts of the expenditures for the cash and non-cash welfare benefits for households in need.⁶ The household-related welfare expenditures decline only if the household earns an income above a threshold that is much higher than the threshold for the *unemployment benefit II*. Therefore, the districts would rather benefit from more stable placements in jobs yielding a higher wage rate.

In centralized job centers, the Federal Employment Agency has a large influence on the placement strategy, while in decentralized job centers only local authorities are in charge. However, with a decentralized job center, the district has to cover also the personnel expenses for the caseworkers providing the employment services. Therefore, a district likely applied for decentralization if it estimated that its own placement strategy would reduce the welfare-dependency by an extra amount that outweighs these additional personnel expenditures.

3.2. Determinants of the number of placements

The main objective of PES is to reduce the coordination frictions in the labor market and thereby to increase the aggregate job finding rate. The PES may also directly influence the job finding rate by placing unemployed job seekers into jobs. This channel is particularly important when the PES, as in the case of the German job centers, con-

⁶The remaining welfare expenditures are covered by the state government.

stitutes the major job search channel for the unemployed. The number of placements then approximates the overall outflow from unemployment to employment.

The amount of placements itself is determined by a variety of other factors (see, for instance, Grubb and Martin, 2001; Sheldon, 2003; Althin and Behrenz, 2005; Hynninen *et al.*, 2009): The business cycle influences the number of unemployed and vacancies on the labor market and therefore the number of potential placements. The share of potential placements that is actually realized then depends on the placement efficiency of the PES, the share of hard-to-place job seekers among the unemployed, and other local labor market conditions. As job seekers may be placed in vacancies of other districts, the placement rate of a PES is also determined by the number and search activities of unemployed and vacancies in surrounding districts.

The relationship between the number of placements and its various determinants can be formalized with the aggregate matching function (see, for instance, Petrongolo and Pissarides, 2001; Gregg and Petrongolo, 2005). The aggregate matching function can be thought of as a production function. In its original version, the aggregate matching function defines the number of placements, or the aggregate outflow from unemployment into employment, M , as an output produced by the stock of unemployed job seekers U and the stock of vacancies V :

$$M = M(\mu(\bullet), U, V). \quad (1)$$

The ‘total factor productivity’ (TFP) $\mu(\bullet)$ of this ‘production’ function reflects the PES’ efficiency, the unemployment structure and all the other local labor market conditions that may have an effect on unemployment outflows. In fact, the ‘TFP’ is often termed ‘aggregate matching efficiency’. Due to its simplicity, the matching function has been used repeatedly to estimate the effect of policy changes on this aggregate matching efficiency (see, for example, Fahr and Sunde, 2009; Holzner and Munz, 2013; Klinger and Rothe, 2012; Launov and Wälde, 2015). In my case, decentralization may

have an impact on aggregate matching efficiency as it may alter the placement efficiency of the respective local PES office.

I use the stock-flow version of the matching function, proposed by Coles and Smith (1998), to address the particular search behavior of PES and their clients. Upon entering unemployment, job seekers are provided by the PES with all relevant job offers currently on the market. If the job seeker does not find an appropriate trading partner among these offers, and therefore remains unemployed, there is no reason for her or the PES to search among the stock of vacancies again, because all relevant job offers have already been declined. Moreover, the clients of the German job centers have usually been unemployed for quite some time, because they usually cannot register at the job centers if they are still entitled to the unemployment insurance benefit. This entitlement usually lasts one year. It can be assumed that job seekers registering at the job centers have already screened the complete stock of vacancies without success. Hence, it is quite unlikely that job-center clients will match with a vacancy from the stock of vacancies. Only new vacancies to be opened up in the following periods will provide potentially relevant job offers. Thus, the stock of unemployed, U , is more likely to match with the inflow of new vacancies, \tilde{V} , rather than the stock of vacancies V ; while the inflow into unemployment, \tilde{U} , is more likely to match with the stock of vacancies, V . This idea of stock-flow matching extends the matching function to

$$M = M(\mu(\bullet), U, V, \tilde{U}, \tilde{V}). \quad (2)$$

The stock-flow matching function has received empirical support both at the macro level (see, for instance, Gregg and Petrongolo, 2005) and the micro level (see Andrews *et al.*, 2013). When estimated at the local level, the matching functions is often further expanded to account for spatial interdependencies between the different geographical units (see, for instance, Robson, 2001; Fahr and Sunde, 2006).

3.3. Determinants of the placement efficiency

Estimating the aggregate matching function will reveal the effect of decentralization on the job centers' placement efficiency, but it is not informative with regard to the channel through which this effect works. A change in the placement efficiency might reflect a change in the PES' objectives, organizational structure or strategies.

For instance, a PES may accept a lower placement rate if it puts more emphasis on the quality than on the quantity of its placements (see Grubb and Martin, 2001, for a discussion). Matches with higher quality will be more stable, which reduces the inflow into unemployment, and they will potentially offer a higher wage rate, which increases the likelihood that placed job seekers do not only leave unemployment but also welfare. However, the search for jobs with a better match quality usually requires more personnel resources and more time. Hence, on average, the job finding rate will be lower the more emphasis a PES puts on the quality than on the quantity of placements (see also Caliendo *et al.*, 2013). Whether decentralized job centers have changed their objectives can easily be assessed by comparing the effects of decentralization on different versions of the aggregate matching function. These alternative versions link the outflow into stable placements or the outflow out of welfare, respectively, to the job center's degree of centralization and all the other determinants discussed above. If decentralization has changed the job centers' objectives, any efficiency gain or loss with regard to the placements of unemployed should correspond to a counteracting efficiency loss or gain with respect to the stability of placements or the overall outflow out of welfare.

Alternatively, changes in the placement efficiency may result from changes in the caseworkers' caseloads. A lower caseload allows the caseworker to spend more time on each assigned job seeker. This enables her to find jobs corresponding to the job seekers' abilities, to increase the frequency of meetings with the job seeker, and to increase the monitoring efficiency. Via all these channels, a lower caseload potentially increases the job-finding rate and the quality of subsequent matches (Grubb and Martin, 2001;

Koning, 2009; Boockmann *et al.*, 2014; Hainmueller *et al.*, 2015).⁷ This makes the lower caseload also cost-effective, if the higher personnel expenses for a given stock of job seekers are offset by savings of unemployment compensation schemes and training measures. Unfortunately, I have no data on caseloads, so I cannot assess this channel directly.

Finally, decentralization may affect the aggregate job finding rate through a change of the monitoring and sanction scheme. Sanctions are temporary reductions of unemployment benefits when job seekers do not comply to their search and meeting duties. There is ample empirical evidence that not only actually imposed sanctions but also the credible threat of being sanctioned increase the job finding rate (see, for instance, van den Berg *et al.*, 2004; Abbring *et al.*, 2005; Lalive *et al.*, 2005; Svarer, 2011; Hofmann, 2012; Hofmann *et al.*, 2013; van der Klaauw and van Ours, 2013). However, sanctions also lower the quality of subsequent jobs in terms of wages, occupational level, and job stability (Arni *et al.*, 2013; Hofmann *et al.*, 2013; van den Berg and Vikström, 2014). Moreover, monitoring may even reduce the job finding rate if informal search channels are more effective than formal ones. This is because monitored job seekers tend to substitute informal search channels by formal search channels like the PES to provide the PES with observable signals of their search efforts (see van den Berg and van der Klaauw, 2006, 2015).

The effect of decentralization on the sanction strategy can be modeled analogously to the aggregate matching function. The number S of new sanctions imposed in a given month depends on the number of persons at risk to be sanctioned, the current business cycle state, and the rigor of the sanction policy. The number of persons at risk equals the number of unemployed job seekers or welfare beneficiaries, respectively. However, sanction rates might differ between the stock and the respective inflow, as caseworkers have to identify misbehavior of the newly registered clients first. The local business cycle affects the sanction rate through the number of vacancies the clients

⁷Behncke *et al.* (2010) provide evidence that it is not only the caseload but also the social similarity between caseworker and job seeker that affects the job-finding rate.

can (and shall) apply to. As before, the inflow of vacancies might be more relevant than its stock. The rigor of the sanction policy is represented by the TFP $\mu(\bullet)$ of this sanction production function. If caseworkers differentiate in their rigor between socio-demographic groups and their individual labor market prospects, the TFP also reflects the different shares of hard-to-place clients among the unemployed or welfare recipients, respectively. Thus, the production function for sanction reads:

$$S = S(\mu(\bullet), U, V, \tilde{U}, \tilde{V}). \quad (3)$$

If decentralization has changed the rigor of the sanction policy, we should observe an effect on the parameter μ .

4. Empirical Strategy

4.1. Parameter of Interest and Identifying Assumptions

The causal effect of decentralization on a job center's placement efficiency is defined as the difference between the potential placements if the job center is decentralized, and the potential placements if the same job center was centralized, while holding all other determinants of placements constant (see Roy, 1951; Rubin, 1978). This definition applies analogously to the stability of placements, the outflow out of welfare, and the number of sanctions as the alternative outcome indicators. Therefore, the following arguments in this section concentrate on the example of the number of placements M .

Let M^1 denote a job center's potential placements if the job center is decentralized, and M^0 the potential placements if it is centralized. The vector X denotes all determinants besides the type of the job center that might affect the number of placements. The binary treatment indicator $D, d \in \{0, 1\}$ equals 1 for job centers which are decentralized in 2012. Finally, let $T = 0$ be a period before the decentralization and $T = 1$ a period

after the decentralization. Then, the average treatment effect of decentralization on the placements of the decentralized job centers is given by

$$E[M^1 - M^0 | X = x, D = 1, T = 1]. \quad (4)$$

I face the usual evaluation problem that for each job center i and each period t only one outcome, $M_{it} = D_i M_{it}^1 + (1 - D_i) M_{it}^0$, is observed. The unobserved potential placements M_{it}^0 for decentralized job centers after their decentralization have to be inferred from the observed outcomes of other periods and other job centers. Estimation has to take into account that the complex selection process described in section 2 potentially led to a non-randomized sample of treated units, where the variables determining the selection into treatment are unknown.

I therefore employ the difference-in-differences estimator (DD). The DD framework allows consistent estimation of the causal effect of decentralization on a job center's efficiency even in the case where selection is based on unobservables. Identification using the DD framework is feasible because labor market institutions were and are the same in all German districts except for the type of the job center (see also Boockmann *et al.*, 2015) and because the 2012 decentralization did not coincide with the reform of any other labor market institution.

Identification of the causal effect requires that job centers that remained centralized are not indirectly affected by the reform (see, for instance, Lechner, 2011). This assumption is violated if there are spillover effects of decentralization on centralized job centers in neighbored districts. For example, decentralized job centers may increase the share of job seekers they place on jobs in neighboring districts, thereby reducing these districts' unemployment outflows. In order to check for such spillover effects I will perform a robustness check where I exclude all districts of the control group which are neighbored to a district of the treatment group (see section 6.4).

Identification furthermore requires that treated and non-treated job centers face com-

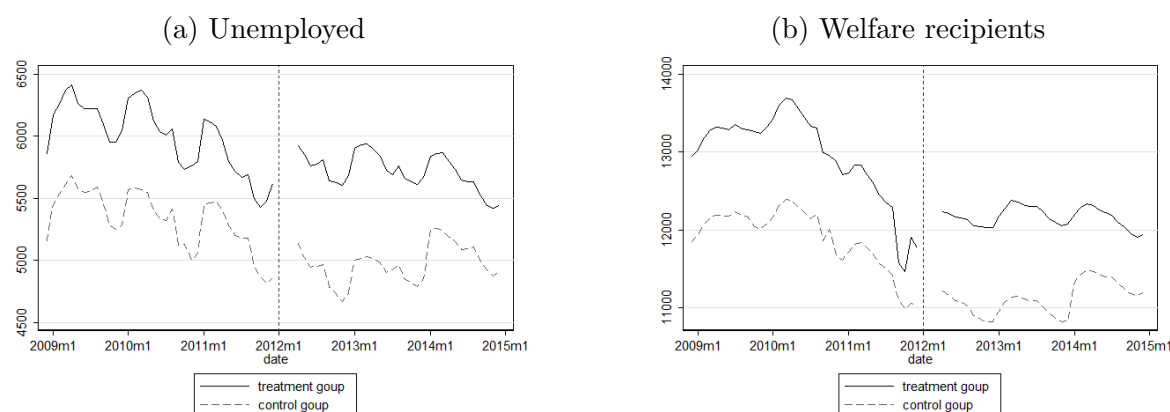
mon fundamental labor market trends, conditional on exogenous variables X (see, for example, Lechner, 2011). Consider, for instance, the issue of population aging. Elder unemployed are usually more difficult to place in jobs than prime-age job seekers. If the treatment-control difference between the shares of elder unemployed remains constant over time, causal inference using the DD estimator is consistent. However, if this difference increases, simply because districts in the treatment group suffer from a faster population aging, we observe a deterioration of a job center's efficiency that is completely independent from decentralization. This holds true for any fundamental labor market trend that may change the share of hard-to-place job center clients over time: Population aging, which affects the shares of younger and elder unemployed; migration, which affects the share of foreigners among the unemployed; and structural change, which affects the share of unemployed whose skills have become obsolete.

Table A1 in the Appendix confirms that districts of the treatment and the control group experienced on average quite the same fundamental labor market trends from 2000 to 2010: a shrinking population at working age, with a reduction especially in the medium age group from 25 to 54 years; a structural change with employment shifts from construction, agriculture and manufacturing toward business services and public and other services; a reduction in overall unemployment, especially in long-term unemployment;⁸ and a reduction in welfare dependency, particularly among the young. The shares of old-age unemployed and old-age welfare recipients increased on average in either group. Among all trend indicators considered, there are only three statistically significant differences between the treatment and the control group: Districts that decentralized their job centers in 2012 experienced a slower increase of the number of foreigners at working age, a smaller reduction of the number of foreigners on welfare, and a smaller reduction of long-term unemployed. However, these differences did not induce different trends between the groups during the pre-treatment period for any

⁸Job seekers are long-term unemployed if they have been unemployed for at least one year without interruption. Interruptions are, for instance, employment, participation in training measures or illness of at least six weeks.

relevant indicator: the number of unemployed and welfare recipients (see Figure 2), their outflows (see Figure 3), or the sanctions issued on them (see Figure 4). Only very shortly before the actual decentralization the differences start to vary. These variations occurred after the list of successful applications for decentralization has been announced and thus likely reflect pre-reform treatment effects. Hence, the common trend assumption of the DD approach holds if the trends observed between 2000 and 2010 continued throughout my sample period or would have were they not changed by the decentralization of job centers.

Figure 2: Unemployed and welfare recipients registered at job centers, by treatment status of the job center



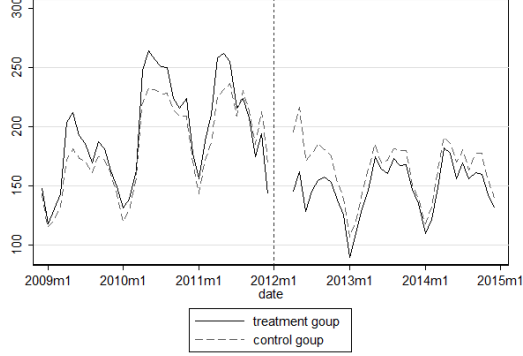
Finally, identification of the causal effects of decentralization using aggregate data requires that the reform affected the job seekers' behavior only through the channel of a potentially changed sanction strategy. In particular, job seekers should not have altered their job search effort simply because the name of their PES has changed or they were assigned to new caseworkers. This requirement is very likely fulfilled.

4.2. Estimation

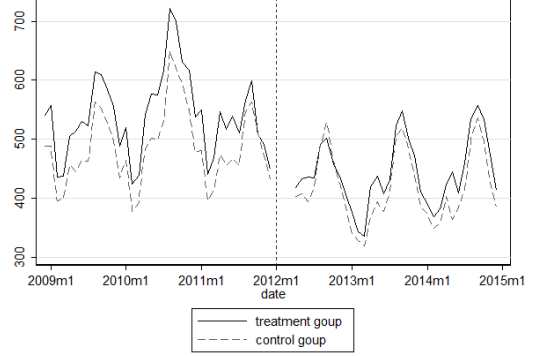
I implement the DD framework in a fixed-effects panel model with districts indexed by i and periods indexed by t . Following Klinger and Rothe (2012), Holzner and Munz

Figure 3: Outflows out of unemployment and welfare, by treatment status of the job center

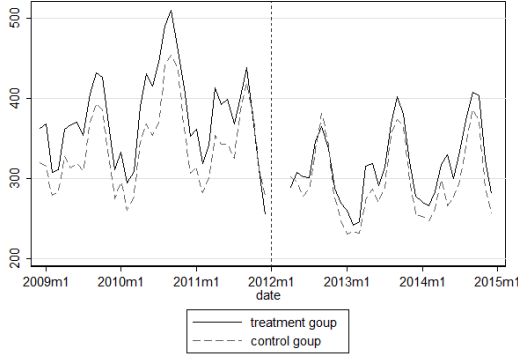
(a) Outflow from unemployment to employment



(b) Outflow out of welfare



(c) Outflow out of welfare for > 3 months

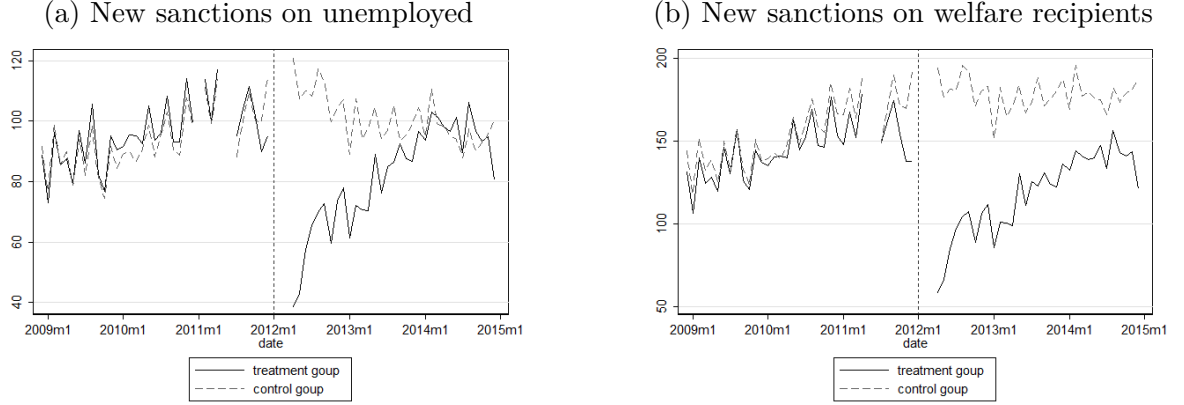


(2013) and many others, I assume the matching function introduced in section 3.2 to be of the Cobb-Douglas type:

$$M_{it} = \mu_{it} U_{it}^{\beta_1} V_{it}^{\beta_2} \tilde{U}_{it}^{\beta_3} \tilde{V}_{it}^{\beta_4}. \quad (5)$$

To check for an effect of decentralization on the placement efficiency, I define the matching efficiency μ_{it} as a product of variables Z_{it} that account for shares of hard-to-place clients, a district-specific effect α_i to account for unobserved time-constant local labor market conditions, a time fixed effect τ_t that accounts for unobserved nationwide business cycle and seasonal effects, and a treatment effect $\delta DECENTRALIZED_{it}$,

Figure 4: New sanctions imposed on unemployed and welfare recipients, by treatment status of the job center



where $DECENTRALIZED_{it}$ is a dummy variable which identifies job centers which are decentralized. The parameter δ gives an estimate of the treatment effect of decentralization on the treated PES.

In order to account for potential spatial spillovers between the districts, all stocks, flows and shares are also included as their spatial lags (see also Holzner and Munz, 2013). The spatial lags are constructed as weighted averages of a variable's outcomes in all other districts, where the weights W are equal to the row-normalized inverse distances between the districts (see LeSage and Pace, 2009).

Stocks and shares are measured at the begin of each period, while flows are measured during a period. Stocks and flows are transformed into logs. I thus estimate the following fixed-effects panel model:

$$\begin{aligned}
 \ln M_{it} = & \beta_1 \ln U_{it} + \beta_2 \ln V_{it} + \beta_3 \ln \tilde{U}_{it} + \beta_4 \ln \tilde{V}_{it} + \gamma_1 Z_{it} \\
 & + \rho_1 W \ln U_{it} + \rho_2 W \ln V_{it} + \rho_3 W \ln \tilde{U}_{it} + \rho_4 W \ln \tilde{V}_{it} + \gamma_2 W Z_{it} \\
 & + \alpha_i + \tau_t + \delta DECENTRALIZED_{it} + \varepsilon_{it}.
 \end{aligned} \tag{6}$$

In order to trace the evolution of the treatment effect over time, I substitute the single uniform treatment effect $\delta DECENTRALIZED_{it}$ by a series of period specific

treatment effects:

$$\begin{aligned}
\ln M_{it} = & \beta_1 \ln U_{it} + \beta_2 \ln V_{it} + \beta_3 \ln \tilde{U}_{it} + \beta_4 \ln \tilde{V}_{it} + \gamma_1 Z_{it} \\
& + \rho_1 W \ln U_{it} + \rho_2 W \ln V_{it} + \rho_3 W \ln \tilde{U}_{it} + \rho_4 W \ln \tilde{V}_{it} + \gamma_2 W Z_{it} \\
& + \alpha_i + \tau_t + \sum_t \delta_t \text{DECENTRALIZED}_{it} + \varepsilon_{it}.
\end{aligned} \tag{7}$$

Equation (7) immediately provides pseudo-treatment effect estimates for the pre-treatment period that will help to assess the quality of my identification strategy. Statistically significant ‘treatment’ effects during the pre-treatment period would indicate serious violations of the common trend assumption.

The treatment and pseudo-treatment effects are estimated relative to the base period, for which no time-fixed effect is included in the model. The choice of the base period is therefore crucial for the size and statistical significance of the estimated treatment effects. In order to prevent any potential anticipation effects to bias my results, I define the last month prior to the beginning of the application period, that is July 2010, as my base month. Alternative specifications, for example using the whole year 2010 as a base category, did not alter my results.

Standard errors are obtained via bootstrapping with 200 replications to account for clustered and autocorrelated error structures (Bertrand *et al.*, 2004; Lechner, 2011). The bootstrapping samples are drawn independently for treated and non-treated districts at the district level, ensuring that all observations of the same district are either included or excluded altogether.

I estimate equations (6) and (7) separately for the unemployed job seekers and the welfare recipients using the respective stocks, flows and shares as inputs. I also estimate similar equations for the imposition of sanctions, substituting the outflow out of unemployment or welfare, respectively, by the number of new sanctions issued in the given month.

5. Data and sample definition

I utilize monthly data on vacancies, unemployment, welfare recipients, and sanctions measured at the level of the German job centers or districts, respectively.⁹ These data are provided by the Federal Employment Agency. My sample period ranges from January 2009 to December 2014, that is from three years before to three years after the decentralization.

The vacancy data measure the stock and the inflow of registered vacancies. Registration of vacancies is not mandatory, hence vacancy figures are subject to measurement error. According to the IAB Job Vacancy Survey, on average approximately 45% of all vacancies have been registered in each quarter of my sample period (see Institute of Employment Research, 2015). Note that the registration requirement imposes a lower bound on the decentralization effect. If decentralized job centers are systematically more (less) efficient in acquiring information about existing vacancies and placing their clients, the estimated impact of decentralization on aggregate matching efficiency is downward (upward) biased toward zero.

Unemployment figures reflect unemployment registered at the job centers. As registration is mandatory for take up of the *unemployment benefit II*, virtually all relevant job seekers are considered in the official unemployment statistics.¹⁰ I observe stock data for all unemployed as well as for various groups of hard-to-place job seekers (unemployed aged 15 to 24, unemployed aged 55 to 64, foreign unemployed and long-term unemployed). I use these stock data to calculate corresponding shares of hard-to-place job seekers in total unemployment. I also readily observe for each job center the total monthly inflow into unemployment as well as the monthly outflow from unemployment

⁹I do not use the official performance indicators on job centers that are provided by the Federal Employment Agency since 2011, because these indicators proved inappropriate for my analysis. For instance, they do not distinguish between unemployment outflows and job-to-job transitions when measuring the number of ‘placements’, and they do not account for temporary returns into unemployment when assessing the ‘stability’ of these placements.

¹⁰Only job seekers eligible for the welfare scheme *unemployment benefit II* are registered at the job centers. All other job seekers are registered at the local employment agencies.

into employment.¹¹ As indicated in section 2, this outflow measure should approximate the number of placements by the job center very well. Unfortunately, I have no data on the quality of these placements.

Data on welfare recipients comprises the stock of all welfare recipients as well as the stocks of various groups of hard-to-place clients (welfare recipients aged 15 to 24, welfare recipients aged 50 to 64, and welfare recipients with foreign citizenships), from which I compute the corresponding shares. I also readily observe the total inflow into and the total outflow out of welfare, as well as the outflow with subsequent returns into welfare within three months. Unfortunately, I do not observe to what extent these outflows out of welfare reflect welfare-to-work transitions or placements by the job center. For instance, the observed number of total outflows may also include transitions into old-age pensions. However, descriptive statistics on the structure of the welfare recipients do not indicate systematic differences between the treatment and the control group (see Table A2). Therefore, I use the monthly outflow out of welfare as a measure of placements of welfare recipients, and acknowledge that this measure is plagued with some measurement error which does not vary systematically between the treatment and the control group. From this placement measure, I subtract the number of outflows with subsequent returns into welfare within three months to derive a measure of sustainable outflows out of welfare.

The sanction data provide the number of new sanctions imposed each month on either the unemployed or the welfare recipients, respectively.

In order to calculate the spatial lag of each variable, all data must correspond to the same geographic units over the whole sample period. This requires some additional adjustments, as some data are measured at the district level, while other data are measured at the level of job centers. Furthermore, some data have not yet been corrected for administrative reforms merging or separating various districts. I therefore convert all data such that they correspond to the 402 districts in their latest territorial

¹¹Assignments into public job creation schemes are not considered as transitions into employment.

boundaries. For these 402 German districts, I calculate the distance-weighted spatial lags of each variable using geodata that are readily provided by the German Federal Agency for Cartography and Geodesy.¹² Data on districts or job centers for which the cross-sectional transformation was not achievable are dropped.¹³

From the remaining observations, my treatment group comprises those districts whose job centers were decentralized in 2012, while my control group contains all districts whose job centers remained joint ventures throughout the sample period. Districts in which job centers were decentralized already in 2005 are not part of my sample as they belong neither to the treatment nor to the control group. I also drop the 11 districts which are divided between job centers of different types. My sample thus consists of 40 districts in the treatment and 294 districts in the control group. Due to missing data for decentralized job centers immediately after decentralization, I have to drop three months from my sample (January, February and March 2012). For the remaining sample period, districts exhibit missing values in some or all variables from time to time, often due to technical or reporting issues in particular months. Sanction data is completely missing in May and June 2011, and for unemployed also in January 2011. Data on welfare recipients are missing systematically until the end of 2010 for some districts in Saxony and Saxony-Anhalt as well as the region of Aachen because of changes in these districts' territorial boundaries. My results do not change significantly when I drop all observations of these districts.

¹²<http://www.geodatenzentrum.de>.

¹³This concerns mainly administrative reforms. If districts were divided, data on the old districts could not be transferred to the new district boundaries. Moreover, some job centers are in charge for more than one district, such that the respective job-center-specific data could not be allocated to the corresponding districts.

6. Empirical results

6.1. District characteristics

I first briefly review the average fundamental characteristics of the treatment and the control group before discussing the treatment effects of decentralization. I assess the characteristics for the year 2010, when districts had to decide whether to apply for decentralization (detailed results can be found in Table A2 of the Appendix). Districts of the treatment group were slightly more densely populated and had a slightly higher population on average, but the differences were not statistically significant. Also, employment rates, unemployment rates and welfare take-up rates did not differ significantly. The same holds true for the socio-demographic structures of the population at working age, of the unemployed, and of the welfare recipients, as well as the sectoral structure of employees (including self-employed). The single exception is the share of long-term unemployed, which was larger in districts that successfully applied for decentralization. This assessment suggests that the treatment group is quite a representative subsample of all German districts, although selection into treatment was not random but involved decisions at the district and the state level.

6.2. The average treatment effect of decentralization

The treatment effect of decentralization is negative and statistically significant for all outcome indicators considered. Table 1 presents the results from estimating equation (6) separately for the two populations of interest (unemployed job seekers and welfare recipients) and the three outcome indicators considered (outflows, sustainable outflows, and sanctions). In particular, the conditional number of monthly transitions from unemployment to employment declined by 10% on average due to the decentralization (see column 1). The effect is significantly larger than the effect on monthly outflows and sustainable outflows out of welfare, for which aggregated matching effi-

Table 1: Difference-in-differences estimates of the effect of decentralizing public employment services (PES)

	Log outflow			Log new sanctions	
	From unempl. to empl.	From welfare	From welfare for > 3 months	On unempl.	On welfare recipients
Decentralized PES	-0.101***	-0.039***	-0.026**	-0.124***	-0.246***
Controls and spatial lags	YES	YES	YES	YES	YES
District fixed effects	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES
N	22,327	22,505	22,505	21,014	21,504
Districts	334	334	334	334	334
R-squared (within)	0.669	0.702	0.669	0.113	0.137

Notes: Statistical significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are bootstrapped with 200 replications. Controls are the logs of begin-of-period stocks and the logs of inflows of vacancies, and of unemployed or welfare recipients, respectively; and the respective begin-of-period shares of young, elderly and foreign citizens. The regressions for unemployed additionally control for the begin-of-period share of long-term unemployed. All variables are also included as spatial lags, using row-normalized inverse distances between districts as weights.

Sample period: 2009m1–2014m12.

ciency declined on average by 4% and 3%, respectively (see columns 2 and 3). The number of new sanctions imposed each month declined as well, and here the effect was on average smaller for unemployed job seekers (-12%, see column 4) than for welfare recipients (-25%, see column 5).

However, each effect varies significantly between the short run and the long run. The evolution of the treatment effects over time as estimated by equation (7) is plotted in Figures 5 and 6. Bullets denote point estimates that are statistically significant at the 95% confidence level.

During the whole pre-treatment period, there are virtually no statistically significant ‘treatment’ effects for any of the outcome indicators considered. This finding supports the perception that prior to the decentralization districts of the treatment group and the control group faced on average the same labor market trends. This backs the identification using the DD framework. Only very shortly before the reform, a few significant treatment effects are observed. However, these effects occurred after the

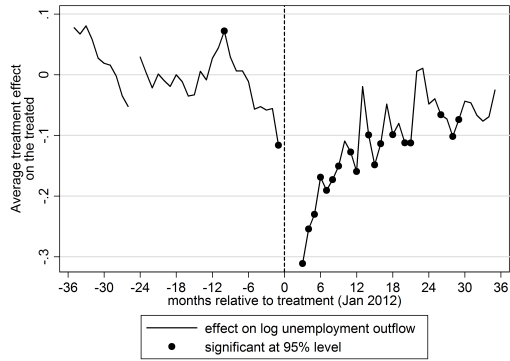
announcement of successful applications for decentralization in 2011, and therefore they likely reflect anticipation or pre-reform treatment effects.

Immediately after the decentralization, the placement efficiency of decentralized job centers with regard to unemployed job seekers dropped significantly. The conditional outflow from unemployment to employment shrunk by 30% (see Figure 5a). Over the course of the following years, the efficiency loss decreased in size. One year after the reform, the loss in conditional monthly outflows amounted to approximately 15%; about two years after the reform the outflow loss was less than 10% and not statistically significant anymore; and three years after the reform the outflow gap amounted to less than 5% and continued to decline. Extrapolating from Figure 5a, we can expect the long-run treatment effect of decentralization on the placement efficiency to be about zero. A similarly negligible long-run treatment effect of decentralization on conditional unemployment outflows was already identified for the first generation of decentralized job centers from 2005 (see Holzner and Munz, 2013; Boockmann *et al.*, 2015). However, these studies do not account for the short-run efficiency loss immediately after decentralization. If the average decentralized job center had remained centralized, it could have placed more than 400 job seekers additionally during the three year post-treatment period, which is a sizeable figure compared to the about 4,100 placements it had realized.

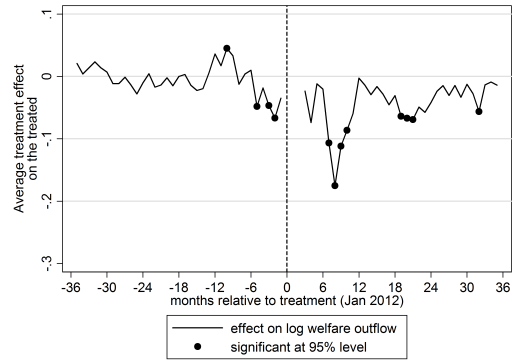
The effects of decentralization on the outflow out of welfare and the sustainable outflow out of welfare were also negative but much smaller and probably more persistent than the effect on the placements of job seekers (see Figures 5b and 5c). The monthly efficiency loss of approximately 4% on average is in fact only on the cusp of statistical significance. Accordingly, three years after decentralization, the cumulated outflow out of welfare for the average decentralized job center amounted to almost 15,500 placements and thus was reduced by only 600 placements due to the centralization. The effect on sustainable placements was equally small (almost -600), while the average decentralized job center realized about 10,600 sustainable transitions out of welfare.

Figure 5: Difference-in-differences estimates of period-specific effects of decentralization of public employment services on outflows out of unemployment and welfare

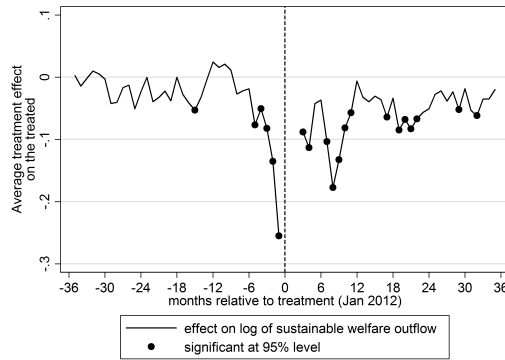
(a) Outflow from unemployment to employment



(b) Outflow out of welfare



(c) Outflow out of welfare for > 3 months

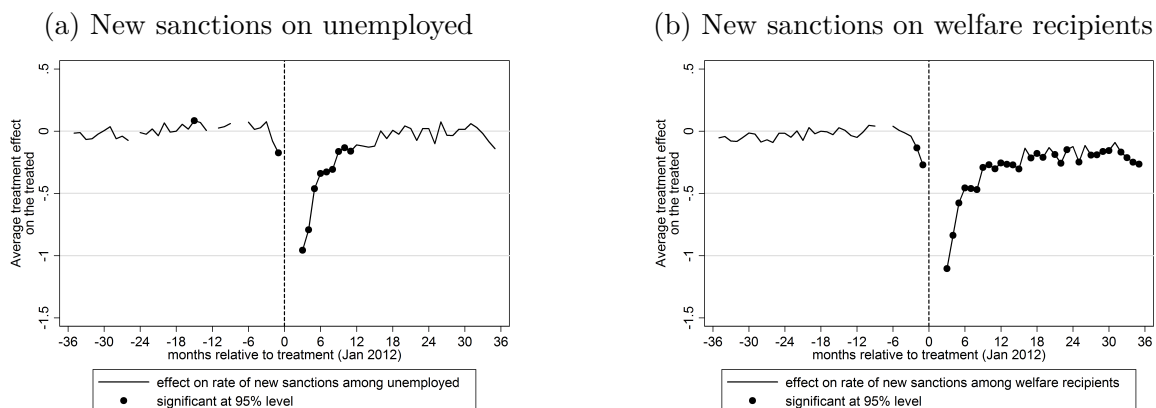


Notes: Standard errors are bootstrapped with 200 replications. Regressions include district and time fixed effects and additionally control for the logs of begin-of-period stocks and the logs of inflows of vacancies, and of unemployed or welfare recipients, respectively; and the respective begin-of-period shares of young, elderly and foreign citizens. The regressions for unemployed additionally control for the begin-of-period share of long-term unemployed. All variables are also included as spatial lags, using row-normalized inverse distances between districts as weights. All models also include interactions of the treatment dummy with monthly time dummies.

Sample period: 2009m1–2014m12.

The immediate effect of decentralization on sanctions was more severe than the effect on outflows. Both, the conditional number of sanctions on unemployed and the conditional number of sanctions on welfare recipients, dropped by an order of magnitude immediately after the decentralization. They then increased sharply again during the second quarter after the reform. The conditional number of sanctions on unemployed then increased further, and one year after the reform, the effect of decentralization was

Figure 6: Difference-in-differences estimates of period-specific effects of decentralization of public employment services on sanctions on unemployed and welfare recipients



Notes: Standard errors are bootstrapped with 200 replications. Regressions include district and time fixed effects and additionally control for the logs of begin-of-period stocks and the logs of inflows of vacancies, and of unemployed or welfare recipients, respectively; and the respective begin-of-period shares of young, elderly and foreign citizens. The regressions for unemployed additionally control for the begin-of-period share of long-term unemployed. All variables are also included as spatial lags, using row-normalized inverse distances between districts as weights. All models also include interactions of the treatment dummy with monthly time dummies.

Sample period: 2009m1–2014m12.

not statistically significant anymore (see Figure 6a). Still, during the whole follow-up period, the average decentralized job center issued almost 200 sanctions to unemployed less than it would have if it had remained centralized. The effect is sizable, as such a job center issued only 1,600 sanctions on unemployed at all. The conditional number of new sanctions on welfare recipients was even permanently affected by the decentralization (see Figure 6b). During the second and third post-reform year, the treatment effect varied around -20% on average and was highly significant throughout the follow-up period. Hence, while the average decentralized job center issued more than 3,300 sanctions on the welfare recipients during the first three years after its decentralization, a comparable centralized job center would have issued almost 1,000 sanctions more in the same time.

6.3. Discussion

To sum up, with regard to unemployed job seekers, the estimation results point to rather transitory negative treatment effects of decentralization on the placement efficiency and the sanction rate. As the effects are transitory, they likely reflect a period of learning, during which caseworkers in the decentralized job centers acquired the relevant placement skills on-the-job. They may also reflect a transition period for the job center as a whole during which the assignment of tasks, internal structures, and the administrative software have been optimized (see also Fay, 1997). However, even after this transition period, which lasted at least two years for the sample at hand, decentralized job centers did not become more efficient than centralized job centers. Hence, decentralized job centers lost efficiency in the short run, but did nothing gain in the long run.

With regard to welfare recipients, the results are even more concerning. I estimate permanent negative treatment effects of decentralization on the placement efficiency and the sanctioning rate. These permanent effects can be jointly explained either by increased caseloads for the job centers' caseworkers or by an easing of the monitoring and sanction strategy in decentralized job centers. The caseloads may have increased if decentralized job centers entrusted caseworkers of the welfare administrations also with tasks of the employment service, or if they transferred caseworkers from the welfare administration to the employment service. An easing of the sanction intensity may have its roots in political economy: If the local authorities in charge for the strategy of the decentralized PES are elected representatives, as is the case in Germany, they aim to maximize their share of votes. For them, reducing the monitoring and sanction intensity is a useful strategy to increase the well-being of the clients of their PES, as these may constitute a relevant share of voters in upcoming elections. It would be illuminating to decompose the observed causal effect of decentralization on the outflow out of welfare into a direct effect resulting from a lower placement efficiency and an indirect effect operating through the sanction channel. Unfortunately, such an analysis

would require additional exogenous variation in the sanction rate besides the observed variation due to the decentralization. As I do not observe such an exogenous variation in my data, I leave this issue for future research.

The absence of any positive treatment effect of decentralization suggests that decentralized job centers did not change the focus of the placement strategy. In particular, my results do not provide evidence that the lower placement rate results from the more time-consuming search for better and more stable matches. If there was such a shift, we should have observed positive treatment effects at least for the sustainable outflow out of welfare.

6.4. Sensitivity analysis

I now examine whether the assumptions underlying my empirical analysis are not violated. First, I check for spillover effects from decentralized on centralized job centers. I re-estimate equation (6) with a reduced sample where I dropped all districts of the control group that are neighbors to at least one district of the treatment group. The results from the full and the restricted sample are virtually the same (see Table 2), indicating that indirect treatment effects on non-treated districts are of no concern.

Second, I apply the conditional difference-in-differences (cDD) estimator originally proposed by Heckman *et al.* (1997, 1998).¹⁴ This estimator improves the balancing between the treatment and the control group with regard to their fundamental characteristics by reweighing the control group before applying the DD estimator. Usually, the reweighing employs propensity score matching, which requires knowledge about and access to all variables that have played a role in the two-sided selection process described in section 2 (see e. g. Schünemann *et al.*, 2015). As mentioned above, I do not have access

¹⁴Smith and Todd (2005) provide an assessment of the quality of the cDD estimator. Abadie (2005) modifies the estimator for repeated cross-section settings. The estimator has been applied, among others, in the context of training programs (Bergemann *et al.*, 2009), development projects (Ravalion and Chen, 2005), educational attainment (Buscha *et al.*, 2012), and educational success (Cho, 2009).

Table 2: Difference-in-differences estimates of the effect of decentralizing public employment services (PES), sample without centralized job-centers that are neighbored to decentralized job-centers

	Log outflow			Log new sanctions	
	From unempl. to empl.	From welfare	From welfare for > 3 months	On unempl.	On welfare recipients
Decentralized PES	-0.097***	-0.037**	-0.025**	-0.114***	-0.233***
Controls and spatial lags	YES	YES	YES	YES	YES
District fixed effects	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES
N	17,503	17,666	17,666	16,413	16,809
Districts	262	262	262	262	262
R-squared (within)	0.674	0.703	0.673	0.117	0.149

Notes: Statistical significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are bootstrapped with 200 replications. Controls are the logs of begin-of-period stocks and the logs of inflows of vacancies, and of unemployed or welfare recipients, respectively; and the respective begin-of-period shares of young, elderly and foreign citizens. The regressions for unemployed additionally control for the begin-of-period share of long-term unemployed. All variables are also included as spatial lags, using row-normalized inverse distances between districts as weights.

Sample period: 2009m1–2014m12.

to these variables. I therefore reweigh my sample using entropy balancing, which directly pursues the balancing of whole distributions of the relevant characteristics using non-parametric methods (see Hainmueller, 2012; Hainmueller and Xu, 2013). I reweigh the control group observations such that the means and variances of their fundamental labor market trends match the corresponding distributions of the respective trends of the treated districts. I then re-estimate equation (6) using the reweighted sample. The cDD approach yields results very similar to my baseline specification (see Table 3). In particular, the average treatment effect of decentralization on the conditional transitions from unemployment to employment is again estimated to be about 10% in the first three years after the decentralization.

Finally, I check whether the decentralization in 2012 coincided with an unobserved but systematic worsening of local labor market conditions in districts where job centers were decentralized. I make use of the fact that job centers are not the only public providers of placement services. There are also local employment agencies that provide

Table 3: Conditional difference-in-differences estimates of the effect of decentralizing public employment services (PES)

	Log outflow			Log new sanctions	
	From unempl. to empl.	From welfare	From welfare for > 3 months	On unempl.	On welfare recipients
Decentralized PES	-0.105***	-0.036***	-0.024***	-0.157***	-0.265***
Controls and spatial lags	YES	YES	YES	YES	YES
District fixed effects	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES
N	22,327	22,505	22,505	21,014	21,504
Districts	334	334	334	334	334
R-squared (within)	0.663	0.688	0.660	0.160	0.205

Notes: Statistical significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are bootstrapped with 200 replications. Controls are the logs of begin-of-period stocks and the logs of inflows of vacancies, and of unemployed or welfare recipients, respectively; and the respective begin-of-period shares of young, elderly and foreign citizens. The regressions for unemployed additionally control for the begin-of-period share of long-term unemployed. All variables are also included as spatial lags, using row-normalized inverse distances between districts as weights.

Sample period: 2009m1–2014m12. Districts of the control group have been reweighted to balance long-run and medium-run trends.

placement services for job seekers who are not entitled to the welfare scheme *unemployment benefit II* and who are therefore not clients of the German job centers. This type of PES is centrally organized and was neither directly nor indirectly affected by the decentralization of job centers. However, it should be equally affected by district specific labor market shocks. Due to data constraints, I focus on the effect of decentralization on the outflow from unemployment to employment. I re-estimate equation (6) for the local employment agencies to detect any unobserved local labor market shocks, that systematically affected districts of the treatment group. The resulting estimate of the pseudo-treatment effect is zero and statistically insignificant (see Table 4, column 1). Hence, if there were any unobserved but systematic local labor market shocks, their effects must have been negligible small.

To further elaborate on this issue, I extend my analysis to a difference-in-differences-in-differences framework (DDD). The DDD approach was first applied by Gruber (1994), Gruber and Poterba (1994) and Yelowitz (1995) in the context of tax and welfare

Table 4: Estimates of (pseudo-)treatment effects of decentralization on the log outflow from unemployment to employment, by type of public employment service (PES)

PES type	Employment agencies	Job-centers
Estimation method	DD	DDD
Pseudo-treatment effect for employment agencies	-0.000	
Treatment effect for decentralized job-centers		-0.084***
Controls and spatial lags	YES	YES
District fixed effects	YES	YES
Time fixed effects	YES	YES
Job-center fixed effect		YES
District of treatment group \times Time fixed effects		YES
District of treatment group \times Job-center		YES
Job-center \times Time fixed effects		YES
N	22,712	45,039
Districts	334	334
R-squared	0.806	0.909

Notes: Statistical significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are bootstrapped with 200 replications. Controls are the log of the inflow and of the lagged stock of vacancies; the log of the inflow and of the lagged stock of unemployed; and the lagged shares of young, elderly, foreign citizens, and long-term unemployed. All variables are also included as spatial lags, using row-normalized inverse distances between districts as weights.

Sample period: 2009m1–2014m12.

programs. They exploit the variation of the respective programs across states and between treated and non-treated individuals within a state. In my DDD application, I exploit the variation across districts, and across types of PES (local employment agencies and job centers) within a district. Comparing different districts differences away all unobserved heterogeneity arising from changes over time that were common to all districts. Comparing the different types of PES within each district differences away district-specific labor market trends that affected both types of PES alike.

I expand equation (6) such that all variables now refer to stocks, flows, and shares for PES type j in district i in period t . Next to the district and time fixed effects I add a job-center fixed effect JC_j that captures the average efficiency difference between job centers and local employment agencies across all districts and over all time periods. I also add three interaction terms where I interact the treatment dummy variable $TREAT_i$, which identifies districts of the treatment group, the job-center dummy

variable JC_j , and the time period-specific dummy variables $PERIOD_t$ pairwise with each other. Finally, I add a modified $DECENTRALIZED_{ijt}$ dummy variable, that equals one in periods since January 2012 for job centers which are decentralized, and zero otherwise. The DDD analogue to equation (6) then reads

$$\begin{aligned}
\ln M_{ijt} = & \beta_1 \ln U_{ijt} + \beta_2 \ln V_{it} + \beta_3 \ln \tilde{U}_{ijt} + \beta_4 \ln \tilde{V}_{it} + \gamma_1 Z_{ijt} \\
& + \rho_1 W \ln U_{ijt} + \rho_2 W \ln V_{it} + \rho_3 W \ln \tilde{U}_{ijt} + \rho_4 W \ln \tilde{V}_{it} + \gamma_2 W Z_{ijt} \\
& + \alpha_i + JC_j + \tau_t \\
& + \delta_1(TREAT_i \times JC_j) + \delta_2(TREAT_i \times PERIOD_t) + \delta_3(JC_j \times PERIOD_t) \\
& + \delta_4 DECENTRALIZED_{ijt} + \varepsilon_{ijt}.
\end{aligned} \tag{8}$$

The coefficient of interest is now δ_4 , which is estimated to be -8.4% (see Table 4, column 2). This is remarkably close to my baseline estimate of the treatment effect. Thus, like my cDD results and the pseudo-treatment effect for the local employment agencies, the DDD estimation results do not indicate a violation of the common trends assumption.

7. Conclusion

In this paper, I exploit a German policy experiment to assess the effect of decentralization of PES on the placement efficiency and the sanction rate. In 2012, 10% of the German districts established decentralized PES in contrast to the usual scheme of centralized PES. This allows to study centralized and decentralized PES offices in a common economic and institutional environment. I identify the causal effect of decentralization on placements by estimating aggregate stock-flow matching functions in a difference-in-differences framework. Analogously, I derive results for the causal effect of decentralization on sanctions. I perform these analyses for two groups of clients of the German PES offices: unemployed job seekers and welfare recipients.

I estimate significant negative treatment effects of decentralization on all outcome indicators. However, the effects vary considerably in their magnitude over time, across outcome measures, and between the two groups of PES clients considered. In particular, the effect on the placement and sanctioning of unemployed appears to be only transitory, while the effect on the placement and sanctioning of welfare recipients is permanent. However, point estimates of the effects of decentralization on the placement of welfare recipients are only at the brink of statistical significance.

These results lead to the following three conclusions. First, like any other major change of an organization, decentralization of PES initiates a transition phase during which the caseworkers and each PES office at a whole acquire the relevant skills and optimize their strategies before they work efficiently (see also Fay, 1997). At least during that period, decentralized PES offices exhibit a lower placement and sanction rate than comparable centralized PES offices. For the German PES offices that were decentralized in 2012, the transition phase lasted on average at least two years.

Second, decentralized PES may ease the sanction regime for their clients, at least if elected local authorities are in charge of the decentralized PES' resources and strategies. A laxer sanction policy will reduce the job seekers' incentive to exert high search effort and thereby the aggregate job finding rate, but may increase the well-being of the welfare recipients. Local authorities might therefore ease the sanction regime to increase their share of voters among the welfare recipients in upcoming elections. Investigating this issue in more detail is beyond the scope of this paper but promises to be a fruitful field of future research.

Third, at least for the first three years after decentralization, decentralized PES do not perform better than centralized PES in any of the outcome indicators considered. However, in the long run they do also not perform particularly worse. Moreover, decentralized job centers apparently do not place more or less emphasis on the quality of their placements than centralized PES. Hence, switching from a well established centralized or decentralized system to the respective other system does not seem worthwhile. There

is much to lose in the short run, and apparently little to gain in the long run.

Future research may complement the picture of the causal effects of decentralization by using individual-level instead of aggregate data. This would allow to check for more detailed effects, for instance, on wages and the actual duration of subsequent jobs, the compliance to job-seeker duties, and the probability to be sanctioned given individual mis-conduct. Additionally, more research is needed on the internal structures and strategies adopted in centralized and decentralized PES. This will help to disentangle the effects of decentralization (external structure of PES offices) from the effects of particular features of the internal organization within the PES offices (see, for instance, Boockmann *et al.*, 2015). Finally, caseload data may help to identify whether the observed permanent losses in the placement efficiency and sanction rate of decentralized PES result indeed from a laxer sanction regime or rather from an overload of work for the caseworkers.

A. Appendix

Table A1: Growth rates of district characteristics, by the type of the public employment service

Variable	decentralized in 2012	remained centralized
<i>Population at working age (15–64 years), 2000–2010</i>		
Total population (in 1000)	–4.1%	–3.5%
Aged 15–24 years	–2.2%	–0.1%
Aged 25–54 years	–4.6%	–4.5%
Aged 55–64 years	–2.7%	–1.9%
Foreigners	1.1%	6.5%**
<i>Employment, 2000–2010</i>		
Total employment (in 1000)	1.1%	2.8%
Agriculture and fishing	–14.6%	–13.3%
Mining and energy	–2.0%	1.2%
Manufacturing	–7.2%	–7.0%
Construction	–15.1%	–16.2%
Consumer-related services	1.0%	3.0%
Business services	21.7%	25.5%
Public and other services	9.0%	9.6%
<i>Unemployed registered at job-centers, 2007–2010</i>		
Total unemployment (in 1000)	–11.5%	–13.6%
Aged 15–24 years	–14.9%	–19.3%
Aged 25–54 years	–13.3%	–15.3%
Aged 55–64 years	7.8%	8.9%
Foreigners	–10.5%	–13.1%
Long-term unemployed	–26.5%	–30.6%*
<i>Welfare recipients, 2007–2010</i>		
Total number of welfare-recipients (in 1000)	–7.1%	–8.7%
Aged 15–24 years	–13.1%	–15.1%
Aged 25–49 years	–9.9%	–11.7%
Aged 50–65 years	5.6%	4.7%
Foreigners	–4.1%	–7.2%*
Districts	40	294

Notes: Statistical significance of differences in group means: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A2: Descriptive statistics of districts in the application year 2010, by the type of their public employment service

Variable	decentralized in 2012	remained centralized
<i>District type</i>		
Share: large city	20.0%	18.4%
Share: urban district	50.0%	31.3%
Share: suburban district	10.0%	25.5%
Share: rural district	20.0%	24.8%
<i>Population at working age (15–64 years)</i>		
Total population (in 1000)	154.2	131.5
Share: aged 15–24 years	16.9%	17.2%
Share: aged 25–54 years	63.8%	63.7%
Share: aged 55–64 years	19.3%	19.2%
Share: foreigners	9.8%	9.0%
<i>Employment</i>		
Total employment (in 1000)	115.3	101.0
Employment rate	72.3%	76.3%
Share: agriculture and fishing	2.1%	2.3%
Share: mining and energy	1.4%	1.4%
Share: manufacturing	20.4%	19.5%
Share: construction	6.6%	6.6%
Share: consumer-related services	25.4%	25.1%
Share: business services	14.2%	14.1%
Share: public and other services	29.9%	31.0%
<i>Unemployed registered at job-centers</i>		
Total unemployment (in 1000)	6.0	5.4
Unemployment rate	4.8%	4.6%
Share: aged 15–24 years	8.0%	7.8%
Share: aged 25–54 years	79.7%	79.9%
Share: aged 55–64 years	12.3%	12.4%
Share: foreigners	19.0%	16.5%
Share: long-term unemployed	43.5%	40.9%**
<i>Welfare recipients</i>		
Total number of welfare-recipients (in 1000)	13.3	12.1
Dependency rate	8.4%	8.1%
Share: aged 15–24 years	18.3%	18.1%
Share: aged 25–49 years	56.0%	56.1%
Share: aged 50–64 years	25.7%	25.8%
Share: foreigners	19.9%	17.6%
Districts	40	294

Notes: Statistical significance of differences in group means: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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