

**Is Local Better?**  
**German Jobcenters and the Decentralization of Welfare Institutions<sup>1</sup>**

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## **Abstract**

Long-term unemployment is a serious economic problem whose importance has not been properly addressed in the fiscal federalism literature so far. Using the devolution of German Jobcenters in 2012, this paper applies a difference-in-differences framework as well as a synthetic control approach to identify the causal effect of decentralization on long-term unemployment. The results do not provide any support for the famous Decentralization Theorem by Oates (1972) but suggest that long-term unemployment rises during the regime change without offering significant medium-term reductions.

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# 1. Introduction

“In a large republic, the public good is sacrificed to a thousand views; it is subordinate to exceptions, and depends on accidents. In a small one, the interest of the public is easier perceived, better understood, and more within the reach of every citizen; abuses are of less extent, and of course are less protected.”

Montesquieu, *The Spirit Of Laws* (1748)

The debate on the question how states should be optimally organized has its roots long before Montesquieu wrote his treatise “The Spirit of Laws” in 1748. The debate has drawn attention from scholars of many academic disciplines including Political Science, Philosophy or Management and is still widely discussed. In Economics, the seminal works of Musgrave (1959), Buchanan (1980) and Oates (1972) set the stage for an extensive literature on Fiscal Federalism which “addresses the vertical structure of the public sector” (Oates, 1999)<sup>2</sup>.

In particular, the Decentralization Theorem formulated by Oates has found great acclaim. The theorem states that in the absence of jurisdictional spillovers and homogeneous preferences, government functions should always be allocated at the lowest possible level. This dissertation applies the theorem to the decentralization of German Jobcenters and attempts to verify whether its predictions hold true.

The 41 German counties<sup>3</sup> who took over the sole responsibility for their local Jobcenters in January 2012 are the focal points of the analysis. These centres, responsible for servicing, training and job-finding of long-term unemployed, were previously run as cooperatives between the respective county and the Federal Employment Agency (FEA). Fortunately, the devolution in 2012 was not accompanied by simultaneous labour market reforms. It can therefore be interpreted as a quasi-experiment. To my and the German government’s best knowledge, this is the first attempt to exploit this unique event scientifically (Deutscher Bundestag, 2012).

Previously, in January 2005, 69 counties already experimentally opted for the decentralization of their Jobcenters. This was evaluated by the federal government and resulted in the publications by Holzner and Munz (2012) and Boockmann et al. (2013). Both papers rely on an unfavourable empirical setting (outlined in the next section) and do not find evidence in favour of decentralisation.

This paper applies a difference-in-differences framework as well as a synthetic control approach to identify the causal effect of decentralization on long-term unemployment. Using county-level data from 2009 to 2013, the results do not offer evidence for declining unemployment as suggested by the decentralization theorem. On the contrary, there is significant evidence that the transition temporarily caused severe

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<sup>2</sup> See Oates (2005) and Vu (2010) for a comprehensive overview.

<sup>3</sup> Note that in a few special cases, county and Jobcenter district boundaries are not identical. For simplicity, the terms “county” and “district” are used interchangeably in the following.

disruptions in the functionality of long-term unemployment assistance as reflected in rising unemployment figures.

In order to establish this paper, a broad literature review was performed. The following paragraphs aim to summarize a selection of studies and to outline the main contributions of this paper in comparison.

Several mechanisms have been suggested as channels mediating the gains from decentralization. Following Hayek (1944), central planning suffers from information asymmetries. Knowledge about regional conditions is often local and dispersed which could become accessible by decentralization. This is particularly relevant if there is a lot of heterogeneity across regions. Buchanan (1980) is concerned about monopolistic behaviour of a central government and emphasizes decentralization to spur competition among local governments, constraining rent extraction. Political economy considerations in the form of Yardstick competition (Salmon (1987); Besley and Case (1995)) as well as Tiebout-sorting (Tiebout, 1956) provide further possible channels. Seabright (1996) presents a similar reason but based on incomplete-contracts theory, stressing the enhanced accountability of decentralized governments. Finally, Oates (1999) and others argue that decentralization may lead to more policy experiments which enables horizontal diffusion of effective policies and therefore improves average policy quality.

However, there are doubts about the advantages of decentralizing government functions. In particular, the Decentralization Theorem may fail if there are significant economies of scale from central provision (Tiebout, 1961), spill-overs across regions are not coordinated (Lockwood, 2002) and governments are not benevolent (Lockwood, 2007; Tommasi and Weinschelbaum, 2007). Besides, deviations from the assumptions of heterogeneity across regions and a central government, only being able to provide a uniform public good level could also lead to a failure of the theorem (Lockwood, 2002; Besley and Coate, 2003).

As the sample of studies in Table 1 illustrates, fiscal decentralization has been subject of extensive empirical testing. These analyses have investigated the impact of decentralization on economic growth, government size, educational attainment, corruption, environmental quality and life satisfaction - just to name a few. Given this ample number of studies, it is surprising to find that no research has been published on the decentralization of labour market institutions. Closing this gap is the first major contribution of this paper.

**Table 1: Empirical studies of fiscal decentralization (Selection)**

<b>Dependent variable</b>	<b>Paper</b>	<b>Sample</b>	<b>Effect of decentralization</b>
Economic growth	Davoodi and Zou (1998)	46 countries	Contingent on country type
	Akai and Sakata (2002)	US states	Contingent on indicator
	Stansel (2005)	US metropolitan areas	(+)
	Thornton (2007)	19 OECD countries	None
Government size	Oates (1985)	US States, 43 countries	None
	Jin and Zou (2002)	32 countries	Contingent on indicator
	Baskaran (2011)	18 OECD countries	(+)
Corruption	Fisman and Gatti (2002a)	59 countries	(-)
	Fisman and Gatti (2002b)	US states	Contingent on policy
	Dincer (2010)	US states	(-)
Educational outcomes	Barankay and Lockwood (2007)	Swiss cantons	(-)
Shadow economy	Teobaldelli (2011)	73 democracies	(-)
Environmental quality	Sigman (2007)	145 countries	None
Life satisfaction	Bjornskov et al. (2008)	66,000 individuals	(+)

Note: (+) represents a positive effect, (-) a negative effect on the dependent variable

Empirical studies usually employ cross-country comparisons, whilst studies using single countries other than the US as their unit of observation are rare<sup>4</sup>. Especially Germany, despite being the largest European economy, has hardly ever been studied. This is also contrary to the fact that the examination of a single country is favourable due to the necessity of cross-country studies to account for the vast heterogeneity among countries. Therefore, enlightening the case of decentralization for Germany and within a single country is my second contribution.

Typical empirical explorations of fiscal federalism use measures of subnational revenue or government expenditure shares as indicators for decentralization. However, these shares do not necessarily reflect the actual allocation of political power since revenues and expenditures might be subject to federal restrictions. The examination of an actual and non-budgetary event of devolution is contribution number three.

The findings may also be relevant to other countries which have undergone similar decentralizations of social benefit administrations. This includes Denmark (Lindsay and McQuaid, 2008), the US 1996 welfare reform (Blank 2002) and certain elements of the UK welfare reform act 2012 (UK Department of Welfare & Pensions, 2013). In particular, the German example resembles the situation in Canada, where different unemployment provider types with varying degrees of decentralization across provinces exist (Gray, 2003). Yet this coexistence of different models of local autonomy cannot be rigorously evaluated due to its singularity<sup>5</sup>. The lack of evidence from these examples increases the need for evidence from other countries, the fourth gap this paper aims to shrink.

<sup>4</sup> See Zhang (1998), Faguet (2004) or Barankay and Lockwood (2007) for examples.

<sup>5</sup> Each province has to sign an individual bi-lateral labour market development agreement (LMDA) with the federal government

The remainder is organized as follows: Section two introduces the German reforms in long-term unemployment assistance. Section three describes the data and identification strategy. Estimation results are presented in section four while section five concludes and rounds off the discussion.

## 2. The Decentralization of Long-Term Unemployment assistance in Germany

In Germany, a political compromise between the government and the opposition-dominated upper house led to the emergence of a situation very favourable for a quasi-natural experiment. In conjunction with a major unemployment benefits reform in 2005, the organization of long-term unemployment assistance was to be reformed as well. While the government favoured a strong role of the Federal Employment Agency (FEA), the opposition wanted local counties to take sole responsibility of unemployment assistance.<sup>6</sup>

In the end, two distinctive types of Jobcenters, the responsible institution for servicing the long-term unemployed, were created: In the standard case, counties formed joint-ventures of the local administration and the federal employment agency to run their Jobcenters. However, 69 counties experimentally opted to become a local approved provider, i.e. to organize their Jobcenters independent of the federal agency. Nevertheless, both types received equal financial support by the federal government and underlay the same law. To ease the process of decentralizing and create a level playing field, the federal government granted 28 million Euro to the 41 affected counties (Deutscher Bundestag, 2012).

In 2008, the government mandated an official evaluation of the two provider types resulting in ambiguous conclusions (Bundesministerium für Arbeit und Soziales, 2008). The evaluation was also made available in two publications by Boockmann et al. (2013) and Holzner and Munz (2012). However, both papers rely on data collected between 2006 and 2007 which is more than 1.5 years after the institutional changes took place. To make things worse, unemployment benefits were reformed at the same time. This is why these evaluations could not gather any observations prior to the institutional reorganization.

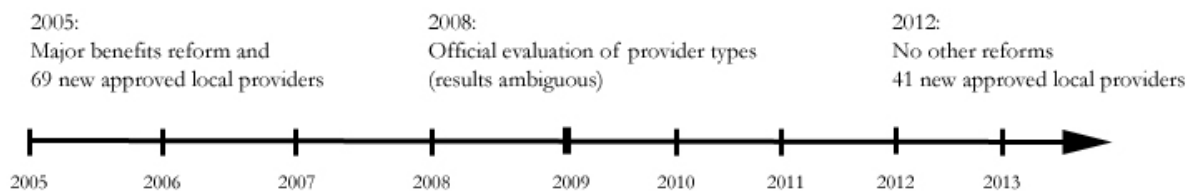


Figure 1: Timeline of events, 2005-2013

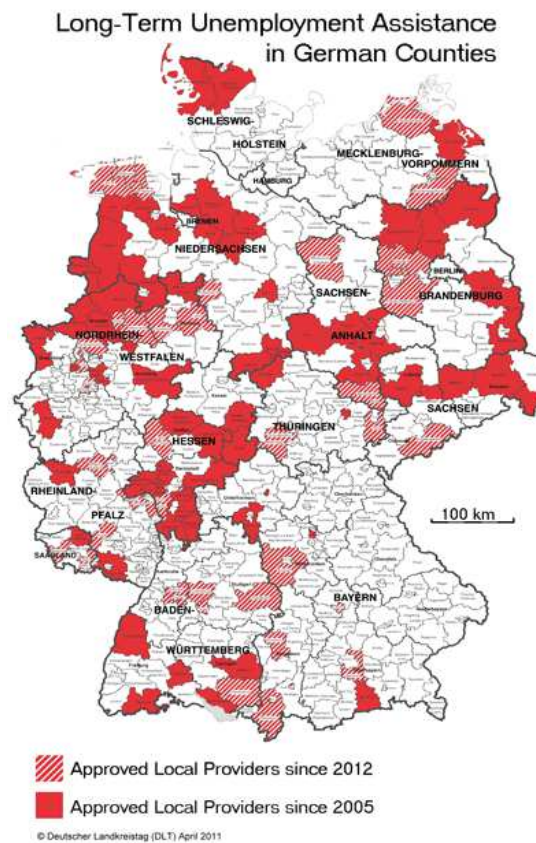
With the governmental evaluation being inconclusive, the initially experimental local approved providers were established permanently. Moreover, in early 2011, another 41 counties were granted permission to fully decentralize their Jobcenters and become local approved providers whose operations started in January

<sup>6</sup> See e.g. Feldkirchen and Sauga, 2005. Das Reform-Monster. *Der Spiegel*, 1/2005, p. 24.

2012. This event is going to be the focus of this paper because there were no simultaneous unemployment benefit reforms. Moreover, a large number of counties whose decentralization status remained unchanged are available as a control group. The decentralization to be studied is then interpreted as the “treatment” or “intervention” of interest.

The analysis here will need to address potential endogeneity since the selection into treatment was not random but a three-step process: First, interested counties had to submit an application to their respective state ministry of labour to become local approved providers. Secondly each state was allocated a certain number of slots for which the state ministries then nominated counties from the application pool. Finally, these nominations had to be approved by the federal government.

**Figure 2: Provider Types of Long-Term Unemployment Assistance (Source: German County Association)**



The goal of this paper is to test whether the decentralization of Jobcenters in 2012 improved the labour market performance in the affected counties as suggested by the Decentralization Theorem.

The gains from decentralization may be dominating in this case study for the following reasons: Decentralized Jobcenters could have superior information to adapt their assistance more efficiently to local labour market conditions which can be expected to be very heterogeneous across Germany. Additionally, these counties are able to realize synergies with other services that they already provide such as regional business promotion, housing assistance and other social infrastructure. Besides it seems realistic that policy innovation and policy diffusion play a major role as the German county associations run a common

“Benchlearning” scheme among local approved providers to promote these practices. The following sections are going to explore whether the hypothesis can be supported by the data.

### 3. Data and Identification Strategy

#### 3.1 Data

The analysis will be based on monthly unemployment data aggregated at the Jobcenter level (territorial status as of June 2013). It was compiled by the German Federal Employment Agency solely for the purpose of this study<sup>7</sup>.

The observations range from January 2009 to December 2013. That is, the data includes three years of the pre-treatment and 2 years of the post-treatment information. The main variables incorporated in the data set are the levels of civilian labour force (ILO definition<sup>8</sup>), short-term unemployment and long-term unemployed for all districts. Each of the components can be further specified by gender, five different age groups and nationality (German/foreign)<sup>9</sup>. This means that for each of the 410 Jobcenter districts, observations are available on nine subgroups over 60 periods. The inclusion of labour force variables allows calculating easily the unemployment rates corresponding to these statistics. A dummy on the districts’ decentralization status was compiled manually from sources by the FEA and German county association (Deutscher Landkreistag, 2012).

Certainly, it would be beneficial to work with individual level data. However, as the treatment varies only across Jobcenters, individual observations within districts cannot be assumed to be independent as emphasized by Moulton (1990) for instance. In such a case, standard errors needed to be clustered at the Jobcenter-level to correct for that bias. This effectively reduces the number of observations to the 410 districts included in this data set which is why the use of aggregated data is not a huge disadvantage after all.

Table 2 presents three time series of unemployment rates for different selections of districts. It allows getting a first impression of the labour market situation for long-term unemployed since 2009 and will be further discussed in the following section with its relevance for the identification strategy.

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<sup>7</sup> Bundesagentur für Arbeit (FEA), Arbeitsmarkt in Zahlen. Nürnberg (Germany), June and August 2013.

<sup>8</sup> The civilian labour force comprises the total labour force minus armed forces. See ILO (1982).

<sup>9</sup> Two subgroups have a few missing observations which only concerns the pre-treatment period, however.

**Table 2: Long-term unemployment 2009-2013**

Year	All Districts	Districts by type	
	Decentralizing + Others	Decentralizing	Others
2009	5.09%	5.06%	5.06%
2010	4.91%	5.09%	4.89%
2011	4.67%	4.83%	4.65%
2012	4.46%	4.79%	4.42%
2013	4.47%	4.77%	4.44%
# of districts	410	43	367

Sources: German Federal Employment Agency, own calculations

Notes: Long-term unemployment ratios are annual averages. Dashed lines indicate the beginning of the treatment.

### 3.2 Identification Strategy

The paper focuses on the 41 counties that changed their status at the beginning of 2012 from joint-ventures with the Federal Employment Agency to independent local approved providers. This change in status will be interpreted as the “treatment” while the Jobcenters whose organization did not change serve as the control group. In addition, two Saxonian counties which extended their decentralized Jobcenters after county mergers to the entire new county area in January 2012 are coded as “treated” too.<sup>10</sup>

An initial inspection of summary statistics as given in Table 2 indicates a general decline of long-term unemployment from about 5.1% to 4.5% between 2009 and 2013. With regard to the decentralizing districts, this process came to a halt as early as 2012 (the time when the treatment occurred) and remained constant thereafter. Unchanged districts though were still experience declining long-term unemployment in 2012. Nonetheless, this relative increase in unemployment between the two groups cannot be interpreted causally as it could be the result of confounding factors other than decentralization.

The special constellation of two provider types simultaneously operating under the same law offers a reasonable starting point for a quasi-natural experiment. In an ideal experiment, selection into treatment would be randomized and the effect of decentralizations should be independent. However, this is clearly not the case in this event study as the decentralizing counties were selected in a political process<sup>11</sup>. Also, there may be interdependencies within counties over time and among counties in a spatial sense. This is why we need a rigorous identification strategy to make causal inferences.

<sup>10</sup> Including these two districts in the treatment group may attenuate the results but increases the credibility of all coefficients still estimated to be significant.

<sup>11</sup> See Section 2 for a detailed description of the process.



Problems may arise if there is a systematic difference between the treatment and control group. One such difference may result from a different regional distribution of the two groups. Thanks to the special allocation process of the rights to decentralize Jobcenters, the geographic distribution of the two groups seems to be fairly even<sup>12</sup>. Even so, there may be systematic differences arising from other dimensions. Table 3, being effectively a close-up of Table 2, offers insights about the composition of long-term unemployment. It reveals that in 2011, the year before the intervention, long-term unemployment was slightly higher in decentralizing counties for all characteristics considered. Thus, the treatment is indicated not to be “as good as randomly assigned” and the research design will need to account for this problem.

**Table 3a: Long-term Unemployment ratios at the transition from**

(Sub-)Group	All Districts		By Type of District			
	Decentr. + Control		Decentralizing		Control	
	2011	2012	2011	2012	2011	2012
Total	4.67%	4.46%	4.83%	4.79%	4.65%	4.42%
Men	4.63%	4.44%	4.76%	4.71%	4.62%	4.41%
Women	5.24%	5.03%	5.40%	5.35%	5.22%	4.99%
15-25 years	3.36%	3.28%	3.50%	3.62%	3.34%	3.24%
25-35 years	6.35%	5.95%	6.49%	6.37%	6.33%	5.90%
35-45 years	4.69%	4.49%	4.76%	4.75%	4.68%	4.46%
45-55 years	4.94%	4.58%	5.15%	4.88%	4.92%	4.54%
55-65 years	4.25%	4.19%	4.48%	4.59%	4.22%	4.15%
German	4.25%	4.02%	4.36%	4.28%	4.23%	3.99%
Foreign	12.05%	11.45%	12.31%	11.58%	12.02%	11.43%

Sources: German Federal Employment Agency, own calculations

Notes: Long-term unemployment ratios are annual averages. Dashed lines indicate the beginning of the treatment.

The first step to estimate a causal effect will be to employ a difference-in-differences (DD) estimation with district fixed effects. On the one hand, the DD-estimator will control for trends common to treatment and comparison group. On the other hand, the fixed effects capture (unobserved) heterogeneity among counties. However, this approach still leaves the problem of time-varying unobserved confounding factors and state-specific shocks which may bias estimation results. It can be argued though that omitted time-varying covariates are not expected to be crucial due to the relatively short period of observation (6-18 months).

<sup>12</sup> Each state was allocated the rights to choose a certain number of decentralizing counties according to its voting weight in the German upper house, reflecting the size of a state. More details in Section 2.

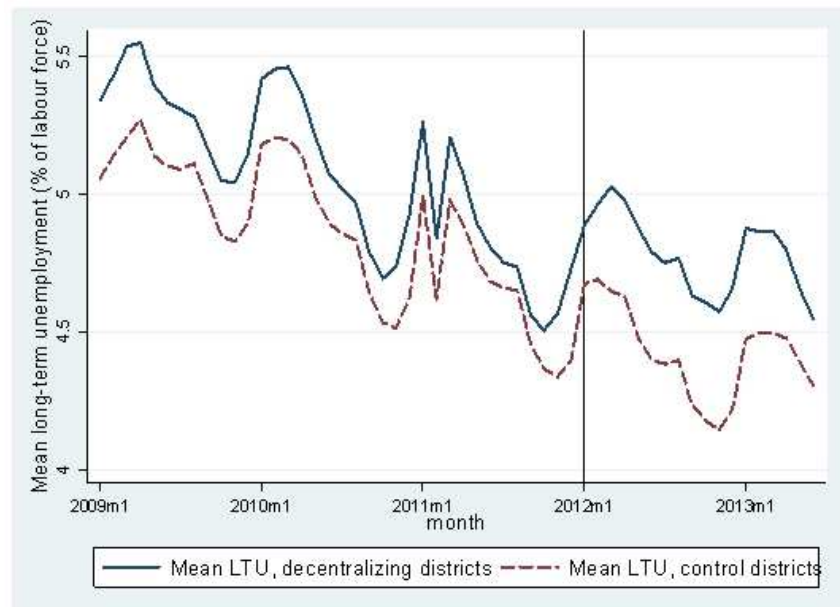
Thirdly, the synthetic control method as pioneered by Abadie and Gardeazabal (2003) which extends the DD-framework may offer a remedy for omitted time-varying covariates<sup>13</sup>. The idea is that creating a synthetic counterfactual for each district in the treatment group from a convex combination of control districts leads to a more suitable comparison group. Detailed specifications and results are outlined below.

## 4. Estimating the Causal Effect of Decentralization

### 4.1 Difference-in-Differences Estimation (DD)

The basic assumption necessary for causal inference with a difference-in-differences estimation is that treatment and control group had followed a common trend prior to the beginning of the treatment. The available data prior to the year 2012 in Table 4 illustrates the validity of this assumption.

Table 4: Common trend of treatment and control districts. (LTU = Long-term unemployment)



Also, it is required that no other relevant policies changed at the same time with the treatment. Unlike in 2005, there were no other simultaneous changes in the labour market regulations which is why this assumption is seen as satisfied.

The general regression equation can then be estimated using OLS and has the following form, providing an estimate of the average treatment effect (ATE):

$$Y_{gist} = \alpha_i + \lambda_g + \gamma Post_t + \delta(Post_t \times Dct_s) + \varepsilon_{gist}$$

$$Y_{ist} = \log(\text{Long-term unemployment of subgroup } g \text{ in county } i \text{ with status } s \text{ at time } t)$$

<sup>13</sup> Another remedy would be the use of an instrumental variable. Following Holzner and Munz (2012), the possibility of using the membership of county presidents in federal county association committees has been explored. However, due to a very weak first-stage relationship, this approach has not been further studied.

$\alpha_i$  = District Fixed Effects  
 $\lambda_g$  = Subgroup Fixed Effects  
 $Post_t$  = Postreform dummy (1 in all periods after December 2011, 0 otherwise)  
 $Dct_s$  = Decentralization Dummy (1 for decentralizing districts, 0 otherwise)  
 $(d_t \times local_s)$  = "Post x Treated" Interaction term  
 $\varepsilon_{gist}$  = Error term

In this regression,  $\delta$  will be the causal coefficient of interest if the model is correctly specified. To make full use of the available data, I stack all observations for each subgroup of unemployed (total, male, female, ... , foreigners) and extend the observations for independent variables accordingly. Including indicator dummies ("Subgroup Fixed Effects") account for the differences among these subgroups. A second regression with "subgroup times decentralization"– interactions added enables the treatment effect to vary among subgroups. District fixed effects are included to substitute for all time-invariant district-specific covariates and the post-dummy allows the regression to vary in the post-decentralization period.

Using the logarithmic transformation of long-term unemployed levels is sensible because the outcome variable distribution would be highly positively skewed otherwise. The log symmetrises the residuals and therefore allows testing based on the normal distribution.<sup>14</sup>

The unemployment variable can be expected to be serially correlated over time and also within the different subgroups in each district. Not accounting for this problem may result in unbiased point estimates but in understated standard errors as described by Bertrand et al. (2003) and others. To accommodate the above, Huber-White standard errors are used throughout. Since there are two nested dimensions of potential serial correlation, standard errors are clustered at the most aggregate level (i.e. district level) as advised by Cameron et al. (2011).

**Table 5: DD estimation results, static model. (LTU=Long-term unemployment)**

VARIABLES	(1) Log(LTU)	(2) Log(LTU)
Postreform Dummy	-0.100*** (0.00480)	-0.104*** (0.00468)
Post x Treated Interaction	0.00604 (0.0188)	0.0361** (0.0143)
Subgroup x Treated Interactions	No	Yes
District Fixed Effects	Yes	Yes
Period Fixed Effects	Yes	Yes
Observations	221,378	221,378
R-squared	0.937	0.937

Ordinary Least Squares estimates given. Clustered standard errors in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

<sup>14</sup> See Appendix A for a graphical demonstration.

Table 5 summarizes the most important results of the estimation. It is thereby important to keep in mind that all coefficients represent percentage effects on long-term unemployment due to the transformation of the outcome variable. The causal effect of decentralization is only significant when allowing for subgroup-specific treatment effects (column 2). However, these specific treatment-subgroup effects are not significant individually (not shown in this table). Contrary to the initial hypothesis, the coefficients of interest have a positive sign and in the second estimation indicate a long-term unemployment increase by 3.6 percent caused by decentralization (column 2). The negative and significant postreform-coefficients confirm the deductions made from Table 3, showing an overall decline in long-term unemployment figures.

To check the robustness of the results, the previous regressions are re-run using short-term unemployed as the outcome variable. Since short-term unemployed did not experience a change in their assistance provider type, the causal coefficient of decentralization should not be significant anymore. It turns out that the desired outcome is only partially obtained (see Appendix B). After the inclusion of subgroup-specific treatment effects (column 2), the coefficient of interest is significant at the 10% level, somewhat weakening the credibility of the DD analysis. Then again, the estimated placebo effect is smaller in magnitude and the significance still does not reach the levels of the long-term unemployment coefficient. Therefore I do not retract the previous results but employ alternative approaches to further authenticate their claims.

So far, Jobcenter decentralization was assumed to have an immediate impact right after the intervention occurred and none beforehand. This assumption will be relaxed in the following to allow for anticipation and delayed effects. Adding leads and lags of the treatment decomposes the single treatment effect estimated in the previous analysis into multiple, time-dependent effects. The impact caused by decentralization could now be accelerating, stabilizing or mean-reverting. Technically, the regression equation now has a slightly modified form:

$$Y_{gist} = \alpha_i + \lambda_g + \tau_t + \sum_{j=-17}^{36} \delta_j Dct_{st}(t = k + j) + \varepsilon_{gist}$$

$Y_{ist}$  = log( Long – term unemployment of subgroup  $g$  in county  $i$  with status  $s$  at time  $t$  )

$\alpha_i$  = District Fixed Effects

$\lambda_g$  = Subgroup Fixed Effects

$\tau_t$  = Period (Year – Month) Fixed Effects

$Dct_{st}$  = Decentralization Lag/Lead (1 for decentralizing districts in  $t$ , 0 otherwise)

$k$  = time at which treatment is switched on, here  $k = 0$  is January 2012

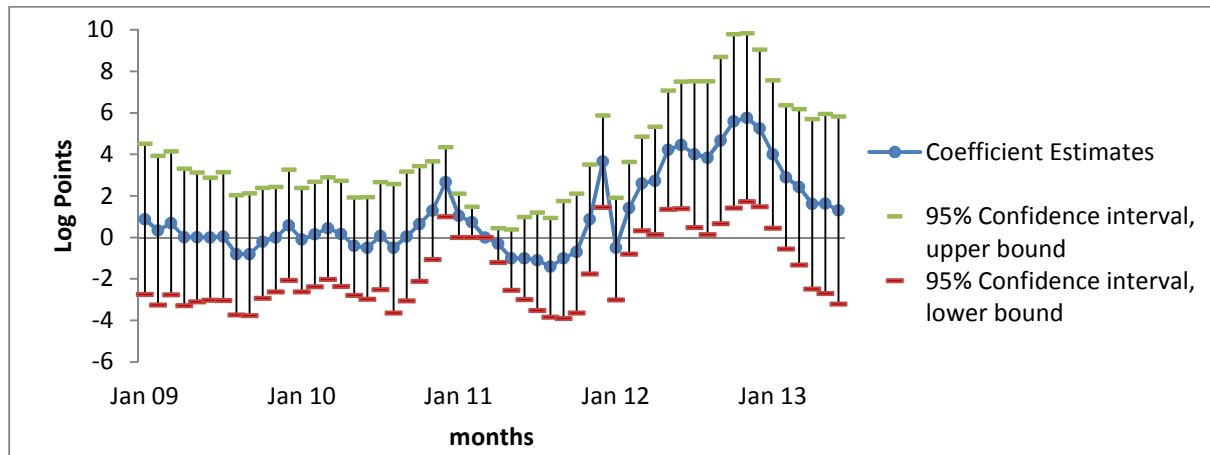
$\varepsilon_{gist}$  = Error term

As previously,  $\delta_t$  is the causal coefficient of interest. However, this time, there is a coefficient specific to each period to study the dynamics of the intervention. To do so,  $Dct_{st}$  equals to one in only one year each per decentralizing county. The omitted category (to avoid perfect collinearity) is March 2011, the month in which German state governments announced the winning decentralizing counties. An alternative estimation

is performed with December 2011, the last month before the decentralization, as a baseline. Year-month fixed effects capture time trends and seasonality in the data. Standard errors are again clustered at the county-level and estimations performed by Ordinary Least Squares.

Graphical representations of the results are presented in “Tables” 6, tabular results are reported in Appendix C to save space and avoid redundancy.

**Table 6: DD dynamic model, March 2011 as omitted category**



The dynamic model confirms a significantly positive effect of decentralization on long-term unemployment. Especially during the first year after the intervention, many monthly lead-/lag-coefficients are significant at the five percent level and quantitatively noteworthy (about four Log Points). However, the effect stabilizes after the first year. Moreover, there is a significant anticipating effect in January 2012, leading to another increase in long-term unemployment for decentralizing states. This might be due to a shortage of resources during the transition process which may require some Jobcenter employees to take care of other responsibilities related to that transition. These resources may be lacking elsewhere in the assistance and support of long-term unemployed, leading to the increase shown above.

If the common trend assumption is supposed to be confirmed, all leads coefficients of the decentralization should not be significantly different from zero. This implies that there is no significant difference in trends between the two groups in the absence of a treatment. Table 6 reveals a partial violation of the assumption in December 2010 for instance. However, in the great majority of pre-treatment periods, the assumption is generally satisfied. Consequently, retaining the assumption still seems to be a fair way of proceeding.

An issue to be kept in mind when running DD estimations is the problem of measurement error. In particular, the use of fixed effects may accentuate this error because these do not only remove “bad” noise but also “good” signals. Since the observations employed in this paper are based on complete count data by the FEA, this problem should not be too severe. Critics could argue though that in the 2005 round of Jobcenter decentralization, unemployment statistics were severely flawed or simply unavailable due to the hectic transition process in affected counties. However, the federal government reports that in 2012, such

problems could be minimized (Deutscher Bundestag, 2012). Even if there was some substantial measurement error left, its effect would be an attenuation of resulting estimates. This implies that coefficients still estimated as being significant bear a higher credibility. In consequence, the paper does not further elaborate on this error.

## 4.2 Synthetic Control Method

To fully take time-invariant changes into account, the final stage of the empirical analysis is to apply the synthetic control method of Abadie and Gardeazabal (2003) for comparative case studies to the decentralization of German Jobcenters.

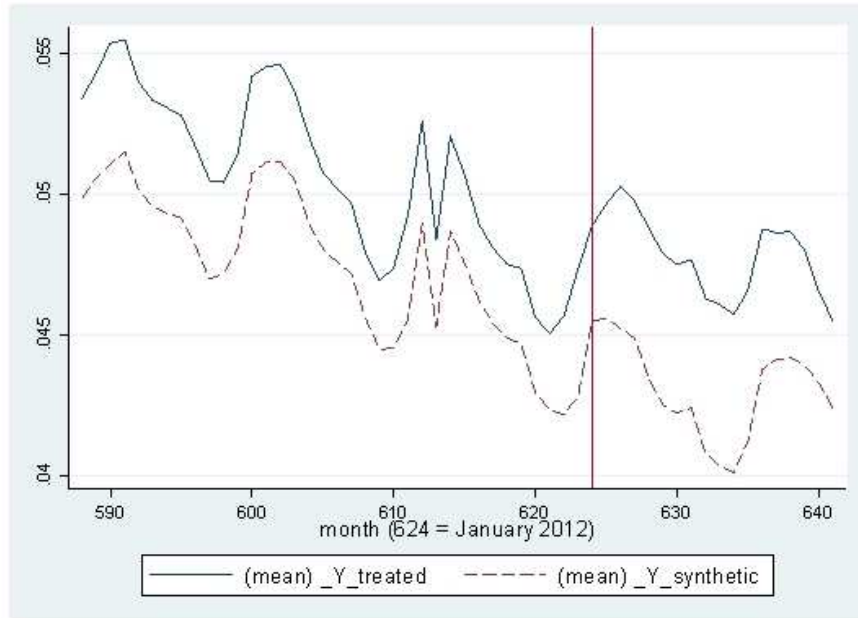
The method is a data-driven approach that assigns weights on linear combinations of variables from unaffected regions to construct a counterfactual based on data for the pre-intervention period. Weights are chosen to minimize the Mean Squared Prediction Error (MSPE) being the average of the squared differences between synthetic and original unit. The resulting artificial control district can then be used to extrapolate the hypothetical evolution of long-term unemployment in treated i.e. decentralizing counties for the post-treatment period. To obtain a valid synthetic control region, the assumption of no interference among counties needs to be satisfied. This is likely to be fulfilled here as the most apparent interdependency, interregional mobility of people, is hardly expected to change because of assistance decentralization, nor is it particularly encouraged by Jobcenters.

Ideally, I would envisage using all non-decentralizing counties as potential controls (i.e. the donor pool). Unfortunately STATA is unable to process 350+ units in the donor pool. Therefore, all geographical neighbours of each treated districts were manually identified and fed into a district-individual donor pool. This practice is also justified by an argument made in Abadie et al. (2010). The authors claim that “researchers trying to minimize biases caused by interpolating across regions with very different characteristics may restrict the donor pool to regions with similar characteristics”. Assuming that neighbouring districts are more similar to the decentralizing district than the average of all non-treated districts may thus be favourable in terms of potential interpolation bias.

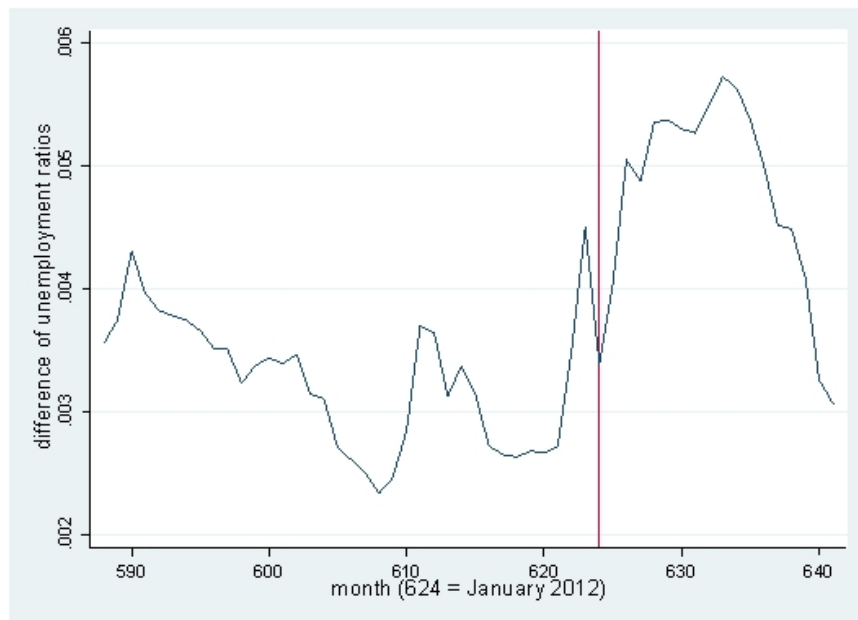
To avoid problems arising from different county sizes, the ratio of long-term unemployment relative to total labour force per district is used as an outcome variable instead of absolute values. As predictor variables, the largest subgroup of each characteristic was chosen (male, 45-55 years old, German) to derive the weight of each neighbouring county in the convex combination.

Finally, using the STATA extension SYNTH<sup>15</sup>, I estimated a synthetic control district for each county. Averaging those counties and their counterfactuals produces the following graphs.

**Table 21: Evolution of long-term unemployment ratios in actual and synthetic counties**



**Table 32: Average difference of unemployment ratios between actual and synthetic districts**



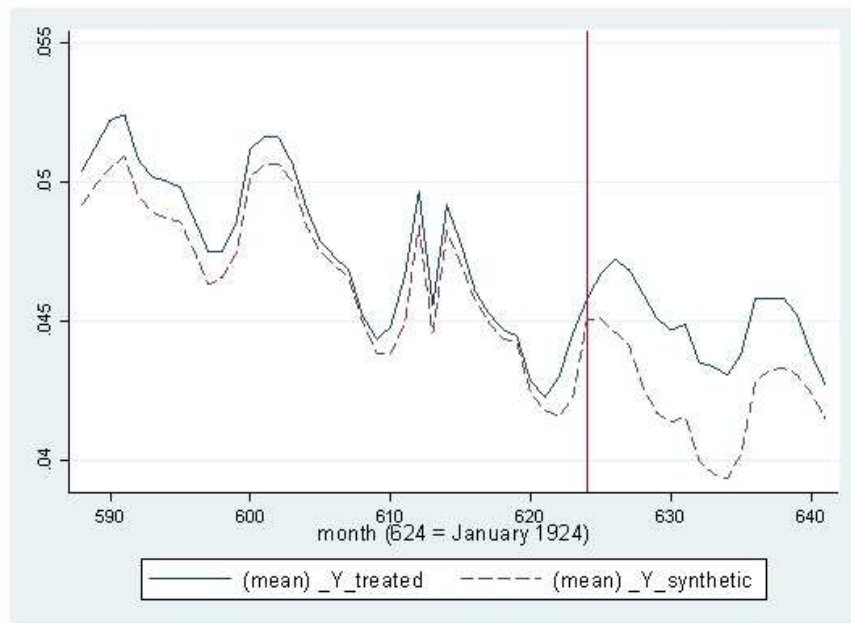
From the previous two figures it can be inferred that there is a noticeable gap between the treated and the synthetic districts, indicating a suboptimal match of the two groups. For a few counties, geographically

<sup>15</sup> Abadie, Alberto, Diamond, Alexis and Hainmueller, Jens, (2013), SYNTH: Stata module to implement Synthetic Control Methods for Comparative Case Studies.

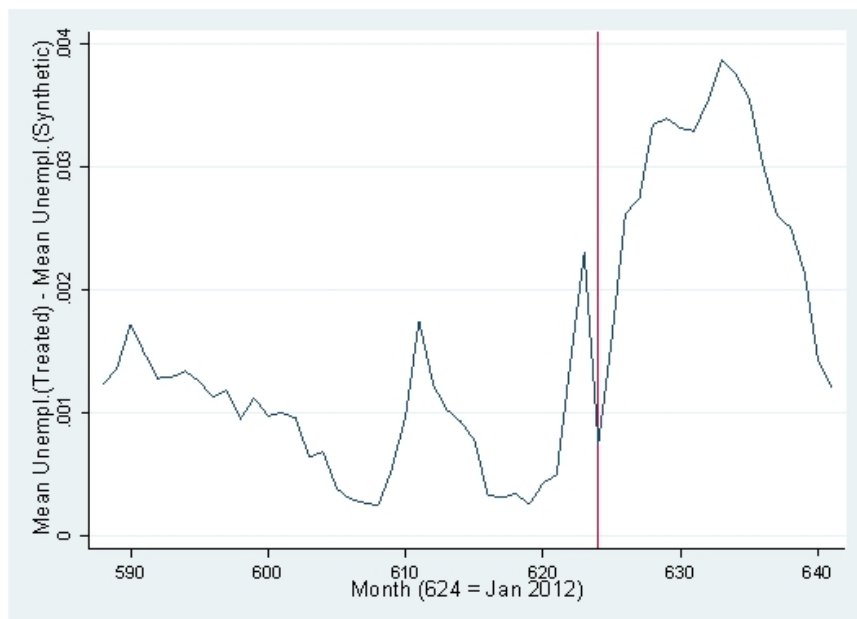
neighbouring districts are revealed to be very inadequate counterfactuals. In particular, there will be a disparity for counties whose neighbours (i.e. potential controls) collectively exhibit either higher or lower long-term unemployment ratios. In such a case there will be no convex combination of unaffected neighbouring districts to generate a hypothetical control which behaves identical to the actual district.

To reduce this problem I repeat the entire process once more and this time exclude the 10 % (i.e. four) counties that produced the highest MSPEs. The rationale is that the MSPE is a measure of the goodness of fit between treated and synthetic control district and excluding districts with bad counterfactuals dilute results.

**Table 13: Evolution of long-term unemployment ratios in actual and synthetic counties (four counties excluded)**



**Table 44: Difference of unemployment ratios (four counties excluded) between actual and synthetic districts**





The resulting new graphs reveal a much closer resemblance between treated districts and their synthetic counterparts. Besides, they reinforce the conclusions of the DD-analysis: Once again, long-term unemployment displays an increase in the months following the decentralization. Yet after about one year, this drift is reversed and the unemployment rate in treated district is quickly approaching the unemployment levels of their control group again. Around its peak, the long-term unemployment in decentralizing district seems to be between 3% and 4% above control group levels which is slightly higher than the upshot estimated in the DDD specification. Furthermore, the graph confirms a notable anticipation effect as indicated previously by the dynamic DD estimation.

Given time, an interesting extension would be to compare these results with placebo treatments in several non-intervention months. Alternatively, the placebo treatment could be run by leaving the treatment timing unchanged but using districts in the treatment group whose Jobcenters actually did not change their provider type. Ideally, these graphs then should not exhibit any irregularities when the placebo is switched on since no actual reform has taken place. Consequently, the additional estimations could provide a suitable robustness check and allow more sophisticated inferences.

## 5. Conclusion and Discussion

Long-term unemployment is a serious problem whose importance has not been properly addressed in the fiscal federalism literature. Furthermore, typical empirical papers of that literature rarely employ within-country studies and seldom examine actual events of decentralization. Opposite to that, this paper exploits the decentralization of 41 German Jobcenters in 2012.

The analysis is based on a unique data set compiled by the Federal Employment Agency (FEA), covering official unemployment statistics at the level of the (in total 410) German Jobcenter districts. On this foundation, the paper applies a difference-in-differences framework to identify the causal effect of decentralization on long-term unemployment. Starting from a simple DD regression model using fixed effects, I then proceed to consider more sophisticated approaches. These approaches include dynamic versions of the DD regression, triple-differences estimations as well as the synthetic control method.

Remarkably, all techniques employed yield a coherent picture of the causal effects. Moreover, the results provide clearer orientation for policy-making and consistently extend the perspective of the 2008 governmental evaluation.

The results question the validity of Oates' Decentralization Theorem with respect to labour market institutions. Even 18 months after the reform, there are no gains from decentralization in sight. Fears of foreigners being disadvantaged by decentralization could not be confirmed either. Nevertheless, the process of decentralization seems to come at a significant cost. Dynamic DD and synthetic control estimations both indicate a significant increase in unemployment in the first year following the change in provider type. This may well be explained by the necessity that Jobcenter employees first need to get familiar with new

organizational structures, bureaucratic procedures or new computer software. As a consequence, they are able to spend less time on assisting long-term unemployed, leading to the result described above. It also implies that the additional resources made available by the government<sup>16</sup> were insufficient to create a level playing field.

However, it remains unclear how decentralizing Jobcenters affects unemployment in the long-term. Since this analysis only included 2 years of post-treatment data, it would be interesting to investigate whether the described trends reverse at a later point in time. Richer data may also allow employing a dependent variable that reflects the performance of Jobcenters more directly than overall unemployment statistics (e.g. job-finding rates). Also, this analysis does not offer insights about the type of employments people actually pursued after dropping out of the Jobcenter statistics. These open questions clearly reflect the need for more research on this topic. The decentralization of welfare institutions may still be a considerable idea after all.

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<sup>16</sup> See section two for details.

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# Appendix

## Appendix A. Difference-in-Differences Estimation: Rationale for Using the Depend Variable's Logarithmic Transformation

Table 15: Before transformation - skewed distribution

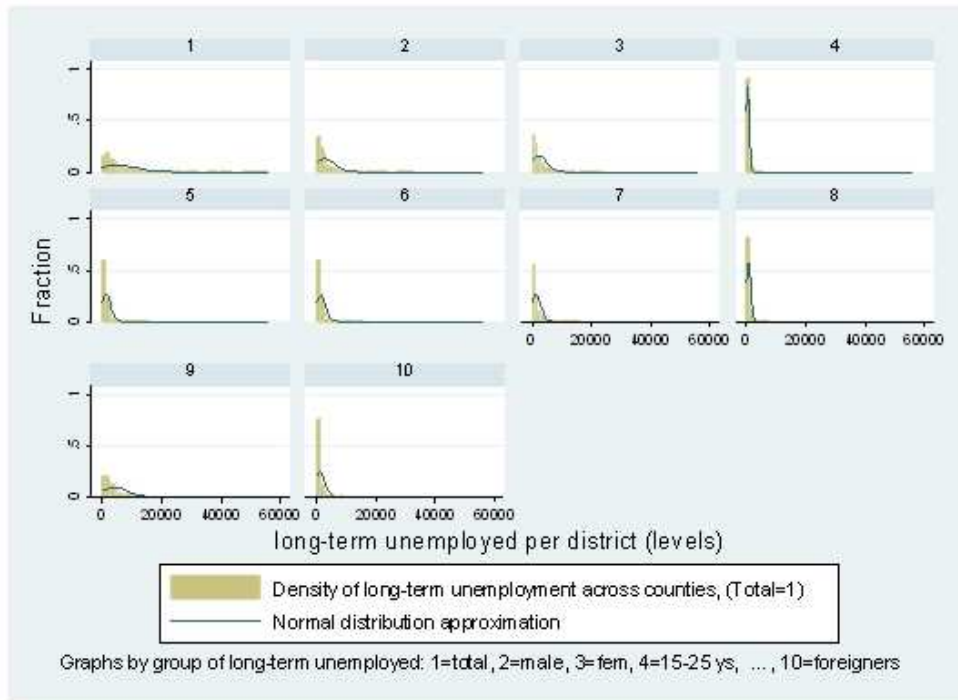
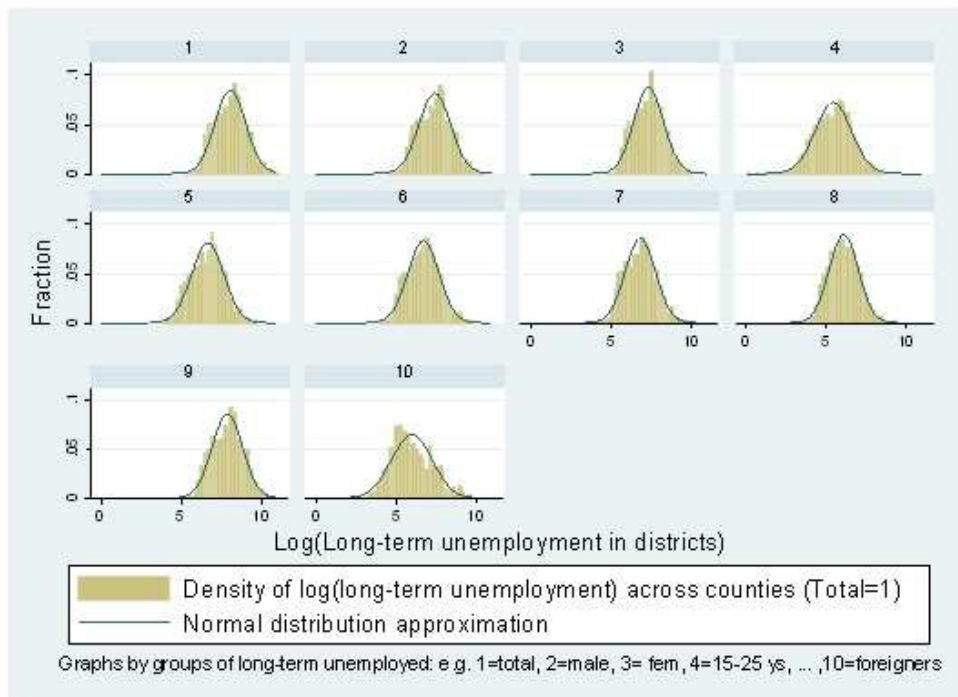


Table 16: After transformation - approximately normal



## Appendix B. Difference-in-Differences Estimation: Robustness Check

Table 17: DD estimation results, static model, robustness check (STU = short-term unemployment)

VARIABLES	(1) Log(STU)	(3) Log(STU)
Postreform Dummy	-0.100*** (0.00390)	-0.103*** (0.00356)
Post x Treated Interaction	-0.00241 (0.0192)	0.0213* (0.0124)
Subgroup x Treated Interactions	No	Yes
District Fixed Effects	Yes	Yes
Period Fixed Effects	Yes	Yes
Observations	221,399	221,399
R-squared	0.922	0.922

OLS estimates given. Clustered standard errors in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$