Job Search Autonomy

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Abstract: This paper studies the effects increasing the job search autonomy of unemployment workers. We exploit a policy change in the Swiss canton Bern, which strongly reduced search requirements and abolished mandatory vacancy referrals. Using detailed administrative data, we find that the policy change led to a reduction and narrowing of average job search. We set up a difference-in-differences design to estimate effects on labor market outcomes. Our results show that the policy change increased the average duration of unemployment spells in Bern by about 8%, while increasing average re-employment earnings by about 3%. Moreover, we find the effects to vary with the relative importance of changes in individual effort versus changes in local labor market tightness.

1 Introduction

Modern unemployment policies tend to follow a paternalistic approach when it comes to the provision of job search effort. In many countries, Public Employment Services (PES) restrict the choice of effort through minimum application requirements and mandatory vacancy referrals. Such restrictions on the decision of how much and how broadly to search are commonly motivated by the concern that job seekers might under-provide search effort, for instance due to the disincentive effects of unemployment insurance (e.g., Schmieder, Wachter, and Bender (2016)) or behavioral phenomena, such as over-optimism (e.g. Spinnewijn (2015); Mueller, Spinnewijn, and Topa (2021)) or hyperbolic time preferences (e.g. DellaVigna and Paserman (2005)).

The degree of autonomy left to job seekers is a controversial policy choice. On the one hand, search effort is a key input to the process that matches unemployed individuals to jobs. Therefore, enforcing high effort may successfully foster labor market re-integration. At the same time, restrictions on search effort carry the risk of reducing intrinsic motivation and self-efficacy.[1] Moreover, effort restrictions involve an important general equilibrium dimension. Making all job seekers in a given labor market exert high effort will affect labor market tightness if job creation does not fully adjust. Depending on the relative importance of changes individual effort versus changes in labor market tightness, it is unclear how the large-scale enforcement of search effort ultimately translates into job finding. Finally, there potentially exists a trade-off between the speed of job finding and job quality. If job seekers use their autonomy to successfully direct their search towards higher paying jobs, this can have meaningful consequences for the individual and fiscal trade-offs involved.

In this paper, we provide comprehensive empirical evidence on the labor market effects of a large-scale policy change which reduced job search restrictions. The policy change *BernTop!* was implemented over the year 2012 in the Swiss canton Bern. It had the declared goal to promote the autonomy and self-efficacy of unemployed job seekers. Moreover, it aimed at reducing the administrative burden and improving the image of the PES. This resulted in a substantial reduction in the number of required job applications that job seekers faced. Job search requirements decreased by about 25% on average and mandatory vacancy referrals were almost completely abolished. This translated into a roughly proportional decrease in provided job applications. Moreover, job seekers reduced the occupational broadness of their job search, focusing on a smaller set of different occupations.

We estimate the reform's effect on labor market outcomes based on individual-level data from Swiss unemployment insurance and social security records. We set up a difference-in-differences framework, using job seekers registered in the rest of Switzerland as the control group. We estimate the effect of the policy change on job seekers' search behavior, duration of unemployment and post-unemployment earnings. For all outcomes, pre-reform trends of job seekers inside and outside Bern evolved in a parallel way.

We first analyze how the average duration of unemployment spells was affected. We find that the reform-induced reduction in effort comes along with an increase in the length of unemployment spells by about 14 days (8%) on average. To fix ideas about the forces underlying this average effect, we discuss a simple conceptual framework. The framework illustrates the decomposition of the overall reform effect into a behavioral effect due to the decrease in individual effort and a tightness effect due to the decrease in aggregate effort. Moreover, the framework yields implications regarding the expected reform effect under different circumstances, which

[1] For evidence on intrinsic motivation, self-efficacy and self-regulation in the job search process, see Bandura (1977), Zimmerman, Boswell, Shipp, Dunford, and Boudreau (2012) and Guan, Deng, Sun, Wang, Cai, et al. (2013).

we assess with analyses of heterogeneity. First, we expect a higher decrease in job finding among job seekers whose individual effort decreased more strongly due to the reform. This conjecture is strongly supported in the data, where we observe that effects are most pronounced among job seekers whose effort is predicted to decrease most strongly due to the reform. Second, we expect job finding to react more in local labor markets with a higher initial vacancy-to-unemployed ratio. In the data, we we find suggestive evidence of stronger effects in labor markets with a high pre-reform vacancy-to-unemployed ratio. Third, we expect job finding to decrease more in local labor markets where the expected change in tightness due to the reform is lower. In line with this notion, we observe that the effect is strongest in labor markets with low commuting time to one of the adjacent cantons. In these markets, tightness is expected to decrease less, due to a higher exposure to commuters from outside Bern, whose search effort remains unchanged after the reform. We also find suggestive evidence that job seekers at the other side of the border benefited from the decrease in effort provided by job seekers in Bern and exited unemployment at a faster rate. Taken together, the evidence suggests that tightness effects play a non-negligible role for the net effect of policies that target search effort.

The analysis of post-unemployment earnings outcomes among job seekers in Bern is preliminary at this stage due to a limited observation window in the social security data. The preliminary results suggest that the increase in autonomy came at the benefit of higher post-unemployment earnings. We estimate that the policy change increased average earnings by two to four log points on average.

In a final step, we use our estimates to discuss the monetary trade-offs related to the reform's average effects. The aim is to quantify how the individual and fiscal costs of longer unemployment spells compare to the benefits of higher post-unemployment earnings. A simple back-of-the envelope calculation suggests that the earnings gains need to persist for about 8 years to offset the fiscal costs of longer unemployment spells . In turn, the worker's individual earnings losses due to longer unemployment are amortized after about 3 years if earnings gains persist.

This study relates to the literature on the effects of unemployment policies. A large body of literature has shown the positive relationship between UI generosity and unemployment duration (e.g., Card and Levine, 2000; Chetty, 2008; Lalive, 2008; Schmieder, Wachter, and Bender, 2012). This empirical relationship usually motivates the control which Public Employment Services (PES) exerts over job seekers' effort in most OECD countries (Venn (2012)). With job search requirements and monitoring being a central measure in the toolbox of the PES, their impact has been studied in a multitude of contexts, with results showing mostly pointing towards a reduced length of unemployment spells (e.g., Johnson and Klepinger, 1994; Meyer, 1995; Klepinger, Johnson, and Joesch, 2002; Ashenfelter, Ashmore, and Deschênes, 2005; Van den Berg and Van der Klaauw, 2006; McVicar, 2008; Manning, 2009; McVicar, 2010; Van den Berg and Vikström, 2014; Arni and Schiprowski, 2019). We contribute with the comprehensive analysis of a large-scale policy change that drastically increased autonomy for all job seekers in a large, well-defined area. This distinguishes our setting from most previous studies, which typically rely on sources of variation that affect only a subset of job seekers in a given labor market. As real-world labor market reforms typically apply to the vast majority of job seekers in a given market, it is key to understand the effect of such large-scale changes, which affect not only individual behavior, but also labor market tightness. As our results show, these tightness effects matter for the outcomes of treated and untreated job seekers competing in the same labor market. As a result, the effect of changes in search effort is likely to be over-estimated when treatment and control group search in the same market.

By accounting for the relevance of tightness effects, we further relate to recent studies which acknowledge and estimate job search externalities. In the context of UI, market externalities have been shown to have an important impact on the optimal level of UI (Landais, Michaillat, Saez, et al., 2010; Landais, Michaillat, and Saez, 2018). Recent empirical evidence addresses this notion by documenting varying externalities of UI extensions (Lalive, Landais, and Zweimüller, 2015; Johnston and Mas, 2018) and job search assistance (Crépon, Duflo, Gurgand, Rathelot, and Zamora, 2013; Gautier, Muller, Klaauw, Rosholm, and Svarer, 2018; Cheung, Egebark, Forslund, Laun, Rodin, et al., 2019). While our study does not have the goal to separately identify the size of job search externalities, it shows that the local scope for search externalities through changes in labor market tightness is decisive for the average effect of large-scale changes in the search intensity of job seekers.

We proceed as follows: In section 2, we describe the institutional setting and data, section 3 describes the policy change and section 5 presents the empirical design and discusses the results. Section 6 discusses the reform's monetary trade-offs and Section 7 concludes.

2 Data & Institutional Background

2.1 Data

Our empirical analysis is based on individual-level data from the Swiss UI registers provided by the Swiss State Secretariat for Economic Affairs (SECO), merged to social security records. The sample covers all unemployment spells starting between 2009 and 2015 of job seekers aged between 28 and 60. Additionally, we use data on the monthly job requirements set by the caseworkers for the cantons Bern, Fribourg, Solothurn and Tessin. These cantons cover about 22% of all UI recipients and three different geographic and language regions in Switzerland (Arni and Schiprowski, 2019). Overall, the dataset covers the date of registration for unemployment, the date and reason for deregistration from unemployment, the number of monthly application requirements in the mentioned cantons, the number and date of official referrals for each spell as well as rich information on socio-demographics and municipality level geolocation data of the place of residence of each job seeker in the dataset.[2] Moreover, we observe the history of pre- and post-unemployment earnings up to the end of 2015.

2.2 The Swiss Unemployment Insurance

In Switzerland, unemployed individuals are entitled to unemployment benefits if they contributed for at least twelve months during the two years prior to unemployment. To be eligible for the full benefit period, the contribution period extends up to 18 months for job seekers up to 55. Usually, the maximum potential benefits duration is 1.5 years for prime age workers, with variation with respect to the job seeker's employment history,

[2]Note that the number of applications reported does not necessarily perfecty reflect actual job search effort. If the marginal costs for job seekers to report their search effort (i.e. reporting one additional application) are significant, then job seekers may only report as many applications as they have to, even if they sent out more. However, we assume that the marginal costs of reporting one additional application are sufficiently low such that job seekers also report applications that exceed the application requirements. This is also supported by our observation that on average, job seekers report more applications than required.

age, and family situation. The replacement rate ranges between 70% and 80% of gross previous earnings, depending on the job seeker's family situation.

The process to claim unemployment benefits is strictly organized. As soon as an individual knows about her (upcoming) unemployment, she registers at the local Public Employment Service (PES) office, called the *Regionale Arbeitsvermittlung* (RAV). After registration, job seekers are assigned to a caseworker. Through regular meetings, caseworkers provide advice and counseling in the search process. Caseworkers also set application requirements and refer job seekers to vacancies according to general guidelines, which are set at the canton level.

2.3 The Application Requirements

The first caseworker meeting usually takes place around two to three weeks after registration. During this meeting, the caseworker sets the first application requirement, that is, the minimum number of monthly job applications which the job seeker must submit to avoid benefit cuts. Job seekers document their application activity in a monthly "protocol of search effort", which includes all types of applications made. The protocols are submitted on a monthly basis to the canton or to the PES office (depending on the canton), where they are collected and registered centrally. Job seekers are required to send in copies of their applications together with the protocols. Upon receiving the protocol, cantons or PES offices record the total number of applications in the central database. Caseworkers are legally obliged to assess whether the provided number of applications satisfies the requirement. They also check whether a minimum quality standard is met. Moreover, caseworkers occasionally verify the truthfulness of reported applications by calling the prospective employer. Once non-compliance with the search requirement is detected, a sanction can be imposed if the job seeker had no special reason or circumstance justifying the non-compliance.

2.4 The Referral Process

Caseworkers can officially refer job seekers to job openings if they believe to have found a fitting match in the PES database. These official referrals are, once made, mandatory to apply to and consist of several forms to be filled out both by the job seeker and the potential employer.

Among policy makers in Bern, the practice of requiring potential employers to give feedback on the applications was perceived as generating a burden to potential employers and leading to fewer employers actually reporting job openings to the PES database. Moreover, the vacancy referrals were considered to worsen the job prospects of job seekers through negative signaling effects.

3 The *BernTop!* Policy Change

Over the course of 2012, the department for economic affairs of the canton Bern enacted a policy change, *BernTop!*, which changed the strategy of the PES. The two main goals were to promote the autonomy and to increase the attractiveness of job seekers for employers, by improving the transparency in the job search process and minimizing "demotivators" in the job search process. Application requirements and vacancy referrals were regarded as the two major demotivators. Their use was decreased substantially.

- One of the most frequently mentioned demotivators and a controversial topic in the PES policy overall were the application requirements. In the official guide to the policy change, the PES notes that “We do not see the expedience of cantonwide application requirements. They do not improve the attractiveness of job seekers for potential employers, do not improve the PES’ image and are inefficient from an administrative point-of-view.” Over the course of *BernTop!*, the application requirements were reduced by approximately 25 %.
- The second “demotivator" tackled was the use of mandatory vacancy referrals. Through *BernTop!*, referrals were almost completely abolished.
- The number and definition of occupations a job seeker declared to search in was changed. Before *BernTop!*, job seekers had a vague catalogue of potential occupations to choose from. Over *BernTop!*, this catalogue was trimmed and job seekers were asked to only fill in occupations they saw as fitting and realistic to achieve.
- The time frame for the policy change was from August to December 2012. During this time, the PES offices trained their caseworkers to adapt the new policy.

3.1 Effects of the Policy Change in the Data

The reform *BernTop!* affected both ongoing and incoming unemployment spells. As a result, job seekers entering in 2013 were fully treated and job seekers entering in the previous months were partially treated. We therefore expect the change in requirements and vacancy referrals to become gradually visible. Panel (a) of Figure 1 confirms this notion. During the years 2010 and 2011, the average requirement amounted to about 9 applications per month in Bern. Over the course of 2012, this number fell down to about 6.5 applications per month for job seekers registered in 2013 or later. Panel (b) reveals that the drop in average application requirements translates to a comparable drop in average reported applications. Over the same time period, requirements and reported applications stayed roughly constant in the other cantons. Appendix Figure C.1 additionally shows how the distribution of requirement and provided applications changed after the reform.

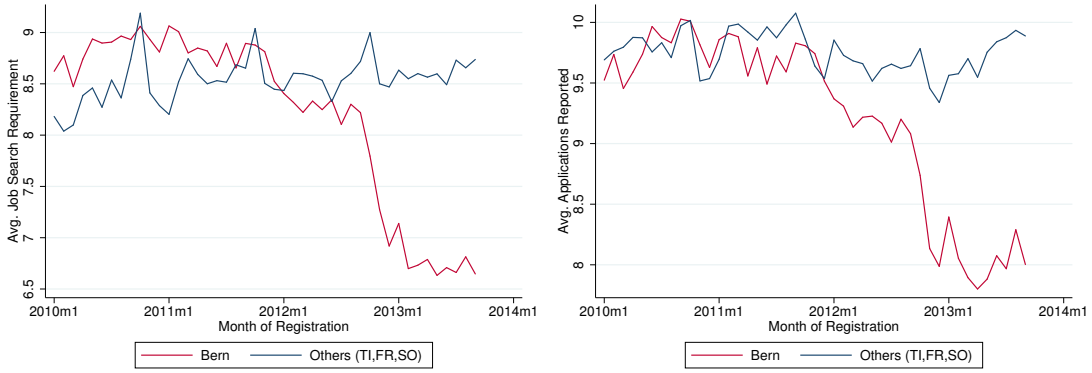
Figure 2a plots the share of unemployment spells that received at least one vacancy referral for Bern and the remaining cantons by month of registration. While this share stays constant in the remaining cantons, it drops from about 50% down to about 5% of spells over the course of the reform implementation (again, note that also spells registered prior to *BernTop!* are affected).

Finally, Figure 2b shows that the number of different occupations in which a job seeker states to search in decreased with the introduction of *BernTop!*. As a result, the self-reported occupational broadness of search also decreased, as shown in Appendix Figure C.3.

In sum, we can see that as an immediate result of the reform, job search intensity decreased, vacancy referrals were stopped and job seekers started searching in fewer, more similar occupations.

4 Empirical Analysis

We aim at providing a comprehensive evaluation of the reform on the outcomes of unemployed job seekers. We consider two main aspects: (1) the effect on the duration of unemployment spells; and (2) the effect on post-unemployment earnings (preliminary).

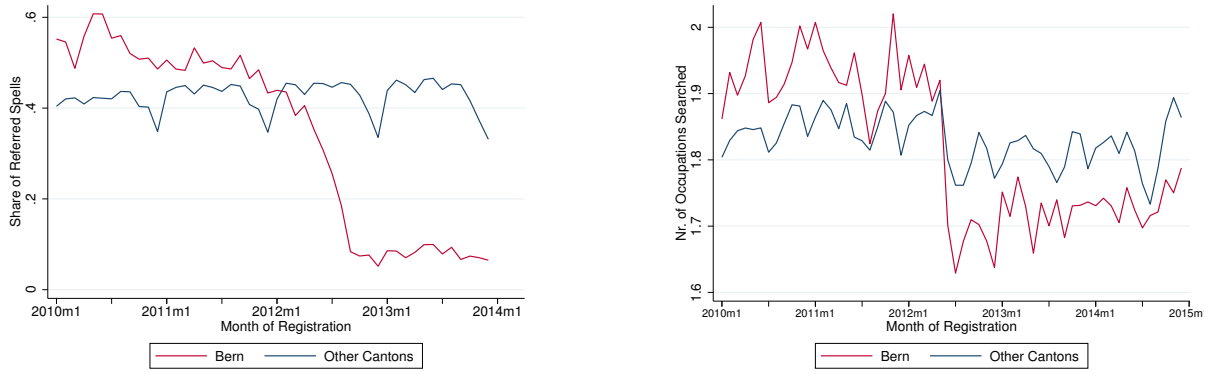


(a) Nr of Required Monthly Applications

(b) Nr of Reported Monthly Applications

Figure 1. Required and Reported Applications

This figure shows the average number of required and reported applications per spell per month in Bern and the other cantons. As not all cantons report requirements in the data, the figure only includes Bern, Tessin, Fribourg and Solothurn. Note that the x-axis denotes the month of registration for unemployment for each spell, which explains why we partly observe the effects of the reform even before it was implemented (Through, for example, unemployment spells that started a few months before the reform was implemented, but were then affected by it once the reform was introduced. This reduced the average number of application requirements of that spell, which is still registered before the reform took place).

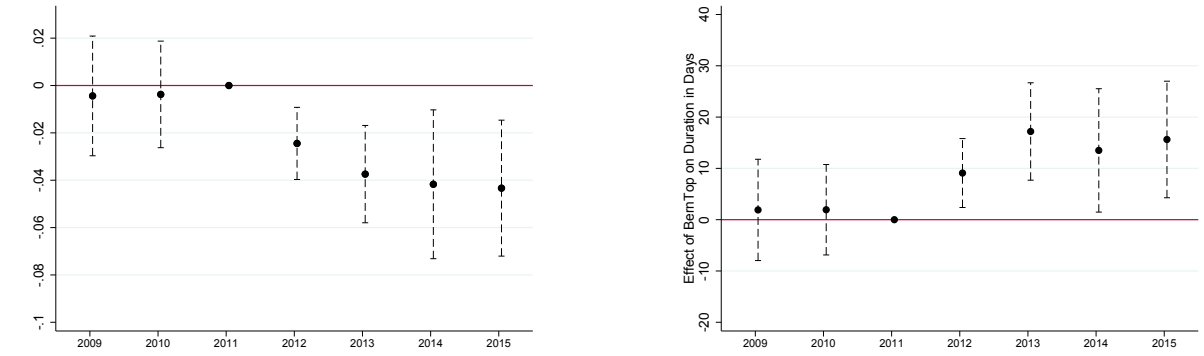


(a) Share of Spells That Were Referred at Least Once

(b) Nr of Occupations Searched

Figure 2. Measures of the Policy Change

In panel 2a, we plot the share of unemployment spells that received at least one referral over the course of the spell. The use of official referrals was almost completely anbolished over the course of *BernTop!*. In panel 2b, we display the average number of occupations job seekers declared to search in during their first meeting with a caseworker. These occupations can be chosen from a catalogue of occupations, which makes occupation classification easier. We see that job seekers declared to search in fewer occupations than before the policy change. This search behavior was actively encouraged by the case workers. Note that in both panels, the x-axis denotes the month of registration into unemployment for the unemployment spells, which explains why the stark drops can be observed before the official period of the policy change.



(a) Unemployment Exit Probability (< 12 months)

(b) Unemployment Duration in Days

Figure 3. Effect on Exit from Unemployment and Unemployment Duration

Estimated coefficients of a dynamic Diff-in-Diff framework (equation 2). In panel (a), the outcome is the probability to exit unemployment within 12 months after entry. In panel (b), the outcome is the duration of unemployment (top-coded at 520 days). The treatment group includes unemployment spells registered in Bern, the control group includes unemployment spells registered in the rest of Switzerland. Regressions include controls for job seeker characteristics. The dashed lines denote 95% confidence intervals. Standard errors are clustered at the PES level (N=150).

4.1 Empirical Framework

To estimate the effect of the policy change on the labor market outcomes of unemployed job seekers in Bern, we set up a difference-in-differences framework. The control group consists of all unemployment spells in Switzerland starting between January 2009 and December 2015 that were not located in Bern. Our dynamic difference-in-differences framework is specified as:

$$Y_i = \sum_{s=2009}^{2015} \gamma_s^{Bern} \mathbf{I}_{(y=s \,\&\, k=Bern)} + \delta \mathbf{I}_{(k=Bern)} + \tau_t + X_i'\beta + \epsilon_i \tag{1}$$

Y_i describes a given labor market outcome of job seeker i. Indicators $I_{(y=s \,\&\, k=Bern)}$ equal one when a spell started in year s in the canton of Bern. As the reform's roll-out started in 2012, we use the year 2011 as the omitted baseline period. The indicator $\mathbf{I}_{(k=Bern)}$ equals one for all spells started in the canton of Bern and controls for time-constant differences between Bern and the other cantons. τ_t includes calendar month fixed effects, which control for aggregate time shocks in Switzerland. Controls for job seeker covariates are included in x_i (see summary statistics in Appendix D). The difference-in-differences coefficients γ_s^{Bern} measure how outcomes in Bern changed compared to 2011, relative to the control group. Their causal interpretation for post-reform years relies on the key assumption that outcomes in Bern would on average have evolved in parallel to those in the other Swiss cantons. The estimates of γ_s^{Bern} for s=2009 and s=2010 inform about the relevance of this assumption during the pre-reform period.

We cluster standard errors at the level of the Public Employment Service (PES) office, which is the level at which requirement and referral policies are implemented.

4.2 Effect on Average Duration of Unemployment

Figure 5 shows the estimates of $\hat{\gamma}_s^{Bern}$. The baseline period is the pre-reform year 2011. In Panel (a), the outcome is the probability to exit unemployment within 12 months after entry. In panel (b), the outcome is

Table 1. Effect on Unemployment Duration and Exit: Pooled Diff-in-Diff Estimates

	UE Duration		P(Exit, 6 mon.)	P(Exit, 12 mon.)	P(Exit, 18 mon.)
	(1)	(2)	(3)	(4)	(5)
DiD	12.231***	14.472***	-0.033***	-0.038***	-0.027***
	(4.679)	(3.728)	(0.010)	(0.009)	(0.009)
Controls	No	Yes	Yes	Yes	Yes
Outcome Mean	266.27	266.27	0.45	0.66	0.80
R^2	0.026	0.114	0.093	0.086	0.094
N	348448	348448	348448	348448	348448

The table reports estimates from a pooled version of the Difference-in-Differences framework defined by equation 2, excluding job seekers who registered in the year 2012. The treatment group includes unemployment spells registered in Bern, the control group includes unemployment spells registered in the rest of Switzerland.In columns (1) and (2), the outcome is the average unemployment duration in days (top-coded at 520 days). In columns (3) to (6), outcomes are the probability to exit unemployment after 6, 12, or 18 months, respectively. Summary statistics on control variables are reported in Appendix D. Standard errors are clustered at the PES level (N=150). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

unemployment duration (top-coded at 520 days). Results show that the overall effect of the policy change on unemployment exit is negative. Among partially treated spells that started in 2012, the probability to exit within 12 months decreases by roughly two percentage points. The effect amounts to about four percentage points for spells starting in 2013 or later, after the reform was fully implemented. Similarly, the duration of unemployment prolongs by about ten days on average for job seekers registered in 2012 and by about 12-18 days for job seekers registered thereafter.

Table 2 shows the corresponding pooled difference-in-differences estimates, excluding spells in 2012 which were partially treated. On average, the policy change prolonged the duration of unemployment by about 14 days (column 2). Compared to the pre-reform mean in Bern, this corresponds to an increase by 8%. As shown by columns (3) to (4), the effect seems to operate mostly during the first year of unemployment, although it remains negative and significant even when considering a time window of 18 months. Table E.1 shows that the results are robust to various levels of clustering and fixed effects as well as different specifications and the inclusion of unemployment spells starting in 2012. Anticipating the possibility of search externalities on job seekers registered outside of Bern, we further exclude job seekers in municipalities within less than 40 minutes commuting time from the control group. We observe only a small decrease in the results.

4.3 Role of Individual Effort and Labor Market Tightness

So far, the estimates have shown how the reform affected the average unemployment duration of job seekers in Bern. The size of this overall effect is likely determined two main forces: a decrease in individual-level effort provision and a change in labor market tightness due to the fact that job seekers in Bern collectively decrease their effort. In the following, we provide heterogeneity analyses that inform about the importance of these two counteracting forces.

To fix ideas, Appendix A presents a simple conceptual framework regarding the reform's expected effect. The framework illustrates that the overall effect of the policy change can be decomposed into a *Behavioral Effect* due to changed individual-level search effort and a *Tightness Effect* due to changing labor market conditions:[3]

[3]Note that we use this decomposition only to describe the different channels affecting the reform's effect. The variation in our data does not allow to separately estimate the Behavioral Effect and the Tightness Effect.

$$\underbrace{e_{post}f(\theta_{post}) - e_{pre}f(\theta_{pre})}_{\text{Overall Effect}} = \underbrace{[e_{post} - e_{pre}]f(\theta_{pre})}_{\text{Behavioral Effect}} + \underbrace{e_{post}[f(\theta_{post}) - f(\theta_{pre})]}_{\text{Tightness Effect}} \quad (2)$$

In this expression, e denotes a job seeker's effort and $f(\theta)$ its return, which depends on labor market tightness θ. As θ describes the ratio of vacancies over total effort in the labor market, it is also affected by the policy change.

The behavioral effect denotes the difference in job finding rates that is solely attributable to the change in search effort at the individual level, keeping labor market conditions constant. The tightness effect, in turn, denotes the difference in the (potential) job finding rates of job seekers in post-reform versus pre-reform labor markets, keeping individual search effort constant. That is, the tightness effect denotes the difference in the job finding rate that is solely attributable to the change in labor market conditions.

Based on the simple framework, we now investigate heterogeneity in the reform effect along three dimensions: (1) individual-level effort change; (2) pre-reform labor market tightness; and (3) competition from untreated job seekers.

Individual-level effort change. We first investigate how the treatment effect varies with respect to the expected change in individual search effort. As a proxy for the expected effort change, we use predicted application requirement levels. Intuitively, if job seeker characteristics predict a high level of job search requirements in the absence of a policy change, then these job seekers would have been more restricted in their job search without the policy change. We thus regress the average number of application requirements per month on a vector of job seeker characteristics (same as x_i in equation 2). Using our estimates from this regression, we predict the individual average number of monthly application requirements that a job seeker would have gotten had she registered in Bern before the policy change. Based on the predicted individual average number of monthly application requirements, we conduct a median split. Figure 6 plots the *actual* individual average number of monthly application requirements for job seekers for whom we predicted a high level of application requirements and for those for whom we predicted a low level of application requirements. The drop in average required applications is larger for job seekers for whom we predict a high level of application requirements without the policy change. That is, the job search effort for this group experiences a larger drop due to the reform.

Column (1) of Table 2 reports the results from a pooled Difference-in-Differences regression for individuals with high versus low predicted requirement levels (median split). The results show that the reform's effects are indeed about twice as strong for job seekers with a high versus low predicted pre-reform job search requirement level, supporting the idea that the size of the individual-level effort change is decisive for the reform's effect.

Pre-reform labor market tightness. As a second insight from section A, we expect the overall reform effect to increase with pre-reform local labor market tightness. Intuitively, effort changes matter more if there are more vacancies per job seeker available. To assess the relevance of this conjecture, we interact the Difference-in-Differences term with a median split on the pre-reform labor market tightness on municipality level. Despite a lack of statistical significance, Column (1) of Table 2 shows suggestive evidence that the reform's effect increases when labor market tightness is high, i.e., when the ratio of vacancies over unemployed is more favorable from the job seekers' perspective.

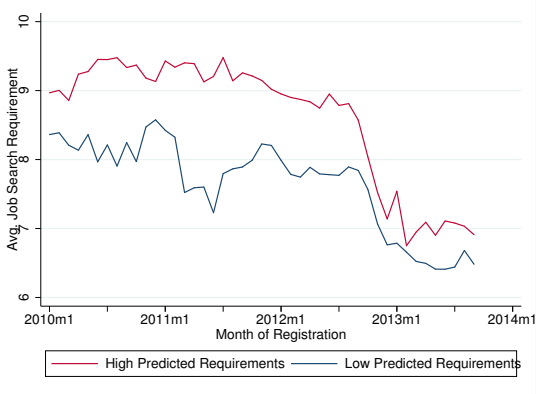


Figure 4. Application Requirements for High- vs Low-predicted Requirement Spells by Month of Registration into Unemployment

This figure displays how actual application requirements developed in Bern and shows the results from a median split based on the number of predicted monthly application requirements. The figure shows the average number of required and reported applications per spell per month in Bern for UE spells separately for UE-spell for which we predicted a high level of application requirements (red) and for those for which we predicted a low level of application requirements, based on the pre-*BernTop!* data.

Table 2. Effect on Unemployment Duration: Heterogeneity

	UE Duration		
	(1)	(2)	(3)
DiD	9.785**	11.005**	12.844***
	(4.016)	(4.334)	(3.458)
DiD x (High treatment intensity)	9.462*		
	(5.231)		
DiD x (High LM tighness)		6.524	
		(4.799)	
DiD x (Close to border)			7.317*
			(3.944)
Controls	Yes	Yes	Yes
Outcome Mean	266.27	266.27	266.27
R^2	0.114	0.116	0.114
N	348448	348431	348431

The table reports estimates from a pooled version of the Difference-in-Differences framework defined by equation 2, excluding job seekers who registered in the year 2012. The outcome is the average unemployment duration in days (top-coded at 520 days). In column (1), "High treatment intensity" equals one if a job seeker's predicted requirement without *BernTop!* is at or above the median. In column (2), "High LM tightness" equals one if the vacancy-to-job seeker ratio in a job seeker's local labor market region is at or above the median. In column (3), "Close to border" equals one if the job seeker's municipality has a travel time of 15 minutes or less to the cantonal border. Standard errors are clustered at the PES level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Change in labor market tightness. The tightness effect sketched out in section A arises because job seekers in Bern collectively decrease their job search effort, positively affecting the probability of the individual job seeker to find a job and thus counteracting the behavioral effect. However, the smaller the share of job seekers in a labor market, who decrease search effort, the smaller should the impact of this channel be.

As all job seekers who live in Bern are treated by the reform, an interesting source of variation in the share of treated job seekers stems from job seekers from other cantons searching for a job in Bern. We expect the tightness effect to matter more in labor markets with less competition from job seekers outside of Bern, who do not decrease their search. Intuitively, consider a closed labor market with no competition from job seekers from outside of the canton. Then a policy change as *BernTop!* would affect all job seekers in this closed labor

market, who would collectively reduce their job search effort following the introduction of the policy change. In contrast, consider Bern to be an open labor market where job seekers living in Bern compete with job seekers from neighboring cantons due to the possibility of commuting.[4] In such an open labor market, we expect the tightness effect to be smaller, as not all job seekers are treated by the reform.

While we cannot estimate the reform's effect under the scenario of a closed labor market, we can make use of the variation in the local competition from job seekers outside Bern. The simplest source of variation is the commuting time from a job seeker's home to the border of Bern and to one of the adjacent cantons. The calculation of commuting times is explained in AppendixF. In the center of Bern, where commuting times to other cantons are longer, the share of job seekers from other cantons who are competing for the same jobs is lower. Indeed, as figure C.4 in the Appendix shows, travel time and the share of commuters from the adjacent cantons are significantly negatively correlated.

To exploit this variation, we perform a split of the commuting time to the border of the canton. As before, we interact this split with the Difference-in-Differences coefficient. Column (3) of Table 2 shows that the reform had an about 30% stronger effect on job seekers in municipalities with a travel time of 15 minutes or less to the border. This result supports the idea that the average effect of policies that target search effort depend a lot on the relative importance of tightness effects.

4.4 Externalities on Job Seekers in Adjacent Cantons

Given the finding that job seekers in Bern are more affected if they live closer to the border, it is natural to ask whether job seekers at the other side of the border were positively affected by the policy change. Intuitively, if job seekers in Bern decrease their effort, this could have positive externalities on the job finding prospects of job seekers outside Bern, who partially compete with job seekers from Bern. To test for potential externalities, we estimate the following regression on the population of job seekers outside Bern:

$$Y_i = \sum_{s=2009}^{2015} \gamma_s^{close} \mathbf{I}_{(y=s \,\&\, close=1)} + \pi_m + \lambda_{y\times c} + \sigma_t + X_i'\kappa + \varepsilon_i \tag{3}$$

The indicators $\mathbf{I}_{(y=s \,\&\, close=1)}$ equal one if a job seeker enters unemployment in year s and lives in a municipality with low commuting time the border to Bern. The calculation of commuting times is explained in AppendixF. π_m includes municipality fixed effects and $\lambda_{y\times c}$ are canton-year fixed effects. As in equation 2, σ_t are calendar month fixed effects and X_i are job seeker covariates. In this specification, we cluster standard errors at the level of the municipality, which defines the travel time to Bern.

The framework compares how the outcomes of job seekers close to the border evolve over time, relative to those of job seekers in the same canton, but further away from the border. This also means that cantons that are non-adjacent to Bern do not serve as a control group, but only identify the effect of covariates and aggregate time factors.

[4] The share of workers living in Bern and commuting to a different canton is 10%. The share of commuters from other cantons working in Bern is 14%. Source: https://www.bve.be.ch/bve/de/index/mobilitaet/mobilitaet_verkehr/mobilitaet/grundlagen_mobilitaet/Pendlerstatistik.html

Table 3. Effect on Unemployment Duration: Adjacent Cantons

	≥20 Mins	≥30 Mins	≥40 Mins
	(1)	(2)	(3)
DiD	0.727	-7.076*	-6.727**
	(4.332)	(3.626)	(3.255)
Controls	Yes	Yes	Yes
Outcome Mean	268.21	268.21	268.21
R^2	0.085	0.085	0.085
N	317120	317120	317120

Notes: This table shows the estimated coefficients based on a pooled Diff-in-Diff regression. For this specification, we excluded observations from the canton of Bern and instead only used unemployment spells from the remaining cantons. As before, the outcome for this specification is unemployment duration (top-coded at 520 days). To produce the displayed coefficients, we generate a DiD-term based on a dummy variable that takes on the value 1 if an unemployment spell is located in a municipality that is no more than 10, 20, 30 or 40 minutes (by car) away from the border of the canton of Bern and coincides with the language spoken in the corresponding part of Bern. The corresponding municipalities are displayed in figure C.2b. Although the results are to noisy if we choose a bandwidth of 20 minutes or less around Bern, we can see that for all specifications of 20 minutes commuting time and upwards average unemployment duration decreased by 4 to 5 days (≈ 50% of the estimated original effect in Bern). We cluster the standard errors at the municipality-level.

Figure 7 plots the estimates of γ_s^{close}, where “close" is defined by municipalities with a travel time of 30 minutes or less to the border of Bern and has the same language as the bordering municipality in Bern.[5] Although imprecisely estimated, the coefficients point towards a slight decrease in the average unemployment duration of job seekers in closeby municipalities. This is confirmed by the estimates from a pooled version of equation 4, as reported in Table 3.

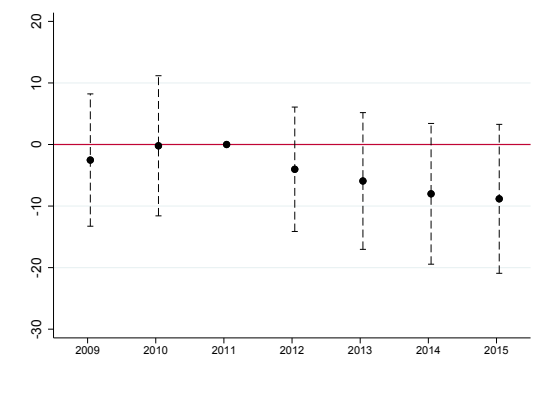


Figure 5. Effect on UE Duration of Job Seekers in Adjacent Municipalities (≤ 30 minutes travel time to Bern)

Estimated coefficients of a dynamic Diff-in-Diff framework (equation 4). The outcome for is the duration of unemployment (top-coded at 520 days). Spells registered in Bern are excluded. The treatment group includes spells located in a municipality that is no more than 30 minutes (by car) away from the border of the canton of Bern and coincides with the language spoken in the corresponding part of Bern. The corresponding municipalities are displayed in figure C.2b. The dashed lines denote 95% confidence intervals. Standard errors are clustered at the level of the municipality.

4.5 Effect on Average Earnings

So far, the results have shown that the increased autonomy in job search prolongued the average duration of unemployment spells. For a comprehensive evaluation of the reform and its monetary trade-offs, it is key to

[5] Most often, this requires a municipality to be German speaking. An exception are muncipalities in the Cantons Jura and Neuchatel, which border the French-speaking part of Bern

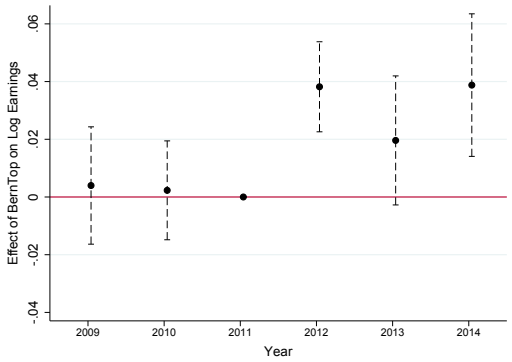


Figure 6. Estimated Dynamic Effect on log Post-Unemployment Wages

Estimated coefficients based on the dynamic Diff-in-Diff regression (2). The outcome is the log of post-unemployment monthly employment earnings. The dashed lines denote 95% confidence intervals. Standard errors are clustered at the PES level.

also understand how job match quality was affected: did job seekers use their autonomy to successfully direct search towards longer-lasting, higher-paying jobs?

To shed light on this question, we study how post-unemployment earnings were affected by the reform. [Please note that these results are preliminary, as our social security data currently range only until 2015. This also prevents us from studying job stability at this stage.] The effect of increased search autonomy on re-employment wages is not easy to predict, even if the effect on the duration of unemployment is known. In a model with directed search (e.g. Nekoei and Weber, 2017), an increased duration of unemployment may reflect that job seekers becoming more selective about the jobs they apply to. This would on average result in higher post-unemployment wages. On the other hand, a longer unemployment duration has been shown to decrease the wage offers. With both effects working in opposite directions, the net effect is an empirical question.

We estimate the reform's effect on post-unemployment earnings by using the same dynamic difference-in-differences model as outlined in equation (2). Instead of unemployment duration, we now use log re-employment earnings from social security records as the outcome. Figure 8 presents the results. We estimate an increase in post-unemployment earnings between two and four log points on average –pointing towards the dominance of the first mechanism. It appears that search autonomy can yield the benefit of allowing individuals to find better job matches. At the current stage, we have a limited observation window and cannot inform about the persistence of these earnings effects. Extending the time window will be an important part of the ongoing work.

5 Monetary Trade-Offs

When estimating the average effect of a policy change with respect to unemployment duration and post-unemployment earnings, a natural follow-up analysis concerns the fiscal trade-off inherent to the policy change. A meaningful analysis of the fiscal gains or losses encompasses multiple components we need to consider: The effect on unemployment, the effect on post-unemployment earnings, the effect on post-unemployment job stability and the persistence of all these effects. As we are still waiting for a data update containing unemployment spell data up to 2020, we cannot yet conduct a meaningful analysis regarding post-unemployment job

stability or the persistence of the effect of the policy change on earnings. However, our estimates regarding unemployment duration and post-unemployment earnings allow for a back-of-the-envelope calculation and provide a starting point for a fiscal cost-benefit analysis. The fiscal costs the average job seeker generates from staying unemployed for 15 additional days are[6]

$$\underbrace{15}_{\text{Effect on avg. UE duration}} \times \underbrace{165\ \textit{CHF}}_{\text{avg. daily wage}} \times \underbrace{(0.75}_{\text{avg. benefit rate}} + \underbrace{0.2}_{\text{avg. income tax}} (1-0.75))$$
$$= 1'980\ \textit{CHF}$$

That is, a job seeker staying unemployed 15 days longer and earning 165 CHFs per day on average results in forgone tax revenues of $15 \times 165 \times 0.2 = 495$ CHFs. On the other hand, job seekers in Switzerland still pay taxes on unemployment benefits, resulting in tax revenues of $15 \times 165 \times 0.75 \times 0.2 = 371.25$ CHFs. Through unemployment benefits the fiscal costs increase by $15 \times 165 \times 0.75 = 1856.25$ CHFs.
In contrast, the fiscal gain from job seekers finding higher-paying jobs can be quantified using the effect on job stability, the effect on post-unemployment earnings and its persistence. As we cannot yet conduct a meaningful analysis regarding job search stability or the persistence of the wage effect, we will exclude them from our analysis and instead calculate the daily fiscal gains from increased post-unemployment earnings assuming that the earnings effect persists indefinitely.

$$\underbrace{0.02}_{\text{Effect on avg. wage}} \times \underbrace{165\ \textit{CHF}}_{\text{avg. daily wage}} \times \underbrace{0.2}_{\text{avg. income tax}} = 0.66$$

We can now calculate that earnings effect would (in the absence of an effect on job stability) need to persist $1980/0.66 = 3'000$ days (≈ 8 years) to cancel out the fiscal costs from the increased unemployment duration.

5.1 Worker Trade-Off

Forgone income:

$$165\ \textit{CHF} \times (1-0.2) = 132$$

Da noch KV und sonstige Abgaben raus? Kürzt sich ja eh raus. Gains:

$$165 \times 0.75(1-0.2) = 99$$

After unemployment:

$$165 \times 0.02(1-0.2) = 2.64$$

[6] Note that we use our data on pre-unemployment earnings and replacement rate to retrieve the average daily wage and replacement rate in our sample.

In total:

$$(132 - 99) \quad \times \quad 15 \quad = \quad 1.815 \quad \textit{CHF}$$

Amortized after $1815/2.64 = 687.5$ days.

Then, putting both together gives us aggregate losses: $1'980 + 1'815$ *CHFs* and daily aggregate gains $0.66 + 2.64$ *CHFs*. Ammortized after 1'150 days.

6 Conclusion

We analyze the effects of increasing thw job search autonomy of unemployed workers. We exploit a policy change in the Swiss canton Bern, during which application requirements were drastically reduced and the use of official job referrals was almost completely abolished. We find that, on average, unemployment duration increased by about 8%. The effect is larger among job seekers whose effort is predicted to reduce more strongly due to the reform. Moreover, we find that the changes in local labor market tightness due to the fact that job seekers collectively decrease their effort mitigate the effect of individual effort changes.

Finally, we present preliminary evidence on the reform's effect on post-unemployment earnings. We estimate that post-unemployment earnings increased by 2 to 4% on average. Apparently, the decreased pressure through increased search autonomy allowed job seekers to find slightly better matching and better paying jobs.

In sum, our estimates indicate that increasing the autonomy and self-responsibility of job seekers carries the risk of prolonging unemployment spells, but these adverse effects can, from a fiscal point-of-view, be eventually compensated by an increase in post-unemployment earnings. Furthermore, results suggest that the success of policies which lead to collective changes in search effort depends on the the local scope for tightness effects and search externalities. In the context of this study, the behavioral effect of a decrease in individual effort was apparently the dominating force. It is open whether this also holds in other labor market with less favorable labor demand conditions.

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Appendix A Conceptual Framework

Setup. Let *post* denote the labor market after the reform and *pre* denote the labor market before the reform. We start by assuming the canton Bern to be a closed labor market, such that all job seekers in the *post* labor market are treated. Let V denote the number of vacancies and E the aggregate amount of search effort provided by job seekers in Bern. Thereby, $\theta = V/E \in \{\theta_{post}, \theta_{pre}\}$ describes labor market tightness in the *post* and *pre* labor market, respectively. The individual search effort of *post* and *pre* job seekers is defined by e_{post} and e_{pre}. Assuming that every job offer is accepted, the job finding probability $e \times f(\theta)$ depends on individual job search effort e and a function f that is increasing in labor market tightness θ, with $f''(\cdot) < 0$. If the reform induces job seekers to exert less (but still positive) search effort, we have $e_{post} < e_{pre}$.

The overall effect of the policy change can be decomposed into a *Behavioral Effect* due to changed individual-level search effort and a *Tightness Effect* due to changing labor market conditions.

$$\underbrace{e_{post}f(\theta_{post}) - e_{pre}f(\theta_{pre})}_{\text{Overall Effect}} = \underbrace{[e_{post} - e_{pre}]f(\theta_{pre})}_{\text{Behavioral Effect}} + \underbrace{e_{post}[f(\theta_{post}) - f(\theta_{pre})]}_{\text{Tightness Effect}} \tag{A.1}$$

The behavioral effect denotes the difference in job finding rates that is solely attributable to the change in search effort at the individual level, keeping labor market conditions constant. The tightness effect, in turn, denotes the difference in the (potential) job finding rates of job seekers in post-reform versus pre-reform labor markets, keeping individual search effort constant. That is, the tightness effect denotes the difference in the job finding rate that is solely attributable to the change in labor market conditions.

The relationship between the overall effect, the behavioral effect and the tightness effect sketched out in equation (A.1) is illustrated in figure A.1. We plot the job finding rate as a function of the job search effort e and the labor market tightness θ. In tighter labor markets (high θ), job seekers face less competition for the existing vacancies, which increases the individual job finding rate. Hence, the job finding rate increases in θ. Secondly, the job finding rate increases in the exerted search effort. Thus, when job seekers decrease search effort following the introduction of *BernTop!* ($e_{post} < e_{pre}$), the individual job finding rate decreases for every potential labor market tightness, as depicted by the blue graph staying below the red graph for all values of θ. Finally, we assume that the returns of labor market tightness to the individual job finding rate diminish, resulting in a concave function.

The (negative) behavioral effect of the policy reform denotes the difference in the job finding rate in the pre-reform labor market between treated and untreated job seekers, that is, the effect of decreasing job search following the introduction of *BernTop!*. The tightness effect denotes the difference between the job finding rate of a post-reform job seeker in a post-reform and a pre-reform labor market, that is, the effect of changing labor market conditions, while keeping individual search effort constant. The overall effect is the sum of the two effects, which work in opposite directions.

Insights for the Empirical Analysis. This simple decomposition yields insights regarding the expected effect of the policy change under different labor market conditions.

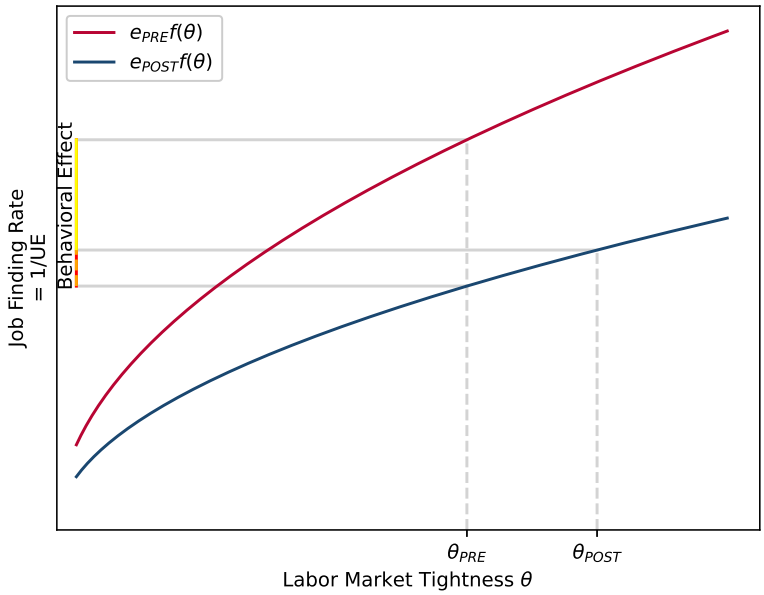


Figure A.1. Decomposition of Reform Effect

We display the graphical decomposition of the overall effect of a policy change that decreased job search effort for all job seekers in a closed economy. The (negative) behavioral effect denotes the decrease in the job finding rate that is purely attributable to the decrease in job search effort from e_{PRE} to e_{POST}, evaluated at the pre-reform market tightness θ_{PRE}. The (positive) tightness effect denotes the increase in the job finding rate that is purely attributable to changing the labor market tightness from θ_{PRE} to θ_{POST}, evaluated at the post-reform search effort e_{POST}.

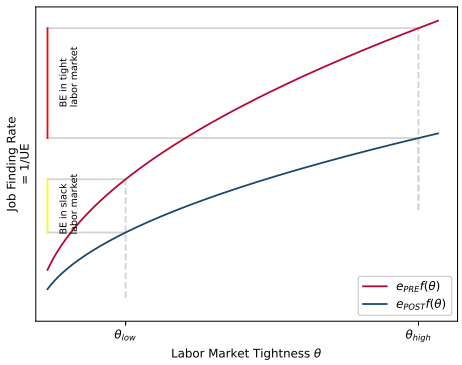
First, it is straight-forward to expect that the overall effect on job finding will be more pronounced for job seekers who reduced their search effort more than others due to the reform, as we expect a stronger behavioral effect for these job seekers.

Second, we expect the policy change to have a weaker effect in slack labor markets, where many job seekers compete for few vacancies, than in tight labor markets with an abundance of vacancies. Intuitively, reducing search effort in a slack labor market with few vacancies per job seeker does not decrease the job finding rate as much as in a tight labor market. This scenario is similar to a game of musical chairs, in which many people try to sit on far too few chairs. If one person excerts less effort to sit on one of the free chairs, the probability to get the free chair, which was already low to begin with, did not decrease much. This scenario is illustrated in figure A.2, panel (a). The behavioral effect of reducing job search effort from e_{pre} to e_{post} on the job finding rate is smaller in slack labor markets than in tight labor markets. The derivative of the expected overall effect with respect to pre-reform labor market tightness θ_{pre} is derived in appendix B.

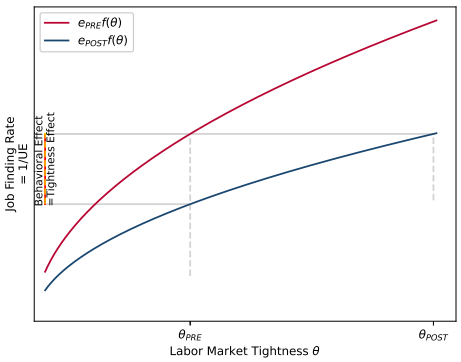
Third, the framework implies that the overall effect decreases with the change in local labor market tightness due to the reform.[7] For illustration, consider a labor market in which only some job seekers are treated by the reform. The lower the share of untreated job seekers, the higher the share of job seekers who decrease search effort. Hence, we expect the tightness effect to play a relatively larger role in labor markets with a high share of treated job seekers. In these markets, we expect the average effect of the policy change on treated job seekers to be lower. This is illustrated in Figure A.2, panel (b). If the difference in the labor market tightness

[7] This result is derived in appendix B under the assumption that the change in labor market tightness $\theta_{post} - \theta_{pre}$ consists **only** of variation in pre- **or** post-reform labor market tightness, keeping post- and pre-reform labor market tightness constant, respectively.

between treated and untreated labor markets is large, the (positive) tightness effect negates the (negative) behavioral effect.



(a) Micro Effect in Tight vs. Slack Labor Markets



(b) The Positive Externality Offsets the Negative Micro Effect

Figure A.2. Effect of Labor Market Conditions

Panel (a) displays the expected behavioral effect in tight vs. slack labor markets, that is, in competitive vs. less competitive labor markets (from a job seeker's perspective). Panel (b) shows how the overall estimated effect can change, depending on the change in labor market conditions that comes along with the policy change.

Appendix B Notes on the Conceptual Framework

We assume that $f' > 0, f'' < 0$

$$\begin{aligned} \underbrace{OE}_{\text{Overall Effect}} &= [e_{post} - e_{pre}]f(\theta_{pre}) + e_{post}[f(\theta_{post}) - f(\theta_{pre})] \\ \Rightarrow \frac{\partial OE}{\partial \theta_{pre}} &= [e_{post} - e_{pre}]f'(\theta_{pre}) + e_{post}\left[f'(\theta_{post})\frac{\partial \theta_{post}}{\partial \theta_{pre}} - f'(\theta_{pre})\right] \end{aligned}$$

We will assume that $\frac{\partial \theta_{post}}{\partial \theta_{pre}} = 1$ and thus

$$\frac{\partial OE}{\partial \theta_{pre}} = \underbrace{\underbrace{[e_{post} - e_{pre}]}_{<0}\underbrace{f'(\theta_{pre})}_{>0} + e_{post}\left[\underbrace{f'(\theta_{post}) - f'(\theta_{pre})}_{<0}\right]}_{<0}$$

Note that the TE is negative, so a negative derivative indicates in increase in magnitude of the TE. But also important:

$$\frac{\partial OE}{\partial \Delta\theta} = \underbrace{[e_{post} - e_{pre}]}_{<0}\underbrace{f'(\theta_{pre})}_{>0}\frac{\partial \theta_{pre}}{\partial \Delta\theta} + e_{post}\left[\underbrace{f'(\theta_{post})}_{>0}\frac{\partial \theta_{post}}{\partial \Delta\theta} - \underbrace{f'(\theta_{pre})}_{>0}\frac{\partial \theta_{pre}}{\partial \Delta\theta}\right] \tag{B.1}$$

With $\theta_{post} - \theta_{pre} > 0$, we will/should assume that $\frac{\partial \theta_{post}}{\partial \Delta\theta} \geq 0$ and $\frac{\partial \theta_{pre}}{\partial \Delta\theta} \leq 0$ and thus

$$\frac{\partial OE}{\partial \Delta\theta} = \underbrace{\underbrace{[e_{post} - e_{pre}]}_{<0}\underbrace{f'(\theta_{pre})}_{>0}\underbrace{\frac{\partial \theta_{pre}}{\partial \Delta\theta}}_{\geq 0}}_{\leq 0} + e_{post}\underbrace{\left[\underbrace{f'(\theta_{post})}_{>0}\underbrace{\frac{\partial \theta_{post}}{\partial \Delta\theta}}_{\geq 0} - \underbrace{f'(\theta_{pre})}_{>0}\underbrace{\frac{\partial \theta_{pre}}{\partial \Delta\theta}}_{\leq 0}\right]}_{\geq 0} \tag{B.2}$$

Finally, we can simplify the expression by assuming $\frac{\partial \theta_{pre}}{\partial \Delta\theta} = 0$ or $\frac{\partial \theta_{post}}{\partial \Delta\theta} = 0$, respectively, such that either

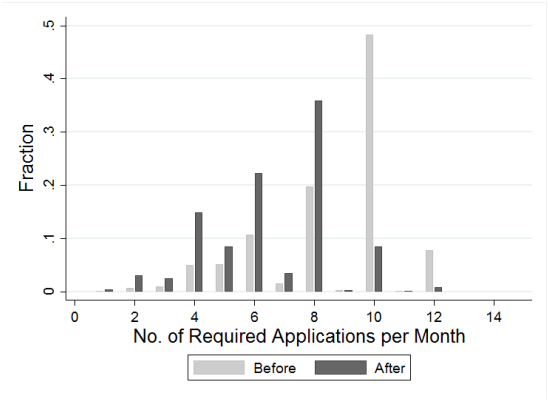
$$\frac{\partial OE}{\partial \Delta\theta} = \underbrace{e_{post}\underbrace{f'(\theta_{post})}_{>0}\underbrace{\frac{\partial \theta_{post}}{\partial \Delta\theta}}_{\geq 0}}_{\geq 0} \tag{B.3}$$

or

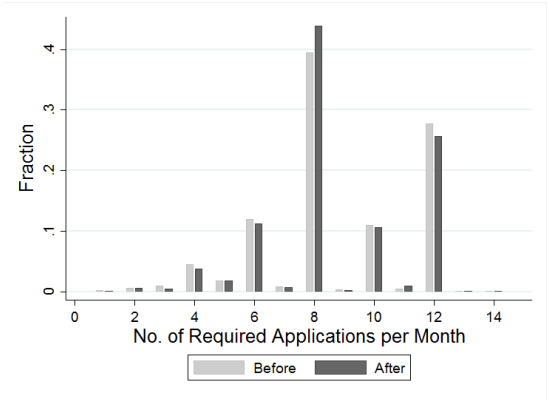
$$\frac{\partial OE}{\partial \Delta\theta} = \underbrace{\underbrace{[e_{post} - e_{pre}]}_{<0}\underbrace{f'(\theta_{pre})}_{>0}\underbrace{\frac{\partial \theta_{pre}}{\partial \Delta\theta}}_{\leq 0}}_{\geq 0} + \underbrace{e_{post}\left[-\underbrace{f'(\theta_{pre})}_{>0}\underbrace{\frac{\partial \theta_{pre}}{\partial \Delta\theta}}_{\leq 0}\right]}_{\geq 0} \tag{B.4}$$

Showing that the (negative) treatment effect decreases in absolute magnitude in the change in labor market tightness.

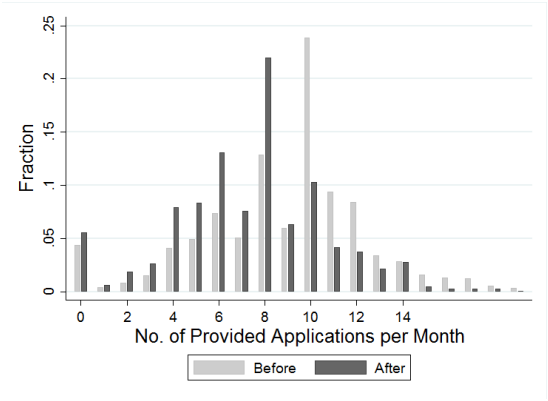
Appendix C Additional Figures



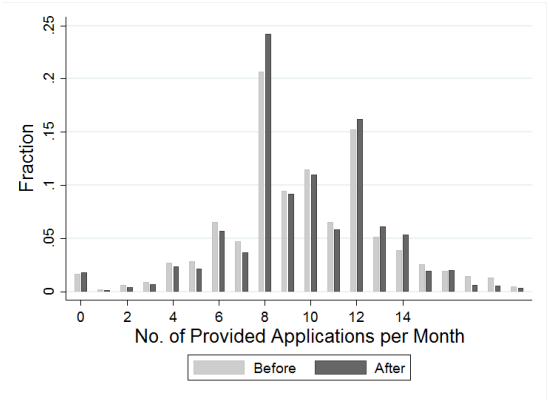
(a) Changes in Distribution of Required Search Effort in Bern



(b) Changes in Distribution of Required Search Effort in the Remaining Cantons



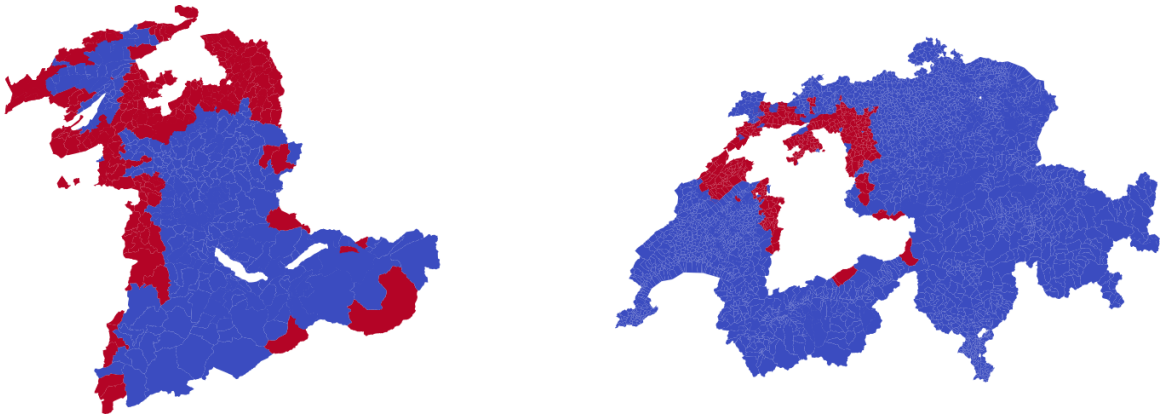
(c) Changes in Distribution of Reported Search Effort in Bern



(d) Changes in Distribution of Reported Search Effort in the Remaining Cantons

Figure C.1. Changes in Distribution of Search Effort

This figure shows the distribution of required and reported search effort (applications) in Bern and the other cantons with data on required and provided effort (Fribourg, Solothurn and Tessin).



(a) Closeness to Border in Bern

(b) Closeness to Border in the Adjacent Cantons

Figure C.2. Closeness to Border

This figure shows the zip code areas in our sample which were used for the distance analyses presented in section 5.4. In figure C.2a, we show the canton of Bern and mark the zip code areas close to the border of Bern. We split the zip code areas by travel time to the next canton, according to Google Maps. We define a zip code area as "close" to the border if it takes less than 20 minutes to reach the border by car. In Figure C.2b we display Switzerland without the canton of Bern and mark the zip code areas close to the canton of Bern. We define a zip code area as "close" to the canton of Bern, if it takes less than 20 minutes to reach the canton of Bern by car and if the language region in the zip coincides with the language region in the corresponding part in Bern. Using this additional criterion, we make sure that these commuters indeed search in the same labor markets as the commuters in Bern and are not restricted by language barriers. On can immediately see that our split defines the southern regions in Bern as well as the regions south of Bern as "not close". This is due to the "Bernese Alps", a mountain range in the south of Bern, which only leaves a handful of passes between the southern canton of Valais and Bern and thus makes commuting between both cantons very time consuming.

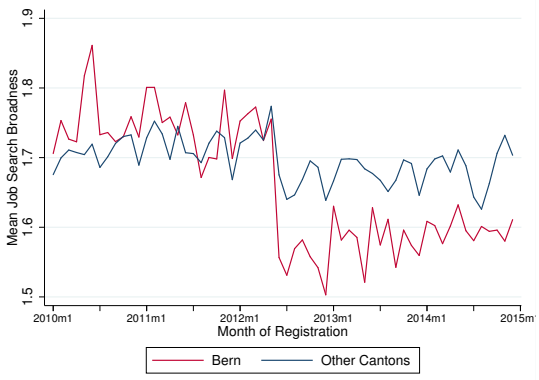


Figure C.3. Broadness of Job Search

This figure displays the average broadness in the classified occupations job seekers declared to search in. Occupational broadness is defined as the variation in the first two digits of the BN-2000 codes of the occupations a job seeker states to search in. The figure reveals that the policy change came along with a drop in the self-reported occupational broadness of search. This is likely due to the fact that *BernTop!* also prescribed caseworkers not to push job seekers towards searching in occupations too far outside their experience and competence. During the first meeting with the caseworkers, job seekers fill in a form stating in which occupations they are searching for a job. These occupations are then classified according to the BN-2000 nomenclature, which classifies occupations using a 5-digit code. Each of the first three digits further specifies an occupation in a lexicographic fashion, while the last two digits pin it down. For example, the occupation of a mason is classified by the code "41101" as follows: "4" denotes "Occupations in Construction & Mining", "4.1" denotes "Occupations in Construction", "4.1.1" denotes "Occupations in Core Construction" and "4.1.1.01" denotes "Masons".

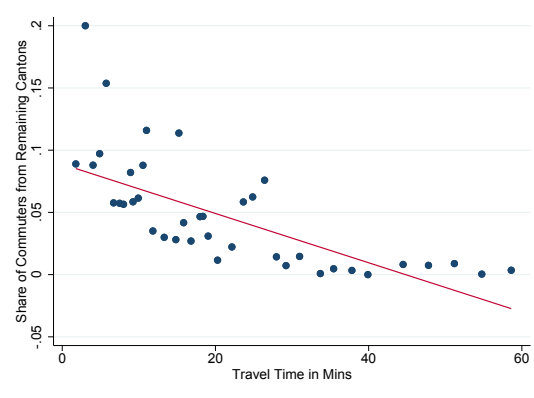


Figure C.4. Travel Time and Share of Outside Commuters

This figure shows a scatterplot of the commuting time in minutes on the x-axis to the share of commuters from another canton (except Bern) for each municipality in Bern. For the travel times, we used the same weighted distances based on the zip code travel times we used in the externality section 5.4. We can clearly see the negative correlation between both variables. The correlation coefficient between both variables is -0.324 (p-value=0.000).

Table D.1. Summary Statistics of Key Covariates

	Mean	Min	Max	Observations
Age	40.965	28	60	417780
Age Squared	1757.216	784	3600	417780
No other Person Affected	0.650	0	1	417780
One Person Affected	0.158	0	1	417780
One to Three Persons Affected	0.175	0	1	417780
More than Three Persons Affected	0.017	0	1	417780
German Language Proficiecy Score	2.217	1	4	417780
French Language Proficiecy Score	2.666	1	4	417780
Sex	0.353	0	1	417780
Low Education	0.285	0	1	417780
Medium Education	0.392	0	1	417780
High Education	0.323	0	1	417780
Potential Benefits Duration 400 Days	0.163	0	1	417780
Married	0.492	0	1	417744
Single, Widowed or Divorced	0.508	0	1	417780
Non-Swiss	0.486	0	1	417780
Non-Permanent Resident	0.207	0	1	417780
Nr. of Months of Employment During the Last 24 Months prior to UE	21.517	2	24	417780
Nr. of Months of Employment During the Last 30 Months prior to UE	26.415	2	30	417780
Log Average Wage over the last 24 Months	8.500	5	13	417780
Log Average Wage over the last 30 Months	8.494	5	13	417780
German language region	0.624	0	1	417780
French language region	0.315	0	1	417780
Italian language region	0.058	0	1	417780
Rhaeto-Romanic language region	0.004	0	1	417780
Occupation in Production	0.151	0	1	417780
Occupation in Engineering	0.068	0	1	417780
Occupation in Construction	0.132	0	1	417780
Occupation in Sales	0.088	0	1	417780
Occupation in Gastronomy/Tourism	0.212	0	1	417780
Occupation in White Collar or Health	0.319	0	1	417780

Notes: This table presents summary statistics for the covariates used in all previous specifications.

[a]: Number of persons affected = Household size - 1

[b]: 0: Male, 1: Female

[c]: Low Education: Max. Compulsory High School, Medium Education: Max. Vocational Training, High Education: Max. College

Appendix D Summary Statistics

Appendix E Robustness Checks

Table E.1. Robustness Checks for the Main Difference-in-Differences Results

	Pooled DiD	With Bootstrapped SEs	Clustering on Canton Level	With 2012	With Canton FEs	With RAV FEs	Control: >40 Mins Traveltime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
dd	14.472***	14.472***	14.472***	14.601***	12.489***	10.661***	13.752***
	(3.728)	(2.270)	(3.498)	(3.358)	(3.675)	(3.352)	(3.828)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	266.27	266.27	266.27	265.98	266.27	266.27	263.67
R^2	0.114	0.114	0.114	0.113	0.142	0.153	0.115
N	348448	348448	348448	417780	348448	348448	300508

Notes: This table shows the results from several pooled Difference-in-Differences regressions. The basis for thee analysis are the regressions shown in table 1. We treat job seekers from Bern, who entered unemployment in 2012 or later in Bern, as treated. Job seekers entering unemployment in 2012 or later in the remaining cantons are used as the control group. We use the same set of covariates used in equation (2). In column (1), we show the estimated DiD-coefficients from the basic pooled Diff-in-Diff analysis in table 1. In column (2), we use bootstrapped standard errors (50 bootstrap replications). In column (3), we cluster all standard errors at the canton level. In column (4), we include observations from the year 2012, which we previously left out. In columns (5) and (6), we add canton- and RAV fixed effects (in addition to controlling for the canton of Bern in the DiD-framework). In column (7), we only include observations from Bern and from municipalities with a commuting time longer than 40 minutes. We can see that our results are robust to different levels of clustering as well as different specifications.

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

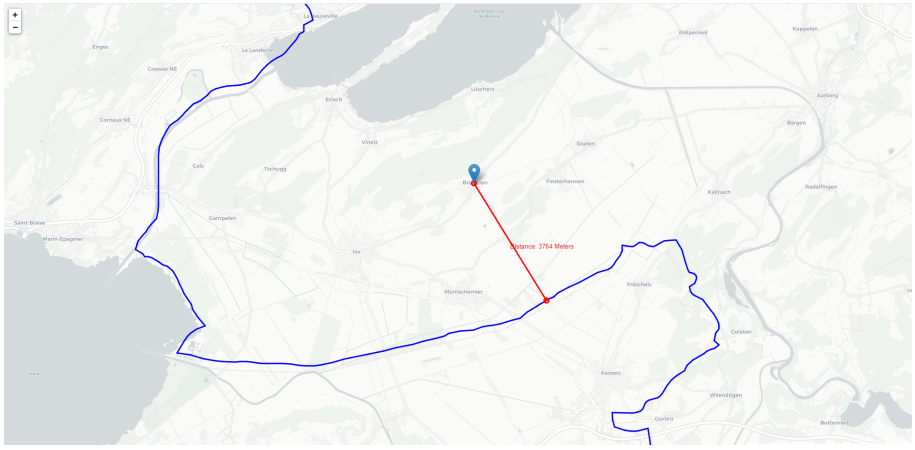


Figure F.1. Calculation of Closest Point on Border

This figure shows an example of how the border point closest to a zip code was calculated. The point delivered by `pgeocode` is the starting point for the calculation. We then calculate the air distance to every point on the border of Bern (Blue Line) and continue with the point for which this distance is the smallest.

Appendix F Distance Calculation

In this section, we describe in detail how the travel distances and travel times used for the analysis in section 5.4 were calculated. We first calculate the travel distances and -times based on zip code level and then use a weighted average of zip code travel distances and -times for each municipality, at which the geolocation information for each unemployment spell is defined. The corresponding weights are the number of unemployment spells being located in a zip code area, relative to the total number of unemployment spells in a municipality.

To calculate the travel distances and -times, we use a two-step-procedure: In the first stage, for each zip code in Switzerland, we use the latitude/longitude-geolocation data generated by the `Nominatim()`-function contained in the `pgeocode` Python library to calculate the center point (or point of the largest village, respectively) for each zip code. Using the latitude/longitude-geolocation data and `geojson`-files containing geodata of the Swiss cantons, we calculate the shortest (air-)distance from a zip code area to the border of Bern. Using this procedure, we generate the start- and end points to calculate travel distances and -times. Note that this first stage of calculating travel distances and -times does not account for geographical special cases like mountains or road difficulties. However, executing the first stage with travelling distances and -times would require to calculate these for every point on the border of Bern, which is not feasible.

In the second stage, we use the assignment of each zip code in Switzerland (both inside and outside of Bern) to the closest point on the Bern border to calculate the travel distance and travel time (by car) between both points using the Google Maps Distance Matrix API. This service by Google allows for large travel calculation queries for a series of locations. Using this approach, we can account for the special geography of Switzerland, with many mountain ranges and lakes that create a discrepancy between air distance and commuting distance. In particular, this approach reveals that, for example, the canton of Valais, despite sharing a large portion of the border, does not allow for quick commuting between both cantons. This is due to the “Bernese Alps”, a mountain range in the south of Bern, which only leaves a handful of mountain passes between both cantons and thus makes commuting between them very time consuming. This mountain range is also the reason why, for some zip codes, the travel distances and -times cannot be calculated using an API, because the closest point on the Bern border lies on a point in the mountain range, which cannot be reached

by car. For these special cases, we calculate the travel distances and travel times by hand based on the closest feasible point (This is the case for some zip codes in Valais and Grisons). Once the travel distances and -times are calculated, we aggregate them on a municipality level using the number of unemployment spells from a zip code area in a municipality relative to the total number of unemployment spells in that municipality as weights.

Appendix G Heterogeneity with Respect to Sociodemographics

Table G.1. Heterogeneity with Respect to Sociodemographics

	Pooled DiD	Female	Male	Low Paid Service Sector	Blue Collar	White Collar	Married Married	Unmarried Unmarried	Low Education	Medium Education	High Education	Non-Swiss	Swiss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
dd	14.472***	8.318*	17.069***	17.069***	24.620***	6.053	17.267***	11.197***	26.069***	12.606***	3.347	21.827***	8.282**
	(3.728)	(4.715)	(4.174)	(4.174)	(4.483)	(7.774)	(4.826)	(3.789)	(7.008)	(4.299)	(4.413)	(4.956)	(4.061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	266.27	280.05	258.74	258.74	244.24	295.71	276.26	256.57	278.34	254.41	270.21	265.88	266.62
R^2	0.114	0.106	0.125	0.125	0.165	0.123	0.126	0.113	0.179	0.107	0.106	0.144	0.107
N	348448	123162	225286	225286	120824	53991	171637	176811	99211	137941	111296	167251	181197

Notes: This table shows the results from pooled Difference-in-Differences regressions, conditional on job seeker/unemployment spell characteristics. In column (1), we display the results from our original Diff-in-Diff specification. Columns (2) and (3) contain the results for female and male job seekers, respectively. In columns (4) - (6), we display the results for White Collar-, Blue Collar-, and Low Paid Service Sector workers, respectively. Columns (7) and (8) present the results for married and unmarried workers, and columns (9) - (11) present the results for low education-, medium education- and high education workers, respectively. Note the definition of these categories below. Columns (12) and (13) present the results for non-Swiss citizens and Swiss Citizens, and columns (12) and (13) for German and French native speakers, respectively.

Low Education: Max. Compulsory High School, Medium Education: Max. Vocational Training, High Education: Max. College

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