Market Externalities of Large Unemployment Insurance Extension Programs*

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Abstract

This paper offers quasi experimental evidence of the existence of spillover effects of UI extensions using a unique program that extended unemployment benefits drastically for a subset of workers in selected regions of Austria. We use ineligible unemployed in treated regions, and a difference-in-difference identification strategy to control for preexisting differences across treated and untreated regions. We uncover the presence of important job search externalities: in treated regions, as the search effort of treated workers plummets, the job finding probability of untreated workers increases, and their average unemployment duration and probability of long term unemployment decrease. These effects are the largest when the program intensity reaches its highest level, then decrease and disappear as the program is scaled down and finally interrupted. We use this evidence to assess the relevance of different equilibrium search and matching models and discuss the policy implications of our results for the EUC extensions in the US.

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1 Introduction

When it comes to understanding the effects of variations in the generosity of unemployment insurance on labor market outcomes, it is striking to see how much we know about partial equilibrium responses, and how little we know about equilibrium (or macro) responses. Concerning partial equilibrium responses, theory unambiguously predicts that higher benefits lead to longer unemployment duration[1], and empirically, a large number of well-identified estimates have been produced. Concerning macro effects, the literature on unemployment insurance has always recognized the potential importance of general equilibrium effects for assessing the optimal level of these programs (see for instance the surveys of Atkinson [1987] or Krueger and Meyer [2002]), but the existence and potential magnitude of these general equilibrium effects is still highly debated. Despite the large literature on equilibrium search-and-matching representations of the labor market, there is no theoretical consensus on the sign and magnitude of equilibrium effects of UI on unemployment and labor market outcomes. And empirically, it has always proven extremely arduous to estimate equilibrium effects. Hence our inability to tell to what extent micro estimates of the effects of UI are valid to infer the macro effects of large variations in the generosity of the UI system on total unemployment. During the Great Recession, for instance studies have found the overall effect of the large UI federal extensions on unemployment to be relatively small (Rothstein [2011]; Valletta and Farber [2011]), especially compared to traditional partial equilibrium micro-evidence on the effects of UI benefits, and some suggest that this might be due to the presence of significant job search externalities.

This paper wishes to shed light on the equilibrium (macro) effects of UI benefits by investigating market externalities of large UI extensions. By doing so, we wish to address two important sets of questions. First of all, do large unemployment insurance (UI) extension programs create market externalities and if yes, can we empirically identify their existence and potential size? The second set of questions that our paper wishes to answer relates to the functioning of labor markets. How elastic are wages with respect to outside options of workers? Is there job-rationing in the short run? And in the longer run? Is the search process in the labor market efficient? By looking into the very nature of search externalities, our paper aims at a better understanding of labor markets by discriminating between different models of search in the labor market.

Our paper contributes to the first set of questions by offering compelling quasi-experimental evidence of the existence of spillover effects of UI extensions using a unique program in Austria that extended unemployment benefits drastically for a large subset of workers in selected regions

[1] Whether this effect is driven by distortionary moral hazard effects or non-distortionary wealth/income effects is still an open question. See Chetty [2008]

of Austria. We use unemployed workers in treated regions who are very similar to treated workers but who are ineligible based on past work experience requirements in the REBP program, and a difference-in-difference identification strategy to control for preexisting differences across treated and untreated regions. Our quasi-experimental setting has a number of advantages.

First, treatment is massive: treated workers received an extra 3 years of covered unemployment with unchanged benefit level. This translated into a huge effect on the effort of treated workers, already documented in Lalive [2008], which makes it the most promising setting to investigate manipulation of equilibrium labor market conditions.

Second, the set-up of the REBP program makes it a perfect quasi-experimental setting to identify the presence of search externalities. REBP was enacted only in a subset of regions and for a large subset of workers. While the choice of treated regions and workers is partially endogenous, we use specific features of the REBP program to build a credible identification strategy. Because of past experience eligibility requirements of the REBP program, we consider workers just below the experience requirement who could not qualify for REBP. These workers are very similar to REBP-eligible workers, they compete in the same labor market but represent a small fraction of the treated labor force. As a consequence, they are very likely to be affected by the drastic drop in search effort of treated workers. Moreover, we can compare them to similar workers in non-REBP regions to uncover the presence of search externalities. Our strategy therefore relies on two important assumptions. First, that there are no region-specific shocks contemporaneous with the REBP program. The choice of regions eligible for REBP was determined by the size of the steel sector, but all workers irrespective of industry were eligible. Given that the size of the steel sector never exceeded 15% of the labor force in REBP regions, we focus on industries that are not related to the steel sector. We show compelling evidence in favour of our parallel trend assumption and argue that if anything, region-specific shocks are likely to bias downwards the magnitude of our spillover estimates. The second assumption requires that there is no change in unobserved characteristics of untreated workers contemporaneous with the REBP program. In favour of this assumption, we show that there is no change in observed characteristics or in the inflow rate into unemployment of untreated workers in REBP relative to non-REBP regions during the REBP program. Again, we argue that if anything, variations in unobserved characteristics of untreated workers would likely bias downwards the magnitude of our spillover estimates. Finally, we show evidence that REBP and non-REBP regions represent isolated labor markets, so that our estimates are not contaminated by geographical spillovers.

The last advantage of our quasi-experimental setting is the availability of great administrative data on the universe of unemployment spells in Austria since 1980. By matching these data with data on the universe of employment spells in Austria since 1949 we were able to compute past work experience at any point in time for all unemployed workers, thus determining with precision eligibility for the REBP program in treated regions. Our data also enables us to look at many different outcomes, from unemployment and non-employment durations, to reemployment characteristics and wages. Moreover, we have data for all periods before, during and after the REBP program so that we are able to show that spillovers totally disappear after the REBP program is repealed.

Our results demonstrate the presence of important job search externalities. In treated regions, as the search effort of treated workers plummets, the job finding probability of untreated workers increases, and their average unemployment duration and probability of long term unemployment decrease. These effects are the largest when the program intensity reaches its highest level, then decrease and disappear as the program is scaled down and finally interrupted. We use this evidence to assess the relevance of different search-and-matching representations of the labor market. In particular, we show that the sign and magnitude of our estimated externalities is only compatible with a model where returns to labor are decreasing and wages are not very flexible to outside options of workers. We show that in fact, REBP benefits had almost no impact on reemployment wages of unemployed workers, even though we can detect a small bargaining effect building over time when controlling for duration dependence effects. We also discuss the policy implications of our results for the EUC extensions in the US. We argue that spillover effects may have been even stronger in the US, which explains the very low elasticities estimated in Rothstein [2011] or Marinescu [2013] using variations in the magnitude and timing of extensions across US states. Our results also confirm that temporary extensions enacted in reaction to business cycles downturns such as EUC are a lot less socially costly than previously thought, but that governments should avoid making these extensions permanent as most European countries have done in the 70s and 80s. When determining the optimal time span of temporary extensions, governments should pay attention to the pace of the decrease in externalities over time. In the absence of direct measures of these externalities, two important indicators should be used: the cross-sectional correlation between UI benefits and wages of new hires, and the time series evolution of the fraction of eligible to non-eligible in the number of new hires.

The remainder of the paper is organised as follows. Section 2 discusses the related literature. Section 3 presents the theoretical framework and explains how different assumptions in search and matching models lead to opposite predictions concerning the sign and magnitude of externalities. Section 4 presents the institutional background of the REBP program and section 5 presents the data. In section 6, we explain our identification strategy and in section 7 we present our results. Section 8 draws policy implications, with an application to the EUC extensions.

2 Related literature

The treatment evaluation literature has long advocated that identifying spillover effects of labor market programs is critical because, if such externalities exist, they will bias traditional estimates of treatment effects of these programs. In particular, studies estimating the impact of active labor market policies such as randomized programs of counselling for job seekers have long raised the issue that part of the treatment effect estimated by comparing treated and untreated unemployed in the same labor market might be due to the existence of displacement effects. Recently, several papers have tried to directly estimate the magnitude of these potential effects.Blundell et al. [2004] study the effect of a counselling program for young unemployed in the UK and find little evidence of displacement effects. Ferracci et al. [2010] study a program for young employed workers in France and find that the direct effect of the program is smaller in labor markets where a larger fraction of the labor force is treated.

Gautier et al. [2012] analyze a randomized job search assistance program organized in 2005 in two Danish counties. Comparing control individuals in experimental counties to job seekers in some similar non-participating counties, their results suggest the presence of substantial negative treatment externalities.

More convincingly, Crepon et al. [2012] analyze a job search assistance program for young educated unemployed in France with two levels of randomization: the share of treated was randomly assigned across labor markets, and within each labor market individual treatment was also randomized. They find significant negative treatment externalities for men (though not for women).

As opposed to active labor market policies, there are very few papers trying to estimate potential spillover effects of unemployment insurance, apart from Levine [1993] who finds, using variations in UI legislation across states and time in the US, that increases in the replacement rate of UI decreases unemployment duration among the unemployed who are ineligible for UI.

3 Theoretical framework

Here, we present a simplified, static version of an equilibrium search and matching model and characterize the comparative static for steady state equilibria. The labor market is characterised by the presence of matching frictions. There are u unemployed workers. Among these workers, there are two groups, $i \in a, b$, with different unemployment insurance benefits, and $u = u_a + u_b$. Each individual worker exerts some effort $e_i = e(B_i)$, where e is a decreasing function of benefits received B. Unemployed workers face v vacancies opened by firms, and the total number of matches done is given by an aggregate matching function $m(\overline{e \cdot u}, v) = \omega_m \cdot (\overline{e \cdot u})^{\eta} \cdot v^{1-\eta}$, where $\overline{e \cdot u} = e_a \cdot u_a + e_b \cdot u_b$ The key assumption is that employers cannot discriminate between unemployed from group a and b and cannot therefore post differentiated vacancies for each group. This assumption seems realistic in the present application because groups a and b are defined based on age and the total number of years of experience in the past 25 years at the moment the individuals become unemployed. It is difficult to strictly condition job openings on these characteristics, and more generally, it is complicated for firms to condition their openings on the usual characteristics affecting unemployment benefits such as wage in the previous job, etc. Therefore, when opening a vacancy, even after conditioning for good proxies for experience or qualifications, a firm can never tailor it perfectly to the level of benefits of different individuals.

As a consequence, there will be only one labor market tightness in equilibrium for the two groups, defined as $\theta \equiv v/(\overline{e \cdot u})$ For each group, the individual job-finding probability is given by $e_i \cdot f(\theta) = e_i \cdot m(1, \theta)$. This job-finding probability is an increasing function of θ (meaning that $\frac{\partial e \cdot f(\theta)}{\partial \theta} > 0$). Equivalently, we can define the vacancy-filling probability for each vacancy opened by the firm as: $q(\theta) = m(1/\theta, 1)$ and we have $\frac{\partial q(\theta)}{\partial \theta} < 0$.

From the equality of flows out and into unemployment, we can write that at the steady state:

$$n = (1 - (u_a + u_b)) + f(\theta) \cdot (e_a \cdot u_a + e_b \cdot u_b) \tag{1}$$

Because at the steady state the share p (resp. $1 - p$) of unemployed of groups a (resp. b) is stable, we can rewrite total employment n supplied in the labor market as a weighted sum of employment supplied by group a, $\tilde{n_a^s}$ and employment supplied by group b, $\tilde{n_b^s}$

$$n^s = p[\underbrace{1 - u(1 - f(\theta) \cdot e_a)}_{\tilde{n_a^s}}] + (1 - p)[\underbrace{1 - u(1 - f(\theta) \cdot e_b)}_{\tilde{n_b^s}}]$$

Following Landais et al. [2010], we interpret $n^s = n^s(\theta, e_a(B_a, \theta), e_b(B_b, \theta), p)$ as a labor supply that we can represent as an increasing function of θ in a $\{n, \theta\}$ diagram.

A representative firm maximizes profit $\pi = f(n) - n(s \cdot w_a + (1-s) \cdot w_b) - \frac{r}{q(\theta)} \cdot (n - (1-u))$ where r is the recruiting cost of opening a vacancy, s (resp. $1-s$) is the share of employed workers coming from group a (resp. b), and w_a (resp. w_b) is the wage of workers from group a (resp. b). We assume that workers from both groups are perfect substitute but that employers cannot discriminate openings. Note that we would get similar results if we allowed for discrimination but had complementarities in the production function[2]. The first-order condition of the firm with respect to employment level[3] n is

$$f'(n) = (s \cdot w_a + (1-s) \cdot w_b) + \frac{r}{q(\theta)} \tag{2}$$

Equation (2) implicitly defines a labor demand function $n^d(\theta, w_a, w_b)$ whose properties depend on the assumptions made on $f(.)$ and on the wage setting process defining w_a and w_b. These properties are critical to determine the sign and magnitude of externalities, as explained below. Moreover, to the extent that $n^d(\theta)$ is a continuous function of labor market tightness, we can define a labor market equilibrium by the condition:

$$n^s(\theta, e_a(B_a, \theta), e_b(B_b, \theta), p) = n^d(\theta, w_a, w_b) \tag{3}$$

Equilibrium condition (3) defines θ as an endogenous variable, affected by the level of benefits B_a and B_b of unemployed individuals. Note also that once θ is determined in equilibrium, we immediately recover the equilibrium level of employment for both groups n_a^{s*} and n_b^{s*}, as shown in figure 1 panel A. Variations in UI benefits, because they directly affect labor supply, dictate equilibrium adjustments in θ, which, in presence of matching frictions, acts as a price equating labor demand and labor supply. Importantly, if the wage setting process is such that $w_a(B_a)$ and $w_b(B_b)$ depend on the outside options of workers, then labor demand n^d also depends on UI benefits. In this case, the equilibrium effects on θ of variations in UI benefits arise from shifts in both labor supply and labor demand, as shown in figure 1 panel B.

Externalities: diminishing returns vs wage flexibility We start from a situation in which both groups have the same UI benefits, so that their labor supply $\tilde{n_a^s}$ and $\tilde{n_b^s}$ are identical. As shown in figure 1, equilibrium is determined by the intersection of labor supply and labor demand at E_1 in the $\{n, \theta\}$ diagram. We now consider the effect of increasing benefits of group a, leaving benefits of group b unchanged. We define UI benefit externalities as $\frac{d(e_b \cdot f(\theta))}{dB_a}$, namely

[2]In this case, the relative price of each type of workers would be given by the relative labor market tightness, and the model would bear strong similarities to a general equilibrium incidence model *a la* Harberger.

[3]Firms take labor market tightness as given, and for them it is equivalent to choose employment level or the number of vacancies, given that v vacancies automatically translate into $v/q(\theta)$ jobs.

the effect on the job finding probability of group b individuals of a change in the benefit level of individuals in group a. The reason such externalities may exist is because in equilibrium, labor market tightness is an endogenous function of B_a, and we have:

$$\frac{d(e_b \cdot f(\theta))}{dB_a} = \frac{\partial e_b}{\partial \theta} \cdot \frac{\partial \theta}{\partial B_a} \cdot f(\theta) + f'(\theta) \cdot \frac{\partial \theta}{\partial B_a} \cdot e_b \tag{4}$$

The sign and magnitude of externalities[4] depends on the sign and magnitude of $\frac{\partial \theta}{\partial B_a}$. Equilibrium adjustments in θ in response to a change in B_a are first coming from variations in labor supply: because unemployed from group a exert less effort, their labour supply decreases and the new aggregate labor supply, which is a weighted sum of labor supply of both groups, shifts to the left, as shown in figure 1. Then, if wages are independent of the outside options of workers, labor demand is unaffected and the new equilibrium tightness is given by a movement along the demand curve, as shown in figure 1 panel A. But if wages are bargained over, an increase in benefits of unemployed from group a will lead to higher bargained wages on average, which decreases the return from opening vacancies for firms. This will shift labor demand to the left, and the new equilibrium tightness is the result of a shift in both labor demand and labor supply as shown in panel B.

Two major forces therefore determine the sign and magnitude of externalities. First, returns to labor in the production function. The first-order condition of the firm (2) which implicitly defines labor demand as a function of θ shows clearly that returns to labor $f'(n)$ determine the steepness of the labor demand function in the $\{n, \theta\}$ diagram. If technology is linear for instance, equation (2) defines a perfectly elastic labor demand as a function of θ, in which case, variations in labor supply have no effects on θ in equilibrium. This will likely be the case if there exists perfect substitutes for workers a and b (other types of workers, or capital). To the contrary if returns to labor are decreasing (capturing the fact that there is no close substitutes to workers a and b in the short run) then labor demand is decreasing function of θ, and a decrease in labor supply will increase θ in equilibrium. And if labor demand is perfectly rigid, a UI benefit-induced decrease in labor supply has no effect on employment, but firms bear the full incidence since θ, and as a consequence recruiting costs, increase sharply.

The second force is the correlation between wages and outside options of workers. This

[4] The externalities defined here are the consequence of an equilibrium mechanism whereby a price (θ) adjusts in order to clear the market. In some sense, they could be thought of as a mere incidence effect. The reasons such price adjustments matter for welfare is twofold. First of course, in our two groups setting, they matter because firms cannot discriminate and therefore cannot reach the first-best allocation of vacancies. But more importantly, even if firms could perfectly discriminate, equilibrium adjustment in θ are not simple incidence effects because of the existence of frictions in the labor market: if the Hosios condition does not hold, then any adjustment in θ has first-order welfare effect.

correlation depends on the wage setting process. In search-and-matching models, there is indeterminacy of the wage setting process, since multiple wage setting processes are compatible with equilibrium, to the extent that they define wages within the band of acceptable wages from both firms and workers (Hall [2005]). If wages are perfectly independent of the outside options of workers for instance, variations in B_a have no effect on w_a, and therefore do not affect labor demand. But if wages are strongly correlated to outside options of workers (which would be the case if wages are bargained over and workers have a large bargaining power), then labor demand could decrease in response to a increase in B_a, leading to a decrease in θ in equilibrium.

The respective importance of these two forces therefore determines the sign of $\frac{\partial\theta}{\partial B_a}$. If wages are independent of benefits, and returns to labor are decreasing, then $\frac{\partial\theta}{\partial B_a} > 0$ and therefore externalities should be positive. This is the situation depicted in panel A of figure 1. If returns to labor are almost constant and wages are strongly correlated to outside options of workers, then both $\frac{\partial\theta}{\partial B_a}$ and externalities might be negative. This situation is depicted in panel B of figure 1.

In the empirical section, we identify externalities of a large UI extension program. These estimates inform us about the functioning of the labor market and the respective importance of returns to labor and wage flexibility in determining the macro effect of UI benefits. We also pay particular attention to the behaviour of wages in order to uncover the mechanics of these externalities.

4 Austrian Unemployment Insurance and the REBP

The Unemployment Insurance System Before August 1989, an unemployed person could draw regular unemployment benefits (UB) for a maximum period of 30 weeks provided that he or she had paid unemployment insurance contributions for at least 156 weeks within the last 5 years.[5] In August 1989 the potential duration of UB payments became dependent not only on previous experience but also on age at the beginning of the unemployment spell. Benefit duration for the age group 40-49 was increased to 39 weeks if the unemployed has been employed 312 weeks within the last 10 years prior to the current spell. For the age group 50 and older, UB-duration was increased to 52 weeks if the unemployed has been employed for at least 468 weeks within the last 15 years. Our empirical analysis below controls for the general change in benefit duration. Voluntary quitters and workers discharged for misconduct can not claim benefits until a waiting period of 4 weeks has passed. UB recipients are expected to search actively

[5] UB duration was 20 weeks for job-seekers who did not meet this requirement. This paper focuses on individuals who were entitled to at least 30 weeks of benefits.

for a new job that should be within the scope of the claimant's qualifications, at least during the first months of the unemployment spell. Non-compliance with the eligibility rules is subject to benefit sanctions that can lead to the withdrawal of benefits for up to 4 weeks. **Job seekers who leave unemployment before exhausting their benefits remain eligible during a period of three years counted from the data when they registered for their first spell.**

Compared to other European countries, the replacement ratio (UB relative to *gross* monthly earnings) is rather low. The amount of UB payments depends on previous earnings and, in 1990, the replacement ratio was 40.4 % for the median income earner; 48.2 % for a low-wage worker who earned half the median; and 29.6 % for a high-wage worker earning twice the median. On top, family allowances are paid. UB payments are not taxed and not means-tested. There is no experience rating.

After UB payments have been exhausted, job seekers can apply for 'transfer payments for those in need' ("Notstandshilfe").[6] As the name indicates, these transfers are means-tested and the job seeker is considered eligible only if she or he is in trouble. These payments depend on the income and wealth situation of other family members and close relatives and may, in principle, last for an indefinite time period. These transfers are granted for successive periods of 39 weeks after which eligibility requirements are recurrently checked. These post-UB transfers are lower than UB and can at most be 92 % of UB. In 1990, the median post-UB transfer payment was about 70 % of the median UB. Note however, that individuals who are eligible for such transfers may not be comparable to individuals who collect UB because not all individuals who exhaust UB pass the means test. The majority of the unemployed (59 %) received UB whereas 26 % received post-UB transfers. In sum, the Austrian unemployment insurance system is less generous than many other continental European systems and closer to the U.S. system (Nickell and Layard, 1999).[7]

Restructuring of the Austrian steel industry and the REBP To protect its assets after World War II from Soviet appropriation and to provide the capital needed for reconstruction, Austria nationalized its iron, steel, and oil industries, large segments of the heavy engineering and electrical industries, most of the coal mines, and the nonferrous metals industries. Firms in the steel sector were part of a large holding company, the Oesterreichische Industrie AG,

[6] This implies that job seekers who do not meet UB eligibility criteria can apply at the beginning of their spell

[7] It is interesting to note that the incidence of long-term unemployment in Austria is closer to U.S. figures than to those of other European countries. In 1995, when our sample period ends, 17.4 % of the unemployment stock were spells with an elapsed duration of 12 months or more. This compares to 9.7 % for the U.S. and to 45.6 % for France, 48.3 % for Germany, and 62.7 % for Italy (OECD, 1995).

OeIAG. By the mid-1970s this holding company was running into serious problems related to shrinking markets, overstaffing, too heavy concentration on outmoded smokestack industries, insufficient research and development, and low productivity. Initially, the Austrian government covered the losses by subsidies. But in 1986, after the steel industry was hit by an oil speculation scandal and failure of a U.S. steel plant project, this protectionist policy was abolished. A new management was appointed and a strict restructuring plan was implemented. This plan aimed at focusing on the holdings' core competencies. The result were layoffs due to plant closures and downsizing, particularly in the steel industry.

To mitigate the labor market problems in the concerned regions the Austrian government enacted a law that extended UB-entitlement to *209 weeks* for a specific subgroup. An unemployed worker became eligible to 209 weeks of UB if he or she satisfied, *at the beginning* of his or her unemployment spell, each of the following criteria: (i) age 50 or older; (ii) a continuous work history (780 employment weeks during the last 25 years prior to the current unemployment spell); (iii) location of residence in one of 28 selected labor market districts since at least 6 months prior to the claim; and (iv) start of a new unemployment spell after June 1988 or spell in progress in June 1988.

The minister for social affairs, a member of the ruling party SPÖ, was in charge of selecting those regions that became eligible to the program. Figure 2 shows the distribution of REBP across the 2361 communities in Austria. Interestingly, the treated regions (communities with blue shading) were all located on a contiguous area located in the Eastern part of Austria and stretching from the Northern border to the Southern border. The program covers parts of the provinces Burgenland, Carinthia (Kärnten), Lower Austria (Niederösterreich), Upper Austria (Oberösterreich), and Styria (Steiermark).

It is interesting to look at some characteristics of the chosen regions.[8] On the one hand, the entitled regions were characterized by a strong concentration of employment in the steel sector. In the REBP regions, roughly 15 % of workers were employed in the steel industry firm, whereas in the non-REBP regions the corresponding figure was below 5 %. On the other hand, it is not possible to detect, *before the REBP starts*, any important differences between treated and non-treated regions in terms of the unemployment rate or the fraction of long-term unemployed.

The REBP was in effect until December 1991 when a reform of these rules took place which

[8]Records of the meetings in which the set of regions eligible to the program was decided are not open to the public. However, the ultimate decision was heavily criticized by opposition parties and media as being biased towards the clientele of the ruling parties.

came into effect in January 1992. This 1991-reform left all claims in progress unaffected. The 1991-reform enacted two important changes. First, the reform abolished the benefit extension in 6 of the originally 28 regions. We exclude from our analysis the set of treated regions that were excluded after the reform. Second, the 1991-reform tightened eligibility criteria to extended benefits: new beneficiaries had to be not only residents, but also *previously employed* in a treated region.

The program was first abolished in August 1993 **but job seekers who satisfied all requirements could still get REBP until some date in 1995.**

Apart from the REBP, the second important measure to alleviate the problems associated with mass redundancies in the steel sector was the so-called 'steel foundation'. Firms in the steel sector could decide whether to join in order to provide their displaced workers with re-training activities that were organized by the foundation. Member firms were obliged to finance this foundations. Displaced individuals who decided to join this out-placement center were entitled to claim regular unemployment benefits for a period of up to 3 years (later 4 years) regardless of age. In 1988, the foundation consisted of 22 firms. We exclude all workers employed or reemployed in the steel sector in order to make sure that REBP-entitled individuals in our sample do not have access to re-training activities or other active labor market programs (see Winter-Ebmer, 2001, for an evaluation of the steel foundation).

Early Retirement Rules: Austrian social security legislation provides for regular old age pensions at age 65 for men and age 60 for women. Pension benefits depend on contributions to the pension system in the 156 months (13 years) prior to leaving the labor force, and on the total number of months contributed to the pension system.

There are two early retirement pathways available at age 60 for men and at age 55 for women. The first is provided for individuals who have a long contribution history, that is, worked for at least 420 months (35 years) prior to claiming early retirement. Also, individuals applying for this early retirement option must have worked for at least 2 out of the previous 3 years before entering early retirement. The second early retirement option is available to individuals who have spent at least 12 out of the previous 15 months claiming unemployment benefits, post-UB transfers, or special income support.

5 Data

The data we use comes from the universe of UI spells in Austria from 1980 to 2010. For each spell we observe the dates of entry and exit into paid unemployment, as well as information on age at the start of the spell, region of residence at the beginning of the spell, education, marital status, etc. This information is merged at the individual level with the universe of social security data in Austria from 1949 to 2010, which contains information on each employment spell (as well as information for each spell in a benefit program and information on pensions and retirement). We use this extra information to compute experience in the past 25 years for each individual at any point in time, in order to determine eligibility status for REBP. We also use social security data to compute wages before and after each unemployment spell, as well as the total duration of non-employment after the end of an employment spell. Finally, the social security data gives us useful information about previous and subsequent employers (such as industry, address, et) for each unemployment spell.

Table 1 gives descriptive statistics for unemployed individuals in our sample, for treated and untreated regions before the introduction of the reform (panel A), and for treated and untreated unemployed in treated regions before the introduction of the reform (panel B).

Sample selection Regions participating in the REBP program are not chosen at random, but because of the importance of their steel sector. To control for this endogeneity bias, we completely remove the steel sector from our analysis. More specifically, we get rid of all individuals who were employed in the steel sector immediately prior to becoming unemployed as well as unemployed whose subsequent employer is in the steel industry. Note that the share of the steel sector in total employment is never larger than 15% in REBP regions at any point int time. To further attenuate concerns about endogeneity, we also explore in our robustness analysis the effects of removing different industries from the analysis, based on their connection to the steel sector. If anything, as we explain below, endogeneity is likely to bias towards zero our estimates of the positive spillovers of REBP on the untreated, so that we can think of the magnitude of the effect estimated here as a lower bound.

Because of early retirement programs in Austria during our period of analysis, women can go directly from REBP to early retirement programs. For women, it is therefore unclear whether the effect of REBP can be interpreted as a reduction in search effort or as a extensive margin decision to exit the labor market. These having very different implications for equilibrium analysis, we mainly focus on men 50 to 54 because they cannot go directly from REBP to early retirement, and therefore the effect of REBP is clearly to reduce search effort of the treated.

6 Identification strategy

Quasi-experimental framework The best way to understand our identification strategy is to compare it to the following experimental framework. Imagine two comparable labor markets, $M = 0, 1$. Labor market $M = 1$ is randomly selected to receive some exogenous treatment between time $t = t_0$ and $t = t_1$. Labor market $i = 0$ is a control. In labor market $M = 1$, a random subset of workers is treated ($T = 1$) while the rest of the workers do not receive treatment ($T = 0$). Following the treatment evaluation and potential outcome literature, under the double randomisation assumption, the average externality of the treatment on outcome y is identified by:

$$AE = E(y|T = 0, M = 1) - E(y|M = 0)$$

In our case, labor markets $M = 1$ are all Austrian counties that received REBP, while markets $M = 0$ are all the remaining Austrian counties in a radius distance of 100 minutes from REBP counties. Treated workers ($T = 1$) are all workers who were eligible for REBP while untreated workers in markets $M = 1$ are all workers who were not eligible because they did not have a continuous work history of 15 years in the past 25 years.

Because of the lack of double randomization, our identification strategy relies on three important assumptions. First, concerning labor markets, we assume that differences in unobserved characteristics of markets $M = 1$ and $M = 0$ are fixed over time. Table **??** panel A shows average characteristics of unemployed workers aged 50 to 54 in treated and untreated counties before the introduction of REBP. This table confirms that the two regions are actually pretty similar, and that unemployment duration or the fraction of long term unemployed are not statistically different across the two regions before REBP. The parallel trend assumption that we are making relates to a standard difference-in-difference identification strategy. Observations of labor markets $M = 1$ and $M = 0$ prior to REBP and after the end of REBP ensures identification of the labor market fixed effects, and the evolution of labor market $M = 0$ during REBP years offers a counterfactual for the evolution of market $M = 1$ during the same period, in the absence of REBP. The main concern with such an assumption is that regions that received REBP treatment where not chosen at random so that the parallel trend assumption might be violated. Indeed, as stated in section 4, treated regions were chosen because of their higher share of employment in the steel sector that was being restructured. We therefore exclude all the steel sector from our sample and only focus on non-steel workers. Because the steel sector only accounts for at most 15% of employment in REBP regions, the spillover effects of the restructuring can be assumed to be small on industries not directly related to the steel industry supply chain. Moreover, if, in the absence of UI extensions, non-steel industries in REBP treated regions had experienced a relatively higher unemployment than non-steel industries in non-treated regions during the REBP period, this would bias the diff-in-diff estimates of REBP job search externalities towards zero, so that our estimates are likely to be a lower bound for the average externality AE. Note that in our robustness checks we investigate whether removing or including certain industries more connected to the steel affects the results. We also include specifications testing for the sensitivity of the results to the inclusion of pretty flexible differential time trends prior to the REBP reform. Not surprisingly, we find that including differential trends increases slightly the magnitude of the estimated externalities.

The second important assumption underlying our identification strategy is that treated and untreated labor markets are (almost) isolated. If this was not the case, unemployed workers in market $M = 0$ might also be subject to treatment externalities, which would again bias towards zero the externalities estimated from comparing untreated workers in market $M = 1$ to workers in market $M = 0$. To get a sense of how geographically integrated treated and non-treated counties are, we follow Manning and Petrongolo [2011] and compute the distance between residence while unemployed and job when reemployed. Table **??** shows that this average distance is relatively small, around 25 minutes, suggesting that in Austria, labor markets are essentially local, with a low level of geographical mobility.

Another useful indicator of the level of geographical integration is the fraction of new hires in non-treated counties coming from treated counties. Figure 3 panel A maps the fraction of men aged 50 to 54 coming from treated counties in the total number of new hires of men aged 50 to 54 in non-treated regions for all the years when the REBP was not in place. Unsurprisingly, there only few counties where this fraction is above 4% and a handful of counties where this fraction is above 20%. Most of these counties are situated in a narrow bandwidth, 10 to 20 minutes to the border of REBP counties. In our baseline analysis, we remove these counties from our control region $M = 0$. But in our robustness analysis, we use these counties to show that we can also detect the presence of externalities in these counties highly integrated to REBP regions.

In figure 3 panel B, we map the fraction of men aged 50 to 54 coming from non-treated counties in the total number of new hires of men aged 50 to 54 intreated regions for all the years when the REBP was not in place. Interestingly, the comparison of panel A and B reveals that the fraction of residents from non-REBP regions finding a job in REBP regions is larger than the fraction of REBP residents finding a job in a non-REBP region. Besides, panel B shows that there is substantial variation in the openness of REBP counties to non-REBP residents, which creates variation in treatment intensity across REBP counties.

The third assumption that our identification strategy relies on is that differences in unobserved characteristics of treated and untreated workers in REBP counties are fixed over time. Table **??** panel B shows average characteristics for treated and untreated unemployed in REBP counties before the introduction of the program. Overall, untreated workers having less experience than treated workers, they tend to have slightly lower wages, and slightly lower unemployment durations before 1988 [9]. The parallel trend assumption that we are making here is fundamentally untestable, but there are a couple of ways one can address the potential concerns for violation of the parallel trend. First, we can test for differential changes in observed characteristics of treated and untreated workers during the REBP period. Second, we control for group specific time trends.

7 Empirical evidence of search externalities

We begin by providing compelling graphical evidence of the presence of spillover effects of the REBP program on untreated workers. Figure 4 plots the evolution of the difference in unemployment duration in REBP and non-REBP regions controlling for observable characteristics of treated and untreated workers[10]. We run the following regressions for each group of workers (unemployed workers with and without 15 years of experience prior to becoming unemployed).

$$y_{it} = \sum \beta_t \mathbb{1}[T = t] + \sum d_t(\mathbb{1}[T = t] \cdot \mathbb{1}[M = 1]) + X'\gamma + \varepsilon_{it} \tag{5}$$

where $\mathbb{1}[T = t]$ is an indicator for the start of the unemployment spell being in year t and $\mathbb{1}[M = 1]$ is an indicator for residing in a region treated with REBP. The vector of controls X include education, 15 industry codes, family status, citizenship and tenure in previous job. We plot in figure 4 for each group of workers the estimated coefficients d_t which gives us the difference between treated and untreated regions. In both panels, the first red vertical line denotes the beginning of the REBP program, and the two dashed red vertical lines denote the last entry into REBP program at the end of 1993, and the end of the REBP program in 1998 when eligible unemployed exhaust their last REBP-related benefits. Panel A plots the estimated coefficient

[9]Note that because of these differences, a related assumption that we are making is that treated and untreated workers do not belong to two totally isolated labor markets. But we do not need to make any particular assumption on the level of integration of treated and untreated workers for identification. For instance, if treated and untreated workers were perfect substitutes or to the contrary perfect complement, we would still be able in both cases to identify spillover effects in our framework. It is only for the interpretation and external validity of the results that the type of integration between treated and untreated workers' labor markets matter.

[10]As explained above, we remove from non-REBP regions all counties who had more than 5% of new hires coming from REBP regions before 1988. This is to make sure that non-REBP regions are not potentially subject to geographical spillovers.

d_t for workers with more than 15 years of experience, and therefore eligible for REBP extensions.

As can be clearly seen on figure 4, the introduction of REBP induced a massive reduction in the search effort of eligible workers in treated regions, which translates into a huge increase in unemployment durations. This difference in the durations of unemployment disappears for workers entering unemployment after 1994, when REBP no longer accepted new entrants. Year 1993 can therefore be seen as the peak of the effect of REBP on aggregate search effort, since this is the moment where the stock of REBP eligible unemployed is the highest, and their search effort is the lowest.

In Panel B, which plots the difference across treated and untreated regions for non-eligibe workers (with less than 15 years of experience in the past 25 years), we see the opposite pattern taking place. After the introduction of REBP, ineligible workers in treated regions tend to experience shorter unemployment spells, and a higher exit rate out of unemployment. This effect culminates at the end of 1993, just after REBP stops accepting new entrants, and therefore when the effect of REBP on aggregate search effort is at its peak. The difference then reverts back to zero as the REBP program scales down.

Another way to document the presence of externalities is to zoom in on the discontinuity at 15 years of experience. In figure 5, we plot the relationship between experience in the 25 years prior to becoming unemployed and unemployment duration in REBP and non-REBP regions, when the extensions were not in action (panel A), and when REBP extensions were in place (panel B). As in a standard RD design, we estimate and plot the predicted values of simple polynomial models of the form:

$$E[Y|W = w] = \sum_{p=0}^{\bar{p}} \gamma_p (w - k)^p + \nu_p (w - k)^p \cdot D \tag{6}$$

where w is experience, the forcing variable, and k is the eligibility threshold for REBP extensions, and $D = \mathbb{1}[W \geq k]$ is an indicator for being above the threshold. We focus on workers with past experience between 10 and 20 years. Because of measurement error in previous experience we cannot implement a strict RD design. Instead we exclude workers with experience within a 1 year bandwidth of the discontinuity.

In panel A, we observe that for all years when the REBP program was not in place, the relationship between unemployment duration and experience was not statistically different between REBP and non-REBP regions, and exhibited no sign of discontinuity around 15 years. In panel

B, we see that in REBP regions, individuals with more than 15 years of experience have longer unemployment duration, which reflects their lower search effort in response to the increase in the potential duration of their UI benefits. In the absence of spillover effects, we should not expect anything happening on the left side of the discontinuity, but interestingly, panel B shows that for individuals with less than 15 years of experience, the relationship between unemployment duration and previous experience has shifted down significantly, compared to non-REBP regions.

In table 2, we present results summing up this graphical evidence, by estimating models of the following form:

$$\begin{aligned} Y_{irt} = \quad & \alpha + \overbrace{\beta_0 \cdot Z_{irt} \cdot R_r \cdot \tilde{T}_t}^{\text{Effect of REBP on treated}} + \overbrace{\gamma_0 \cdot (1 - Z_{irt}) \cdot R_r \cdot T_t}^{\text{Effect of REBP on non-treated}} \\ & + \eta_0 R_r + \eta_1 B_{irt} + \eta_2 B_{irt} \cdot R_r \\ & + \sum \nu_t + \sum \eta_3 B_{irt} \cdot \iota_t + X'_{it}\rho + \varepsilon_{irt} \end{aligned} \tag{7}$$

where Y_{irt} are different search outcomes of interest, R_r is an indicator for residing in REBP region, T_t is an indicator for spells starting between June 1988 and December 1997, and $\tilde{T}_t$ is an indicator for spells starting between June 1988 and August 1993. $B_{irt} = \mathbb{1}[exp > 15]$ is an indicator for individuals with more than 15 years of experience in the past 25 years at the time they become unemployed. $Z_{irt} = B_{irt} \cdot \tilde{T}_t$ is an indicator for being eligible to REBP extensions. β_0 identifies the effect of REBP on treated workers, while γ_0 identifies spillovers of REBP on non-treated workers in REBP regions. To correct for the presence of common random effects, we cluster standard errors at the region-year level[11]. In column (1), we estimate this model without any other controls. In column (2) we add a vector of controls X which includes education, 15 industry codes, family status, citizenship and tenure in previous job. In column (3) and (4) we add controls for preexisting trends by region, and by region$\times$experience. Results are very stable across all specifications. All estimates of β_0 confirm that REBP increased unemployment duration by 55 to 60 weeks for eligible unemployed compared to similar workers in non-REBP regions. All estimates of γ_0 also confirm that non-treated workers in REBP regions experienced a highly significant 7 to 12 weeks decrease in their unemployment duration compared to similar workers in non-REBP regions. Column (5) confirms that these externalities are of similar

[11] Note that we obtain similar precision when we aggregate observations at the region-year level. Large positive serial correlation might still be an issue (cf. Mullainathan et al. [2004]). To analyse the extent of the issue, we computed the correlogram of the unemployment duration residuals (and other outcomes). We estimated first, second, and third autocorrelation coefficients for the mean treatment-year residuals from a regression of the outcome on treatment and year dummies. The autocorrelation coefficients are obtained by a simple OLS regression of the residuals on the corresponding lagged residuals. Only the first lag residual are significant and positive. Second lag is negative and not significant. Also, serial correlation is usually an issue when treatment is serially correlated as well. Because we have introduction and repeal of the REBP, serial correlation should not be an issue for inference in our case.

magnitude on the duration of total non-employment. Columns (6) and (7) investigate spillover effects on the probability of experiencing unemployment spells longer than 26 weeks and 100 weeks respectively, and show that the reduction in unemployment durations for the non-treated is due to both a reduction in short and long unemployment spells.

There are two main potential confounders to our identification strategy. The first confounder is the presence of differential region-specific shocks at the time the REBP program was in place. In particular, because REBP regions were not chosen at random, one may question the validity of our parallel trend assumption. Two important points should nevertheless greatly mitigate this concern. First, even if REBP regions were chosen because of the relative importance of their steel sector, the fraction of steel sector employees never exceeds 17% of the labor force in these regions, and we restrict our sample to individuals who never were employed in the steel sector. As shown in figure 4, in our sample, the parallel trend assumption between REBP and non-REBP regions for both eligible and ineligible workers seems to hold surprisingly well before and after the REBP period. Second, and most importantly, because REBP regions were experiencing a restructuring of the steel sector, we should expect the region-specific shock to be negative during the REBP period for REBP regions, which would lead to higher unemployment durations for non-treated workers. In this sense, the bias introduced by the presence of region-specific shock is likely, if anything, to attenuate our estimates of the search externalities for the non-treated.

To further investigate the robustness of our results to the presence of region-specific shocks, we pursue two strategies. We begin by using regions with high labor market integration to REBP regions that we had previously excluded from our analysis. We focus on workers with more than 15 years of experience, and use workers in non-REBP regions with high labor market integration to REBP regions as the spillover group. Counties with high level of labor market integration are defined as counties with a fraction of new hires coming from REBP regions in total number of new hires above 10%. Results are reported in table 3. They confirm that workers in counties highly integrated to REBP regions also experienced spillovers from the REBP program, and that their unemployment duration decreased and search outcomes improved compared to other non-REBP counties. The magnitude of these externalities is nevertheless much smaller than those of table 2 for ineligible workers living in REBP regions. This result is not surprising given that the effect of REBP treatment in these labor markets is substantially smaller on aggregate search effort than in REBP regions. Because there are few counties with high labor market integration to REBP regions, the precision of the estimates is also substantially smaller than in our baseline analysis of table 2.

The second potential confounder would be the presence of important selection effects among ineligible workers in REBP counties that would affect the distribution of their unobserved characteristics correlated with search outcomes. In particular, one may be concerned that because entry into unemployment is endogenous, the unobserved characteristics of ineligible workers might change during the REBP period. To investigate this concern, we look at inflow rates into unemployment for eligible and ineligible workers in REBP counties, again compared to non-REBP counties. We run the same diff-in-diff model as previously on the log separation rate and results are reported in column (1). The REBP program has had a large positive effect on the log separation rate of eligible workers in REBP regions but has not affected the log separation rate of ineligible workers in REBP regions. In column (2) and (3), we run the same diff-in-diff model on the log wage in previous job prior to becoming unemployed, controlling for observable characteristics. We again cannot detect any effect of the REBP program on the distribution of residual wages in previous job of ineligible workers in treated regions. These findings alleviate the concern of an important change in unobserved characteristics of ineligible workers in REBP regions at the time of the REBP program.

Wages As highlighted in the theoretical section, one of the key mechanism for externalities to be positive is that wages do not react much to outside options of workers. Here, we test explicitly this aspect by looking at the effect of REBP on reemployment wages and other characteristics of jobs at reemployment. In table 5, we begin by looking at the effect of REBP on the reemployment wage of eligible and non-eligible workers, following the baseline diff-in-diff strategy of equation 7. Results suggest that reemployment wages for both eligible and non-eligible workers are almost unaffected by REBP. If anything, there is a slight decline in reemployment wages of eligible workers.

But because eligible workers experience longer unemployment durations during REBP, it might be the case that the absence of effect on reemployment wages is due to variations in the distribution of wage offers over the duration of a spell. To investigate this question, we follow the methodology of Schmieder et al. [2012]. We focus on the age eligibility discontinuity at 50 in REBP regions and estimate RD effects of the REBP extensions controlling for the effect of duration on reemployment wages by adding a rich set of dummies for the duration of the spell prior to finding the job.

$$E[Y|A=a] = \sum_{p=0}^{\bar{p}} \gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[A \geq k] + \sum_{t=0}^{T} \mathbb{1}[U=t] \tag{8}$$

where Y is real reemployment wage, A is age at the beginning of the unemployment spell, $k = 50$

is the age eligibility threshold, and U is the duration of the unemployment spell prior to finding the new job. Results are displayed in figure 6, where we have estimated this model for three periods: before REBP 1985-1987, at the beginning of REBP (1988-1990), at the peak of REBP (1991-1993). Before REBP, we can detect no sign of discontinuity at age 50 in reemployment wages. But interestingly, we can detect a small discontinuity at the beginning of REBP, and this discontinuity increases over time at is the largest in 1991-1993. This evidence suggests that when controlling for the effect of duration on reemployment wages, we can identify a positive yet small effect of the REBP program on wages, and that this effect increases over time. This suggests that wages are relatively rigid in the short run, but that in the longer run, wages adjust to variations in outside options of workers.

8 Policy implications and application to the Emergency Unemployment Compensation extensions

Relationship to micro elasticity and macro elasticity estimates of UI benefits Our empirical findings carry important policy implications. First of all, the presence of search externalities imply that the micro and the macro effect of UI benefits will differ, so that estimates of the partial equilibrium effects of UI benefits on search effort do not provide enough information to assess the welfare implications of variations in UI benefits. As explained in Landais et al. [2010], in equilibrium search and matching models of the labor market, the traditional partial equilibrium Baily-Chetty formula for the optimal level of benefits needs to be extended to take into account the difference between partial equilibrium (micro) and macro effects of UI benefits.

Importantly, our analysis offers direct insights on the relative magnitude of micro and macro effects of variations in benefits. The total effect on job finding probability of changing UI benefits for the entire population of unemployed is given by:

$$\frac{d(e \cdot f(\theta))}{dB} = \underbrace{\underbrace{\frac{\partial e}{\partial B} f(\theta)}_{\text{Micro effect}} + \overbrace{\frac{\partial e}{\partial \theta} \cdot \frac{\partial \theta}{\partial B} \cdot f(\theta) + f'(\theta) \cdot \frac{\partial \theta}{\partial B} \cdot e}^{\text{Equilibrium adjustment}}}_{\text{Macro effect}} \tag{9}$$

The difference between the micro and the macro effect of UI benefits is given by the equilibrium adjustment effect in labor market tightness, which is directly equivalent to the externality (as shown in equation 4) in the case where the treated group is close to the entire population. In the REBP setting, where more than 80% of unemployed over 50 were treated, the equilibrium adjustment effect can be approximated by the externality effect γ_0 on the 20% of untreated unemployed, while the macro effect is given by the diff-in-diff estimate on the treated β_0. This gives us a ratio of micro to macro effect $\frac{e^m}{e^M} = \frac{\beta_0 - \gamma_0}{\beta_0} \approx 1.4$ if we take the baseline estimates of column (5) in table 2.

This relatively large ratio of micro to macro effects of UI benefits has interesting implications for understanding the small magnitude of the estimates of the effect of the EUC extensions in the US during the Great Recession. All studies (Rothstein [2011], Valletta and Farber [2011] and Marinescu [2013]) have found small effects of EUC extensions on unemployment, with elasticities around .1 to .15. Because these studies use variations in the timing and magnitude of extensions across US states, they essentially identify a macro elasticity. Therefore, these estimates do not mean that EUC extensions do not have larger effects on individual search effort, but that search externalities might be large, driving an important wedge between the micro and macro effect of EUC extensions. In particular, in the case of EUC, it is very likely that the ratio $\frac{e^m}{e^M}$ is even larger than in the REBP case. The reason is that the fraction of the population treated by the EUC extensions is much larger than in the REBP case, where only unemployed aged 50 and over were eligible. A larger fraction of treated workers means a larger shift in labor supply, driving larger equilibrium adjustment in labor market tightness. Moreover, because a larger population is treated, it is likely that the availability of close substitutes to the treated unemployed is smaller than in the REBP case: this means large diminishing returns to labor, and a steeper demand curve in the $\{n, \theta\}$ diagram, and therefore larger search externalities. With a ratio $\frac{e^m}{e^M} \approx 2$ for instance, the implied micro effect of the EUC extensions would be around .3, in line with the most recent estimates of the micro effects of unemployment extensions on search effort in the US using the regression kink design (Landais [2013])

Short run vs long run effects As explained in section 3, externalities are likely to be larger in the short run. There are two reasons for this: first, in the short run, returns to labor are likely to be strongly decreasing, and second, because of multiple frictions, it might take time for wages to adjust to a change in UI benefits. Our empirical evidence nevertheless suggests that even after three to four years, REBP externalities are still detectable. Because the REBP program was only temporary, we cannot properly estimate the speed at which externalities decrease over time. In the long run, however, it is likely that these externalities would have decreased. First, because, as we have shown in the previous section, it seems that wages started to react more importantly to REBP extensions over time. The effect of REBP on wages seems however to have been quite limited even in the long run, which suggests that wages are somewhat rigid with respect to outside options of workers, even in the long run, But second and most importantly, in the long run, labor demand is likely to flatten a lot, as substitution away from the treated segment of the labor market increases. These substitution effects can take the form of increased hirings of new entrants not eligible for large benefits (increased immigration, new entrants in the labor market, etc), but also investment in capital, changes in production technology, etc. Eventually, it is even possible that externalities change sign, so that the macro effect becomes larger than the micro effect. This may explain why cross-sectional estimates comparing countries or US states tend to find much larger elasticities than reform-based (short term) estimates. This may also explain why, eventually, European countries with very generous UI coverage experience high level of structural long term unemployment despite the fact that most reform-based estimates in Europe find relatively modest elasticities in the short run.

In terms of policy implications, this means that temporary extensions enacted in reaction to business cycles downturns are a lot less socially costly than previously thought, but that governments should avoid making these extensions permanent as most European countries have done in the 70s and 80s. When determining the optimal time span of temporary extensions, governments should pay attention to the pace of the decrease in externalities over time. In the absence of direct measures of these externalities, two important indicators should be used: the cross-sectional correlation between UI benefits and wages of new hires, and the time series evolution of the fraction of eligible to non-eligible in the number of new hires.

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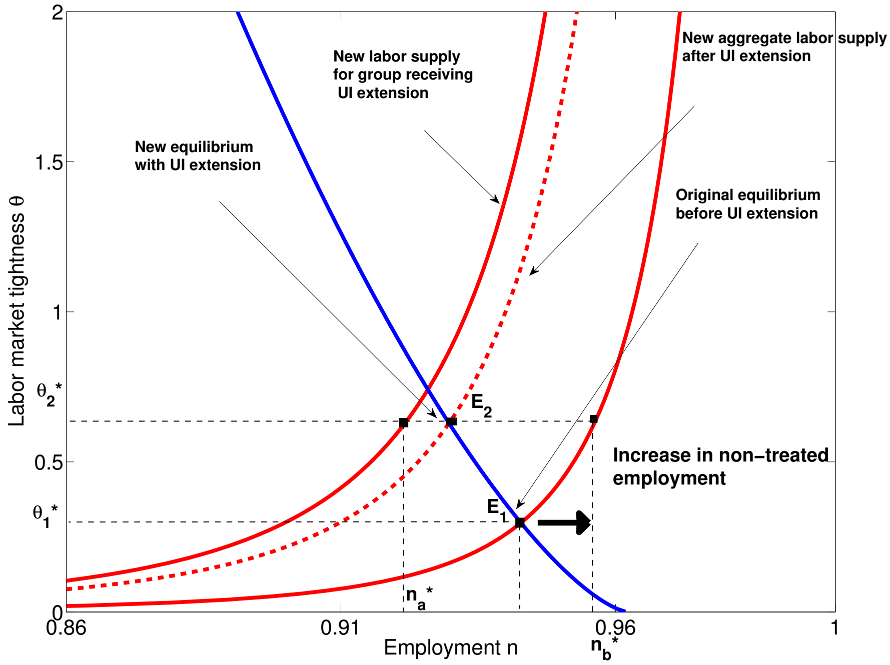
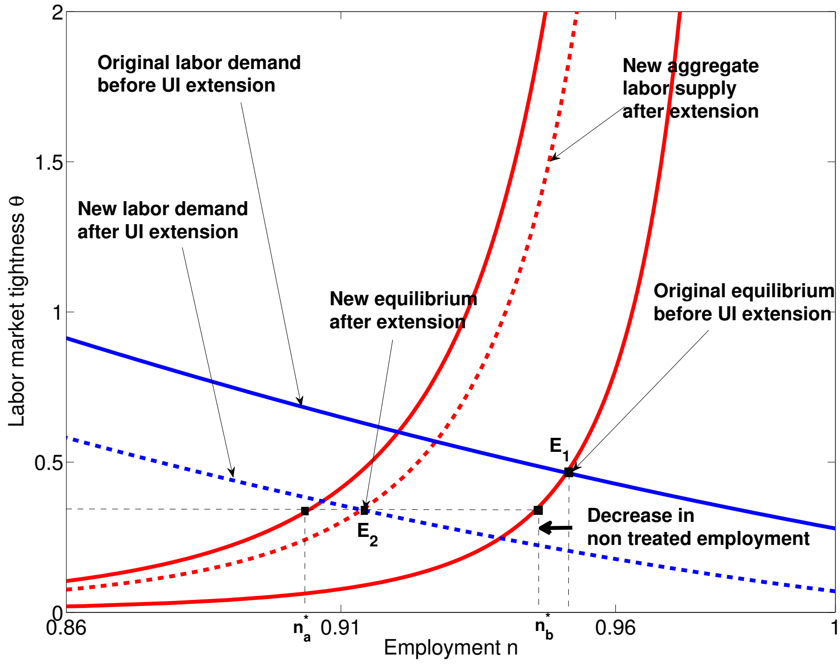
Figure 1: Externalities of UI extensions in an equilibrium search-and-matching model:

A. Rigid wages & diminishing returns



B. Flexible wages & close to linear technology



Notes: Both panels describe the effect on labor market equilibrium of a change in benefits for a subsample of the workforce, when firms cannot discriminate vacancies between groups. In both panel, we start from equilibrium E_1, where all workers get the same UI benefits. A group of workers then receives a higher level of benefits, which shifts their labor supply to the left. The new aggregate labor supply is a weighted average of labor supply of both groups, depicted by the dashed red line. In case of rigid wages (panel A), labor demand is not affected, and, if returns to labor are decreasing, the new equilibrium E_2 is characterised by higher labor market tightness θ_2^* and positive search externalities on untreated workers. When wages adjust to the change in benefits (panel B), firms reduce their vacancy openings, and if returns to labor are almost constant, it can lead to a decline in θ and negative externalities on untreated workers.

Figure 2: Regional distribution of REBP

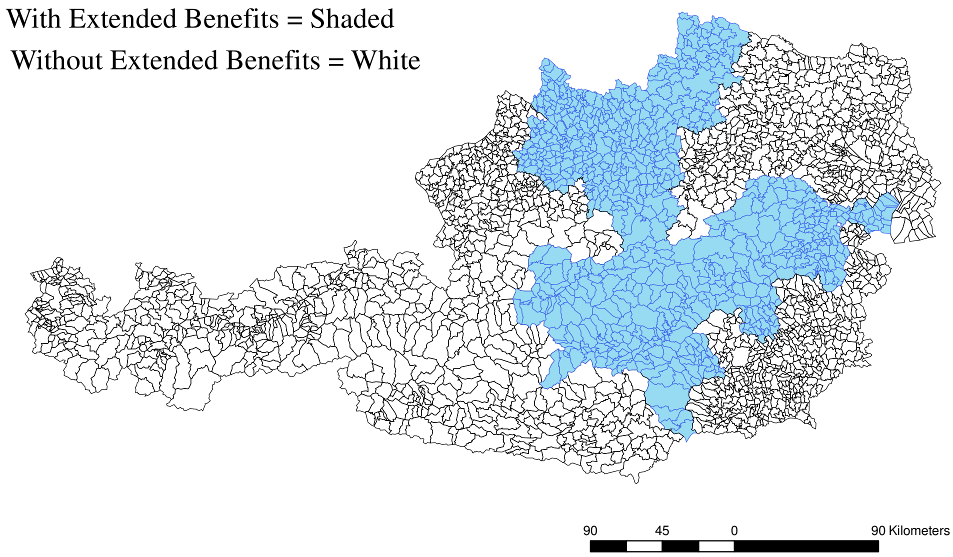
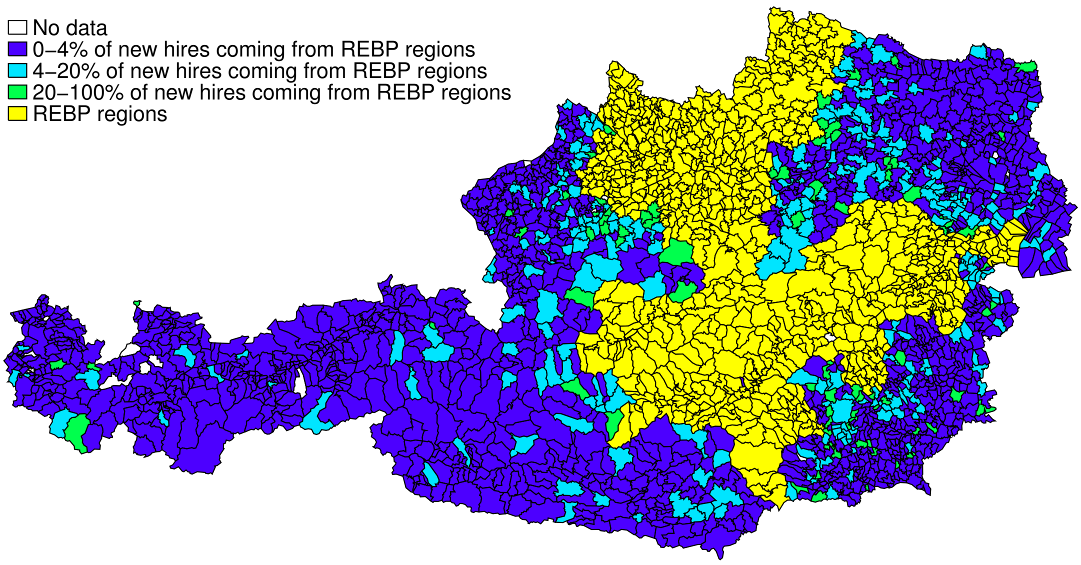
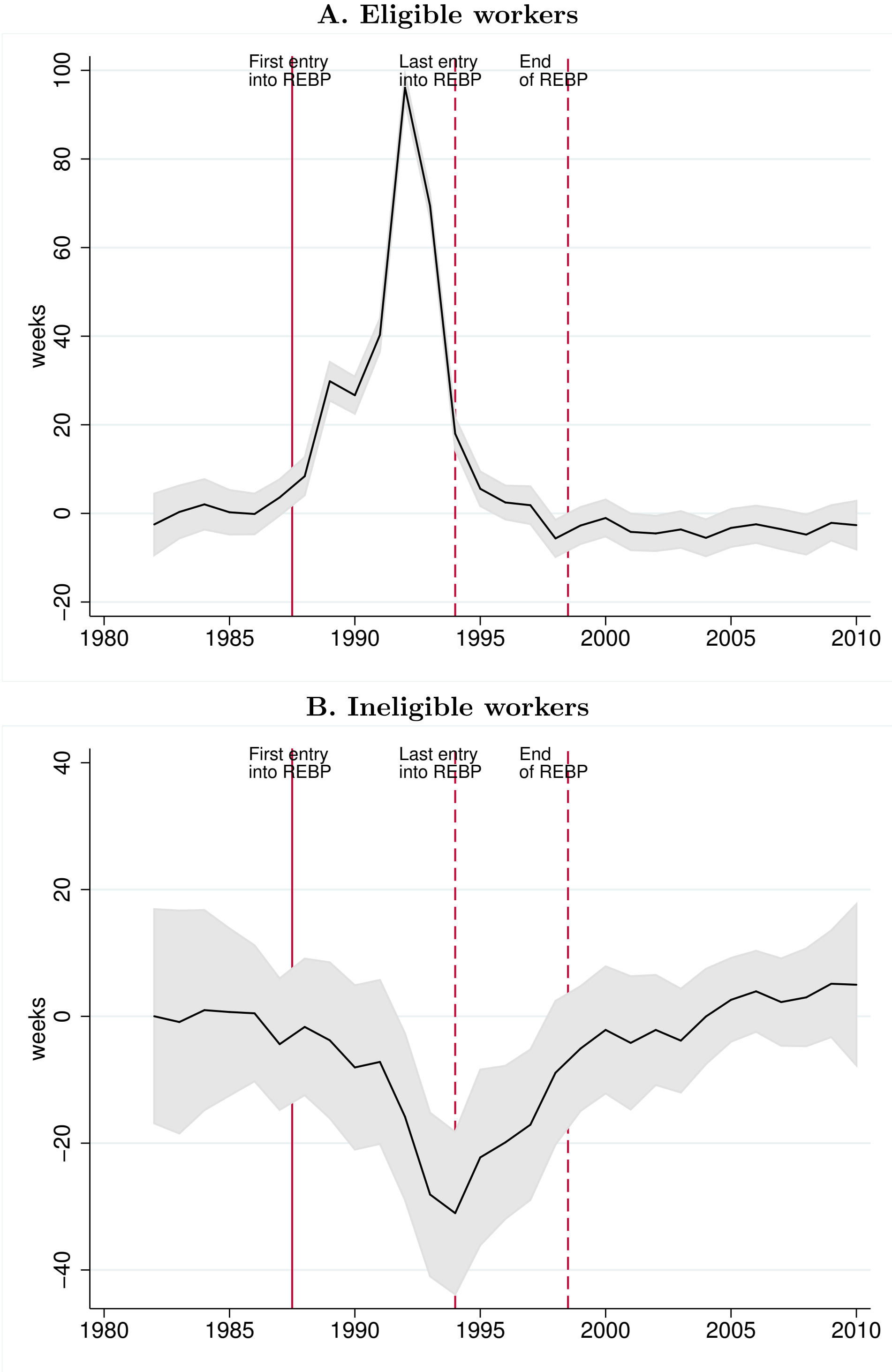


Figure 3: Local labor markets integration: fraction of new hires from REBP regions in total number of new hires by county



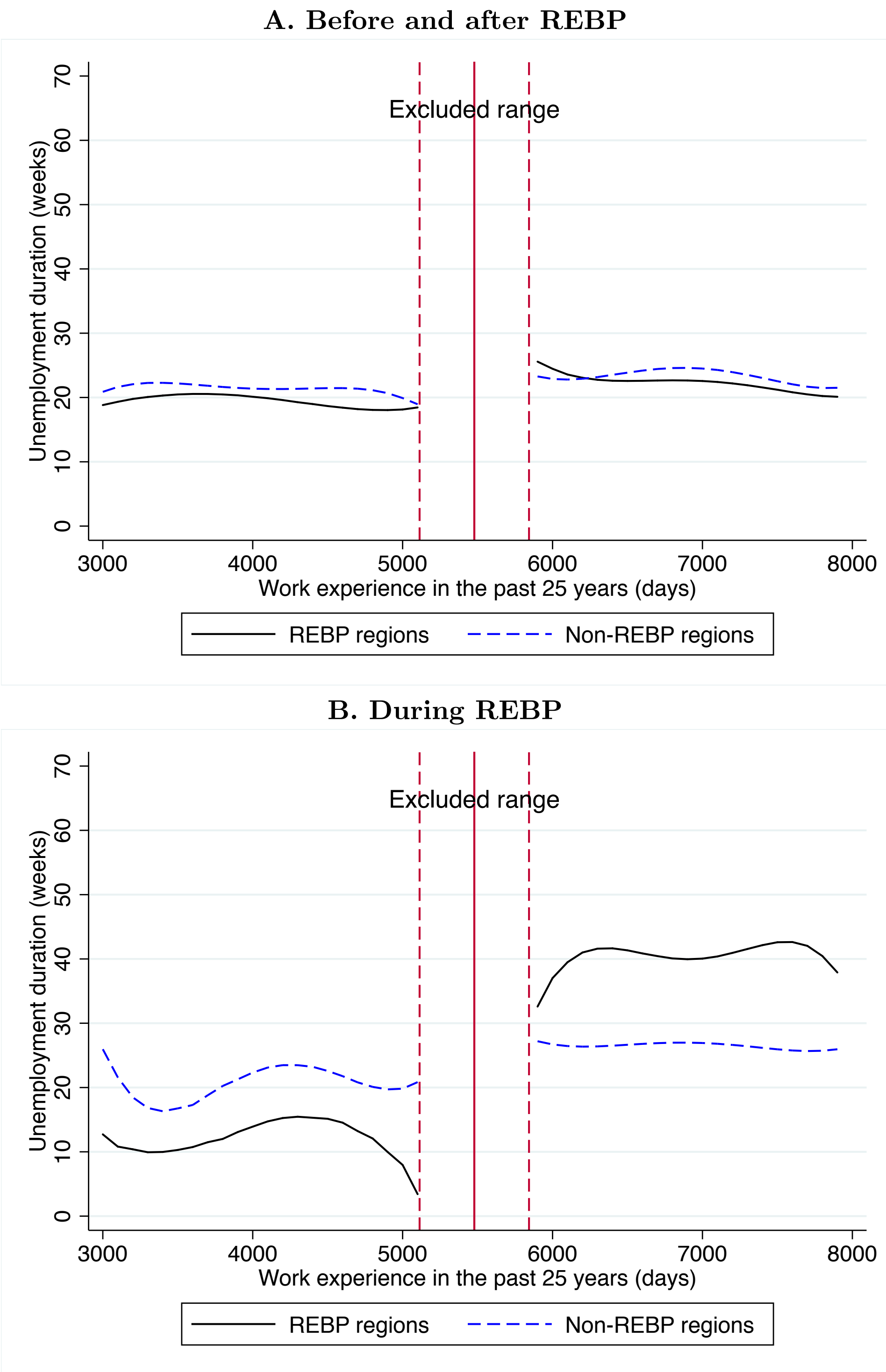
Notes: The figure maps the average quarterly fraction of men aged 50 to 54 coming from treated counties in the total number of new hires of men aged 50 to 54 in non-treated regions **for all years when the REBP was not in place**. This measures the degree of labor market integration between counties on both sides of the REBP treatment border. The map shows that this degree of integration is small, except for a few counties close to the border. To make sure our control and treatment regions are isolated labor markets we remove from our estimation sample the few counties with more than 4% of new hires coming from REBP regions.

Figure 4: Difference in unemployment durations between REBP and non-REBP regions by year of entry into unemployment, for eligible and ineligible unemployed:



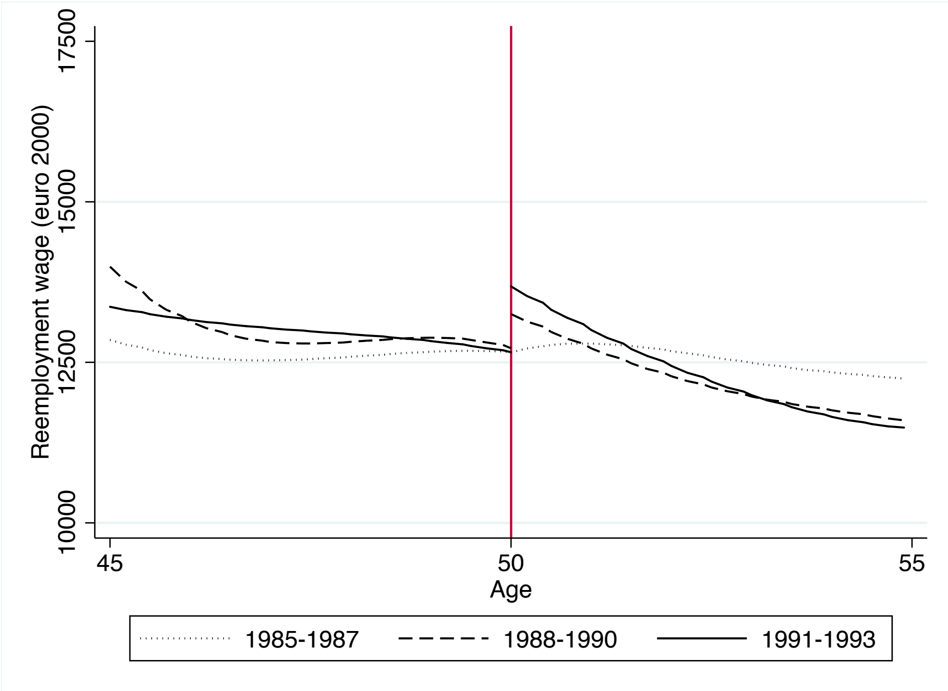
Notes: The figure plots d_t, the yearly difference in unemployment duration between REBP and non-REBP regions, obtained from regression specification 5, where controls include education, 15 industry codes, family status, citizenship and tenure in previous job. Panel A plots the difference for workers with more than 15 years of work experience in the past 25 years prior to becoming unemployed, who are therefore eligible for REBP. Panel B plots the difference for ineligible workers (less than 15 years of experience). Non-REBP counties with high labor market integration to REBP regions are excluded from the sample. See text for details.

Figure 5: Relationship between previous work experience and unemployment duration in REBP and non-REBP regions:



Notes: the figure plots the relationship between experience in the 25 years prior to becoming unemployed and unemployment duration in REBP and non-REBP regions, when the extensions were not in action (panel A), and when REBP extensions were in place (panel B). We estimate and plot the predicted values of a simple polynomial model of the form: $E[Y|W = w] = \sum_{p=0}^{5} \gamma_p (w-k)^p + \nu_p (w-k)^p \cdot D$ where w is experience, the forcing variable, and k is the eligibility threshold for REBP extensions, and $D = \mathbb{1}[W \geq k]$ is an indicator for being above the threshold. Because of measurement error in previous experience we cannot implement a strict RD design. Instead we exclude workers with experience within a 1 year bandwidth of the discontinuity.

Figure 6: RD evidence on wage bargaining over time: relationship between age and reemployment wages in REBP regions



Notes: the figure displays for REBP regions the relationship between age at the beginning of unemployment spell and reemployment wages for workers with more than 15 years of experience in the past 25 years prior to becoming unemployed. Workers aged 50 or more are eligible for REBP extensions while workers aged less than 50 are not eligible. We follow the methodology of Schmieder et al. [2012] and estimate RD effects of the extensions controlling for duration by adding a rich set of dummies for the duration of the spell prior to finding the job. $E[Y|A=a] = \sum_{p=0}^{\bar{p}} \gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[A \geq k] + \sum_{t=0}^{T} \mathbb{1}[U=t]$, where Y is real reemployment wage, A is age at the beginning of the unemployment spell, $k = 50$ is the age eligibility threshold, and U is the duration of the unemployment spell prior to finding the new job. The graph plots the predicted values of this regression for 3 periods: before REBP 1985-1987, at the beginning of REBP (1988-1990), at the peak of REBP (1991-1993).

Table 1: Summary statistics:

	(1)	(2)	(3)	(4)
	A. All unemployed treated vs untreated counties before 1988			
	M=0	M=1	Difference	p-value
Age	51.9	51.9	0	.366
U duration	18.7	19.4	-.7	.12
Non employment duration	31.7	29.9	1.8	.018
Fraction spells > 100 wks	.033	.039	-.006	.023
Fraction spells >26 wks	.135	.122	.013	.016
Real wage before spell	52.1	50.5	1.6	0
Real wage after spell	51.8	50.8	1.1	0
White Collar	.063	.035	.028	0
Fraction not in construction	.38	.369	.011	.148
	B. Treated vs untreated unemployed in treated counties before 1988			
	T=0	T=1	Difference	p-value
Age	51.8	51.9	-.1	.181
Experience	4089.365	8292.634	-4203.269	0
U duration	16.3	19.6	-3.3	.025
Non employment duration	52.5	28	24.5	0
Fraction spells > 100 wks	.018	.041	-.023	.022
Fraction spells > 26 wks	.091	.124	-.033	.056
Real wage before spell	47.3	50.8	-3.6	0
Real wag after spell	47.4	51	-3.6	0
White Collar	.01	.037	-.027	.006
Fraction not in construction	.345	.371	-.026	.307

Notes: The table displays summary statistics from the Austrian social security and unemployment insurance files for male unemployed aged 50 to 54 before the introduction of the REBP program in 1988. Panel A compares all unemployed in treated and untreated regions. P-value is for a test of equality of means for treated and untreated counties. Panel B compares in treated counties before 1988 unemployed workers with more than 15 years of continuous work history in the past 25 years to unemployed workers with less than 15 years of continuous work history in the past 25 years. P-value is for a test of equality of means for these two groups

Table 2: BASELINE ESTIMATES OF THE TREATMENT EFFECT OF REBP ON TREATED UNEMPLOYED AND NON-TREATED UNEMPLOYED

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Unemployment duration				Non-empl. duration	Spell >100 wks	Spell >26 wks
β_0 (treatment effect on treated)	62.41***	54.57***	55.48***	58.14***	26.03***	0.233***	0.236***
	(9.565)	(8.345)	(9.051)	(9.159)	(5.797)	(0.0312)	(0.0290)
γ_0 (externality on non-treated)	-6.941***	-7.165***	-11.86***	-8.979***	-9.725***	-0.0186***	-0.0297**
	(1.690)	(2.017)	(1.640)	(1.433)	(1.487)	(0.00509)	(0.0116)
Educ., marital status, industry, citizenship		×	×	×	×	×	×
Preexisting trends							
by region			×				
by region×exp				×	×	×	×
N	127802	126091	126091	126091	106164	126091	126091

Notes: S.e. clustered at the year×region level in parentheses. * p<0.10, ** p<0.05, *** p<0.010.
The table presents estimates of models of $Y_{irt} = \alpha+\beta_0 \cdot Z_{irt} \cdot R_r \cdot \tilde{T}_t+\gamma_0 \cdot (1-Z_{irt}) \cdot R_r \cdot T_t+\eta_0 R_r+\eta_1 B_{irt}+\eta_2 B_{irt} \cdot R_r+\sum \nu_t+\sum \eta_3 B_{irt} \cdot \iota_t+X'_{it}\rho+\varepsilon_{irt}$. β_0 identifies the effect of REBP on treated workers, while γ_0 identifies spillovers of REBP on non-treated workers in REBP regions. In column (1), we estimate this model without any other controls. In column (2) we add a vector of controls X which includes education, 15 industry codes, family status, citizenship and tenure in previous job. In column (3) and (4) we add controls for preexisting trends by region, and by region×experience. Results are very stable across all specifications. Column (5) confirms that these externalities are of similar magnitude on the duration of total non-employment. Columns (6) and (7) investigate spillover effects on the probability of experiencing unemployment spells longer than 26 weeks and 100 weeks respectively.

Table 3: Using regions close to REBP border with high labor market integration as spillover group

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment duration			Non-empl. duration	Spell >100 wks	Spell >26 wks
β_0 (treatment effect on treated)	66.20***	58.24***	65.09***	27.68***	0.254***	0.251***
	(10.13)	(8.865)	(9.869)	(6.298)	(0.0339)	(0.0316)
γ_0 (externality on non-treated)	-1.813	-1.588	-3.110	-3.446	-0.0117	-0.0602**
	(3.323)	(2.954)	(3.261)	(2.563)	(0.0118)	(0.0257)
Educ., marital status, industry, citizenship		×	×	×	×	×
Preexisting trends by region			×	×	×	×
N	160714	157578	159104	135702	159104	159104

Notes: S.e. clustered at the year×region level in parentheses. * p<0.10, ** p<0.05, *** p<0.010.
The table presents estimates of a model similar to that of table 2 but where the spillover group is now composed only of workers with more than 15 years of experience in non-REBP counties that are highly integrated to REBP regions. Counties with high level of labor market integration are defined as counties with an average quarterly fraction of new hires coming from REBP regions in total number of new hires above 10% for all years before 1988.

Table 4: Testing for selection: inflow rate into unemployment and log real wage in previous job

	(1) log separation rate	(2) log real wage in previous job	(3)
eligible workers	0.287***		
	(0.0355)		
non-eligible workers	-0.0346		
	(0.0306)		
β_0 (REBP effect on eligible)		0.144**	0.132**
		(0.0691)	(0.0614)
γ_0 (REBP effect on non-eligible)		-0.0638	-0.0479
		(0.0629)	(0.0608)
Educ., marital status, industry, citizenship			×
Preexisting trends by region			×
N	1733	114770	112242

Notes: For columns (2) and (3), standard errors are clustered at the year×region level. * p<0.10, ** p<0.05, *** p<0.010. The table investigates the presence of selection effects of the REBP program affecting the distribution of unobserved characteristics of non-eligible workers in REBP regions. Column (1) presents the diff-in-diff effect of the REBP program on the quarterly log separation rate of eligible and non-eligible workers in REBP counties compared to non-REBP counties. In this column, observations are at the county×quarter level. Column (2) and (3) present specifications similar to that of table 2 but where the outcome variable is the log wage in the previous job prior to becoming unemployed.

Table 5: Effects of REBP on subsequent wages and match quality

	(1)	(2)	(3)	(4)	(5)	(6)
	log real wage in next job		wage drop from next to previous job		distance to next job (min)	
β_0 (REBP effect on eligible)	-0.0236	-0.0381**	-0.157	-0.0904	-0.456	0.223
	(0.0154)	(0.0152)	(0.214)	(0.208)	(0.554)	(0.549)
γ_0 (REBP effect on non-eligible)	0.00515	-0.0477	0.269	0.462	-0.233	2.476*
	(0.0448)	(0.0441)	(0.591)	(0.562)	(1.138)	(1.240)
Educ., marital status, industry, citizenship		×		×		×
Preexisting trends by region		×		×		×
N	90345	88634	94503	92719	103678	101715

Notes: Standard errors are clustered at the year×region level. * p<0.10, ** p<0.05, *** p<0.010. The table investigates the effect of REBP on reemployment wages and other characteristics of the job at reemployment and presents estimates of a model similar to that of specification 7. In column (1) and (2), we look at the effect of REBP on the log real wage at reemployment. In column (3) and (4) we look at the wage drop between previous and reemployment job defined as $\log w_n - \log w_{n-1}$. In column (5) and (6), we look at the distance in minutes between residence and the reemployment job.