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Rafael Lalive
Camille Landais
Josef Zweimüller

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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 non-eligible 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 spillover effects: 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 assumptions on technology and the wage setting process in equilibrium search and matching models and discuss the policy implications of our results for the EUC extensions in the US.

JEL-Code: J650, J210, J220.

Keywords: unemployment insurance, benefit extension, market externality, macro effects.

Rafael Lalive
Department of Economics
University of Lausanne
Switzerland – 1015 Lausanne-Dorigny
Rafael.Lalive@unil.ch

Camille Landais
Department of Economics
London School of Economics
Houghton Street / London, WC2A 2AE / UK
c.landais@lse.ac.uk

Josef Zweimüller
Department of Economics
University of Zurich
Mühlebachstrasse 86
Switzerland – 8008 Zurich
josef.zweimueller@econ.uzh.ch

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1 Introduction

Partial-equilibrium effects of variations in the generosity of unemployment insurance (UI) on labor market outcomes are well-understood. Theory unambiguously predicts that higher benefits lead to longer unemployment duration[1], and empirically, a large number of well-identified estimates of these effects have been produced.[2] But much less is known about the equilibrium (macro) responses. The literature on unemployment insurance has always recognized the potential importance of 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 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.

Partial equilibrium effects and macro effects of a labor market policy (treatment) will differ whenever the treatment induces equilibrium adjustments in the labor market. The presence of these equilibrium adjustments can be identified by the existence of spillover effects of treatment on the untreated in the same labor market. The treatment evaluation literature has long advocated that identifying spillover effects of labor market programs is critical because, if such spillovers 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

[1]Mortensen [1977] discusses the effects of unemployment benefits on job search decisions, and van den Berg [1990] provides a general discussion of job search in non-stationary environments. Whether this effect is driven by distortionary moral hazard effects or non-distortionary wealth/income effects is still an open question. See Chetty [2008]

[2]Early studies, including Moffitt and Nicholson [1982], Moffitt [1985], and Grossman [1989] find significantly negative incentive effects. European studies also finds strong effects. Hunt [1995] finds substantial disincentive effects of extended benefit entitlement periods for Germany. Carling et al. [1996] find a big increase in the outflow from unemployment to labour market programs whereas the increase in the exit rate to employment is substantially smaller. Winter-Ebmer [1998] uses Austrian data and finds significant benefit duration effects for males but not for females. Roed and Zhang [2003] find for Norwegian unemployed that the exit rate out of unemployment increases sharply in the months just prior to benefit exhaustion where the effect is larger for females than for males. van Ours and Vodopivec [2006] studying PBD reductions in Slovenia find both strong effects on the exit rate out of unemployment and substantial spikes around benefit exhaustion. Schmieder et al. [2012b] discuss the effects of extended PBD for benefit duration and non-employment duration over 20 years for Germany. A common objection against these studies is policy endogeneity. Benefits are typically extended in anticipation of a worse labour market for the eligible workers. Card and Levine [2000] exploit variation in benefit duration that occurred independently of labour market condition and show that policy bias is substantial. Lalive and Zweimüller [2004a,b] show similar evidence for the Austrian labour market.

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 spillovers. 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. More recently, Hagedorn et al. [2013] try to estimate macro effects of UI directly using EUC extensions in the US during the Great Recession.

This paper aims to shed light on the equilibrium (macro) effects of UI benefits by investigating market externalities of large UI extensions. We define market externalities as spillover effects of UI extensions on non-eligible individuals (who do not experience a change in their UI benefits) and that are arising from the simple fact of being in the same labor market as eligible individuals[3]. We call these particular spillover effects "externalities" because, as explained in Landais et al. [2010], the equilibrium adjustments that they identify have first-order welfare effects. The questions that we want to address are twofold. First of all, do large unemployment insurance extension programs create market externalities and if yes, can we empirically identify their existence and potential size? And second, what can the very nature of these externalities, tell us about the functioning of the labor market and about optimal UI policies?

Our paper contributes to the first set of questions by offering compelling quasi-experimental evidence of the existence of market externalities of UI extensions using a unique program (REBP) in Austria that extended unemployment benefits drastically for a large subset of workers in selected regions of Austria. We use unemployed workers in REBP regions who are very similar to eligible workers but who are non-eligible because of past work history requirements in the REBP program, and a difference-in-difference identification strategy to control for preexisting differences across REBP and non-REBP regions. Our quasi-experimental setting has a number of advantages.

First, treatment is massive: treated workers received an extra three years of covered unemployment with unchanged benefit level. This translated into a huge effect on the effort of

[3]Note that UI extensions may also induce other type of spillover effects: they may affect the labor supply of spouses of eligible individuals, in case of joint labor supply decisions within the household, or the labor supply of other relatives in case of social interaction effects. These spillover effects, which are orthogonal to market externalities, are beyond the scope of this paper.

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 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 work history eligibility requirements of the REBP program, we consider workers just below the work history 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 labor market. As a consequence, they are very likely to be affected by the drastic drop in search effort of eligible workers. Moreover, we can compare them to similar workers in non-REBP regions to uncover the presence of externalities.

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 eligibility status 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 externalities. In REBP regions, as the search effort of treated workers plummets, the job finding probability of non-eligible 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. On average, the REBP program decreased by 10 weeks the duration of non-employment spells of non-eligible workers in REBP regions relative to similar workers in non-REBP regions. Besides, we show compelling evidence that the magnitude of the externalities on non-eligible workers increases with the intensity of the REBP treatment across local labor markets. We also identify the presence of geographical spillovers of the REBP program on non-REBP regions that have labor markets that are highly integrated to REBP regions.

In our robustness analysis, we address the two main potential confounders for our results. First, we provide evidence that our results are unlikely to be driven by region-specific shocks contemporaneous with the REBP program. Second, we show that our results are unlikely to be confounded by selection, *i.e.* a change in unobserved characteristics of non-eligible workers contemporaneous with the REBP program.

We use our empirical 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 best rationalized by a model *a la* Michaillat [2012] 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 up 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], Valletta and Farber [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.

The remainder of the paper is organised as follows. Section 2 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 3 presents the institutional background of the REBP program and section 4 presents the data. In section 5, we explain our identification strategy and in section 6 we present our results. Section 7 draws policy implications, with an application to the EUC extensions.

2 Theoretical framework

We present a simplified, static version of an equilibrium search and matching model and characterize the comparative static for steady state equilibria. The representation of the labor market that we use was developed by Michaillat [2012]. It is also strongly related to Landais et al. [2010], where search effort is endogeneized and unemployment insurance is introduced in the model of Michaillat [2012]. Here we extend the model to a two-group equilibrium in order to relate more closely the theory to the policy experiment that we analyze empirically. This presentation, in turn, will be critical to relate our empirical findings to the issue of optimal UI in section 7.

The labor market is characterized by the presence of matching frictions. We normalize the size of labor force to unity and assume there are p workers of group a who are eligible to unemployment benefits B_a and $1-p$ workers workers of group b who are eligible to unemployment benefit B_b. The group shares p and $1-p$ are exogenously given. There are $u = u_a + u_b$ unemployed workers. When unemployed, 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 realized 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 $\overline{e \cdot u} = e_a \cdot u_a + e_b \cdot u_b$ We assume that employers cannot discriminate between unemployed from group a and b and cannot therefore post differentiated vacancies for each group. The validity of the assumption depends on the ability of firms to discriminate job vacancy postings based on characteristics that are correlated with unemployment benefits received by the unemployed. This assumption seems realistic in the present application because groups a and b are defined based on the county of residence, the county of the previous employer, 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. More generally, it is often complicated for firms to condition their openings on the 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. Note however that in some cases discrimination is more likely to happen, especially when the characteristics that determine UI benefits are unique and strongly salient, such as age for instance. We discuss below the consequences of discrimination for the existence and magnitude of search externalities.

In the absence of discrimination in vacancy posting, 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 $e_i \cdot f(\theta) = e_i \cdot m(1, \theta)$, where $e_i = e(B_i, \theta)$ is the optimal search effort of individuals of group i given benefits and labor market tightness. 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 $\frac{\partial q(\theta)}{\partial \theta} < 0$.

We denote by n_i^s the probability that a worker of type i is employed. Because at the steady state the employment and unemployment of both groups are stable, we have from the equality of flows in and out of unemployment that

$$n_i^s = \frac{e_i f(\theta)}{\psi + e_i f(\theta)}$$

where ψ is the exogenous separation rate. Following Michaillat [2012], we interpret $n_i^s = n^s(\theta, e(B_i, \theta))$ as a labor supply that we can represent as an increasing function of θ in a $\{n, \theta\}$ diagram. To further simplify the presentation, we assume that $\frac{\partial e_i}{\partial \theta} = 0$ so that $n_i^s = n_i^s(\theta, e(B_i))$. The assumption that the elasticity of job search effort with respect to the job-finding rate is close to zero seems reasonable empirically. As emphasized by Shimer [2004] labor market participation and other measures of search intensity are, if anything, slightly countercyclical even after controlling for changing characteristics of unemployed workers over the business cycle.

The aggregate employment level n^s at the steady state is a weighted sum of employment supplied by group a, n_a^s and employment supplied by group b, n_b^s

$$n^s = p[\underbrace{\frac{e(B_a)f(\theta)}{\psi + e(B_a)f(\theta)}}_{n_a^s}] + (1-p)[\underbrace{\frac{e(B_b)f(\theta)}{\psi + e(B_b)f(\theta)}}_{n_b^s}] \tag{1}$$

A representative firm maximizes profit $\pi = \phi(n) - n(s \cdot w_a + (1-s) \cdot w_b) - \frac{r}{q(\theta)} \cdot \psi \cdot n$ where $\phi(.)$ is total output, r is the recruiting cost of opening a vacancy, $s = \frac{p \cdot n_a}{p \cdot n_a + (1-p) \cdot n_b}$ (resp. $1-s$) is the share of employed workers coming from group a (resp. b). We assume that workers from both groups are perfect substitute but that employers cannot discriminate openings. 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 $v \cdot q(\theta)$ job creations.

The first-order condition of the firm with respect to employment level n is:

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

Equation (2) implicitly defines a labor demand function $n^d(\theta, w_a, w_b)$ whose properties depend on the assumptions made on $\phi(.)$ 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.

Note that we would get similar results if we allowed for discrimination in vacancy posting but had complementarities in the production function. In this case, there would be two labor market tightness for each group of workers (θ_a, θ_b) and firms would be maximizing profits $\pi = \phi(n_a, n_b) - pn_a \cdot w_a - (1-p)n_b \cdot w_b - \psi \cdot (\frac{r}{q(\theta_a)} \cdot pn_a - \frac{r}{q(\theta_b)} \cdot (1-p)n_b)$. The first order condition with respect to n_a and n_b would be: $\frac{\partial \phi(n_a,n_b)}{\partial n_a} = pw_a + \frac{r\psi p}{q(\theta_a)}$ and $\frac{\partial \phi(n_a,n_b)}{\partial n_b} = (1-p)w_b + \frac{r\psi(1-p)}{q(\theta_b)}$ And if there are complementarities in the production function, such that $\frac{\partial^2 \phi(n_a,n_b)}{\partial n_b \partial n_a} \neq 0$ and $\frac{\partial^2 \phi(n_a,n_b)}{\partial n_a \partial n_b} \neq 0$, then the optimal level of employment for each group depends on the employment level of the other group, and therefore on the labor market tightness for the other group. But in the absence of complementarities, the level of employment for each group is independent of the employment level of the other group, and we should see no externalities.

Going back to the no-discrimination case, 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:

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

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 $n_a^* = n_a^s(\theta^*)$ and $n_b^* = n_b^s(\theta^*)$, as shown in figure 1. 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 n_a^s and 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 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. We have $\frac{d(e_b \cdot f(\theta))}{dB_a} = \frac{\partial e_b}{\partial \theta} \cdot \frac{\partial \theta}{\partial B_a} \cdot f(\theta) + e_b \cdot f'(\theta) \cdot \frac{\partial \theta}{\partial B_a}$.

Assuming, as we did that $\frac{\partial e_b}{\partial \theta} = 0$ we can define externalities as:

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

where $\frac{\partial \theta}{\partial B_a}$ comes from implicitly differentiating equation (3). The sign and magnitude of externalities[4] depend 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. We call this effect a "labor-demand externality", following Landais et al. [2010]. 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 down. We call this effect the "wage externality". And the new equilibrium tightness is the result of a shift in both labor demand ("wage externality") and labor supply ("labor-demand externality") as shown in panel B.

Two major forces determine the respective magnitude of the "labor-demand externality" and of the "wage externality" and in turn, the sign and magnitude of the total externality on non-treated workers. 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 exist 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 are no close substitutes to workers a and b in the short run) then labor demand is a 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 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

[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, as explained in Landais et al. [2010].

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 low bargaining power), then labor demand could decrease in response to an 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.

Finally, equation (3) shows that equilibrium adjustments in θ ($\frac{\partial\theta}{\partial B_a}$) depend on the fraction of treated workers p. Therefore treatment intensity, defined as the fraction of individuals eligible to longer benefits in the labor force, also determines the magnitude of the externalities. Interestingly, in a model with rigid wages and diminishing returns, this implies that as treatment intensity increases, the positive externality on untreated unemployed should increase in magnitude. This can be understood intuitively from figure 1 panel B: the larger the fraction p of unemployed receiving the unemployment benefit extension relative to those not receiving the extensions, the larger the shift in labor supply, which is a weighted average of labor supply of both groups of workers. In the absence of a shift in labor demand, a larger p implies a larger increase in θ, and therefore larger externalities for non-eligible unemployed.

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, and the potential magnitude of the "wage externality".

3 Austrian Unemployment Insurance and the REBP

We explain here briefly the functioning of the UI system in Austria and the most important features of the REBP program needed to understand our empirical analysis. More detailed information are available in Lalive [2008].

The Unemployment Insurance System Workers who become unemployed can draw regular unemployment benefits (UB), the amount of which depends on previous earnings. Interestingly, compared to other European countries, the replacement ratio (UB relative to *gross* monthly earnings) is rather low, and similar to that in the US. 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.

The maximum number of weeks that one can receive UB (potential duration) depends on work history (number of weeks worked prior to becoming unemployed) and age. For the age group 50 and older, UB-duration is 52 weeks and 39 weeks for the age group 40-49. Voluntary quitters and workers discharged for misconduct can receive UB but are subject to a waiting period of 4 weeks. UB recipients are expected to search actively 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 date when they registered for their first spell.

After UB payments have been exhausted, job seekers can apply for 'transfer payments for those in need' ("Notstandshilfe").[5] 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).[6]

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, 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 unem-

[5] Job seekers who do not meet UB eligibility criteria can apply for *Notstandshilfe* at the beginning of their spell

[6] 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, in the middle of our sample period, 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).

ployed 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 for 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 8 shows the distribution of REBP across the 2361 "communities" (counties) in Austria[7]. Interestingly, the treated regions (counties with red shading in panel A) 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 states Burgenland, Carinthia (Kärnten), Lower Austria (Niederösterreich), Upper Austria (Oberösterreich), and Styria (Steiermark).

The REBP was in effect until December 1991 when a reform of these rules took place which came into effect in January 1992. This 1991-reform left all claims in progress unaffected but enacted two changes for new spells. 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 1991-reform. Second, the 1991-reform tightened eligibility criteria to receive extended benefits: new beneficiaries had to be not only residents, but also *previously employed* in a treated region.

The program was abolished in August 1993[8] so that REBP accepted entrants until 31 July 1993. But job seekers who established eligibility to REBP continued to be covered, and maintained their eligibility to the extended benefits even if they left unemployment for a job (until 4 years after getting eligible for the first time.)

Apart from the REBP, the second 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 these 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.

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 also two early retirement pathways available at age 60 for men and at age 55 for women. The existence of these early retirement programs creates

[7]In the remainder of the paper, we refer to these communities when we use the term county. "Regions" refer to the political and territorial division just above counties. There are on average 20 counties per region.

[8]Law number 503/1993

potential complementarities with the REBP program that are susceptible to affect search effort and labor supply in non-trivial ways (Inderbitzin et al. [2013]). In order to minimize these complementarity effects and concentrate on the effects of the REBP program alone, we focus primarily our analysis on individuals who cannot use REBP or unemployment benefits as a pathway to other programs (such as early retirement), as explained in the next section.

4 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 (Austrian Social Security Database - ASSD)[9] 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 continuous work history 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.

Because of early retirement programs in Austria during our period of analysis, women can go directly from REBP or from regular unemployment benefits 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 an extensive margin decision to exit the labor market. The same issue applies for men above 55, who can use REBP or unemployment programs as a direct channel towards retirement. Search responses to UI along the intensive margins and exits from the labor markets have potentially very different implications for equilibrium analysis. Because our focus is on search externalities arising from responses to UI along the intensive margin, we mainly focus on unemployed men aged 50 to 54 because they cannot go directly from unemployment to early retirement. There is therefore no potential complementarity between the effects of REBP and the effects of early retirement programs on these individuals, and their search response to UI is essentially along the intensive margin. This enables us to isolate the search externalities of the REBP program. In our robustness analysis, we nevertheless show that our results are robust to these sample restrictions, and that similar externalities can be detected on women, and on all men aged 50 to 59.

Table 1 gives descriptive statistics for REBP and non-REBP counties before the introduction of the reform (panel A), and for eligible and non-eligible unemployed in REBP counties regions before the introduction of the reform (panel B). In panel A, we begin by showing simple labor

[9]For more information about the ASSD, see Zweimüller et al. [2009]. The standard ASSD traditionally available covers employment spells from 1972 onwards, but we used a newly available version covering employment spells from 1949 on.

market indicators for REBP and non-REBP counties. Regions participating in the REBP program are not chosen at random, but because of the importance of their steel sector. The average quarterly fraction of employment in the steel sector in REBP counties was 13% before 1988 versus 7% in non-REBP counties. The monthly unemployment rate was nevertheless exactly the same on average (8%) before 1988 in REBP and non-REBP counties. Still, to control for the potential endogeneity bias in the choice of REBP counties, 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. Because the share of the steel sector in total employment is never larger than 15% in REBP counties, this leaves us with a very large sample. We also explore in our robustness analysis a series of sensitivity checks destined to further address concerns about endogeneity. If anything, as we explain below, endogeneity is likely to bias towards zero our estimates of the spillovers of REBP on the untreated, so that we can think of the magnitude of our estimated effect as a lower bound.

In the remainder of table 1 panel A, we show descriptive statistics on our restricted estimation sample of unemployed men, aged 50 to 54, who never work in the steel sector. First, the fraction of unemployed with more than 15 years of continuous work history in the past 25 years (and therefore potentially eligible for REBP) is very large: 92% in REBP counties before the introduction of REBP. Second, REBP and non-REBP counties were extremely similar before the introduction of REBP in terms of labor market outcomes: the duration of unemployment spells, the duration of non-employment[10] spells and the fraction of spells longer than 52 weeks were roughly the same for unemployed in REBP and non-REBP counties. Finally gross (unconditional) wages, both before and after unemployment spells, were slightly higher in REBP counties.

In table 1 panel B, we display descriptive statistics for eligible and non-eligible unemployed workers in REBP counties in our restricted estimation sample of unemployed men, aged 50 to 54, who never work in the steel sector. Eligible unemployed are defined as unemployed with more than 15 years of continuous work history in the past 25 years who reside in REBP counties and whose previous employer was also in a REBP county. Non-eligible unemployed are those who have worked less than 15 years out of the previous 25 years but are identical to the eligible otherwise. Eligible and non-eligible unemployed had the same age on average before the introduction of REBP, and had roughly similar job search outcomes. Non-eligible unemployed had a slightly lower duration of unemployment, but equivalent duration of non-employment. Non-eligible unemployed had slightly lower (unconditional) gross real wages, but had equivalent level of education, and were also similar in terms of other socio-demographic characteristics such as marital status.

[10] All duration outcomes are expressed in weeks. Non-employment is defined as the number of weeks between two employment spells. Unemployment duration is the duration of paid unemployment recorded in the UI administrative data.

5 Identification strategy

Quasi-experimental framework Our identification strategy can be related to the following experimental framework. There are two labor markets, $M = 0, 1$. Labor market $M = 1$ is randomly selected to receive some exogenous treatment. Labor market $M = 0$ does not receive treatment and acts as 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 ($T = 0$). There are three potential outcomes y_{iM}^T (where i indexes individuals): y_{i1}^1, when being treated in a treated labor market, y_{i1}^0, when being untreated in a treated labor market, and y_{i0}^0 when being in a non-treated labor market. We are interested in the average externality of the treatment on outcome y_i, $AE = E(y_{i1}^0 - y_{i0}^0)$. Following the treatment evaluation literature, we can relate observed outcomes to the average externality on the non-treated in treated labor markets, AE_T^{NT} :

$$E(y_{i1}^0|T=0, M=1) - E(y_{i0}^0|T=0, M=0) = \overbrace{E(y_{i1}^0 - y_{i0}^0|T=0, M=1)}^{AE_T^{NT}} + \underbrace{E(y_{i0}^0|T=0, M=1) - E(y_{i0}^0|T=0, M=0)}_{\text{selection}} \quad (5)$$

The average treatment effect on the treated ($ATET \equiv E(y_{i1}^1 - y_{i0}^0|T = 1, M = 1)$) can of course also be related to observed outcomes:

$$E(y_{i1}^1|T=1, M=1) - E(y_{i0}^0|T=0, M=0) = ATET + E(y_{i0}^0|T=1, M=1) - E(y_{i0}^0|T=0, M=0)$$

Under double randomization (of treated labor markets and of treated individuals within labor markets), the selection term in equation 5 is zero and AE_T^{NT} can be identified by comparing observed outcomes for the non-treated in labor market $M = 1$ to observed outcomes for workers in labor market $M = 0$.

In our case, labor markets $M = 1$ are Austrian counties that received REBP, while markets $M = 0$ are a set of control Austrian counties that did not receive REBP. 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). Despite the lack of double randomization, we can still identify AE_T^{NT} based on a standard diff-in-diff strategy, under the assumption that the unobserved differences between non-eligible unemployed workers in REBP counties and unemployed workers in non-REBP counties are fixed over time. Observations of labor markets 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.

There are two potential concerns with regard to our parallel trend assumption. The main concern is that regions that received REBP treatment were not chosen at random so that the parallel trend assumption might be violated because of region-specific shocks in REBP vs non-REBP counties. Indeed, as stated in section 3, treated regions were chosen because of their higher share of employment in the steel sector that was being restructured. This is the reason why we focus our analysis on a sample restricted to non-steel workers only. Because the steel sector only accounts for at most 15% of employment in REBP counties, the spillover effects of the restructuring can be assumed to be small on industries not directly related to the steel industry supply chain. We show compelling graphical evidence in favor of our parallel trend assumption in the next section. We also provide in our sensitivity analysis several robustness tests to control for region-specific shocks. Moreover, if, because of the restructuring of the steel sector, non-steel industries in REBP regions had experienced a negative shock, then we would expect non-eligible workers in REBP regions to do worse in terms of job search outcomes than unemployed workers in non-REBP regions during the REBP program. But, as we will show in the next section, we find, to the contrary, that non-eligible workers in REBP regions did better in terms of job search outcomes. So, if anything, region-specific shocks would bias our diff-in-diff estimates of REBP job search externalities in the opposite direction, and our estimates are likely to be a lower bound for the average externality AE.

The second concern with regard to our parallel trend assumption is that the unobserved characteristics of non-eligible workers in REBP counties may change over time. Such a change in unobserved characteristics of non-eligible workers is fundamentally untestable, but there are a couple of ways one can address this potential concern 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. We show in particular in section 6 that there was no change in the inflow into unemployment of non-eligible workers in REBP counties at the time of the REBP and that average observed characteristics of non-eligible unemployed remained unchanged. Second, we also control for group-specific time trends within REBP and non-REBP counties.

A final important requirement for the validity of our identification strategy is that treated and untreated labor markets are 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 the labor markets of REBP and non-REBP counties are, we use two indicators[11]. First, we compute the fraction of new hires in non-REBP counties who come from REBP counties. In figure 2 panel A, we map the average quarterly fraction of men aged 50 to 54 coming from REBP counties in the total number of new hires of men aged 50 to 54 in non-REBP regions for all the years when the REBP was not in place. There are only few counties where this fraction is above 5% and a handful of counties where this fraction is above 20%. Most of these counties are situated in a narrow bandwidth, at a distance of 20 to 30 minutes to the border of REBP counties. Because workers in these counties face competition from workers coming from REBP counties, they might be affected by spillover effects of the REBP program. Thus, in our baseline analysis, we remove the few counties with more than 5% of new hires coming from REBP regions from our estimation sample. But in our robustness analysis, we use these counties to show that we can also detect the presence of

[11] Manning and Petrongolo [2011] also suggest an interesting indicator, which is the distance between residence while unemployed and job when reemployed. We computed this average distance in our sample, and it is relatively small, around 25 minutes, suggesting that in Austria, labor markets are essentially local, with a relatively low level of geographical mobility.

geographical externalities in these counties highly integrated to REBP regions.

In figure 2 panel B, we map the average quarterly fraction of men aged 50 to 54 coming from non-REBP regions in the total number of new hires of men aged 50 to 54 in REBP counties for all years when the REBP was not in place. This measures the degree of competition from non-REBP workers faced by workers in REBP counties. The map shows that this competition is on average limited, except for a few counties close to the REBP border. Besides, panel B shows that there is interesting variation in the openness of REBP counties to non-REBP residents, which creates variation in treatment intensity across REBP counties. We will rely on this variation to identify the geographic spillovers of the REBP program.

6 Empirical evidence of market externalities

We begin by providing graphical evidence of the presence of externalities of the REBP program on non-eligible unemployed workers in REBP counties. Figure 3 plots the evolution of the difference in unemployment duration in REBP and non-REBP counties controlling for observable characteristics of treated and untreated workers[12]. 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 July 1993, and the end of the REBP program when eligible unemployed exhaust their last REBP-related benefits. Panel A plots the estimated difference d_t each year between REBP an non-REBP counties for workers with more than 15 years of continuous work history, and therefore eligible for REBP extensions.

As can be clearly seen on figure 3, 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 from 1994 on, 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-eligible workers (with less than 15 years of continuous work history in the past 25 years), we see the opposite pattern taking place. After the introduction of REBP, non-eligible workers in REBP 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,

[12] More specifically, we run the following regression, separately for each group of workers (unemployed workers with and without 15 years of continuous work history prior to becoming unemployed).

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

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 county 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 3 for each group of workers the estimated coefficients d_t which gives us the difference between treated and untreated regions. We remove from non-REBP counties all counties who had more than 5% of new hires coming from REBP counties before 1988. This ensures that non-REBP counties are not potentially subject to geographical spillovers.

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 continuous work history. In figure 4, we plot the relationship between continuous work history 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 simple polynomial models of the form:

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

where h is continuous work history, the forcing variable, and k is the eligibility threshold for REBP extensions, and $\mathbb{H} = \mathbb{1}[H \geq k]$ is an indicator for being above the threshold. We focus on workers with past continuous work history between 10 and 20 years. Because of measurement error in previous continuous work history we cannot implement a RD design. Instead we exclude workers with continuous work history 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 continuous work history 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 continuous work history 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 externalities of the REBP program, 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 continuous work history, the relationship between unemployment duration and previous work history has shifted down significantly, compared to non-REBP regions. This is evidence that the effect of the REBP program was to decrease the unemployment duration of non-eligible workers in REBP counties.

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

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

where Y_{it} are different search outcomes of interest, M is an indicator for residing in a REBP county[13], T_t is an indicator for spells starting between June 1988 and July 1997, and $\tilde{T}_t$ is an indicator for spells starting between June 1988 and July 1993. $\mathbb{H} = \mathbb{1}[H > 15]$ is an indicator

[13]We remove the very few observations of individuals who reside in REBP counties and whose previous employer was in a non-REBP county, since their eligibility to REBP changed in 1991.

for individuals with more than 15 years of continuous work history in the past 25 years at the time they become unemployed. β_0 identifies the effect of REBP on treated workers, while γ_0 identifies spillovers of REBP on non-treated workers in REBP regions. $\sum \nu_t$ is a series of year fixed effects. Because we control for work history fixed effects ($\mathbb{H}$) interacted with both REBP counties fixed effects (M) and year fixed effects, specification 7 amounts to pooling two diff-in-diff together, one for the effect of REBP on unemployed workers with more than 15 years of work history (whereby eligible workers in REBP counties are compared to unemployed with similar experience in non-REBP counties), and one for the effect of REBP on unemployed workers with less than 15 years of work history (whereby non-eligible workers in REBP counties are compared to unemployed with similar experience in non-REBP counties).

To correct for the presence of common random effects, we cluster standard errors at the region-year level[14]. In column (1) of table 2, 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, and citizenship and tenure in previous job. In column (3) and (4) we add controls for preexisting trends by region, and by region$\times$work history. Results are very stable across all specifications. All estimates of β_0 confirm that REBP increased unemployment duration by roughly 40 weeks for eligible unemployed compared to similar unemployed workers in non-REBP counties. All estimates of γ_0 also confirm that non-eligible workers in REBP counties experienced a highly significant decrease in their unemployment duration of 7 to 12 weeks compared to similar workers in non-REBP counties. In column (5), we estimate the same model using as an outcome the duration of total non-employment (conditional on finding a job after the unemployment spell). Interestingly, now that we condition the sample on finding a job at the end of the unemployment spell, the direct effect of REBP on eligible unemployed is a little smaller in magnitude (+28 weeks), which suggests that some eligible workers did max out their unemployment benefits and never got back to work. But column (5) confirms that the REBP externalities on non-eligible workers are of similar magnitude on the duration of total non-employment and on the duration of unemployment ($\approx$ -10 weeks), which means that the positive effect of REBP on non-eligible workers is really about finding a job faster. 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. Finally, in columns (8) and (9), we investigate the sensitivity of our results to our sample restrictions. In our baseline estimates, we exclude women and men aged 55 to 59 because these individuals can channel directly from unemployment to early retirement, and therefore it is unclear whether these unemployed are effectively searching for a job or have just dropped out

[14]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.

of the labor force. In column (8) we estimate the same model for women only, aged 50 to 54. The sample size is much smaller, because the labor force participation rate of women aged 50 to 54 was much smaller at the time of REBP. The effect of REBP on eligible women is larger than for men, which confirms that a lot of women just took the full REBP benefits before going into retirement. But results show that we can also detect significant externalities of the REBP program for women, although a little smaller in magnitude than the externalities detected for men. In column (9) we estimate the model for men aged 50 to 59 and find similar externalities for non-eligible unemployed.

Robustness to potential confounders 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 counties 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 counties were chosen because of the relative importance of their steel sector, the fraction of steel sector employees never exceeds 15% of the labor force in these counties, and we restrict our sample to individuals who never were employed in the steel sector. As shown in figure 3, in our sample, the parallel trend assumption between REBP and non-REBP counties for both eligible and non-eligible workers seems to hold remarkably well before and after the REBP period. Second, and most importantly, because REBP counties were experiencing a restructuring of the steel sector, we should expect the region-specific shock to be, if anything, negative during the REBP period for REBP counties, which would lead to higher unemployment durations for non-eligible 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-eligible.

To further investigate the robustness of our results to the presence of region-specific shocks, we look at the estimated externalities of REBP on various age groups below the REBP age requirement by running models equivalent to that of equation (7) where we replace $\mathbb{H}$ by $\mathbb{A} = \mathbb{1}[Age > 50]$. We therefore compare the job search outcomes of individuals in REBP counties who are not eligible to REBP because their age is below the age requirement to similar unemployed in non-REBP counties[15]. If our identification strategy is confounded by a region-specific shock common to all unemployed in REBP counties at the time of the REBP, then we would expect to find for all age groups the same results than the ones we found in table 2 for non-eligible workers aged 50 to 54. If to the contrary, our identification strategy is not confounded by region-specific shocks then we may expect to see externalities for other age groups, but these externalities should: (*i*) be smaller than for non-eligible in the 50 to 54 age group, and (*ii*) decline in magnitude as we look at age groups that face less competition from the 50 to 54 age group. First, among the 50-54 group, non-eligible are very likely to be substitutable to the eligible 50-54 group, while due to age differences, a much smaller fraction of the 45-49 is likely to be perfectly substitutable to the 50-54 eligible. Second, the larger the difference in age between

[15] In these regressions we focus on individuals with more than 15 years of work history to make sure that eligible and non-eligible workers are comparable across all dimensions except age.

the group receiving extensions and the group not receiving extensions, the more likely it is that firms can discriminate job openings and therefore job search externalities should disappear. Results are reported in table 3 panel A and strongly support the view that our baseline are not confounded by a shock specific to REBP counties and contemporaneous to REBP. We first find that there are some much smaller though significant externalities on unemployed men aged 45 to 49[16] (columns (1) and (4)). Second, these externalities decrease as we look at age groups that are further apart (columns (2) and (5)), and then totally disappear for unemployed men aged 35 to 39 (columns (3) and (6)). The absence of externalities on men aged below 40 suggests another more direct strategy to control for region-specific shock, namely to use men aged below 40 in REBP counties as a control instead of men 50 to 54 in non-REBP counties. We implement this strategy in table 3 panel B, where we run on a sample restricted to unemployed aged 30 to 39 and 50 to 54 in REBP counties a diff-in-diff specification equivalent to equation (7) where we replace $\mathbb{M}$ by $\mathbb{A} = \mathbb{1}[Age > 50]$. This specification enables us to control for shocks to the labor markets of REBP counties contemporaneous to REBP that affect all job seekers in the same way. Our estimated externalities on non-eligible unemployed aged 50 to 54 are virtually unaffected compared to table 2, which is strong evidence in favor of our identification strategy. Besides, the treatment intensity analysis and geographical spillovers presented below also strongly suggest that region-specific shocks are not confounding our results.

The second potential confounder would be the presence of important selection effects. In particular, one may be concerned that because entry into unemployment is potentially endogenous, unobserved characteristics correlated with job search outcomes might change during the REBP period for non-eligible workers. To investigate this concern, we look at inflow rates into unemployment for eligible and non-eligible workers in REBP regions versus non-REBP regions. We run the same diff-in-diff model as previously on the quarterly log separation rate by region. Results are reported in column (1) of table 4. 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 non-eligible workers in REBP regions. In the remainder of table 4, we look at the effect of REBP on characteristics that are likely to be correlated with productivity and job search outcomes. In column (2) and (3), we run the diff-in-diff model of equation 7 on the log wage in previous job (prior to becoming unemployed), controlling for observable characteristics. We cannot detect any effect of the REBP program on the distribution of residual wages in previous job of non-eligible workers in REBP regions. For eligible workers, there is a small though not significant positive effect, which suggests that eligible unemployed who took up REBP had slightly better wages in their previous job. In column (4) and (5) we look at a measure of education level, the fraction of unemployed reporting having completed compulsory education. Again, we find at best a very small negative effect for non-eligible workers, and a small positive effect for eligible workers, which suggests that the education level of the eligible and non-eligible unemployed in REBP regions was hardly affected by REBP. Overall, these findings alleviate the concern of an important change in unobserved characteristics of non-eligible workers in REBP

[16]These externalities are, compared to table 2 column (1), more than 3 times smaller than for non-eligible men aged 50-54

regions at the time of the REBP program.

Treatment intensity Theory predicts, in a model with rigid wages and diminishing returns, that the relative size of the treated group of eligible unemployed compared to the non-treated group, which determines treatment intensity, has an impact on the magnitude of the externalities. To investigate this question, we look at measures of treatment intensity and interact these measures with the effect of REBP on non-eligible workers. The estimated specification is

$$\begin{aligned} Y_{it} = \quad & \alpha + \beta_0 \cdot \mathbb{H} \cdot M \cdot \tilde{T}_t + (\gamma_0^H \cdot \mathbb{1}[\text{Treat.=High}] + \gamma_0^L \cdot \mathbb{1}[\text{Treat.=Low}]) \cdot (1 - \mathbb{H}) \cdot M \cdot T_t \\ & + \eta_0 \cdot M + \sum \nu_t + \eta_1 \cdot \mathbb{H} + \eta_2 \cdot M \cdot \mathbb{H} + \sum \iota_t \cdot \mathbb{H} + X_{it}'\rho + \varepsilon_{it} \end{aligned} \quad (8)$$

where $\mathbb{1}[\text{Treat.=High}]$ and $\mathbb{1}[\text{Treat.=Low}]$ are indicators for a proxy of treatment intensity being above or below some threshold. We use two methods to characterize treatment intensity. The first method computes the average yearly fraction of eligible workers for each region×industry×education cell during REBP years. Across these cells, the median fraction of eligible workers is very close to 90%, but there is quite a lot of dispersion in the distribution across cells. We define high treatment intensity as being in a cell where more than 90% of unemployed were eligible and low treatment intensity for cells with less than 90% of eligible unemployed. Results are displayed in table 5 panel A. For all duration outcomes, except the fraction of spells superior to 26 weeks, externalities are significantly higher for non-eligible unemployed in high treatment intensity labor markets. We confirm the robustness of these results using a second proxy for treatment intensity. We compute the average quarterly fraction of new hires coming from non-REBP counties for each REBP county *when the REBP was not in place* as shown in figure 2 panel B. Counties that, absent REBP, had on average a high fraction of hires coming from non-REBP regions have labor markets that are more integrated to non-REBP regions and the effect of REBP on aggregate search effort within these counties is likely to be smaller than in counties that hardly ever hire individuals from non-REBP regions. We define high treatment intensity counties as counties where the fraction of new hires coming from non-REBP counties is lower than 5%. Figure 5 plots the yearly difference in unemployment duration between REBP and non-REBP counties for non-eligible workers (less than 15 years of work history) as in figure 3 panel B, but breaking down REBP counties in higher and lower treatment intensity counties as defined above. The relative decrease in unemployment duration for non-eligible unemployed during REBP has the same pattern in both higher and lower treatment intensity counties, but is much more pronounced in higher treatment intensity counties. Table 5 panel B confirms these results and shows that the effect of REBP on non-eligible unemployed was significantly stronger in counties with a very low level of integration to non-REBP counties.

Geographical spillovers So far, we have excluded from our sample unemployed residing in non-REBP counties that had labor markets highly integrated to REBP counties before REBP, defined as counties with an average quarterly fraction of new hires coming from REBP regions in the total number of new hires above 5% for all years before 1988. The reason is that these counties are likely to experience spillover effects from REBP counties and thus, cannot serve as a proper control in our diff-in-diff strategy. We now investigate directly whether we can detect the presence of externalities of REBP on unemployed workers residing in these counties. We begin by running a simple diff-in-diff specification comparing unemployed workers in non-REBP counties with high integration to REBP counties to unemployed workers in non-REBP counties with low level of integration[17]. Results are reported in table 6 panel A and suggest that REBP reduced the duration of unemployment spell by 4 weeks for unemployed workers in non-REBP counties with high labor market integration to REBP counties relative to similar workers in non-REBP counties with little labor market integration to REBP counties. Across specification and duration outcomes, γ_0, the diff-in-diff estimate of the geographical spillover of REBP, is always negative, roughly three times smaller in magnitude than in the baseline, but not statistically significant apart for the fraction of spells longer than 26 weeks. In order to increase statistical power, we use in panel B of table 6 a finer measure of labor market integration by looking at county$\times$industry$\times$education cells, and we compare unemployed workers in cells where the average fraction of hires from REBP counties in total yearly hires was larger than 20% before REBP to unemployed in cells where it was lower than 20%. These cells are a better measure of the relevant labor markets in which competition with REBP eligible workers is taking place. Our estimates show that REBP significantly improved job search outcomes for unemployed workers in cells where REBP greatly reduced competition from REBP workers. The magnitude of the effect is approximately three to four times smaller than in our baseline estimates of table 2.

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 investigate explicitly this question by looking at the effect of REBP on reemployment wages and other characteristics of jobs at reemployment.[18] In table 7, 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 in columns (1) and (2) suggest that reemployment wages for non-eligible workers are almost unaffected by REBP or if anything experience a slight increase during REBP. For eligible unemployed there is a slight ($\approx 5\%$) decline in reemployment wages. But because eligible workers experience longer unemployment durations during REBP while non-eligible workers exhibit shorter spells, it might be the case that these effects on reemployment wages are due to variations in the distribution of wage offers over the duration of a spell. If reemployment wages depend on the duration of the unemployment spell $w = w(D, B)$ (because of human capital depreciation, or discrimination from the employers), then the effect of a change in benefits B on reemployment wage can be decomposed into two effects:

[17]We again restrict the sample to unemployed aged 50 to 54 with more than 15 years of work history who are therefore comparable to eligible unemployed in REBP counties except for their county of residence.

[18]Lalive [2007] discusses the effects of benefit extension programs on re-employment wages without conditioning on elapsed unemployment duration.

$$\frac{dw}{dB} = \underbrace{\frac{\partial w}{\partial D} \cdot \frac{\partial D}{\partial B}}_{\text{Duration effect}} + \overbrace{\frac{\partial w}{\partial B}}^{\text{Reservation wage effect}}$$

If reemployment wages decline over the duration of a spell ($\frac{\partial w}{\partial D} < 0$), the total effect of an increase in benefits on reemployment wages might be zero or even negative even though the reservation wage effect is positive. We follow the methodology of Schmieder et al. [2012a] and estimate the effect of variations in benefits on reemployment wages *conditional on unemployment duration*. We do this first in the diff-in-diff setting of equation 7, and then in a RD setting taking advantage of the age eligibility discontinuity at 50. Note that in both cases, the identifying assumption requires that there is no correlation between unobserved heterogeneity and unemployment benefits *conditional on unemployment duration* which is a much stronger assumption than in the standard diff-in-diff or RD assumptions where we only need that the correlation between unobserved heterogeneity and unemployment benefits is zero.

We begin by plotting in figure 6 post-unemployment wages conditional on the duration of the unemployment spell in REBP and non-REBP counties for workers aged 50 to 54 with more than 15 years of experience. The difference between REBP and non-REBP counties at each duration point in panel B (when REBP was in place) compared to the same difference in panel A (when REBP was not in place) gives us a diff-in-diff estimate of the "reservation wage" effect. This evidence suggests that there was no significant reservation wage effect of REBP. We confirm this result in column (3) of table 7 by running the diff-in-diff model of equation 7 and adding a rich set of duration dummies to condition on the time spent unemployed prior to finding a job. We cannot detect any statistically significant effect of REBP on reemployment wages, neither for the eligible unemployed nor for the non-eligible, once we control for duration.

To complement our diff-in-diff approach, we then focus on the age eligibility discontinuity at 50 in REBP counties 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.

$$E[Y|A = a] = \sum_{p=0}^{\bar{p}} \gamma_p(a-k)^p + \nu_p(a-k)^p \cdot \mathbb{1}[A \geq k] + \sum_{t=0}^{T} \mathbb{1}[D = t] \tag{9}$$

where Y is real reemployment wage, A is age at the beginning of the unemployment spell, $k = 50$ is the age eligibility threshold, and D is the duration of the unemployment spell prior to finding the new job. Results are displayed in figure 7, where we have estimated this model for six periods to look at the dynamics of the wage response. 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 (1988-1990). This discontinuity increases over time and is the largest in 1991-1993, at the peak of REBP. The implied RD estimate of the elasticity of wages with respect to UI benefits is .08 (.03). This discontinuity then decreases and disappears when REBP is over. In other words, when controlling for the effect of duration on reemployment wages, we can identify a positive yet small effect of the REBP program on wages at the age eligibility discontinuity, and this effect increases over time. This suggests that wages are relatively rigid in the short run, but that in the longer run, wages might adjust a little to variations in outside options of workers. Note that for all periods, we ran a McCrary test, which ruled out the presence of a discontinuity in the probability density function of the assignment variable (age) at the cutoff (50 years), except for the 1991-1993 where a discontinuity can be detected. This implies that the larger wage effects found in 1991-1993 could also partly be driven by selection (sorting) at the 50 years age cut-off.

What can we learn on the wage setting process[19] from this empirical evidence? Is this evidence, combined with other available evidence, compatible with Nash bargaining? In a standard DMP model with Nash bargaining, the wage w is a weighted average of the productivity of the worker Π (which determines the reservation price of the employer) and of the value of remaining unemployed z (which determines the reservation price of the unemployed):

$$w = \beta\Pi + (1-\beta)z$$

The weight β corresponds to the bargaining power of the unemployed. Therefore $\frac{dw}{d\Pi} = \beta$ and $\frac{dw}{dz} = 1-\beta$. In other words, the bargaining power of the workers could be identified by the variation of wages to a change in Π or z. The main problem is that we never observe p nor $z = z(B, X)$, which depends not only on unemployment benefits B but also on many other different things such as the disutility of work, etc. The Nash bargaining model is therefore fundamentally non-identifiable. Are there nevertheless credible values of Π, z and β that would rationalize the empirical evidence presented here? First, all the evidence in the macro literature (see for instance Shimer [2005] and Hagedorn and Manovskii [2008]) suggests that wages do not react much to productivity shocks, so that $\frac{dw}{d\Pi}$ is likely to be small. This, implies that β is small. But if β is small, then wages should react a lot to variations in the outside options of workers, *i.e.* the value of remaining unemployed: $\frac{dw}{dz}$ and $\varepsilon_z = \frac{dw}{dz} \cdot \frac{z}{w}$ should be large. Of course, we never directly observe ε_z. Here for instance we observe the variation of wages to a change in unemployment benefits $\frac{dw}{dB} \cdot \frac{B}{w} = \varepsilon_z \cdot \frac{\partial z}{\partial B} \cdot \frac{B}{z}$. Given that we found $\frac{dw}{dB} \cdot \frac{B}{w} \approx 0$, it is difficult to believe that ε_z is very large, unless $\frac{\partial z}{\partial B} \cdot \frac{B}{z} << 1$. In other words, it is difficult to reconcile the small elasticity of w w.r.t z and the small elasticity of w w.r.t p in the Nash bargaining model. The only solution is to assume that $\frac{B}{z} << 1$ as in Hagedorn and Manovskii [2008]. But two pieces of evidence argue against such an assumption. First, if we follow their preferred calibration for β, our largest estimate of ε_z would imply[20] that $B \leq .05 \cdot z$ which seems absurdly low. In other words the value of remaining unemployed would be more than 20 times larger than the value of the unemployment benefits received by an unemployed. Second, if $\frac{B}{z} << 1$, this in turn implies that accounting profits of firms $\Pi - w$ are small, so that even small increases in w have very large effects on vacancy openings by firms, driving labor market tightness down. This means that

[19] Note that union membership is not extremely high in Austria, and the wage setting process is less centralized and rigid than in most continental European countries. Austria has (formally) a decentralized system of wage negotiations. 400 collective agreements determine a minimum wage in the particular sector/occupation where the contract applies and the wage growth for effective wages, leaving some room for individual bargaining.

[20] Assuming an additive specification $z = B + f(X)$ so that $\frac{\partial z}{\partial B} = 1$.

the "wage externality" would be very large, shocking labor demand down as in figure 1 panel B. This would also mean that the externalities of large unemployment extension programs like REBP would likely go in the opposite direction compared to our estimates. Overall, it seems reasonable to think that the Nash bargaining model is maybe not the best way to describe the data. A model of wage setting with some wage stickiness, at least in the short to middle run seems more appropriate. Still, it does not mean that Nash bargaining is not appropriate to describe the longer run. Indeed, the effects of REBP on wages seems to build up slightly over time and with treatment intensity. In the very long run, wages may adjust more to B than what we observe in the REBP experiment, suggesting that $\frac{dw}{dz}$ can be larger in the long run. This has important implications for the design of UI policies, which we discuss below.

7 Discussion and policy implications

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 which captures equilibrium adjustments in labor market tightness. The reason is that, when the Hosios condition does not hold and the the economy is inefficient, UI-induced variations in labor market tightness may have first-order welfare effects by affecting workers' job-finding probability per unit of effort. When the economy is slack, more UI is desirable if UI increases tightness and less UI is desirable if UI decreases tightness.

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

$$\varepsilon^M = \frac{d(e\cdot f(\theta))}{dB}\cdot\frac{B}{e\cdot f(\theta)} = \underbrace{\frac{\partial e}{\partial B}\cdot\frac{B}{e}}_{\varepsilon^m \text{ (Micro elasticity)}} + \overbrace{f'(\theta)\cdot\frac{\partial\theta}{\partial B}\cdot\frac{B}{f(\theta)}}^{\varepsilon^f \text{ (Equilibrium adjustment)}} \tag{10}$$

The microelasticity accounts only for the response of job search to UI while the macroelasticity also accounts for the response of the job-finding rate to UI[21]. The effect of UI on tightness (equilibrium adjustment effect) is measured by the wedge between microelasticity and macroelasticity of unemployment with respect to UI.

In the REBP setting, where more than 90% of unemployed over 50 were treated in REBP counties, the macro elasticity ε^M in the labor market of male workers aged 50 to 54 is given by

[21] Here, we have again assumed that $\frac{\partial e}{\partial\theta}\approx 0$. Otherwise total equilibrium adjustment is given by $\frac{\partial e}{\partial\theta}\cdot\frac{\partial\theta}{\partial B}\cdot\frac{B}{e}+\varepsilon^f$.

the diff-in-diff estimate on the treated, comparing eligible unemployed to similar unemployed in non-REBP counties. Using the approximation that the job finding rate is somewhat constant over an unemployment spell, we have that $\varepsilon^M \approx -\frac{\partial D}{\partial B} \cdot \frac{B}{D} = -\varepsilon^M_D$, where D is the duration of non-employment of eligible workers. Using our baseline estimates of column (5) in table 2 on the duration of non-employment, we find that $\varepsilon^M = -.185$.

The equilibrium adjustment effect can be approximated by the externality effect on the 10% of non-eligible unemployed aged 50 to 54 in REBP counties. $\varepsilon^f \approx -\frac{\partial D_n}{\partial B} \cdot \frac{B}{D_n}$, where $D_n \approx 1/(e_n \cdot f(\theta))$ is the duration of non-employment for non-eligible unemployed in REBP counties aged 50 to 54. This gives us an estimate of $\varepsilon^f = .067$. The micro elasticity can then be obtained as $\varepsilon^M - \varepsilon^f = -.252$ and the wedge between the micro and macro effect is therefore $1-(\varepsilon^M/\varepsilon^m) \approx .265$. An alternative but formally equivalent way of estimating the wedge consists in estimating the micro elasticity directly instead of the equilibrium adjustment ε_f. This can be done by estimating ε^m in triple-difference by comparing the duration of non-employment for eligible versus non-eligible workers in REBP counties. We obtain of course the exact same results for the elasticity wedge $1-(\varepsilon^M/\varepsilon^m)$ with both methods.

This relatively large wedge between the micro and the 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. Most 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 $\frac{\varepsilon^m}{\varepsilon^M}$ 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 a 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 in turn implies large diminishing returns to labor in the production function, and a steeper demand curve in the $\{n, \theta\}$ diagram, and therefore larger search externalities.

Short run vs long run effects As explained in section 2, 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 become more elastic to labor market tightness (in other words, the labor demand curves flattens a lot in the $\{n, \theta\}$ diagram), 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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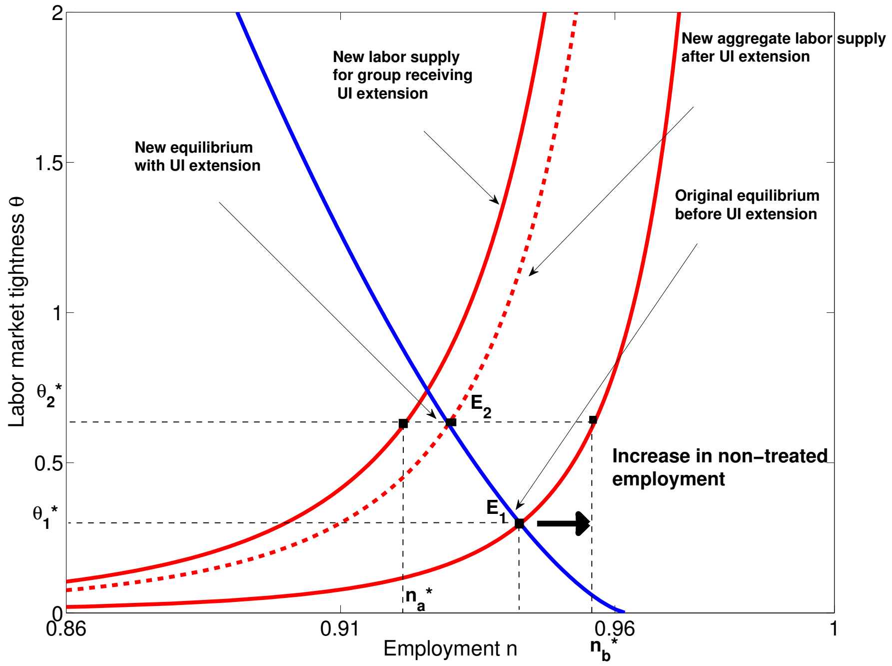
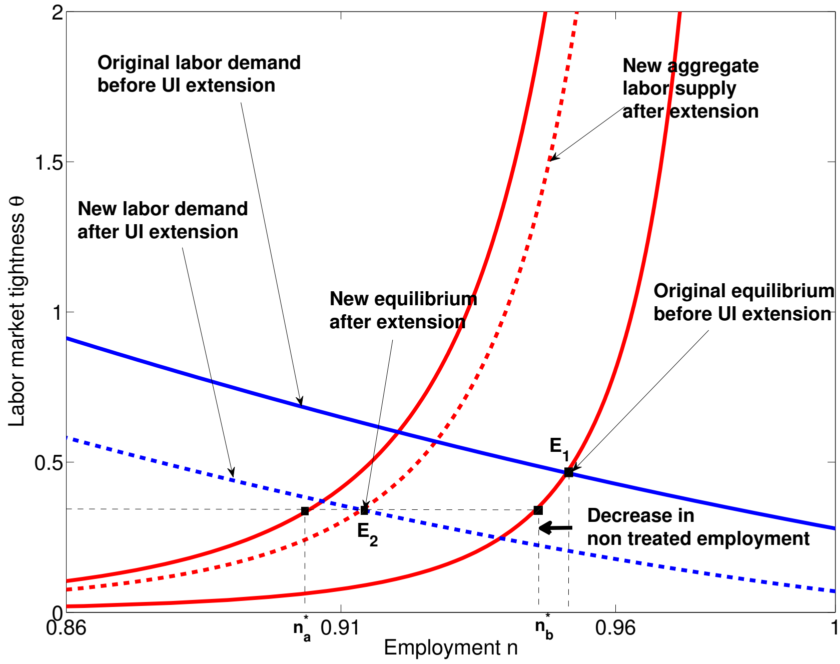
Figure 1: EXTERNALITIES OF UI EXTENSIONS IN AN EQUILIBRIUM SEARCH-AND-MATCHING MODEL:

A. Rigid wages & diminishing returns



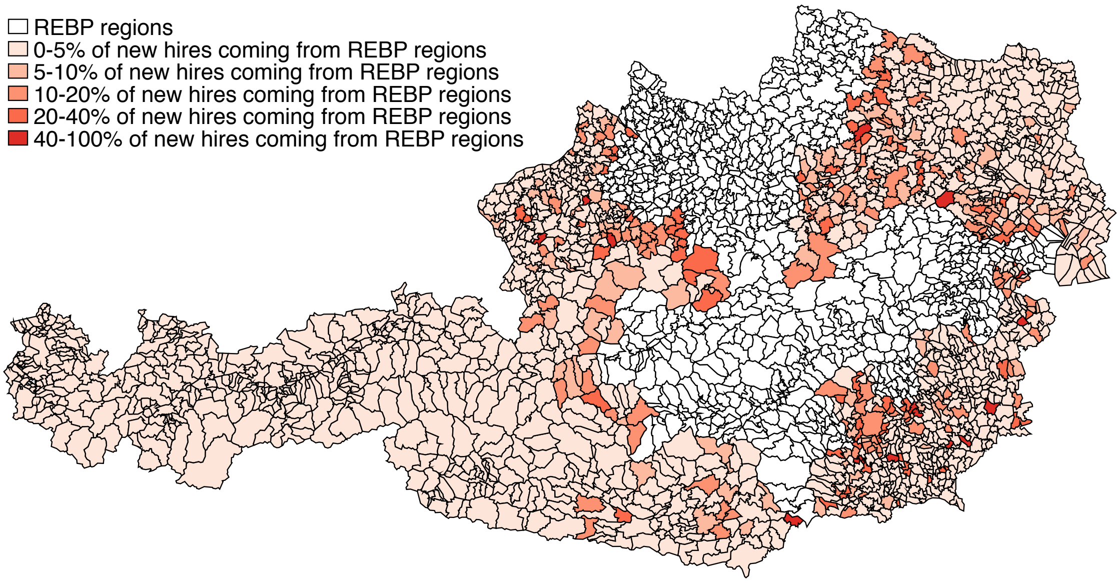
B. Flexible wages & close to linear technology



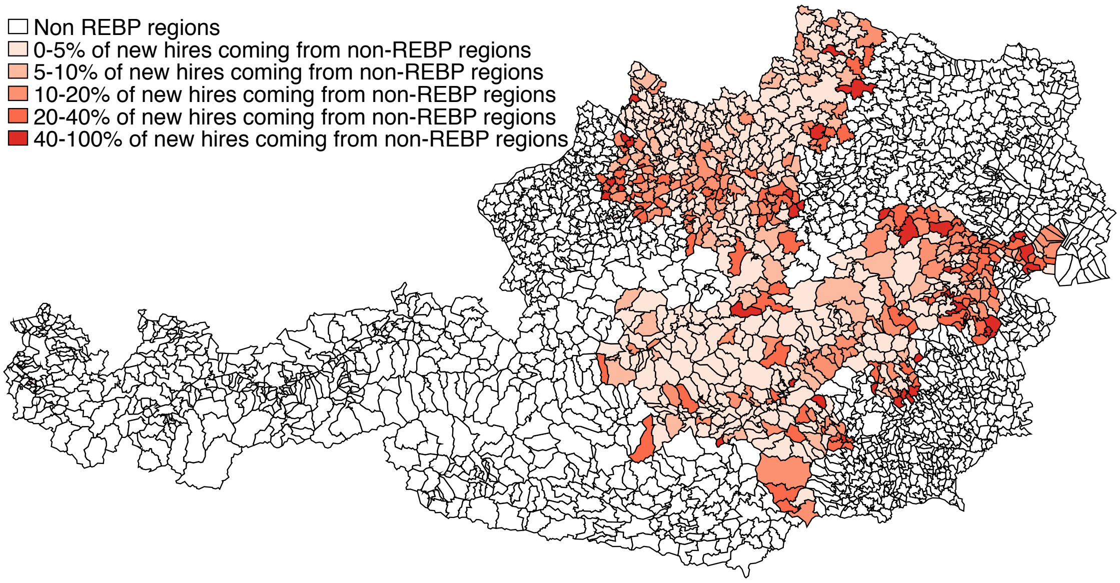
Notes: Both panels describe the effect on labor market equilibrium of a change in benefits for a subsample of the workforce, when firms cannot discriminate vacancies between groups. In both panel, we start from equilibrium E_1, where all workers get the same UI benefits. A group of workers then receives 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) as in the model of Michaillat [2012], labor demand is not affected, and, if returns to labor are decreasing, the new equilibrium E_2 is characterized 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 and local labor market integration

A. Fraction of new hires from REBP regions in total number of new hires by county

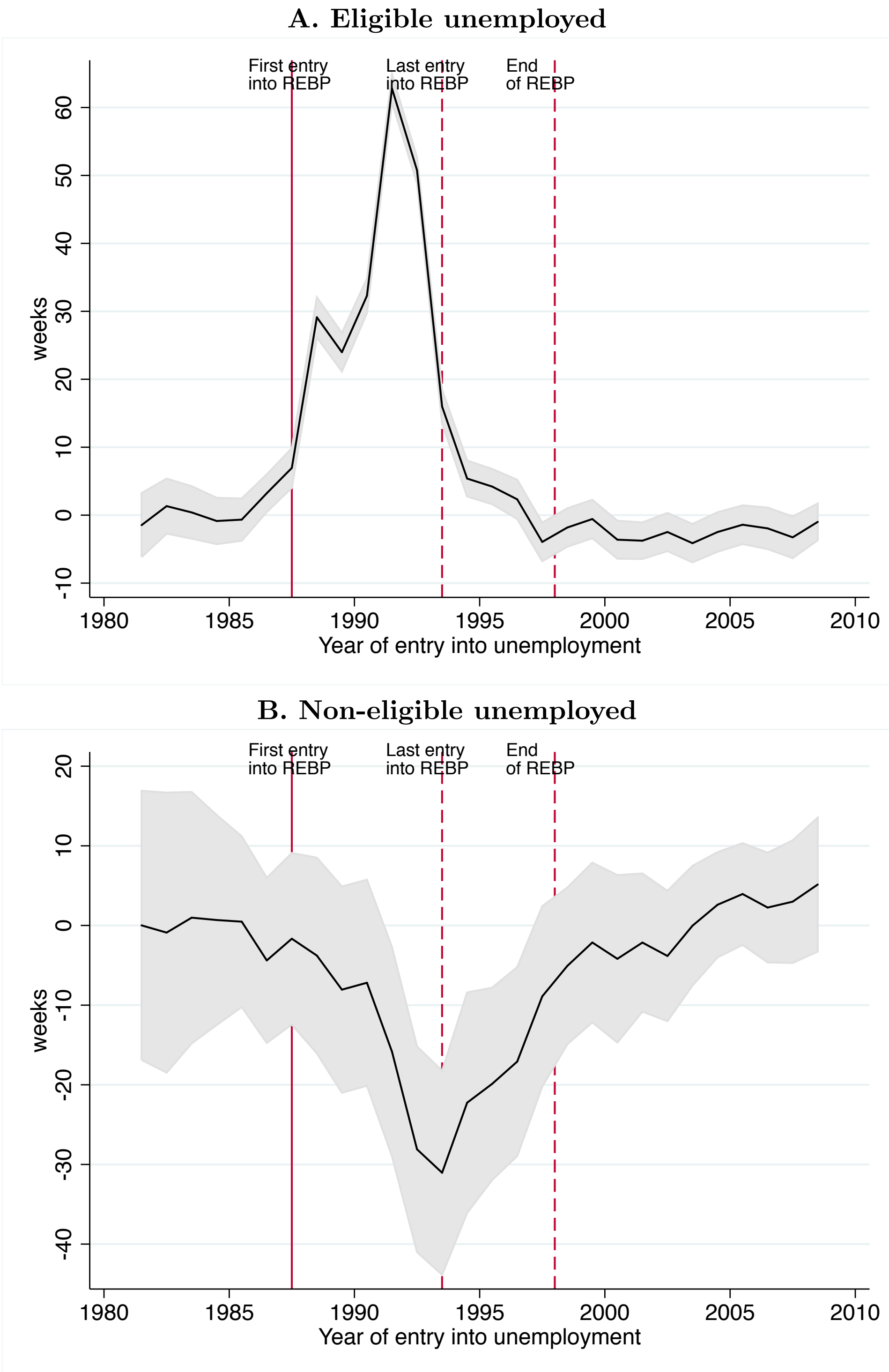


B. Fraction of new hires from non-REBP regions in total number of new hires by county



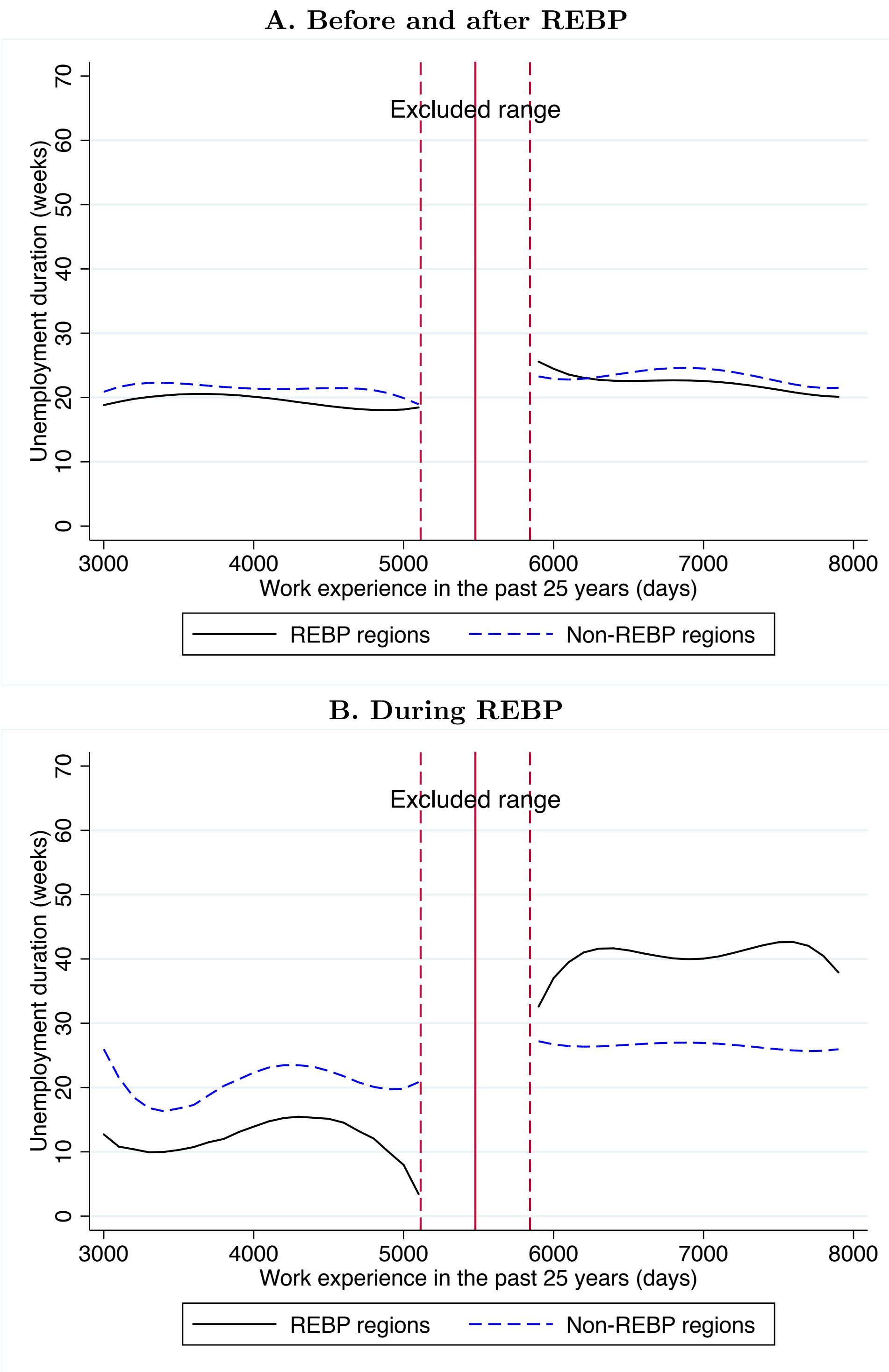
Notes: the figure shows the distribution of REBP across the 2361 communities (counties) in Austria. The treated regions (REBP regions) are all counties with red shading in panel B and include parts of the provinces of Burgenland, Carinthia (Kärnten), Lower Austria (Niederösterreich), Upper Austria (Oberösterreich), and Styria (Steiermark). Both panels also give important information about the level of local labor market integration across REBP and non-REBP regions. Panel A maps the average quarterly fraction of men aged 50 to 54 coming from REBP regions in the total number of new hires of men aged 50 to 54 in non-REBP counties **for all years when the REBP was not in place**. The map shows that the degree of competition from REBP workers faced by workers in non-REBP counties is very 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 5% of new hires coming from REBP regions. Panel B maps the average quarterly fraction of men aged 50 to 54 coming from non-REBP regions in the total number of new hires of men aged 50 to 54 in REBP counties **for all years when the REBP was not in place**. This measures the degree of competition from non-REBP workers faced by workers in REBP counties. The map shows that this competition is relatively small except for a few counties close to the REBP border.

Figure 3: Difference in unemployment durations between REBP and non-REBP counties by year of entry into unemployment, for eligible and non-eligible unemployed:



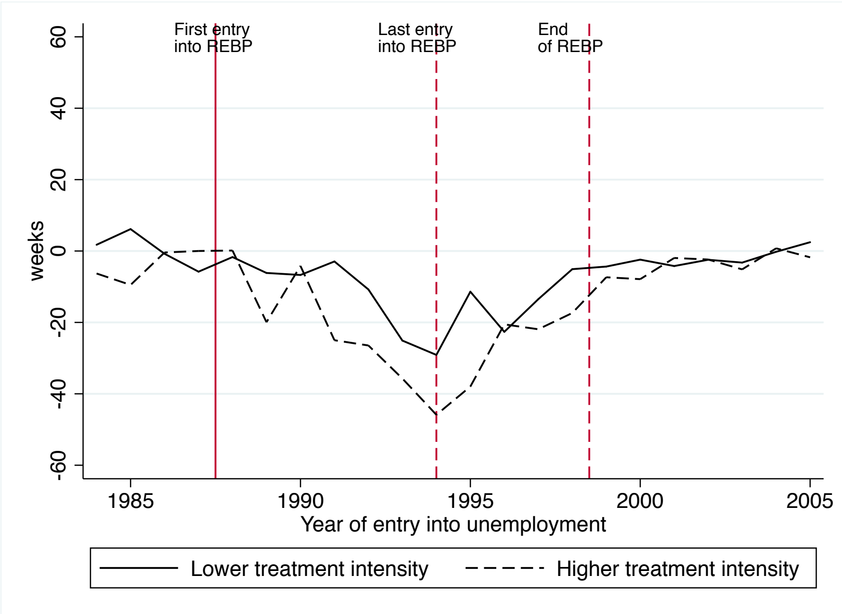
Notes: The figure plots d_t, the yearly difference in unemployment duration between REBP and non-REBP counties, obtained from regression specification 12, where controls include education, 15 industry codes, family status, citizenship and tenure in previous job. Panel A plots the difference for workers with more than 15 years of work history in the past 25 years prior to becoming unemployed, who are therefore eligible for REBP. Panel B plots the difference for non-eligible workers (less than 15 years of work history). Non-REBP counties with high labor market integration to REBP regions are excluded from the sample. See text for details.

Figure 4: Relationship between previous work experience and unemployment duration in REBP and non-REBP counties:



Notes: the figure plots the relationship between work history in the 25 years prior to becoming unemployed and unemployment duration in REBP and non-REBP counties, 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: $E[Y|H=h] = \sum_{p=0}^{5} \gamma_p (h-k)^p + \nu_p (h-k)^p \cdot \mathbb{H}$ where h is work history, the forcing variable, and k is the eligibility threshold for REBP extensions, and $\mathbb{H} = \mathbb{1}[H \geq k]$ 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.

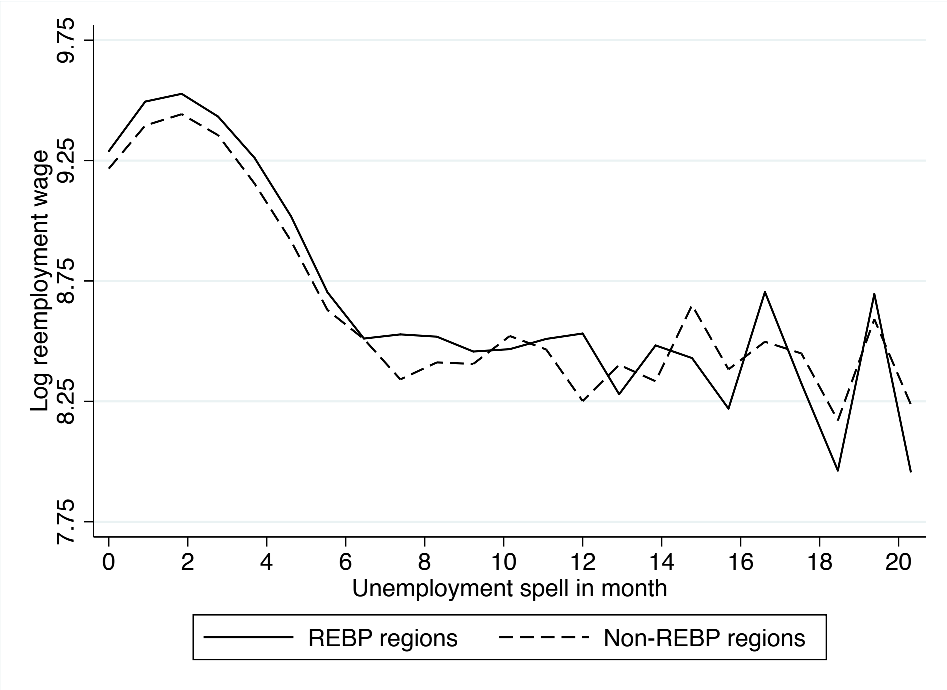
Figure 5: Effects of REBP on non-eligible workers by treatment intensity



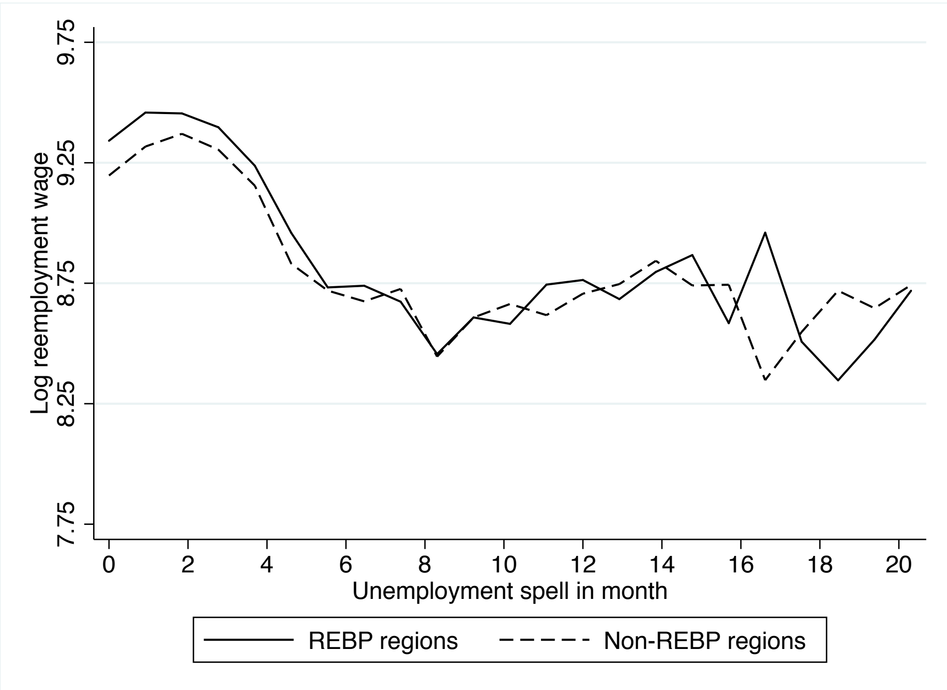
Notes: the figure plots the yearly difference in unemployment duration between REBP and non-REBP counties for non-eligible workers (less than 15 years of work history) as in figure 3 panel B, but breaking down REBP counties in higher and lower treatment intensity counties. To define treatment intensity, we compute the average quarterly fraction of new hires coming from non-REBP counties for each REBP county when the REBP was not in place as shown in figure 2 panel B. Counties that have on average a high fraction of hires coming from non-REBP regions have labor markets that are more integrated to non-REBP regions and the effect of REBP on aggregate search effort within these counties is likely to be smaller than in counties that hardly ever hire individuals from non-REBP regions. We define high treatment intensity counties as counties were the fraction of new hires coming from non-REBP counties is lower than 5%.

Figure 6: REEMPLOYMENT WAGES CONDITIONAL ON DURATION OF UNEMPLOYMENT SPELL IN REBP AND NON-REBP COUNTIES

A. Before and after REBP

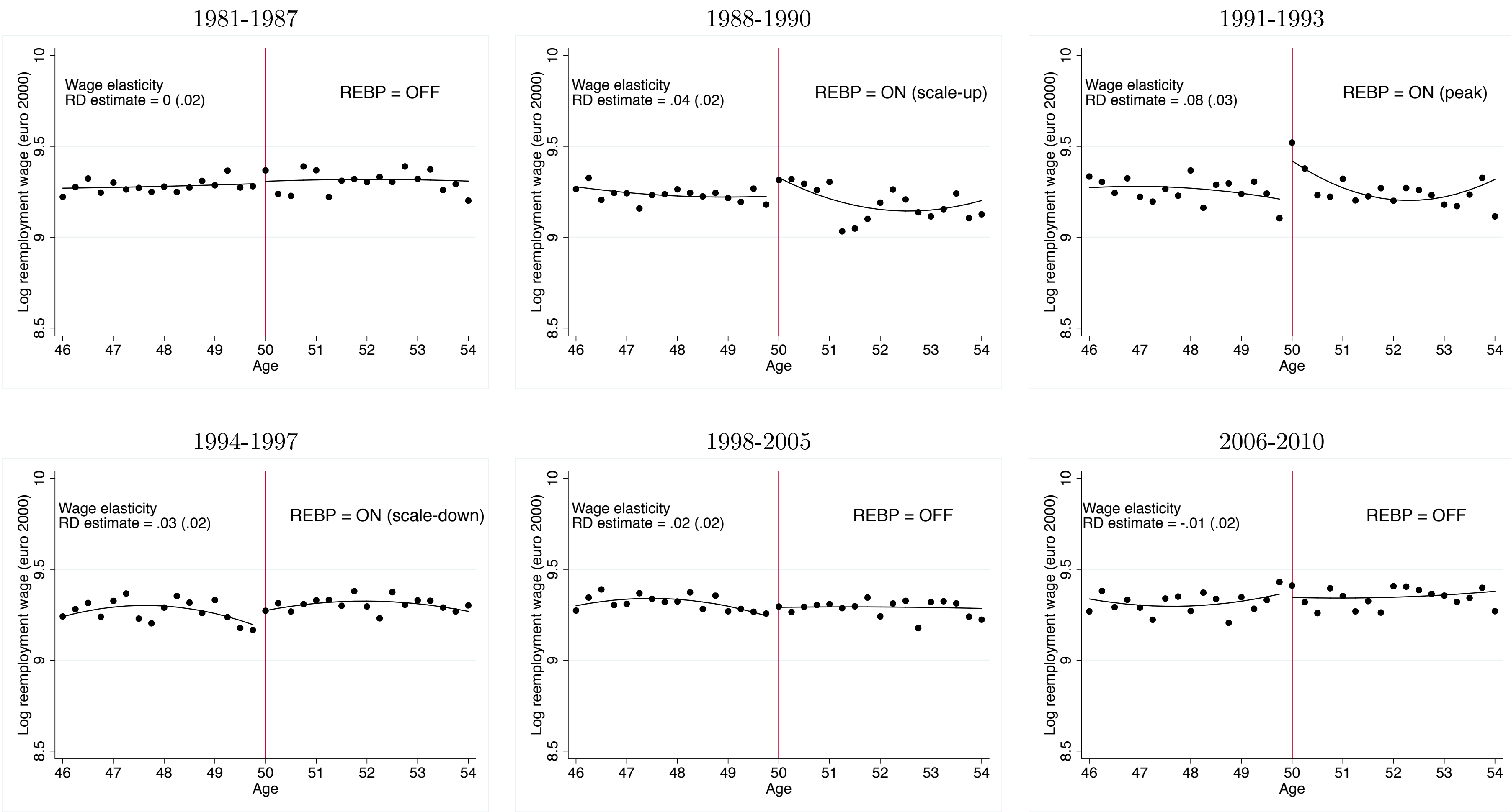


B. During REBP



Notes: the figure plots post-unemployment wages conditional on the duration of the unemployment spell in REBP and non-REBP counties for workers aged 50 to 54 with more than 15 years of experience in the past 25 years prior to becoming unemployed. Following the methodology of Schmieder et al. [2012a], by conditioning on the duration of unemployment, we control for the fact that REBP eligible workers experienced longer unemployment spells during the REBP period, which may impact reemployment wages if the distribution of wages depend on time spent unemployed (because of skill depreciation or discrimination from employers for instance). The difference between REBP and non-REBP counties at each duration point in panel B (when REBP was in place) compared to the same difference in panel A (when REBP was not in place) gives us a diff-in-diff estimate of the “reservation wage” effect. This evidence suggests that there was no significant reservation wage effect of REBP.

Figure 7: RD evidence on wage bargaining over time: relationship between age and reemployment wages in REBP counties



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. [2012a] 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. $E[Y|A=a] = \sum_{p=0}^{\bar{p}} \gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[A \geq k] + \sum_{t=0}^{T} \mathbb{1}[D=t]$, where Y is real reemployment wage, A is age at the beginning of the unemployment spell, $k = 50$ is the age eligibility threshold, and D is the duration of the unemployment spell prior to finding the new job. The graph plots the predicted values of this regression for 6 periods: before REBP 1981-1987, at the beginning of REBP (1988-1990), at the peak of REBP (1991-1993), when REBP was scaled down (1994-1997) and then for two periods after the end of REBP (1998-2005 and 2006-2010). Note that for all periods, we ran a McCrary test, which ruled out the presence of a discontinuity in the probability density function of the assignment variable (age) at the cutoff (50 years), except for the 1991-1993 where a discontinuity can be detected.

Table 1: Summary statistics

	(1)	(2)	(3)	(4)
	A. REBP vs non-REBP counties before 1988			
	Non-REBP counties	REBP counties	Difference	p-value
Labor market outcomes				
Fraction employed in the steel sector	.07	.13	-.06	0
Monthly 50-54 unemployment rate	.0787	.0793	.0006	.69
Unemployed in estimation sample				
Fraction with work history > 15 yrs	.907	.921	-.014	.004
Age	51.9	51.9	0	.596
Unemployment duration	21	22.6	-1.6	.028
Non employment duration	26.9	27.5	-.6	.558
Fraction spells >52 wk	.056	.062	-.006	.132
Wage before U spell (€2000)	11,735	12,313	-578	0
Wage after U spell (€2000)	11,512	12,164	-6,511	0
	B. REBP-eligible vs non-eligible unemployed in REBP counties before 1988			
	Non-eligible unemployed	Eligible unemployed	Difference	p-value
Unemployed in estimation sample				
Age	51.8	51.9	-.1	.095
Unemployment duration	20.5	25.1	-4.6	.118
Non employment duration	30	28.8	1.3	.715
Fraction spells > 52 wks	.032	.064	-.032	.011
Wage before U spell (€2000)	10,403	12,476	-2,072	0
Wage after U spell (€2000)	10,3733	12,318	-1,945	0
Fraction with compulsory education	.705	.659	.046	.066
Fraction married	.832	.811	.021	.321

Notes: The table displays summary statistics from the Austrian social security and unemployment insurance files before the introduction of the REBP program in 1988. Panel A compares REBP and non-REBP counties. P-value is for a test of equality of means for REBP and non-REBP counties. The fraction of employment in the steel sector is defined as the average quarterly fraction of individuals aged 50 to 54 employed in the steel industry by county. The unemployment rate is the average monthly number of unemployed men aged 50 to 54 recorded in the unemployment insurance files as a fraction of the sum of unemployed and employed male workers aged 50 to 54 by county. The estimation sample of unemployed workers is restricted to men, aged 50 to 54, who never work in the steel sector. Panel B compares, in REBP counties and before 1988, eligible unemployed workers with more than 15 years of continuous work history in the past 25 years to non-eligible 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. All duration outcomes are expressed in weeks. Wages are annually adjusted and expressed in constant €2000. Non-employment is defined as the number of weeks between two employment spells. Unemployment duration is the duration of paid unemployment recorded in the UI administrative data.

Table 2: Baseline estimates of the treatment effect of REBP on eligible unemployed and non-eligible unemployed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Unemployment duration				Non-empl. duration	Spell >100 wks	Spell >26 wks	Unemployment duration	
								Women 50 to 54	Men 50 to 59
β_0 (treatment effect on eligible)	47.43***	41.54***	39.26***	40.69***	28.32***	0.224***	0.226***	61.00***	53.72***
	(5.660)	(4.865)	(4.507)	(4.609)	(5.131)	(0.0267)	(0.0216)	(2.312)	(5.046)
γ_0 (externality on non-eligible)	-6.941***	-6.573***	-12.02***	-10.88***	-10.20***	-0.0274***	-0.0431***	-8.184***	-11.99***
	(1.690)	(1.668)	(1.847)	(1.704)	(1.739)	(0.00675)	(0.0124)	(3.076)	(1.631)
Educ., marital status, industry, citizenship		×	×	×	×	×	×	×	×
Preexisting trends									
by region			×						
by region×work history				×	×	×	×	×	×
N	127802	126091	126091	126091	106164	126091	126091	59831	225100

Notes: S.e. clustered at the year×region level in parentheses. * p<0.10, ** p<0.05, *** p<0.010.
All duration outcomes are expressed in weeks. The table presents estimates of the model presented in equation (7). β_0 identifies the effect of REBP on eligible unemployed, while γ_0 identifies spillovers of REBP on non-eligible unemployed in REBP counties. 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. For columns (1) to (7), the sample is restricted to men aged 50 to 54. In column (7) we estimate the same model for women only, aged 50 to 54. In column (9) we estimate the model for men aged 50 to 59.

Table 3: Robustness to REBP-counties-specific shocks

A. Externalities of REBP on different age groups in REBP counties

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment duration			Non-employment duration		
	Age group 50-54 vs			Age group 50-54 vs		
	45-49	40-44	35-39	45-49	40-44	35-39
β_0 (treatment effect)	47.43***	47.43***	47.43***	30.27***	30.27***	30.27***
	(5.659)	(5.659)	(5.659)	(5.866)	(5.866)	(5.866)
γ_0 (externality)	-1.936**	-0.780**	-0.0384	-2.464***	-2.159***	-0.771
	(0.745)	(0.332)	(0.323)	(0.685)	(0.523)	(0.607)
N	269310	283458	283266	237836	254961	257631

B. Externalities on non-eligible aged 50 to 54 using unemployed aged 30 to 39 in REBP counties as a control

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment duration		Non-empl. duration		Spell >26 wks	
β_0 (treatment effect)	54.32***	51.04***	30.30***	30.17***	0.312***	0.274***
	(7.480)	(6.857)	(7.639)	(7.163)	(0.0432)	(0.0360)
γ_0 (externality)	-7.878**	-6.719*	-7.643***	-6.176**	-0.0742***	-0.0544**
	(3.880)	(3.573)	(2.156)	(2.420)	(0.0222)	(0.0211)
Educ., marital status, industry, citizenship		×		×		×
N	182689	180098	170388	168163	182689	180098

Notes: S.e. clustered at the year×county level in parentheses. * p<0.10, ** p<0.05, *** p<0.010.
All duration outcomes are expressed in weeks. In panel A, we investigate the presence of externalities on different age groups within REBP counties to detect the presence of some common potential shock specific to REBP counties and contemporaneous to REBP. We replicate the results of baseline specification 7 presented in table 2 column (1) but in the specification, we replace $\mathbb{H}$ by $\mathbb{A} = \mathbb{1}[Age > 50]$ and focus on individuals with more than 15 years of work history. β_0 identifies the effect of REBP on eligible workers, while γ_0 identifies spillovers of REBP on unemployed workers in REBP regions who are non-eligible because they are less than 50 at the start of their unemployment spell. In panel B, we use the same strategy as in table 2 but we use men aged 30 to 40 in REBP counties as a control instead of men 50 to 54 in non-REBP counties. We run on a sample restricted to unemployed aged 30 to 39 and 50 to 54 a diff-in-diff specification equivalent to equation (7) where we replace $\mathbb{M}$ by $\mathbb{A} = \mathbb{1}[Age > 50]$. This specification enables us to fully control for shocks to the labor markets of REBP counties contemporaneous to REBP.

Table 4: TESTING FOR SELECTION: IMPACT OF REBP ON INFLOW RATE INTO UNEMPLOYMENT, LOG REAL WAGE IN PREVIOUS JOB AND EDUCATION LEVEL OF ELIGIBLE AND NON-ELIGIBLE UNEMPLOYED

	(1)	(2)	(3)	(4)	(5)
	log separation rate	log real wage in previous job		Fraction with compulsory educ.	
Eligible workers	0.299***				
	(0.0356)				
Non-eligible workers	-0.0347				
	(0.0304)				
β_0 (REBP effect on eligible)		0.0604	0.0346	0.0136*	0.0179**
		(0.0600)	(0.0573)	(0.00811)	(0.00733)
γ_0 (REBP effect on non-eligible)		0.00728	-0.00588	-0.0515*	0.0288
		(0.0418)	(0.0410)	(0.0279)	(0.0227)
Industry, citizenship		×	×	×	×
Preexisting trends by region			×		×
N	1734	114757	114757	114757	114757

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 regions compared to non-REBP regions. In this column, observations are at the region×quarter level. Columns (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. Columns (4) and (5) repeat the same regressions using the fraction of unemployed having completed compulsory education as an outcome.

Table 5: Externalities on non-eligible unemployed by REBP-treatment intensity

REBP effect on non-treated	(1) Unemployment duration	(2) Non-empl. duration	(3) Spell >100 wks	(4) Spell >26 wks
	Treatment intensity - Method 1: Fraction treated in region×education×industry cell			
γ_0^L (fraction treated $\leq .9$)	-3.633	-4.723**	-0.00292	-0.0400***
	(2.339)	(1.810)	(0.00734)	(0.0125)
γ_0^H (fraction treated $> .9$)	-8.319***	-7.680***	-0.0188***	-0.0264
	(1.939)	(2.287)	(0.00574)	(0.0262)
F-Test $\gamma_0^L = \gamma_0^H$	[0.0505]	[0.0732]	[0.0485]	[0.668]
	Treatment intensity - Method 2: County share of hires from non-REBP counties			
γ_0^L (share of non-REBP hires $> .05$)	-2.943	-5.128**	-0.00166	-0.0153
	(2.043)	(2.050)	(0.00689)	(0.0145)
γ_0^H (share of non-REBP hires $\leq .05$)	-11.93***	-7.924***	-0.0286***	-0.0756***
	(2.570)	(2.579)	(0.00726)	(0.0234)
F-Test $\gamma_0^L = \gamma_0^H$	[0.00267]	[0.298]	[0.00928]	[0.0388]
Educ., marital status, industry, citizenship	×	×	×	×
N	167920	143922	167920	167920

Notes: S.e. clustered at the year×region level in parentheses. * p<0.10, ** p<0.05, *** p<0.010.
Sample restricted to male workers aged 50-54 working in non-steel related sectors. All duration outcomes are expressed in weeks. The table presents estimates of the effects of REBP on non-eligible workers broken down by REBP-treatment intensity. The estimated specification is that of equation (8). γ_0^H identifies spillovers of REBP on non-treated workers in high REBP-treatment intensity regions, γ_0^L identifies spillovers of REBP on non-treated workers in low REBP-treatment intensity regions. We use two methods to characterize treatment intensity. Method 1 computes the average yearly fraction of eligible workers for each region×industry×education cell during REBP years and we define high treatment intensity as being in a cell where more than 90% of unemployed were eligible (see text for details). Method 2 computes the average quarterly fraction of new hires coming from non-REBP counties for each REBP county when the REBP was not in place and we define high treatment intensity counties as counties where the fraction of new hires coming from non-REBP counties is lower than 5%.

Table 6: Geographical spillovers: Effect of REBP on unemployed workers in non-REBP counties with high labor market integration to REBP counties

	(1)	(2)	(3)	(4)	(5)
	Unemployment duration		Non-empl. duration	Spell >100 wks	Spell >26 wks
	Labor market integration - Measure 1: Fraction of hires coming from REBP regions in county cell				
γ_0 (geographical spillovers)	-4.318	-4.639	-3.201	-0.00827	-0.0285*
	(3.618)	(3.388)	(2.460)	(0.0124)	(0.0144)
	Labor market integration - Measure 2: Fraction of hires coming from REBP regions in county×industry×education cell				
γ_0 (geographical spillovers)	-5.889***	-4.720***	-2.253***	-0.0112***	-0.0142**
	(1.013)	(0.919)	(0.601)	(0.00314)	(0.00538)
Educ., marital status, industry, citizenship		×	×	×	×
N	104881	102840	88702	102840	102840

Notes: S.e. clustered at the year×region level in parentheses. * p<0.10, ** p<0.05, *** p<0.010.
Sample restricted to male workers aged 50-54 working in non-steel related sectors with more than 15 years of experience in the past 25 years prior to becoming unemployed. All duration outcomes are expressed in weeks. The table presents estimates of a simple diff-in-diff specification comparing unemployed workers in non-REBP counties with high integration to REBP counties versus unemployed workers in non-REBP counties with low level of integration as a control. In panel A, 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. In panel B, we use a finer measure of labor market integration by looking at county×industry×education cells, and we compare unemployed workers in cells where the average fraction of hires from REBP counties in total yearly hires was larger than 20% before REBP to unemployed in cells where it was lower than 20%.

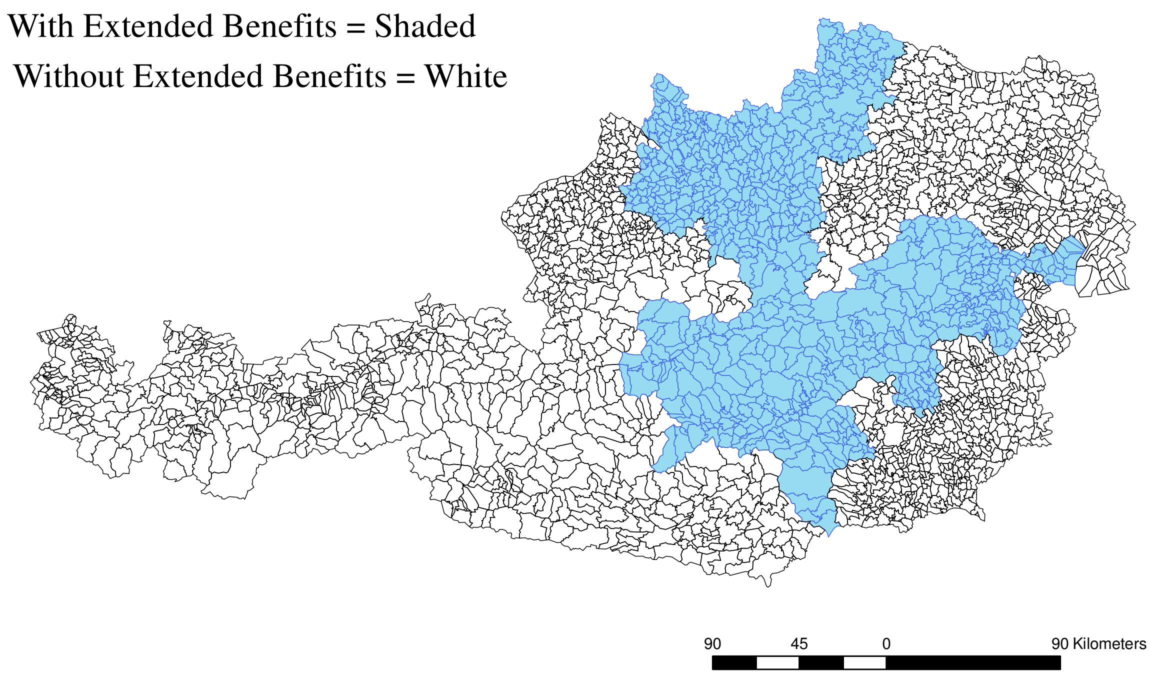
Table 7: Effects of REBP on wages

	(1)	(2)	(3)
	log reemployment wage		
β_0 (REBP effect on eligible)	-0.0491***	-0.0477***	-0.0225
	(0.0176)	(0.0156)	(0.0148)
γ_0 (REBP effect on non-eligible)	0.0190	0.0786**	0.0410
	(0.0448)	(0.0340)	(0.0301)
Educ., marital status, industry, citizenship		×	×
Preexisting trends by region		×	×
Set of dummies for duration of U spell			×
N	89290	88691	88610

Notes: Standard errors are clustered at the year×region level. * p<0.10, ** p<0.05, *** p<0.010. The table investigates the impact of REBP on real reemployment wages. Column (1) and (2) run the baseline diff-in-diff specification of equation 7 using log reemployment wages as an outcome. β_0 identifies the effect of REBP on eligible unemployed, while γ_0 identifies spillovers of REBP on non-eligible unemployed in REBP counties. In column (3), following the methodology of Schmieder et al. [2012a], we condition on the duration of unemployment using a rich set of dummies for the duration of unemployment prior to finding a new job. This is in order to control for the fact that REBP eligible workers experienced longer unemployment spells during the REBP period, which may impact reemployment wages if the distribution of wages depend on time spent unemployed (because of skill depreciation or discrimination from employers for instance). The difference between REBP and non-REBP counties at each duration point when REBP was in place compared to the same difference when REBP was not in place gives us a diff-in-diff estimate of the "reservation wage" effect. This evidence suggests that there was no significant reservation wage effect of REBP.

Appendix - Not for publication

Figure 8: Regional distribution of REBP



Notes: the figure shows the distribution of REBP across the 2361 communities (counties) in Austria. The treated regions (REBP regions) are all counties with blue shading and include parts of the provinces of Burgenland, Carinthia (Kärnten), Lower Austria (Niederösterreich), Upper Austria (Oberösterreich), and Styria (Steiermark).