Market Externalities of Large Unemployment Insurance Extension Programs*

Rafael Lalive† University of Lausanne

Camille Landais‡ London School of Economics

Josef Zweimüller§ University of Zurich

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Abstract

In this paper we shed new light on the equilibrium effects of UI benefits. First, we show how market externalities of UI can be identified in a quasi-experimental setting by looking at the effect of UI variations in a given labor market on job search outcomes of workers who are not treated by UI variations but are in the same labor market. We define a labor market as the place where workers are competing for the same vacancies, and propose a new method to determine the scope of a labor market using vacancy data. Second, we implement this strategy and offer evidence of the existence of market externalities of UI extensions using the Regional Extension Benefit Program (REBP) in Austria. This program extended unemployment benefits drastically for a large subset of workers in selected regions of Austria in the period from June 1988 until August 1993. We focus on unemployed workers in REBP regions who are similar to the eligible unemployed, compete for the same vacancies, but are not eligible for REBP because they fail an eligibility requirement of the REBP program. We show that in treated 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. 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.

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†Department of Economics, University of Lausanne, CH-1015 Lausanne-Dorigny, Rafael.Lalive@unil.ch. Rafael Lalive is also associated with CESifo, IFAU, IZA, IfW and University of Zurich (IEW).

‡Dept of Economics, Houghton Street London, WC2A 2AE +44(0)20-7955-7864, c.landais@lse.ac.uk

§Department of Economics Mühlebachstrasse 86 CH-8008 Zurich; josef.zweimueller@econ.uzh.ch

1 Introduction

The probability that an unemployed individual finds a job depends on her job search strategy[1]. It also depends on the labor market conditions that determine how easy it is to be matched to a potential employer. Changes in unemployment insurance (UI) policies affect the search strategy of unemployed workers which in turn affects their job search outcomes. This is the *micro effect* of UI. It can be identified by comparing two individuals with different levels of UI generosity in the same labor market. Empirically, a large number of well-identified estimates of the micro effect of UI on unemployment duration have been produced[2]. Changes in UI benefits also affect labor market conditions and the job finding rate through equilibrium effects in the labor market. We call this second effect *market externalities* of UI.

Market externalities of UI are important for at least two reasons. First, the overall effect of variations in UI on search outcomes, the *macro effect*, will be the sum of the micro effect and market externalities. Studies comparing individuals with different UI benefit within the same labor market identify the micro effect and cannot shed light on the true effect of such UI extensions if externalities are important. Second, market externalities have first order welfare effects, as shown in Landais et al. [2010]. This implies that the sign and magnitude of market externalities is critical to determine the optimal level of UI.

There is no theoretical consensus on the sign and magnitude of market externalities of UI. And empirically, it has always proven challenging to estimate equilibrium effects of UI on unemployment and labor market outcomes. Recent papers have tried to directly estimate equilibrium effects of active labor market policies such as randomized programs of counselling for job seekers (Blundell et al. [2004], Ferracci et al. [2010], Gautier et al. [2012][3]). But results of these studies reach no consensus. More recently, 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 evidence of significant displacement effects for unemployed men who were not in the program. But take-up of the training program was low (35 %) and many job seekers were already employed at the time of the experiment substantially limiting the statistical power to detect displacement effects.

Contrary to UI, active labor market programs do not directly affect outside options of workers in the wage bargaining process, and miss a potentially important element of equilibrium adjustments through wages. Active labor market programs are therefore only partially informative about the market externalities of UI. We are aware of only one paper that studies market

[1] Setting a job search strategy involves making various decisions such as: how hard to search, what jobs to search for, how to set one's reservation wage, etc.

[2] See for instance Krueger and Meyer [2002] for a survey of early studies. More recent studies include Landais [2013] for the US, Schmieder et al. [2012b] for Germany or Lalive and Zweimüller [2004a,b] for Austria.

[3] 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.

externalities of UI. Levine [1993] finds that increases in the replacement rate of UI decreases unemployment duration among the unemployed who are ineligible for UI. Hagedorn et al. [2013] estimate a macro elasticity of unemployment with respect to UI variations for the U.S. by comparing counties on the border of states with different potential benefit duration. Our estimates are compatible with the macro elasticity they find. Our results complement their findings in suggesting that the micro effect is larger than the macro effect, due to the existence of the market externalities.

In this paper we shed new light on the equilibrium effects of UI benefits. First, we show how market externalities of UI can be identified in a quasi-experimental setting by looking at the effect of UI variations in a given labor market on job search outcomes of workers who are not treated by UI variations but are in the same labor market. We define a labor market as the place where workers are competing for the same vacancies, and propose a new method to determine the scope of a labor market using vacancy data. Second, we implement this strategy and offer evidence of the existence of market externalities of UI extensions using the Regional Extension Benefit Program (REBP) in Austria. This program extended unemployment benefits drastically for a large subset of workers in selected regions of Austria in the period from June 1988 until August 1993. We focus on unemployed workers in REBP regions who are similar to the eligible unemployed, compete for the same vacancies, but are not eligible for REBP because they fail an eligibility requirement of the REBP program. Using a difference-in-difference identification strategy, we compare these non-eligible unemployed to similar non-eligible unemployed in non-REBP regions to identify the effect of REBP on duration of job search of non-eligible unemployed in treated markets.

The REBP is an interesting empirical setting to study market externalities of UI. First, treated workers received an *extra three years* of covered unemployment with unchanged benefit level. This huge UI extension generated a strong increase in unemployment duration of treated workers thereby manipulating equilibrium labor market conditions [Lalive, 2008]. Second, REBP was enacted only in a subset of regions and for a large subset of workers (90% of workers above 50 years old). Partial eligibility for the program means that we can study ineligible jobs seekers in markets affected by large UI extensions both in REBP regions and outside. 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. Finally, administrative data on the universe of unemployment spells is available in Austria since 1980. By matching these data with data on the universe of employment spells in Austria since 1949 we were able to precisely determine eligibility status for the REBP program along all eligibility dimensions. 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 study whether externalities appear during the program and whether they disappear after the program is repealed.

Our results demonstrate the presence of sizeable market externalities of UI. REBP induced a 2 to 4 weeks *decrease* in the average unemployment duration of all non-eligible workers aged 46 to 54 compared to similar workers from non REBP regions. For non-eligible workers aged 50 to 54, who are competing similar vacancies as treated workers, unemployment duration decreases by 6 to 8 weeks. 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. 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 show 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.

The remainder of the paper is organised as follows. Section 2 presents our theoretical framework, explains where market externalities stem from and how they can be identified within a labor market. Section 3 presents the institutional background of the REBP program. Section 4 presents the data and our identification strategy. In particular, it explains how non-treated groups of workers can be identified to be in the same labor market as treated workers using vacancy data. Section 5 present the results as well as our robustness and heterogeneity analysis. Section 6 draws welfare and policy implications.

2 Market externalities of UI and their identification

The probability that an individual finds a job depends on how hard that individual searches for a job and/or on how selective she is in her acceptance decisions. It also depends on the labor market conditions that determine how easy it is to locate jobs or to be matched to a potential employer. These two forces are usually represented in equilibrium search and matching models by the stylized decomposition: $h_i = e_i \cdot f(\theta)$. h is the hazard rate out of unemployment. e_i captures the search effort / selectiveness component. θ is the ratio of job vacancies to total search effort, and represents the tightness of the labor market. $f(\theta)$ therefore captures the effect of labor market conditions on the job finding probability per unit of effort[4]. If there are no job vacancies created by employers, then $f(\theta) = 0$ and no amount of search effort by an unemployed worker would yield a positive probability of obtaining a job.

Changes in unemployment benefit policies affect the search intensity and selectiveness of unemployed workers. We call this effect the *micro effect* of UI. It can be identified by comparing two individuals with different levels of UI generosity in the same labor market. Changes in unemployment benefit policies also affect labor market conditions and the job finding rate per unit of search effort. We call this second effect *market externalities.* It stems from equilibrium adjustments in labor market tightness θ in response to a change in UI generosity. The overall effect on the job finding rate of a change in UI, the *macro effect* of UI, is therefore the sum of the micro effect and market externalities.

There are at least two reasons why we care about identifying the presence of market exter-

[4]Note that $f, f' > 0, f'' < 0$ characterizes the matching process in a labor market with frictions.

nalities of UI. First, when the generosity of UI varies, for instance due to UI benefit extensions such as the recent EUC program in the US, the total effect on unemployment will be the sum of the micro effect and of market externalities. Studies comparing individuals with different UI benefit within the same labor market will typically identify only the micro effect, and cannot shed light on the true effect of such UI extensions. Second, as shown in Landais et al. [2010], market externalities have first order welfare effects whenever the Hosios condition is not met. The sign and magnitude of market externalities is therefore critical to determine the optimal level of UI.

As explained in Landais et al. [2010], using the framework developed by Michaillat [2012], the sign and magnitude of market externalities depends on two forces: the rat race effect and the wage effect. Appendix A gives a detailed theoretical presentation of the framework, derives the formula for market externalities and the decomposition into the rat race effect and the wage effect.

The rat race effect arises when labor demand is not perfectly elastic and does not fully adjust to variations in search effort of unemployed workers, which will be the case when technology exhibits diminishing returns to labor[5]. Intuitively, in the extreme case where labor demand is perfectly rigid, an increase in an individual's search effort induced by a decrease in UI generosity will increase her probability of finding a job, but this must come at the expense of the probability of all other unemployed to find a job as the total number of jobs is fixed. The rat race effect therefore implies that an increase in UI generosity in the labor market, by decreasing aggregate search effort, should increase the probability of finding a job per unit of search effort $f(\theta)$ and create a positive market externality.

The wage effect arises when wages are correlated with outside options of workers, which will be the case when wages are bargained over. An increase in UI benefits will tend to increase wages, which will decrease the return from opening vacancies for firms leading to a decrease in labor demand and in turn, a decrease in labor market tightness, creating a negative market externality. The overall effect of a change in UI benefits on equilibrium labor market tightness will therefore depend on the relative magnitude of these two effects. When wages do not react to a particular policy, the rat race effect will be the only driver of labor market tightness adjustments to the policy. Studies estimating spillover effects of active labor market or training programs such as Crépon et al. [2013] therefore tend to capture a pure rat race effect as these training programs are unlikely to affect bargained wages.

Identifying market externalities can be arduous, as is usually the case when trying to identify equilibrium effects. Our strategy consists in using two groups of workers that are searching in the same labor market. The first group of workers is "treated" and experiences an exogenous shock on UI benefits while the other group is no treated and does not experience any change in UI benefits. The individual search effort of treated workers will respnd, affecting their job finding probability. This change in search effort will also affect equilibrium labor market tightness and

[5]Diminishing returns is a sufficient but not a necessary condition for the presence of a downward sloping labor demand. Landais et al. [2010] show for instance that an "aggregate demand model" with a quantity equation for money and nominal wage rigidities will feature a downward sloping labor demand even with linear technology.

therefore the job finding probability per unit of search effort, creating labor market externalities. And the change in job finding probability of non-treated workers will capture these market externalities.

In appendix A.2, we show precisely under which conditions the change in job finding probability of non-treated workers can identify market externalities in the labor market. The key identification requirement is that treated and non-treated workers are *in the same labor market*, where a labor market is defined as the market place where workers compete for the exact same vacancies. From a search-theoretic standpoint, this definition is the most natural: it follows the law of one price, each labor market being defined by one labor market tightness in equilibrium. Practically, this means that each labor market is characterized by a vacancy type, and matching between the workers competing for these vacancies and employers posting these vacancies exhibits randomness. A firm opening one such vacancy cannot know whether it will be matched to a treated or to a non-treated worker. When this is the case, we show that variations in the job finding probability of non-treated workers in response to a change of UI for treated workers will identify market externalities of UI. The size of the treated group compared to the non-treated group increases, market externalities on non-treated workers converge to identifying the equilibrium effects of treating the whole market.[6]

In appendix A.3, we also discuss that changes in the job finding probability of non-treated workers will no longer directly identify variations in labor market tightness for the treated labor market if non-treated workers are not be in the same labor market as treated workers. Yet, UI variations for treated workers may nevertheless still create externalities in the form of substitution effects for non-treated workers.

Identification of market externalities of UI extensions within a labor market requires the ability to find two groups of workers with different UI levels within the same labor market, *i.e.* competing for similar vacancies. Using vacancy data, we propose below a simple method to determine whether two groups of workers are competing for similar job vacancies by looking at how characteristics of job vacancies predict the group affiliation of the individual filling the vacancy.

3 Austrian Unemployment Insurance and the REBP

The Unemployment Insurance System The Austrian unemployment insurance system is more restrictive than many other continental European systems and closer to the U.S. system in terms of generosity (Nickell and Layard, 1999).

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

[6]Note that market externalities identified through the change in the job finding probability of non-treated workers will capture the wage effect even if wages are bargained at the individual level. The intuition is that within a labor market, because of random matching, the expected profit of opening vacancies is the weighted average of the profits of opening vacancies for each group of workers. Therefore the increase in bargained wages of treated workers will reduce the expected profit of opening vacancies and will then affect overall vacancy posting in the market.

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. 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.[7] Voluntary quitters and workers laid off for misconduct can receive UB but are subject to a waiting period of 4 weeks. UB recipients need to search actively for a new job within the scope of the claimant's qualifications. After UB payments have been exhausted, job seekers can apply for post-UB transfers ("Notstandshilfe"). These transfers are means-tested and depend on income and wealth of other family members and close relatives. They are granted for successive 39-week periods after which eligibility requirements are recurrently checked and can last for an indefinite time period. Post-UB transfers can be at most 92 % of UB. In 1990, the median post-UB transfer payment was about 70 % of the median UB. The majority of the unemployed (59 %) received UB whereas 26 % received post-UB transfers.

Restructuring of the Austrian steel industry and the REBP After World War II, Austria nationalized large parts of its heavy industries (iron, steel, etc). Firms in the steel sector were part of a large holding company owned by the state, the Oesterreichische Industrie AG, OeIAG. In 1986, after the steel industry was hit by an oil speculation scandal and failure of a US steel-plant project, a new management was appointed and a strict restructuring plan was implemented resulting in plant closures and downsizing.

To mitigate the labor market consequences of the restructuring plan, the Austrian government enacted the Regional Extended Benefit Program (REBP) that extended UB-entitlement to *209 weeks* for specific workers. An unemployed worker became eligible to 209 weeks of UB if he or she satisfied, *at the beginning* of his or her unemployment spell, each of the following criteria: (i) age 50 or older; (ii) a continuous work history (780 employment weeks during the last 25 years prior to the current unemployment spell); (iii) location of residence in one of 28 selected labor market districts 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. Note that REBP did not impose any sector or industry requirement. All unemployed who met criteria (i) to (iv) were eligible irrespective of whether they previously worked in the steel sector or not.

REBP was reformed once in January 1992. This reform 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 1, 1993. Job seekers who established eligibility to REBP before August 1993 continued to be covered. We therefore set the end of the REBP program in August 6, 1997

[7]Before August 1989, potential UB duration was limited to 30 weeks for all workers. From August 1989 onwards the potential UB duration became dependent on age and experience.

(209 weeks after August 1, 1993).

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 and experience. 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. Note that no other insurance program or active labor market policy were put in place in Austria during the REBP period that may be susceptible of confounding the effect of REBP. Lalive and Zweimüller [2004b] provide an extensive discussion of the context and institutional background of REBP and discuss the validity of REBP as a research design.

REBP could also be used as a pathway to early retirement through disability insurance creating complementarities with the REBP program [Inderbitzin et al., 2013]. We focus our analysis on individuals who cannot use REBP or unemployment benefits as a pathway to other programs to mitigate complementarities with other programs.

4 Data and identification strategy

Data The data we use comes from the universe of UI spells in Austria from 1980 to 2009. We focus on all unemployed men with age between 46 and 54 at the start of a spell. 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)[8] from 1949 to 2009, 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 work history in the past 25 years for each individual, in order to determine eligibility status for REBP[9]. 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, location, etc.) for each unemployment spell.

Because of early retirement programs in Austria during our period of analysis, women above

[8]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.

[9]From our understanding, the UI administration used the same source of information on individual experience to determine eligibility to REBP. Yet, we do not observe final eligibility to REBP. Our approach is therefore an intent-to-treat approach. We can nevertheless detect the presence of a few observations with an experience level below the REBP eligibility threshold who still received more than 52 weeks of paid UI. We get rid of these few obviously misclassified observations in our estimation sample.

50 and men above 55 can go directly from REBP or from regular unemployment benefits to early retirement programs. For these workers, 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. Search responses to UI along the intensive margin and exits from the labor markets have potentially 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 below 55 because they cannot go directly from unemployment to early retirement. In our robustness analysis, we show that our results are robust to these sample restrictions, and that externalities can be detected on women, and on all men aged up to 59.

To determine which workers are competing for the same vacancies as REBP eligible workers, we use detailed micro data on job vacancies posted in public employment agencies available for the period 1994-1998[10]. This data has two important features. First, the data records detailed information about the characteristics of the vacancy.[11] Second, the vacancy data contains the personal identifier of the person who was hired for the position. We use the identifier to see whether the successful job seeker was eligible for REBP or not.

Identification in an experimental setting We first discuss identification in an experimental framework and discuss below how we implement it in the actual REBP setting. There are two labor markets, $M = 0, 1$. Labor market $M = 1$ is randomly selected to receive some exogenous treatment, *i.e.* an increase in the potential duration of UI benefits. 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 ($T = 1$) and receives a larger potential duration of UI benefits 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} :

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

Under double randomization (of treated labor markets and of treated individuals within labor markets), the selection term in equation 1 is zero and AE_T^{NT} can be identified by comparing

[10] We also have some crude vacancy data available for the period 1990-1994 that we use to compute initial labor market tightness in appendix table 9. Unfortunately, we were not able to find or construct consistent data throughout the period enabling us to analyze vacancy responses to the REBP.

[11] This includes the firm identifier of the firm posting the vacancy, the date (in month) at which the vacancy is opened and the date at which it is closed, the reason for closing the vacancy, the identifier of the public employment service where the vacancy is posted, the industry and job classifications of the job, details on the duration and type of the contract, the age requirement if any, the education requirement if any, the gender requirement if any, and the posted wage or range of wage if any.

observed outcomes for the non-treated in labor market $M = 1$ to observed outcomes for workers in labor market $M = 0$.

In our case, REBP treatment was not allocated at random, neither across nor within labor markets. Our empirical strategy identifies AE_T^{NT} adopting a difference-in-difference design. This design is valid if unobserved differences between non-treated workers in markets $M = 0$ and $M = 1$ remain fixed over time. We discuss below whether this assumption is plausible and probe it in the context of robustness analyses.

In our context, treated workers ($T = 1$) are all workers who are eligible for REBP, based on the three eligibility criteria, namely age, experience and geography. To implement our diff-in-diff strategy, *(i)* we need to properly define treated labor markets $M = 1$ and *(ii)*, we also need to properly define control labor markets $M = 0$.

Defining treated labor markets There are three dimensions of eligibility to REBP, which means that we can use groups of non-eligible workers based on three characteristics: age, experience and geography. Geography is the most straightforward characteristic to define treated labor markets. In the next subsection, we show that in Austria during our period of analysis the level of geographical integration of labor markets is very limited. Therefore, to define treated labor markets, we focus on non-eligible workers within REBP counties, which means non-eligible workers who both live and had previous employment in REBP counties. Nevertheless, to properly define treated labor markets, we want to focus on non-eligible workers within REBP counties who compete for the same job vacancies as treated workers. If treated and non-treated workers are competing for similar vacancies, the effect of the REBP on non-treated workers can identify equilibrium variations in labor market tightness in the labor market. If treated and non-treated workers are competing for different vacancies, there are in practice two search markets for labor, and the effect of the program on non-treated workers identify market externalities due to substitution effects.

To determine which groups of workers within REBP counties are competing for the same vacancies as REBP eligible workers, we propose a method based on detailed micro data on job vacancies. The vacancy data records for each vacancy detailed information about the characteristics of the vacancy and the personal identifier of the person who filled the vacancy. Our strategy consists in using all the information that we have on each vacancy, and estimate how well the characteristics of each vacancy predicts the REBP eligibility status of the worker who fills the vacancy. All the details about our data and strategy are given in appendix B.

To implement this strategy, we regress the probability that the worker filling a given vacancy is eligible to REBP on a vector of all the characteristics of the vacancy and run the model separately for various categories of non-eligible workers against eligible workers. For each of the categories of non-eligible workers, we then analyze the predictive power of the model using various goodness-of-fit measures.

In figure 2 panel A, we plot the fraction of observations that are incorrectly predicted by the model for all categories of non-eligible workers. The fraction of misclassified observations is less than 7.5% for the model comparing eligible workers to non-eligible workers aged 30 to 40, but increases up to more than 25% for the model comparing eligible workers to non-eligible workers aged 50 to to 54. We also plot in the same figure the p-value from the Pearson's χ^2 test for categories of non-eligible workers[12] which also confirms that the model fits the data very well for comparing eligible workers to non-eligible workers aged 30 to 40, but tends to perform more and more poorly as we use non-eligible workers that are older. When comparing eligible workers to non-eligible workers aged 50 to 54, the p-value is very close to zero, and the goodness-of-fit of the model is extremely poor.

In panel B of figure 2, we plot the fraction of type I errors, *i.e.* the fraction of true non-eligible workers that are predicted as being eligible to REBP by the model. Type I errors are particularly relevant in our context. They provide information about how likely it is that a non-eligible worker is competing for a vacancy that has been "tailored" to eligible workers based on its characteristics. In this sense, type I errors provide direct information about the intensity of the competition that eligible workers receive from various groups of non-eligible workers when a vacancy is opened in "their" search market. The figure indicates that type I errors seem to be particularly severe when comparing eligible workers to non-eligible workers aged 50 to 54. Because classification is sensitive to the relative sizes of each component group, and always favors classification into the larger group, we also investigate the fraction of type I errors relative to a perfectly random matching and plot the fraction of type I errors of the original model compared to the fraction of type I errors in the perfectly random matching case. This gives us a sense of how close to perfectly random the matching is between eligible workers and the different groups of non-eligible, controlling for the different relative sizes of these groups. Again, we find that the matching process is almost not random at all between eligible workers and workers aged 30 to 40, while it is 50% random between between eligible workers and non-eligible workers aged 50 to 54.

These results help inform our identification strategy and choose the proper groups of non-eligible workers to identify the presence of externalities. Workers aged 30 to 40 seem to fill vacancies that have characteristics that are very different from the vacancies filled by eligible workers. But eligible and non-eligible workers above 50 seem to fill vacancies that have very similar characteristics. This suggests that workers aged 30 to 40 are likely to be in a different job search market than eligible workers. But as we focus on older workers, they seem to be more and more competing for the same vacancies as eligible workers. For non-eligible workers aged 50 to 54, this competition seems the most intense. As a consequence, in our baseline sample, we focus attention to workers with age between 46 and 54 at the start of a spell.

Defining control labor markets To define control labor markets, we exploit primarily the geographical dimension of REBP and use workers of non-REBP counties who have similar characteristics as workers in our treated labor markets. This approach will only be valid if labor markets in non-REBP counties are not too integrated to labor markets in REBP counties. Otherwise, workers in non-REBP counties might also be subject to treatment externalities, which

[12] This test is a standard goodness-of-fit test for logistic regressions. A low p-value for the test indicates a poor fit of the data.

would bias towards zero the externalities estimated from comparing non-eligible workers in REBP and non-REBP counties.

To get a sense of how geographically integrated the labor markets of REBP and non-REBP counties are, we compute[13] the fraction of new hires in non-REBP counties who come from REBP counties. In figure 1 panel A, we map the average quarterly fraction of men aged 46 to 54 coming from REBP counties in the total number of new hires of men aged 46 to 54 in non-REBP regions for all the years when the REBP was not in place (1980-1988 and 1998-2009). 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 located 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 sample, we remove the few counties with more than 5% of new hires coming from REBP regions. In our robustness analysis, we use these counties to show that we can also detect the presence of geographical externalities in these counties highly integrated to REBP regions.

In figure 1 panel B, we map the average quarterly fraction of men aged 46 to 54 coming from non-REBP regions in the total number of new hires of men aged 46 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. 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 that we use in section 5.

Identifying assumption Our strategy relies on comparing before, during and after REBP, workers in REBP counties who are non-eligible to REBP either because of their age or experience level at the start of their unemployment spell to similar workers in non-REBP counties. This diff-in-diff strategy relies on a parallel trend assumption for non-eligible workers in REBP and non-REBP counties.

The main concern with regard to our parallel trend assumption is the presence of region-specific shocks in REBP vs non-REBP counties contemporaneous to the REBP program. 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. To address this issue, we start our analysis on a sample restricted to non-steel workers only, which means workers who are never observed working in the steel sector, either before, during or after REBP. 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

[13] 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.

shocks and to explore the sensitivity of our results to this sample restriction.

Descriptive statistics Table 1 gives descriptive statistics of our baseline estimation sample for the REBP and non-REBP periods. In panel A, we compare REBP and non-REBP counties and begin by showing simple labor 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 15% versus 5% in non-REBP counties. To control for the potential endogeneity bias in the choice of REBP counties, we completely remove the steel sector from our analysis[14]. There is not difference in the monthly 46 to 54 unemployment rate in non-REBP periods. We also report descriptive statistics for the REBP period. Unemployment increases substantially in REBP counties, from 5.4 % to 11.3 %, and it increases substantially less in non-REBP counties, from 5.5 % to 7.3 %.

In the remainder of table 1 panel A, we show descriptive statistics on our estimation sample of unemployed men, aged 46 to 54, who never work in the steel sector. In our sample, the fraction of unemployed eligible to REBP (above 50 years old or with more than 15 years of continuous work history in the past 25 years) is between 40 and 50%. REBP and non-REBP counties are extremely similar for all non-REBP years in terms of labor market outcomes: the duration of unemployment spells and the duration of non-employment[15] spells were roughly the same for unemployed in REBP and non-REBP counties. Finally gross unconditional wages were slightly higher in REBP counties. In the REBP period, both unemployment and non-employment durations increase substantially in REBP counties. For instance, non-employment duration more than doubles from 21 weeks to 45 weeks in REBP counties, but only from 23 weeks to 33 weeks in non-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 46 to 54, who never work in the steel sector. Eligible unemployed are defined as unemployed aged above 50 at the start of their spell or 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 were below 50 at the start of their spell or who have worked less than 15 years out of the previous 25 years. Eligible workers are therefore slightly older in our sample, but have similar job search outcomes. Non-eligible unemployed have a slightly lower duration of unemployment during the non-REBP period. 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 education or marital status. During REBP, both unemployment and non-employment durations increase tremendously for eligible job seekers and less so for non-eligible unemployed. For instance, eligible job seekers searched for a job during 25 weeks in non-REBP periods, but exactly four times longer, during

[14] We show in online appendix table 7 that our results are not sensitive to this sample restriction.

[15] 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.

100 weeks, during REBP. Ineligible job seekers also searched for jobs longer, 31 weeks, during REBP than before or after (22 weeks). The increase in job search durations for eligible job seekers is due, in part, to REBP. The increase in ineligible job search durations is due to the recession that hit most Continenantal Eruopean countries in the early 1990s, possibly masking the market externality of the UI extenion due to REBP.

5 Empirical evidence of market externalities

Graphical evidence 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 between REBP and non-REBP counties for eligible and non-eligible workers. More specifically, for each group of workers (eligible workers in panel A, all non-eligible workers in panel B, and non-eligible workers aged 50 to 54 in panel C), we run the following regression:

$$y_{it} = \sum \beta_t \mathbb{1}[T = t] + \sum d_t(\mathbb{1}[T = t] \cdot \mathbb{1}[M = 1]) + X'\gamma + \varepsilon_{it} \tag{2}$$

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 REBP and non-REBP regions. In all 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 above 50 years old and with more than 15 years of continuous work history, and therefore eligible for REBP extensions. Figure 3 shows that the introduction of REBP induced a large reduction in labor supply of eligible workers in treated regions, which translates into a large 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 labor supply, since this is the moment where the stock of REBP eligible unemployed is the highest, and their labor supply is the lowest.

Panel B plots the difference across REBP and non-REBP regions for all non-eligible workers aged 46 to 54 (below 50 years old or 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 in 1993, when the effect of REBP on aggregate labor supply of eligible workers is at its peak. The difference then reverts back to zero as the REBP program is scaled down.

Panel C plots the difference across REBP and non-REBP regions focusing on non-eligible workers aged 50 to 54 (with less than 15 years of continuous work history in the past 25 years). The exact same pattern is visible, and even more pronounced. While they experience similar unemployment durations prior to REBP, non-eligible workers above 50 experience much shorter unemployment spells during the REBP period in REBP regions compared to similar non-eligible workers in non-REBP regions, and the effect culminates in 1993. The difference then quickly reverts back to zero as the REBP program is rolled back.

Figure 4 shows the relationship between age and unemployment durations for all non-eligible workers in REBP and non-REBP counties when REBP was not in place (panel A), and the peak period when REBP was in action (January 1992 to December 1995, panel B). The figure presents the average duration of unemployment in bins of age at the start of unemployment where the bin size is two months of age. In REBP counties, to make the distinction more visible between non-eligible workers due to age (below 50) and due to work experience only (age 50 to 54), we plot them in different marker shapes. We also fit the data with a third-order polynomial for REBP and non-REBP counties.

Panel A shows that during the non-REBP period, the relationship between age and unemployment duration is almost flat and extremely similar for non-eligible workers in REBP and non-REBP regions. Panel B shows that non-eligible workers experienced shorter unemployment spells in REBP regions compared to non-REBP regions. Interestingly, this difference in unemployment duration between REBP and non-REBP counties is sharply increasing with age: unemployed individuals below 45 in REBP regions do not fare very differently from similar unemployed in non-REBP regions during the REBP period, but unemployed individuals above 50 in REBP counties experienced much shorter spells than similar unemployed in non-REBP counties.

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

$$\begin{aligned} Y_{it} = \quad & \alpha + \overbrace{\beta_0 \cdot \mathbb{H} \cdot M \cdot \tilde{T}_t}^{\text{Effect of REBP on eligible}} + \overbrace{\gamma_0 \cdot (1 - \mathbb{H}) \cdot M \cdot T_t}^{\text{Effect of REBP on non-eligible}} + \eta_0 \cdot M + \sum \nu_t \\ & + \eta_1 \cdot \mathbb{H} + \eta_2 \cdot M \cdot \mathbb{H} + \sum \iota_t \cdot \mathbb{H} + X'_{it}\rho + \varepsilon_{it} \end{aligned} \tag{3}$$

where Y_{it} are different search outcomes of interest, M is an indicator for residing in a REBP county[16], 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. $\mathbb{H}$ is an indicator of eligibility to REBP and is equal to one for unemployed individuals above 50 years old and 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 eligible workers, while γ_0 identifies spillovers of REBP on non-eligible workers in REBP regions. $\sum \nu_t$ is a series of year fixed effects. Because we control

[16] We remove the 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 eligibility fixed effects ($\mathbb{H}$) interacted with both REBP counties fixed effects (M) and year fixed effects, specification (3) amounts to pooling two diff-in-diff together, one for the effect of REBP on eligible unemployed workers and one for the effect of REBP on non-eligible unemployed workers.

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, citizenship and tenure in previous job. In column (3) to (6) we also add controls for preexisting trends by region. Panel A displays estimates of β_0, the diff-in-diff estimate of the effect of REBP on eligible workers. Results confirm that REBP increased unemployment duration by roughly 45 weeks for eligible unemployed compared to similar unemployed workers in non-REBP counties. In column (4), we estimate the same model using as an outcome the duration of total non-employment (conditional 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 (+29 weeks), which suggests that some eligible workers did exhaust their unemployment benefits and never got back to work. Columns (5) and (6) focus on the probability of having a spell longer than 100 and 26 weeks respectively, and confirm that REBP shifted the whole survival function of unemployed eligible to REBP.

Panel B displays estimates of γ_0, the effect of REBP on all non-eligible workers aged 46 to 54 in REBP counties. Results confirm that non-eligible workers in REBP counties experienced a significant decrease in their unemployment duration of 2 to 4 weeks compared to similar workers in non-REBP counties. Column (4) shows that the effect is of similar magnitude on the duration of total non-employment which means that the positive effect of REBP on non-eligible workers is truly about finding a job faster. Columns (5) and (6) show that the reduction in unemployment durations for non-eligible unemployed is due to a significant reduction in both short and long unemployment spells.

Section 4 has shown that we should expect heterogeneity in the magnitude of externalities across different groups of non-eligible workers. In particular, non-eligible workers above 50 seem the most likely to compete for the same vacancies as workers eligible to REBP and therefore more likely to experience larger externalities. To investigate heterogeneity in market externalities, we split the results between non-eligible workers based on age and non-eligible workers based on the work history requirement. In panel C, we focus on the effect of REBP for non-eligible workers age 46 to 49 who are non-eligible based on age. Results show that REBP significantly reduced the duration of unemployment and of total non-employment of non-eligible workers aged 46 to 49 by 2 to 3 weeks. Panel D shows the effect of REBP for non-eligible workers aged 50 or above who are non-eligible based on the experience requirement. Results confirm our earlier graphical evidence showing that market externalities for this group of non-eligible workers are larger. REBP significantly reduced the duration of unemployment and of total non-employment of non-eligible workers above 50 by 6 to 9 weeks.

Standard errors To correct for the presence of common random effects, we cluster standard errors at the region-year level. We have checked sensitivity of inference in three ways. First, we allow for clustering by regions defined by county-by-industry-by-education cells (see appendix

C, table 6). Results indicate that standard errors are robust to clustering by region. Second, clustering by region is fully flexible in terms of clustering in time but assumes no correlation across regions. Conley [1999] proposes a more flexible approach to inference that allows for arbitrary tempo-spatial dependence in shocks within a distance and an autocorrelation cutoff, so-called spatial HAC standard errors. We report results that use a distance cutoff of 33 km – the median commuting distance for job seekers in Austria – and an autocorrelation cutoff of two quarters. Spatial HAC standard errors are similar to our baseline standard errors. Third, both clustering on region and spatial HAC standard errors rely on assumptions regarding the tempo-spatial dependence of standard errors. Permutation is a way to assess sensitivity to these assumptions. Permutation works as follows: we first construct a set of 235 placebo REBP estimates on non-REBP periods and then conduct inference using the distribution of placebo REBP effects. Permutation based standard errors for the market externality are somewhat larger than baseline standard errors, and substantially smaller for the effect of REBP on the eligible. But our inference remains robust to adopting this permutation procedure.[17]

Robustness In appendix table 7, we start by exploring the sensitivity of our results to our sample restrictions. In our baseline sample, we have excluded workers above 54 and women to minimize the concern that male workers between 55 and 59 and female workers can use REBP as a direct pathway to retirement. In panel A, we run specification 3 on a sample including all men up to 59. In panel B, we also include women in the estimation sample. In both panels, estimates are extremely similar to our baseline results, with significant externalities on unemployment durations of non-eligible workers of 2 to 3.5 weeks. In panel C, we also include steel sector workers[18] in the estimation sample, which had been excluded from the baseline sample to alleviate the concern of non-parallel trends between REBP and non-REBP counties. Estimated externalities on non-eligible workers are again very similar to our baseline results. Given that steel sector workers represent a relatively small fraction of treated labor markets in REBP counties, these results are not very surprising.

The second potential concern with regard to our results is that unobserved characteristics correlated with job search outcomes might change during the REBP period for non-eligible workers. Such a change in unobserved characteristics of non-eligible workers would lead to a violation of our parallel trend assumption and bias our estimates of the market externalities of REBP on 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 for all male workers age 46 to 54, broken down by REBP eligibility status. Results are reported in column (1) of table 3. The REBP program has had a large positive effect on the log separation

[17] Kline and Moretti [2014] have adopted the spatial HAC approach in their analysis of the Tennesess Valley Authority. Chetty et al. [2014] use permutation to study sensitivity of inference in active savings decisions in a regression discontinuity design. Lalive et al. [2013] use permutation to test sensitivity of disabled employment to financial incentives in a threshold design.

[18] Steel sector workers are defined as workers who ever had employment in the steel sector between 1980 and 2009 [TBC!!!].

rate of eligible workers in REBP regions[19] but has not affected the log separation rate of non-eligible workers in REBP regions. In the remainder of table 3, 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 3 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 the logarithm of tenure in the previous job (prior to becoming unemployed). Again, we find almost no effect for non-eligible workers, and a small positive effect for eligible workers. Overall, these findings alleviate the concern of an important change in unobserved characteristics of non-eligible workers in REBP regions at the time of the REBP program.

The third concern with our baseline estimates is the possible presence of differential region-specific shocks at the time the REBP program was in place. This concern is valid given that REBP counties were not chosen at random but because of the relative importance of their steel sector. Yet note that the fraction of steel sector employees never exceeds 15% of the labor force in these counties, and we restrict our baseline sample to individuals who never were employed in the steel sector. Also, because REBP counties were experiencing a restructuring of the steel sector, we should expect the region-specific shock to be negative during the REBP period for REBP counties, which would lead to higher unemployment durations for non-eligible workers. This concern thends to attenuate our estimates of the search externalities for non-eligible workers.

To further investigate the robustness of our results to the presence of region-specific shocks, we use men aged below 40 in REBP counties as a control, instead of workers from non-REBP counties. To do so, 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 (3) 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. Results are reported in appendix table 8. Estimated externalities on non-eligible unemployed aged 50 to 54 are virtually unaffected compared to table 2 panel D. This suggests that our estimated externalities are not driven by labor market shocks specific to REBP counties and contemporaneous to the REBP period.

Treatment intensity The magnitude of market externalities depends on treatment intensity, *i.e.* the relative size of the treated group of eligible unemployed compared to the non-treated group of non-eligible workers (appendix A.2). To investigate how estimated externalities vary

[19] We discuss in online appendix section A.4 the theoretical consequences of this increase in the separation rate of eligible workers. When layoffs are endogenous to UI, an increase in the separation rate of eligible workers is equivalent to a downward shift in labor supply, and is therefore analogous to a decrease in search effort. But an increase in the separation rate may also decrease labor demand by decreasing the net return from opening vacancies. The relative magnitude of these two effects will therefore determine if endogenous layoffs deepens or attenuates the effect of UI on equilibrium labor market tightness and therefore the magnitude of market externalities.

with treatment intensity, we look at different measures of treatment intensity and interact these measures with the effect of REBP on non-eligible workers. The estimated specification is

$$\begin{aligned} Y_{it} = \quad & \alpha + \beta_0 \cdot \mathbb{H} \cdot M \cdot \tilde{T}_t + (\gamma_0^H \cdot \mathbb{1}[\text{Treat.=High}] + \gamma_0^L \cdot \mathbb{1}[\text{Treat.=Low}]) \cdot (1 - \mathbb{H}) \cdot M \cdot T_t \\ & + \eta_0 \cdot M + \sum \nu_t + \eta_1 \cdot \mathbb{H} + \eta_2 \cdot M \cdot \mathbb{H} + \sum \iota_t \cdot \mathbb{H} + X_{it}'\rho + \varepsilon_{it} \quad (4) \end{aligned}$$

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. In the first method, we start by computing the average quarterly fraction of new hires coming from non-REBP counties among all new hires of men aged 46 to 54 for each REBP county *when the REBP was not in place* as shown in figure 1 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% which corresponds to the median value across REBP counties. Table 4 panel A displays the 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. REBP induced a reduction in non-employment durations of non-eligible workers of only .7 weeks in low treatment counties but of 4.2 weeks in high treatment counties. When zooming on non-eligible workers aged 50 and above, this pattern is even more striking, with a reduction of 4 weeks of unemployment durations for low treatment counties and of more than 10 weeks for high treatment counties.

We confirm the robustness of these results using a second measure of treatment intensity. To do so, we compute the average yearly fraction of eligible workers among the 50+ for each region×industry×education cell[20] during REBP years and we define high treatment intensity as being in a cell where more than 90% of the 50+ unemployed were eligible, which is the median value across all region×industry×education cells. Results are displayed in table 4 panel B and confirm the pattern found using our first measure of treatment intensity. In low treatment intensity cells, the estimated externalities of REBP on non-eligible workers are approximately two times smaller than in high treatment intensity cells, and this pattern is valid for all non-eligible workers, as well as for non-eligible workers above 50.

Landais et al. [2010] show that in the presence of "job rationing", externalities should be larger when initial labor market tightness is low as job rationing will be more intense, exacerbating the rat race effect. In appendix table 9 we therefore also explore heterogeneity in estimated externalities with respect to the initial level of labor market tightness. Unfortunately, the first year for which we have some vacancy information by county is 1990 and we cannot compute labor market tightness prior to REBP. We compute initial labor market tightness as of 1990 by dividing the average monthly number of vacancies posted in 1990 in each county×industry×education

[20] A region is defined as the first two digits of the municipality identifiers.

cell, by the average monthly number of unemployed in the same county×industry×education cell. And we define low tightness cells as county×industry×education cells where initial tightness is below the median of initial tightness across all cells. Results, displayed in table 9, suggest that non-eligible workers in low tightness cells experienced significantly shorter unemployment spells due to REBP than non-eligible workers in high initial tightness cells. When focusing on non-eligible workers above 50, we also find strong suggestive evidence that REBP externalities were significantly stronger in labor markets with low tightness at the start of REBP.

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. These counties are likely to experience spillover effects from REBP counties and 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 residing in non-REBP counties with high integration to REBP counties to unemployed workers residing in non-REBP counties with low level of integration.[21] We restrict our sample to male unemployed workers aged 50 to 54 with more than 15 years of experience, who would be eligible to REBP if residing in REBP counties. Results are reported in panel A of table 5 and suggest that REBP reduced the duration of unemployment spells 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.

In panel B of table 5, 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%. Our estimates show that REBP significantly improved job search outcomes for unemployed workers in cells where competition with REBP workers was the strongest: unemployed in these cells experienced a decline of two and half to five weeks in the duration of their unemployment spells relative to similar workers residing in non-REBP counties in cells with low competition from REBP workers.

Wages The sign and magnitude of the market externalities of REBP that we estimated suggest that wages did not react much to outside options of eligible workers. Higher wages would have triggered a decrease in the number of job vacancies opened by firms and would have muted or even reversed the externalities on non-eligible workers. Here, we investigate explicitly this question by looking at the effect of REBP on reemployment wages of eligible workers.

Analyzing the effect of REBP on wages is very different from our previous market externality analysis, as we now wish to compare eligible workers to non-eligible workers. Identification of the effect of REBP on wages is difficult for at least three reasons. First, REBP prolongs unemployment duration, which may directly affect wages through duration dependence effects.

[21]High integration to REBP counties is defined as having an average quarterly fraction of new hires coming from REBP regions in the total number of new hires above 15% for all non-REBP periods.

Second, REBP treatment affects the probability of entering into unemployment and REBP recipients may therefore be selected along unobserved characteristics that are correlated with wages. Treatment is also correlated with the probability of ever reentering the labor force, which creates additional selection issues. Finally, REBP affects labor market tightness, which will in turn affect the bargaining power of workers.

Given these difficulties, our analysis remains tentative and most of the details and caveats are discussed more extensively in appendix section D. We start by comparing eligible workers in REBP counties and non-REBP counties. Because eligible workers in REBP counties experienced longer unemployment durations during REBP than eligible workers in non-REBP counties, reemployment wages of eligible workers in REBP and non-REBP counties may simply differ because of variations in the distribution of wage offers over the duration of a spell. To control for this issue, we follow the methodology of Schmieder et al. [2012a] and estimate the effect of variations in benefits on reemployment wages *conditional on unemployment duration*. Identification is based here on the assumption that there is no correlation between unobserved heterogeneity and unemployment benefits *conditional on unemployment duration*.

We plot in appendix figure 6 post-unemployment wages conditional on the duration of the unemployment spell in REBP and non-REBP counties for eligible 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 effect of REBP on reemployment wages conditional on spell duration. This evidence suggests that there was no significant effect of REBP on reemployment wages.

We formally assess this result in appendix table 10 by running a simple diff-in-diff model where we compare workers eligible to REBP (treatment) to non-eligible workers (control). Each panel uses a different control group. In panel A, we use workers aged 50 to 54 with more than 15 years of experience but residing in non-REBP regions. In panel B we use workers aged 50 to 54 residing in REBP regions but with less than 15 years of experience. In panel C we use workers aged 46 to 49 with 15 years of experience and residing in REBP regions. In our preferred specification of column (4), we condition on the duration of unemployment using a rich set of dummies for the duration of unemployment prior to finding a new job. Irrespective of the control group we are using, we always find no significant effect of REBP on reemployment wages.

To complement our diff-in-diff approach, we also 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.

$$E[Y|A=a] = \sum_{p=0}^{\bar{p}} \gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[A \geq k] + \sum_{t=0}^{T} \mathbb{1}[D=t] \tag{5}$$

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. We use a third-order polynomial specification. Results are displayed in appendix 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 .14 (.04). This discontinuity then decreases and disappears when REBP is over. This suggests that wages are relatively rigid in the short run, but that in the longer run, wages might adjust to variations in outside options of workers. Note however that the McCrary test rejects continuity in the probability density function of the assignment variable (age) at the cutoff (50 years) during REBP. This implies that the wage effects could also partly be driven by selection (sorting) at the 50 years age cut-off.[22]

Overall, this evidence, although tentative, suggests that wages of eligible workers did not strongly respond to REBP, which is in line with the market externalities that we find. Yet, we cannot exclude that these results are confounded by selection, nor can we exclude that wages would have adjusted in the very long run.

6 Discussion and policy implications

Micro vs macro effects of UI extensions Our empirical findings carry important implications. The overall effect of a change in UI on the job finding rate, the macro effect of UI, is the sum of the micro effect and of market externalities. The presence of significant market externalities implies that the micro and the macro effect of UI extensions are not the same. Estimates of the effects of UI benefits on search effort using variations in UI across individuals within a labor market capture micro effects of UI and do not provide enough information to assess the full welfare implications of variations in UI benefits.

Importantly, our analysis also offers direct insights on the relative magnitude of micro and macro effects of variations in benefits in a labor market. We are interested in recovering the wedge between micro and macro effects when changing UI for the whole labor market. This wedge is $W = 1 - e^M/e^m$ where e^M is the total effect on job finding rate of treating the whole market by an increase dB in UI benefits ("macro effect") and e^m is the "micro effect". This wedge can be recovered from our two groups quasi-experimental setting (appendix A.2):

$$W = \frac{\frac{1}{p}\frac{dD_b}{dB_a}}{\frac{dD_a}{dB_a} - \frac{dD_b}{dB_a}} \tag{6}$$

The numerator $\frac{dD_b}{dB_a}$ is the effect of the REBP increase in UI, dB_a, for eligible workers on the duration of unemployment of non-eligible workers, D_b, and captures market externalities of REBP. Intuitively, the effect of REBP on non-treated workers will create externalities that are

[22] We finally exploit the experience eligibility discontinuity in REBP counties using the same methodology. Results are displayed in appendix figure 9. Here, we find no evidence of an effect of REBP on reemployment wages.

smaller than if the whole market was treated one needs to rescale estimated externalities in our experiment by $1/p$ where p is the fraction of eligible workers in the market. The denominator is the micro effect of REBP. It is equal to the total effect of REBP on the spell duration of eligible workers $\frac{dD_a}{dB_a}$ minus externalities of REBP identified by $\frac{dD_b}{dB_a}$.

We can now calibrate the wedge W of equation (6) for the labor market of eligible 50 to 54 in REBP regions. To calibrate the numerator $\frac{dD_b}{dB_a}$, we use the externalities estimate γ_0 of table 2 column (4) for non-eligible workers aged 50 to 54: $\gamma_0 = -6.91$. These non-eligible workers are the most likely to be competing in the same labor market as eligible workers and of capturing the full extent of externalities in this labor market. For $\frac{dD_a}{dB_a}$, we use the estimate of the full effect of REBP on eligible workers β_0 from table 2 column (4): $\beta_0 = -29.17$. For p, we use the average fraction of eligible workers among 50-54 workers in REBP regions prior to REBP $\approx .9$. This gives us a wedge of $W \approx .21$.

To what extent is this wedge informative about the micro and macro effects of treating all labor markets by having a country-wide or region-wide unemployment insurance extension? To answer this question, it is important to realize that, compared to a setting where all labor markets would be treated, in the REBP setting there exists untreated labor markets (for workers aged below 50 for instance) offering substitution opportunities to treated workers. We explain in appendix section A.3 the consequences of the existence of substitution possibilities across markets on the magnitude of market externalities of UI. The intuition is that when the treated labor market is small, and the elasticity of substitution with workers from other markets is large, then the treated market is like a small open economy: its labor market tightness is close to infinitely elastic and set by the labor market tightness of substitution markets. Labor market tightness in the treated market will therefore not react strongly to variations in UI for workers in that market and market externalities of UI will be small. In other words, the more substitutes are available for firms, the smaller the market externalities of UI in the treated market. This suggests that the wedge between the micro and macro effects of country-wide or region-wide UI extensions could be greater than the wedge we found in the REBP context for the treated market of male workers aged 50 to 54.

Implications for welfare effects of UI extensions Our results bear important implications for optimal UI policies. As explained in Landais et al. [2010], in equilibrium search and matching models, the traditional partial equilibrium Baily-Chetty formula for the optimal level of benefits (Chetty [2006]) 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 will 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.

Given that we find a positive wedge between the micro and the macro effects of UI, this implies that more generous UI increases labor market tightness. As a consequence, the optimal level of UI will be larger than suggested by the partial equilibrium Baily-Chetty formula. UI extensions are less distortionary than based on estimation of micro estimates of the effects of UI.

Our results in appendix table 9 further suggest that market externalities are larger when initial labor market tightness is low. This would imply that the wedge between micro and macro effects is likely to be larger during recessions (low tightness) than during booms (high tightness). This would therefore offer a natural justification for countercyclical extensions of UI on efficiency grounds, as hypothesized in Landais et al. [2010].

Market 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 more likely to be decreasing (capital not being able to adjust as quickly as labor). Second, because of various frictions in the wage-setting process, 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, positive REBP externalities are still detectable on non-eligible workers. Because the REBP program was only temporary, we cannot properly estimate the speed at which externalities may decrease over time. In the long run, however, it is likely that these externalities could have decreased because wages could have increased and labor demand could have become more elastic to labor market tightness. Eventually, it is even possible that externalities change sign in the long run, so that the macro effect of UI variations becomes larger than the micro effect.

In terms of policy implications, this means that temporary extensions enacted in reaction to business cycles downturns are 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.

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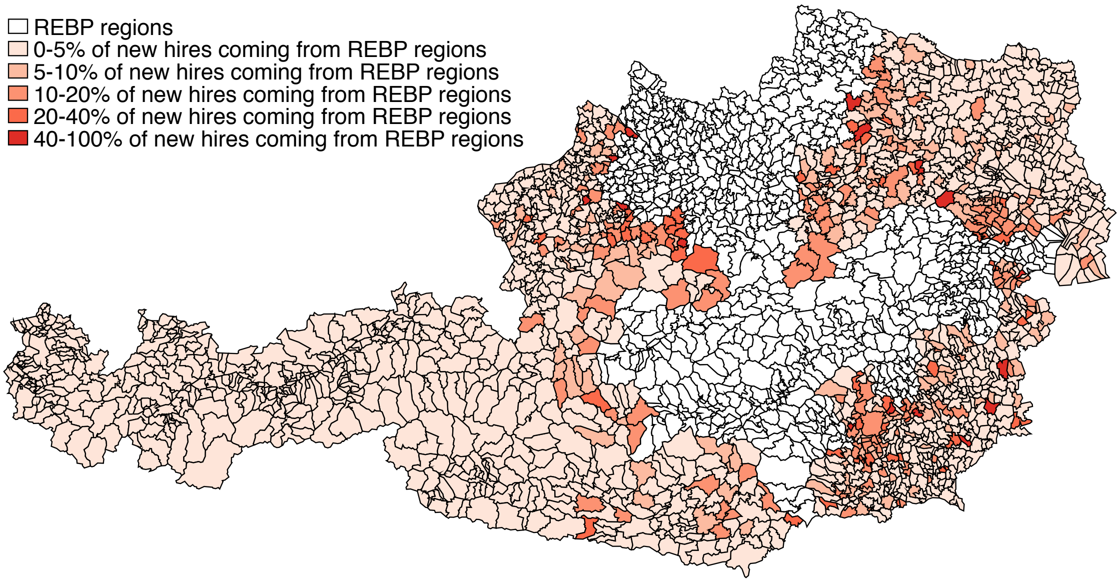
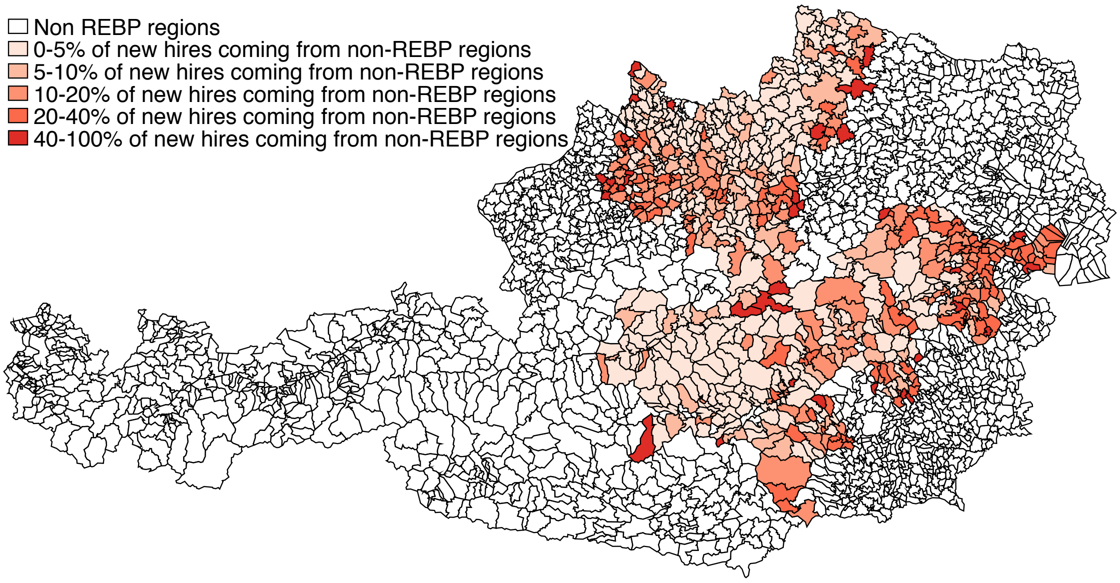
Figure 1: Regional distribution of REBP and local labor market integration during non-REBP years (1980-1988 and 1998-2009)

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



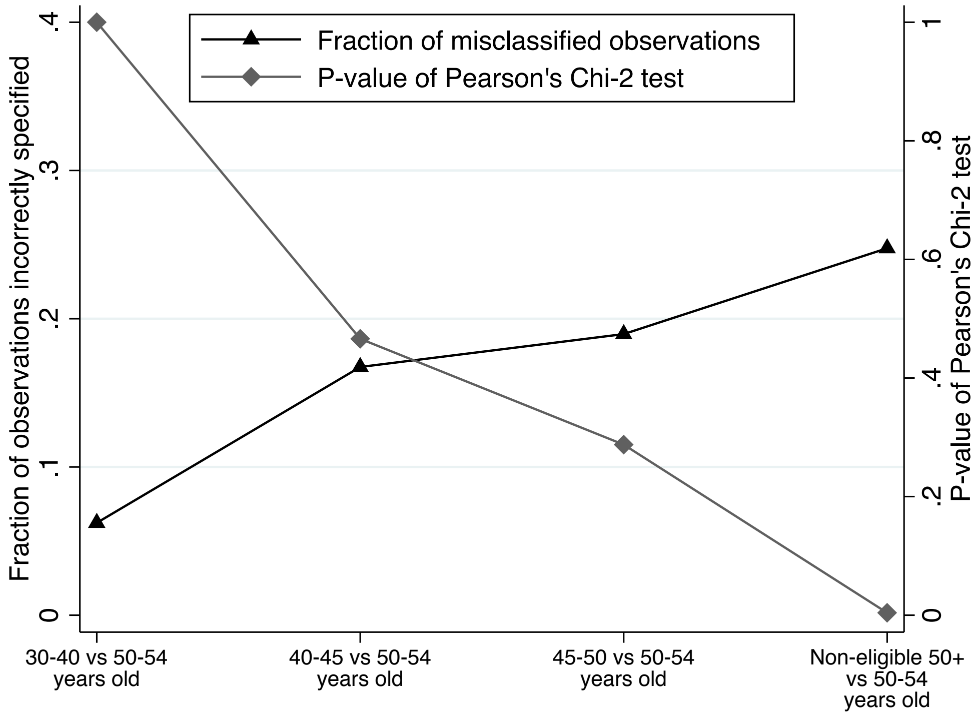
B. Fraction of new hires from non-REBP regions in total number of new hires by county



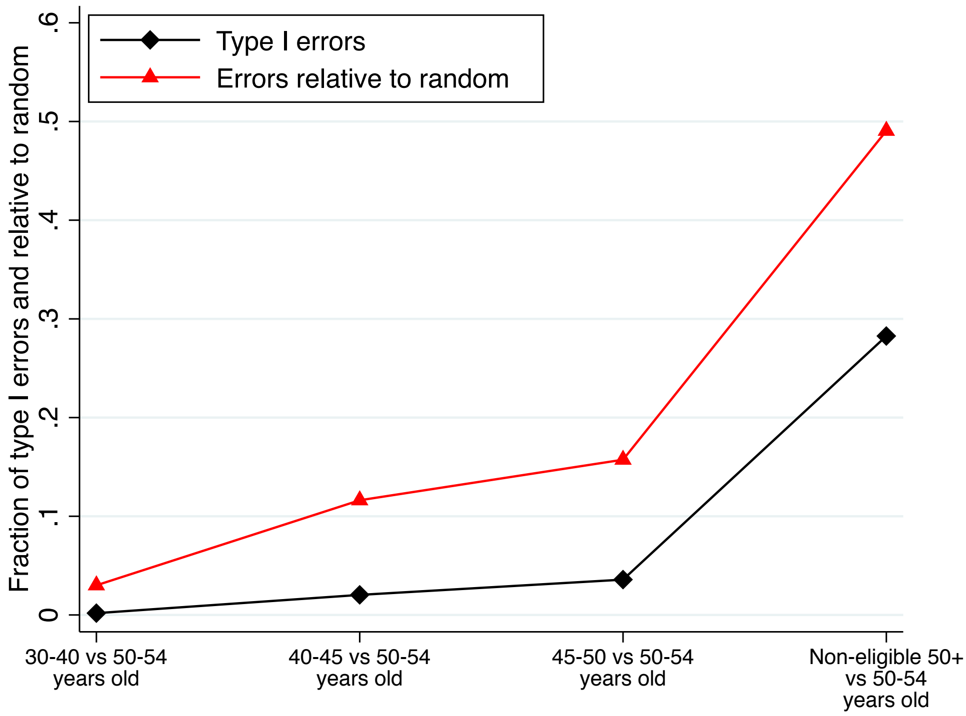
Notes: the figure shows the distribution of REBP across the 2361 communities (counties) in Austria. The treated regions (REBP regions) are all counties with red shading in panel B and include parts of the provinces of Burgenland, Carinthia (Kärnten), Lower Austria (Niederösterreich), Upper Austria (Oberösterreich), 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 46 to 54 coming from REBP regions in the total number of new hires of men aged 46 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 46 to 54 coming from non-REBP regions in the total number of new hires of men aged 46 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 2: Evaluating the degree of competition for identical vacancies between REBP eligible workers and different groups of non-eligible workers:

A. Fraction of misclassified observations & p-value of Pearson's goodness-of-fit test

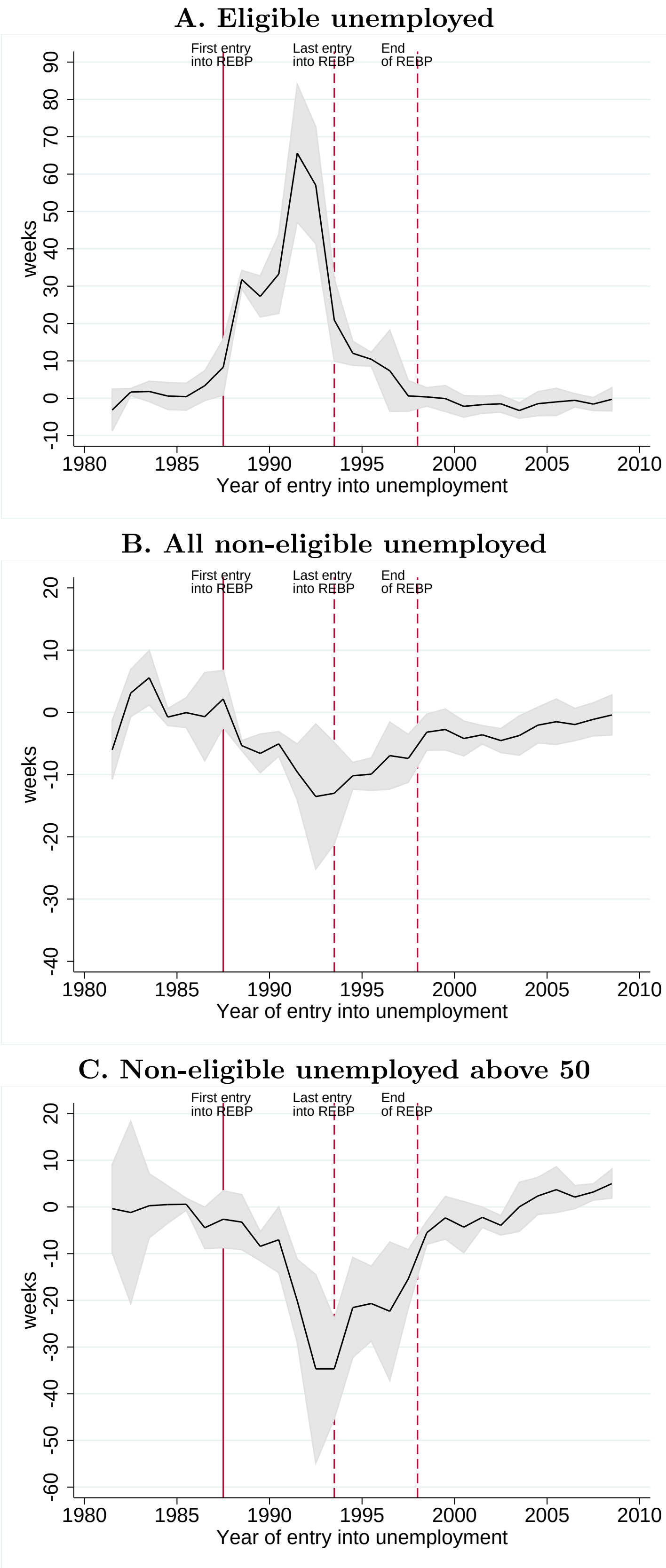


B. Fraction of type I errors compared to random matches



Notes:This figure reports various goodness-of-fit measures of a logit model where the REBP-eligibility status of the worker filling a vacancy is explained by all the characteristics of the vacancy. We estimate this model separately for different groups of non-eligible workers against eligible workers. A good fit of the model indicates that non-eligible workers fill vacancies that are very different from the vacancies filled by eligible workers. A poor goodness-of-fit indicates that eligible and non-eligible workers fill vacancies that have very similar characteristics. In panel A, we plot the fraction of observations that are misclassified by our model (the predicted status is different from the true status of the worker filling the vacancy). We also plot the p-value of the Pearson's χ^2 goodness of fit test. A low p-value indicates poor fit and low predictive value of the model. In panel B, we plot the fraction of type I errors of the model. Because classification is sensitive to the relative sizes of each group of workers, we also plot the fraction of type I errors relative to a sample of perfectly random matches where the size of each component group is kept unchanged. A high fraction indicates that the model is closer to a perfectly random matching process. All the details are given in the online appendix section B.

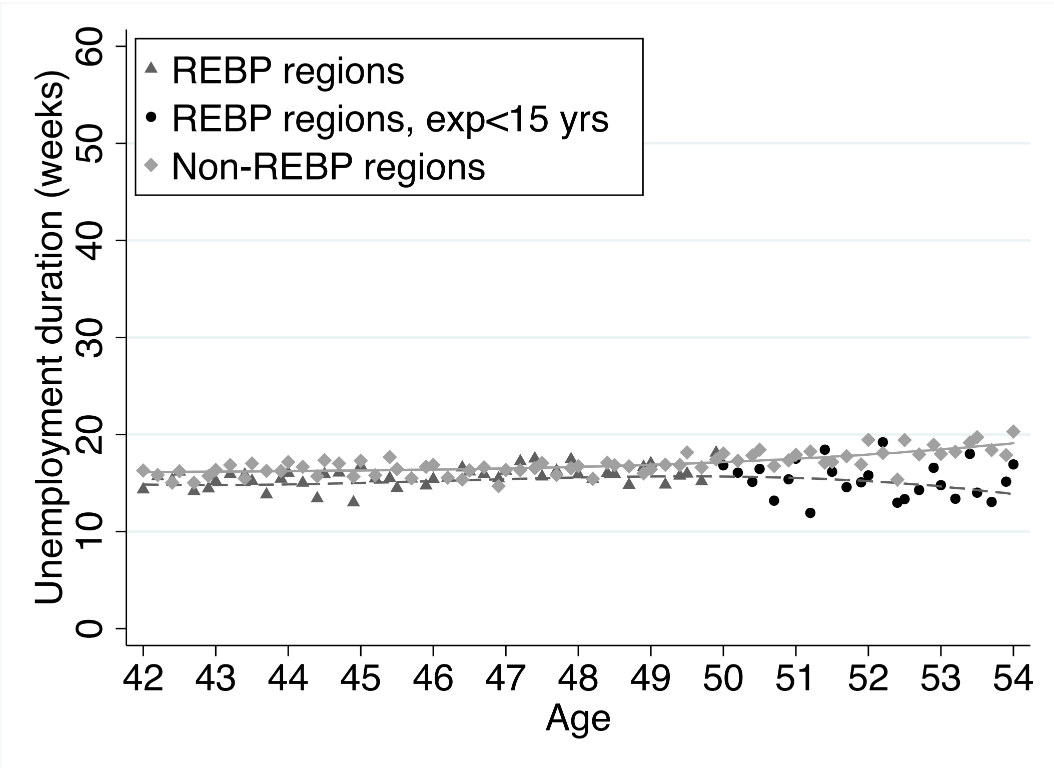
Figure 3: Difference in unemployment durations between REBP and non-REBP counties by year of entry into unemployment:



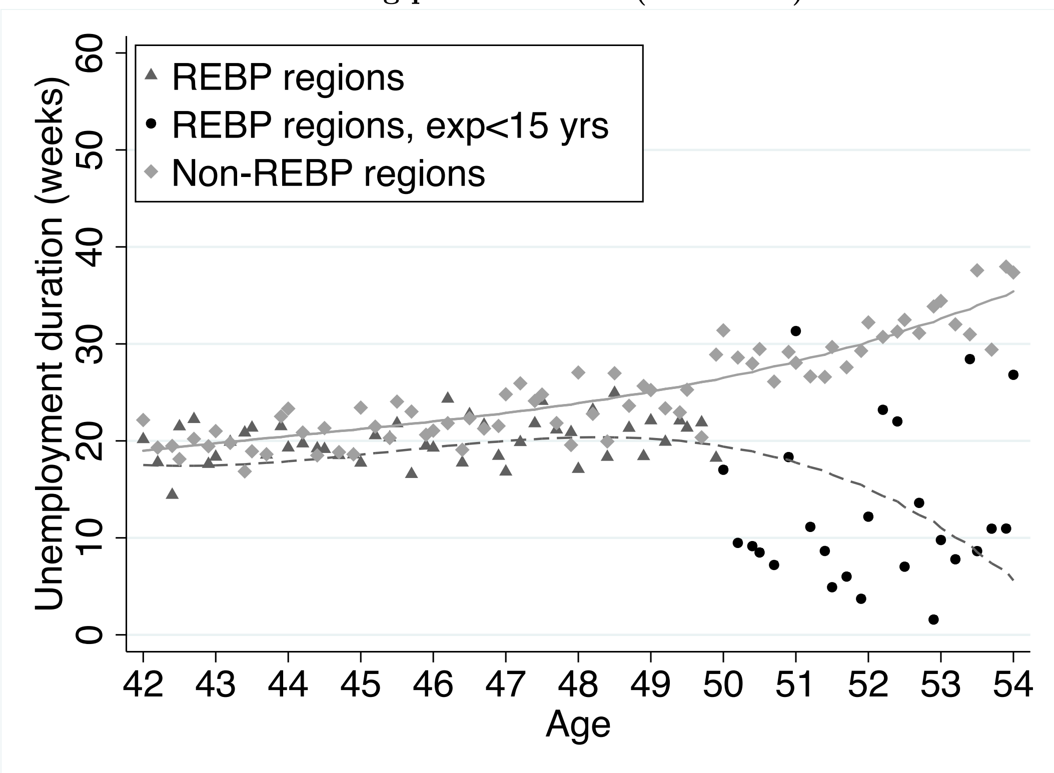
Notes: The figure plots d_t, the yearly average difference in unemployment duration (in weeks) between REBP and non-REBP counties, obtained from regression specification 2, where controls include education, 15 industry codes, family status, citizenship and tenure in previous job. The reference year is 1981. Standard errors cluster at the region $\times$ year level. Sample includes all unemployed individuals between 46 and 54 in REBP and non-REBP counties. Non-REBP counties with high labor market integration to REBP regions are excluded from the sample. Panel A plots the difference for workers above 50 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 all non-eligible workers (less than 50 and/or less than 15 years of work history). Panel C plots the difference for non-eligible workers based on work history only (above 50 but less than continuous 15 years

Figure 4: Unemployment durations as a function of age in REBP and non-REBP counties for non-eligible unemployed:

A. Before and after REBP



B. During peak of REBP (1992-1995)



Notes: the figure plots the relationship between age and unemployment durations for all non-eligible workers in REBP and non-REBP counties when REBP was not in place (panel A), and during the peak of the REBP period (January 1992 to December 1995). We plot the average duration of unemployment in bins of age at the start of unemployment where the bin size is two months of age. In REBP counties, to make the distinction more visible between non-eligible workers due to age (below 50) and due to work experience only (age 50 to 54), we plot them in different marker shapes. We fit the data with a third-order polynomial for REBP and non-REBP counties. Panel A shows that during the non-REBP period, the relationship between age and unemployment duration is extremely similar for non-eligible workers in REBP and non-REBP regions. Panel B shows that during the peak of the REBP period (January 1992 to December 1995) non-eligible workers experienced shorter unemployment spells in REBP regions compared to non-REBP regions. And this difference in unemployment duration is sharply increasing with age.

Table 1: Summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. REBP vs non-REBP counties							
	Non-REBP period				**REBP period**			
	Non-REBP counties	REBP counties	Difference	p-value	Non-REBP counties	REBP counties	Difference	p-value
Fraction employed in the steel sector	.055	.152	-.097	0	.057	.156	-.099	0
Monthly 46-54 unemployment rate	.055	.054	.001	.864	.073	.113	-.04	0
Fraction eligible to REBP	.382	.396	-.014	0	.449	.533	-.084	0
Age	49.7	49.7	0	.343	49.8	50.1	-.3	0
Unemployment duration	13.6	14.3	-.7	0	15.9	29	-13.1	0
Non employment duration	22.7	21.2	1.4	0	32.9	45.4	-12.4	0
Wage before U spell (€2000)	13448	14306	-857	0	13122	14498	-1375	0
	B. Eligible vs non-eligible unemployed in REBP counties							
	Non-REBP period				**REBP period**			
	Non-eligible unemployed	Eligible unemployed	Difference	p-value	Non-eligible unemployed	Eligible unemployed	Difference	p-value
Age	48.2	51.9	-3.7	0	48	52	-4	0
Unemployment duration	17.5	20.8	-3.2	0	23.2	88.8	-65.6	0
Non employment duration	21.6	24.7	-3.1	0	31.4	99.6	-68.2	0
Wage before U spell (€2000)	14096	14623	-527	0	13316	15549	-2232	0
Fraction with compulsory education	.529	.501	.028	0	.511	.506	.005	.44
Fraction married	.744	.751	-.007	.076	.748	.803	-.055	0

Notes: The table displays summary statistics from the Austrian social security and unemployment insurance files. Panel A compares REBP and non-REBP counties in the non-REBP period (1980 to May 1988 and August 1997 to 2009) and during the REBP period (June 1988 to July 1997). 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 46 to 54 employed in the steel industry. The unemployment rate is the average monthly number of unemployed men aged 46 to 54 recorded in the unemployment insurance files as a fraction of the sum of unemployed and employed male workers aged 46 to 54. All remaining rows in this table are computed for our estimation sample of unemployed workers which is restricted to men, aged 46 to 54, who never work in the steel sector. Panel B compares, in REBP counties, in the non-REBP period (1980 to May 1988 and August 1997 to 2009) and during the REBP period (June 1988 to July 1997), eligible unemployed workers (above 50 and 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 or below 50). 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)
	Unemployment duration			Non-empl. duration	Spell >100 wks	Spell >26 wks
	A. Treatment effect on eligible unemployed					
β_0	47.13***	43.35***	43.37***	29.17***	0.240***	0.237***
	(5.602)	(5.129)	(5.069)	(5.444)	(0.0293)	(0.0240)
N	267966	262344	262344	232135	262344	262344
	B. Externality - all non-eligible unemployed					
γ_0	-2.462***	-1.979***	-3.740***	-2.327***	-0.0130***	-0.0165**
	(0.818)	(0.708)	(0.758)	(0.629)	(0.00311)	(0.00660)
N	267966	262344	262344	232135	262344	262344
	C. Externality - non-eligible unemployed below 50					
γ_0	-2.004**	-1.446**	-3.321***	-2.030***	-0.0104***	-0.0166***
	(0.829)	(0.699)	(0.616)	(0.539)	(0.00205)	(0.00526)
N	254934	249894	249894	220754	249894	249894
	D. Externality - non-eligible unemployed above 50					
γ_0	-6.638***	-6.124***	-8.862***	-6.913***	-0.0244***	-0.0494***
	(2.156)	(2.194)	(2.226)	(2.100)	(0.00915)	(0.0142)
N	125088	122277	122277	102677	122277	122277
Educ., industry, citizenship, marital status		×	×	×	×	×
Region-specific trends			×	×	×	×

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 (3). 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) to (6) we add controls for preexisting trends by region. Panel A presents the effect of REBP on labor market outcomes of eligible workers. Panel B presents the effect of REBP on labor market outcomes of all non-eligible workers aged 46 to 54. In panel C, we focus on the effect of REBP for non-eligible workers age 46 to 50 who are non-eligible based on age. For this specification, we exclude from the estimation sample non-eligible workers based on experience. Panel D shows the effect of REBP for non-eligible workers age 50 or above who are non-eligible based on the experience requirement. For this specification, we exclude from the estimation sample workers with age below 50.

Table 3: TESTING FOR SELECTION: IMPACT OF REBP ON INFLOW RATE INTO UNEMPLOYMENT, LOG REAL WAGE IN PREVIOUS JOB AND LOG TENURE IN PREVIOUS JOB OF ELIGIBLE AND NON-ELIGIBLE UNEMPLOYED

	(1) log separation rate	(2) log real wage in previous job	(3)	(4) log tenure in previous job	(5)
Eligible workers	0.286***				
	(0.0356)				
Non-eligible workers	0.0162				
	(0.0218)				
β_0 (REBP effect on eligible)		0.109	0.128*	0.646***	0.487***
		(0.0688)	(0.0686)	(0.0767)	(0.0563)
γ_0 (REBP effect on non-eligible)		0.0110	-0.00873	-0.0450	-0.0581*
		(0.0112)	(0.0108)	(0.0355)	(0.0305)
Educ., marital status, industry, citizenship			×		×
N	3390	240947	240923	267929	267901

Notes: For columns (2) to (5), 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 eligibility group×region×quarter level. In columns (2) to (5), sample include all unemployed age 46 to 54. 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 log tenure in previous job as an outcome.

Table 4: 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
	A. Treatment intensity - Method 1: County share of hires from non-REBP counties			
	All non-eligible			
γ_0^L (share of non-REBP hires $>$ .05)	-1.599**	-0.676	-0.00275	-0.00289
	(0.747)	(0.693)	(0.00224)	(0.00661)
γ_0^H (share of non-REBP hires $\leq$ .05)	-2.866***	-4.170***	-0.00612*	-0.0266***
	(0.844)	(0.917)	(0.00324)	(0.00733)
F-Test $\gamma_0^L = \gamma_0^H$	[0.0674]	[0.0001]	[0.138]	[0.0002]
	Non-eligible 50+			
γ_0^L (share of non-REBP hires $>$ .05)	-4.048**	-4.191*	-0.00300	-0.0119
	(1.894)	(2.309)	(0.00788)	(0.0136)
γ_0^H (share of non-REBP hires $\leq$ .05)	-15.24***	-10.66*	-0.0519**	-0.111***
	(5.164)	(5.831)	(0.0230)	(0.0372)
F-Test $\gamma_0^L = \gamma_0^H$	[0.0245]	[0.310]	[0.0354]	[0.00566]
	B. Treatment intensity - Method 2: Fraction treated in region×education×industry cell			
	All non-eligible			
γ_0^L (fraction treated $\leq$ .9)	-0.849	-1.022	0.00426	-0.00918
	(0.933)	(1.161)	(0.00421)	(0.00886)
γ_0^H (fraction treated $>$ .9)	-2.238***	-1.908**	-0.00560*	-0.0102
	(0.828)	(0.802)	(0.00307)	(0.00725)
F-Test $\gamma_0^L = \gamma_0^H$	[0.252]	[0.545]	[0.104]	[0.928]
	Non-eligible 50+			
γ_0^L (fraction treated $\leq$ .9)	-4.207	-3.661	-0.00126	-0.0351*
	(2.807)	(2.378)	(0.0110)	(0.0188)
γ_0^H (fraction treated $>$ .9)	-8.831***	-8.022***	-0.0274***	-0.0235
	(2.016)	(2.426)	(0.00952)	(0.0215)
F-Test $\gamma_0^L = \gamma_0^H$	[0.0789]	[0.0503]	[0.0272]	[0.668]
Educ., marital status, industry, citizenship	×	×	×	×

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 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 (4). 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 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%, which corresponds to the median value across REBP counties. Method 2 computes the average yearly fraction of eligible workers among the 50+ 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 the 50+ unemployed were eligible, which is the median value across all region×industry×education cells. A region is defined as the first two digits of the municipality identifiers.

Table 5: 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)	-3.997***	-3.500**	-1.043	-0.00658	-0.0239**
	(1.428)	(1.440)	(1.439)	(0.00558)	(0.0119)
	Labor market integration - Measure 2: Fraction of hires coming from REBP regions in county×industry×education cell				
γ_0 (geographical spillovers)	-6.373***	-5.242***	-2.515***	-0.0141***	-0.0169***
	(1.213)	(1.109)	(0.659)	(0.00368)	(0.00603)
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 15% for all years when REBP was not in place. 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% (for all years when REBP was not in place) to unemployed in cells where it was lower than 20%.

Appendix - Not for publication

A Externalities in search and matching models and their identification

The probability that an individual finds a job in a given time period t depends on how hard that individual searches for a job and/or on how selective he is in his acceptance decisions. It also depends on the aggregate labor market conditions that determine how easy it is to locate jobs or to be matched to a potential employer for each unit of search effort. These two forces are usually represented in equilibrium search and matching models by using the stylized decomposition: $h_{it} = e_{it} \cdot f(\theta_t)$. h is the hazard rate out of unemployment (the probability to find a job in period t for individual i). e_{it} captures the search effort / selectiveness component. θ_t is the ratio of job vacancies to total search effort, and represents the tightness of the labor market. $f(\theta_t)$ therefore captures the effect of labor market conditions on the job finding probability per unit of effort. If there are no job vacancies created by employers, then $f(\theta_t) = 0$ and no amount of search effort by an unemployed worker would yield a positive probability of obtaining a job.

Changes in unemployment benefit policies affect the search intensity /selectiveness of unemployed workers. We call this effect the *micro effect* of UI. It can be identified by comparing two individuals with different levels of UI generosity in the same labor market. Changes in unemployment benefit policies also affect the aggregate job finding rate per unit of search effort through equilibrium effects. We call this second effect *market externalities*. It stems from equilibrium adjustments in labor market tightness θ_t in response to a change in UI generosity. The first aim of this appendix is to provide a simple theoretical framework explaining the mechanisms shaping the sign and magnitude of these market externalities. The second aim is to explain how to identify these market externalities empirically.

We start by presenting a one group equilibrium to explain the forces shaping equilibrium adjustments in labor market tightness in response to variations in UI. Then we extend the model to a two-group equilibrium in order to explain how to identify market externalities empirically and connect more closely the framework to the policy experiment that we analyze in the paper. In particular, we detail how to choose groups of workers to identify market externalities. We also explain how the sign and magnitude of market externalities depend on the structure of the labor market treated by the change in UI generosity and its connection to other labor markets.

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]. Readers are referred to these two papers for further details on the set-up and equilibrium analysis.

A.1 One group equilibrium

The labor market is characterized by the presence of matching frictions. We normalize the size of labor force to unity. We present a simplified, static equilibrium analysis of search and matching models and characterize the comparative static for steady state equilibria. We start by looking at a one group equilibrium, as in Landais et al. [2010], where all workers are eligible to the same unemployment benefits B, and explain the two main mechanisms that shape the equilibrium response in labor market tightness to a variation in unemployment benefits: the **rat race effect** (or labor demand effect) and the **wage effect**.

Unemployed workers face v vacancies opened by firms, and the total number of matches realized is given by an aggregate matching function $m(\overline{e \cdot u}, v) = \omega_m \cdot (\overline{e \cdot u})^{\eta} \cdot v^{1-\eta}$. Labor market tightness $\theta = \frac{v}{e \cdot u}$ is defined as the ratio of vacancies to the aggregate search effort in the labor market.

The individual job-finding probability is $h = e \cdot f(\theta) = e \cdot m(1, \theta)$, where $e = e(B, \theta)$ is the optimal search effort of individuals given benefits and labor market tightness. Effort is a decreasing function of unemployment benefits $\partial e / \partial B < 0$. To further simplify the presentation, we assume that $\frac{\partial e}{\partial \theta} = 0$. 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 job-finding probability is an increasing function of θ ($f'(\theta) > 0$). From the definition of the matching function we can also define the vacancy-filling probability for each vacancy opened by the firm $q(\theta) = m(1/\theta, 1)$ which is a decreasing function of labor market tightness $\frac{\partial q(\theta)}{\partial \theta} < 0$.

We denote by n^s the probability that a worker is employed (and by $u = 1 - n^s$ the corresponding unemployment probability). Using the steady state equality of flows in and out of unemployment, we have that

$$n^s = \frac{ef(\theta)}{\psi + ef(\theta)} \tag{7}$$

where ψ is the exogenous separation rate. Following Michaillat [2012], we interpret $n^s = n^s(\theta, e(B))$ as a labor supply that we can represent as an increasing function of θ in a $\{n, \theta\}$ diagram.

A representative firm maximizes profit $\pi = \phi(n) - n \cdot w - \frac{r}{q(\theta)} \cdot \psi \cdot n$ where $\phi(.)$ is total output, n is employment and r is the recruiting cost of opening a vacancy. 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:

$$\phi'(n) = w + \frac{r\psi}{q(\theta)} \tag{8}$$

Equation (8) implicitly defines a labor demand function $n^d(\theta, w)$ whose properties depend in particular on the assumptions made on $\phi(.)$ and on the wage setting process defining w. These properties are important to determine the sign and magnitude of externalities, as explained below. In particular, note that when technology exhibits diminishing returns to labor, with $\phi'(n) > 0$ and $\phi''(n) < 0$, we have by implicit differentiation of equation (8): $\frac{\partial n^d}{\partial \theta} < 0$. So in this case, labor demand will be a downward sloping function of θ as in Michaillat [2012]. The intuition for this negative relationship between labor demand and labor market tightness is the following: as labor market tightness goes up, the cost of opening vacancies goes up, as it takes longer to fill vacancies. Firms will post fewer vacancies, bringing their level of employment down, which will increase labor productivity and restore the profit from opening vacancies. It is also immediate to see that when technology is linear and in the absence of aggregate demand effects, equation (8) implicitly defines labor demand as a perfectly elastic function of labor market tightness.

Note also that, depending on the wage setting process, labor demand implicitly defined by equation (8) can also be a function of unemployment benefits. If wages are bargained over and workers have limited bargaining power, then wages will react to outside options of workers and thus to variations in unemployment benefits B: $w = w(B)$. As can be seen from equation (8), an increase in B leading to a increase in wages w will, everything else equal, decrease the net return from opening a vacancy and lead to a decrease in labor demand n^d.

We can now define a labor market equilibrium by the condition:

$$n^s(\theta, B) = n^d(\theta, w(B)) \tag{9}$$

Market externalities:

Equilibrium condition (9) defines θ as an endogenous variable, affected by the level of benefits B of unemployed individuals in equilibrium. Because of this equilibrium adjustment of θ in response to a change in UI benefits, the effect of UI on the job finding probability $e \cdot f(\theta)$ can be decomposed into two parts, a micro-effect capturing the change in search effort keeping labor market tightness constant and a "market externality", capturing the effect of the change in labor market tightness:

$$\frac{dh}{dB} = \frac{d(e \cdot f(\theta))}{dB} = \underbrace{\frac{\partial e}{\partial B} \cdot f(\theta)}_{\text{Micro effect}} + \overbrace{e \cdot f'(\theta) \cdot \frac{\theta}{B} \cdot \varepsilon_B^\theta}^{\text{Market externality}} \tag{10}$$

where $\varepsilon_B^\theta = \frac{d\theta}{dB}\frac{B}{\theta}$ is the elasticity of labor market tightness with respect to the generosity of UI B. The second term on the right-hand side of equation (10) is the market externality, which is defined as the variation in the job finding rate caused by equilibrium adjustments in labor market tightness, keeping search effort level constant.

The reason why we call this effect a "market externality" instead of a mere incidence effect is because, as shown in Landais et al. [2010], these equilibrium adjustments in labor market tightness have first-order welfare effects when the Hosios condition is not met.

Equilibrium adjustment of θ in response to a change in UI benefits ($\frac{d\theta}{dB}$) is given by fully differentiating equation (9).

$$\frac{d\theta}{dB} = \frac{\frac{\partial n^d}{\partial w}\frac{\partial w}{\partial B} - \frac{\partial n^s}{\partial B}}{\frac{\partial n^s}{\partial \theta} - \frac{\partial n^d}{\partial \theta}} \tag{11}$$

Equation (11) can also be rewritten in terms of elasticities:

$$\varepsilon_B^\theta = \frac{\varepsilon_w^{n^d} \cdot \varepsilon_B^w - \varepsilon_B^{n^s}}{\varepsilon_\theta^{n^s} - \varepsilon_\theta^{n^d}} \tag{12}$$

where the notation ε_Y^X refers to the elasticity of X w.r.t Y. From the previous equation, we can now discuss the forces determining equilibrium adjustments of θ in response to a change in benefits B. We focus in particular on two opposing forces: the rat-race effect (or labor-demand effect), and the wage effect.

Rate race effect

The **rate race effect** is determined by the elasticity of labor-demand ($\varepsilon_\theta^{n^d}$). If labor demand is downward sloping ($\varepsilon_\theta^{n^d} < 0$) then the denominator in (12) is positive. Given that $\varepsilon_B^{n^s} < 0$, it follows that, **conditional on wages**, equilibrium labor market tightness will increase when UI benefits increase $\varepsilon_B^\theta\big|_w > 0$. The more inelastic labor demand is with respect to labor market tightness, the larger the rat race effect. If labor demand is perfectly rigid, then the rat race effect is at its maximum: firms will fully compensate a UI-induced decrease in search effort by opening more vacancies to keep the level of employment constant.

Intuitively, a downward sloping labor demand ($\varepsilon_\theta^{n^d} < 0$) captures the fact that the net profits from opening vacancies are a decreasing function of employment. When search effort decreases, it decreases labor supply, which increases the profits of opening vacancies for firms: vacancies increase, which increases labor market tightness, and the probability of finding a job per unit of effort increases for all workers. Landais et al. [2010] discuss various search and matching models and show under which conditions such "rat race" effect is likely to arise. In particular, Landais et al. [2010] show that technology can be an important factor. In the presence of diminishing returns to labor, as explained above, labor demand is a downward sloping function of tightness and the larger the diminishing returns to labor, the larger the labor demand effect on equilibrium tightness. When technology is close to linear in labor, labor demand will in general be close to perfectly elastic, and therefore ε^{θ}_{B} tends to zero. Note however that diminishing returns is a sufficient but not a necessary condition for the presence of a downward sloping labor demand. Landais et al. [2010] show for instance that an "aggregate demand model" with a quantity equation for money and nominal wage rigidities will feature a downward sloping labor demand even with linear technology.

The rat race effect will be the only driver of labor market tightness adjustments to the policy when wages do not react to the policy ($\varepsilon^{w}_{B} = 0$). Studies estimating spillover effects of active labor market programs such as training programs therefore tend to capture a pure rat race effect as these training programs do not generally affect bargained wages.

Wage effect

If the wage setting process is such that wages are correlated with outside options of workers, then an increase in UI benefits will increase wages $\varepsilon^{w}_{B} > 0$ which will in turn affect the vacancy posting behavior of firms. Higher wages will decrease the return from opening vacancies for firms leading to a decrease in labor demand ($\varepsilon^{n^d}_{w} < 0$) and in turn, a decrease in labor market tightness. We call this effect the **wage effect** (or job creation effect). The wage effect is going in the opposite direction to the rate race effect. The overall effect of a change in UI benefits on equilibrium labor market tightness will therefore depend on the relative magnitude of these two effects. If the wage effect is large enough, the numerator in (12) may become negative ($\varepsilon^{n^d}_{w} \cdot \varepsilon^{w}_{B} < \varepsilon^{n^s}_{B} < 0$) and equilibrium labor market tightness will decrease in response to an increase in benefits. If the wage effect is small in magnitude, then the rat race effect will dominate: the numerator in (12) will be positive ($\varepsilon^{n^s}_{B} < \varepsilon^{n^d}_{w} \cdot \varepsilon^{w}_{B} < 0$) and labor market tightness will increase in response to an increase in UI benefits.

A.2 Identification of market externalities in a two group equilibrium

Identification of the "micro effect" in equation (10) is relatively straightforward. The ideal experiment is to offer higher unemployment benefits to a randomly selected and small subset of individuals within a labor market and compare unemployment durations between these treated individuals and the other jobseekers. In practice, the micro effect is estimated by comparing individuals with different benefits in the same labor market at a given time, while controlling for individual characteristics.

Identification of market externalities in equation (10) is more complicated, in large part due to the lack of good measures of labor market tightness[23]. We show here how one can use la-

[23] A notable exception is Marinescu [2014] who uses very detailed information on vacancies and job applications from `CareerBuilder.com`, the largest American online job board, to compute the effects of UI extensions on aggregate search effort ($e \cdot u$) measured by job applications and on vacancy posting (v) at the state level. She

bor market outcomes of different group of workers *in the same labor market* to identify market externalities of UI benefits. We introduce two groups of workers a and b 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. We start from a situation where $B_a = B_b$ and look at the effect on the steady state equilibrium of an increase in benefits for workers of group a: $dB_a > 0$.

We denote by n_a^s (resp. n_b^s) the probability that a worker of group a (resp. b) is employed (and by $u_a = 1-n_a^s$ the corresponding unemployment probability) There are $u = u_a + u_b$ unemployed workers. When unemployed, each individual worker exerts some effort $e_i = e(B_i),\ i = (a, b)$, where e is a decreasing function of benefits received B.

Workers of both groups are assumed to be in the same labor market and **we define a labor market as the place where workers compete for the same job vacancies.** A labor market is therefore characterized by a unique labor market tightness in equilibrium, and matching is random between identical job vacancies posted by firms and all the (potentially different) workers who apply for these identical vacancies. From the firms' point of view, this means that when opening vacancies, firms take as given labor supply of group a and group b, and opening v vacancies translates into $p \cdot n_a/q(\theta)$ jobs of workers from group a and $(1-p)n_b/q(\theta)$ jobs of workers from group b. Wages are determined at the individual level, once the match is done and depends on the outside option of each worker. We therefore allow for two different wage levels w_a and w_b for both groups of workers in equilibrium.

This definition of labor market is the most natural definition from a search theoretic standpoint. As labor market tightness (and not the wage rate) is the "price" variable equating labor supply and labor demand in labor market characterized by search frictions, our definition of a labor market strictly follows the *law of one price*. From an empirical perspective, this definition captures the fact that a labor market is the place where workers compete for the same jobs.

As in the one group case before, firms choose the level of employment that maximizes profits, which is equivalent to choosing the number of vacancies to open in order to maximize profits (taking labor market tightness as given). There is only one labor market tightness for the two groups of workers, so opening v vacancies translates into $p \cdot n_a/q(\theta)$ jobs of workers from group a and $(1-p) \cdot n_b/q(\theta)$ jobs of workers from group b. We can therefore write firms profits as:

$$\pi = \phi\Big(p \cdot n_a, (1-p) \cdot n_b\Big) - p \cdot n_a \cdot w_a - (1-p) \cdot n_b \cdot w_b - \frac{r}{q(\theta)} \cdot \psi \cdot (p \cdot n_a + (1-p) \cdot n_b) \quad (13)$$

finds a negative effect of UI extensions on job applications but no effect of UI extensions on vacancy posting. Since $\theta = v/(e \cdot u)$, these results imply that more generous UI benefits increase labor market tightness.

$$p\underbrace{\left\{\frac{\partial\phi}{\partial n_a} - w_a - \frac{r\psi}{q(\theta)}\right\}}_{n_a^d} + (1-p)\overbrace{\left\{\frac{\partial\phi}{\partial n_b} - w_b - \frac{r\psi}{q(\theta)}\right\}}^{n_b^d} = 0 \tag{14}$$

Similarly to equation (8), equation (14) implicitly defines the optimal employment level demanded by firms as a function of labor market tightness θ. Importantly, equation (14) defines the optimal employment level $n^d = pn_a^d + (1-p)n_b^d$ as a weighted sum of the optimal employment level of workers of group a and group b. In other words, the labor demand curve in the two-group case is the weighted sum of the demand curve for workers of group a and the demand curve for workers of group b.

Equilibrium in the labor market is now defined by the following condition:

$$pn_a^d(\theta, w_a) + (1-p)n_b^d(\theta, w_b) = pn_a^s(\theta, B_a) + (1-p)n_b^s(\theta, B_b) \tag{15}$$

Equilibrium condition (15) defines θ as an endogenous variable, *affected by the level of benefits B_a and B_b of both groups of unemployed individuals* in equilibrium. Let us start from a situation where $B_a = B_b = B$ and workers of both groups are identical so that $e_a = e_b$, and investigate the effect of a small change $dB_a > 0$ on hazard rates of workers of group a and group b. Because of the equilibrium adjustment of θ in response to a change in UI benefits B_a, the effect of UI on the job finding probability of workers of group a, $e_a \cdot f(\theta)$ can again be decomposed into two parts, a micro-effect capturing the change in search effort of workers of group a keeping labor market tightness constant and a "market externality", capturing the effect of the change in labor market tightness:

$$\frac{dh_a}{dB_a} = \frac{d(e_a \cdot f(\theta))}{dB_a} = \underbrace{\frac{\partial e_a}{\partial B_a} \cdot f(\theta)}_{\text{Micro effect}} + \overbrace{e_a \cdot f'(\theta) \cdot \frac{\theta}{B} \cdot \varepsilon^{\theta}_{B_a}}^{\text{Market externality}} \tag{16}$$

But workers of group b also experience a change in their job finding probability, even if their unemployment benefits are unaffected, due to the equilibrium adjustment of θ in response to a change in UI benefits B_a:

$$\frac{dh_b}{dB_a} = \frac{d(e_b \cdot f(\theta))}{dB_a} = e_b \cdot f'(\theta) \cdot \frac{\theta}{B} \cdot \varepsilon^{\theta}_{B_a} \tag{17}$$

Equation (17) shows that the effect of a change in benefits B_a for a treated group of workers on the job finding probability of non-treated workers of group b identifies the market externality. This result motivates our empirical strategy. By looking at how the job finding probability of non-treated workers varies in response to a change in unemployment benefits of similar workers *in the same labor market*, one can identify equilibrium adjustments in labor market tightness.

We now explain how market externalities in the two group experiment relate to market externalities in the one group experiment where all workers of the labor market are treated. Equilibrium adjustments in tightness in the two group experiment is given by implicitly differentiating equilibrium condition (15):

$$\frac{d\theta}{dB_a} = p \frac{\frac{\partial n^d_a}{\partial w_a}\frac{\partial w_a}{\partial B_a} - \frac{\partial n^s_a}{\partial B_a}}{\frac{\partial n^s}{\partial \theta} - \frac{\partial n^d}{\partial \theta}} \tag{18}$$

Using the fact that we start from $n_a = n_b$, we can rewrite equation (18) in terms of elasticities:

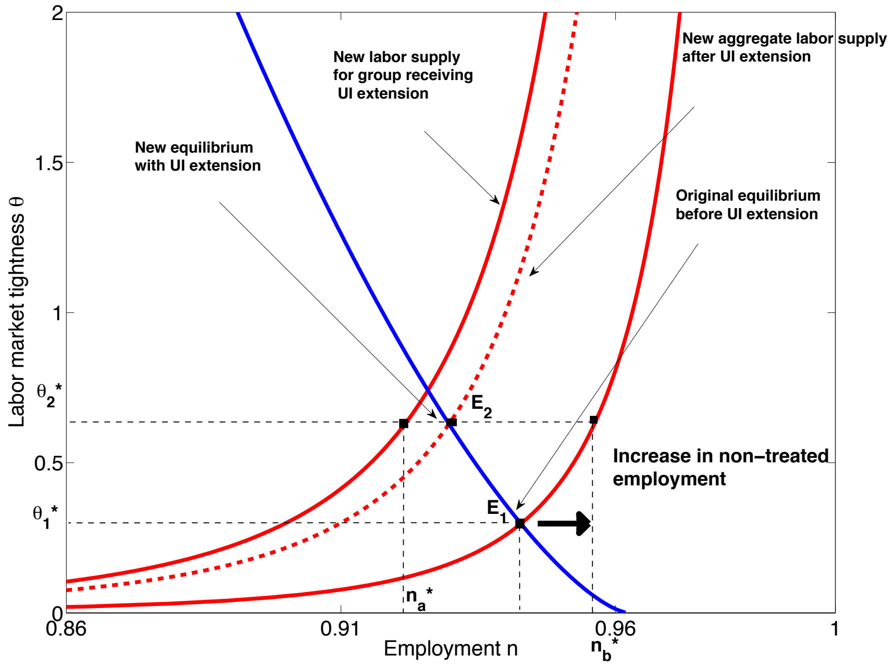
$$\begin{aligned} \varepsilon^{\theta}_{B_a} &= p \cdot \frac{\varepsilon^{n^d_a}_{w} \cdot \varepsilon^{w_a}_{B_a} - \varepsilon^{n^s_a}_{B_a}}{\varepsilon^{n^s}_{\theta} - \varepsilon^{n^d}_{\theta}} \\ &= p \cdot \varepsilon^{\theta}_{B} \end{aligned} \tag{19}$$

A few points[24] are worth noting about equation (19). First, equilibrium adjustments in labor

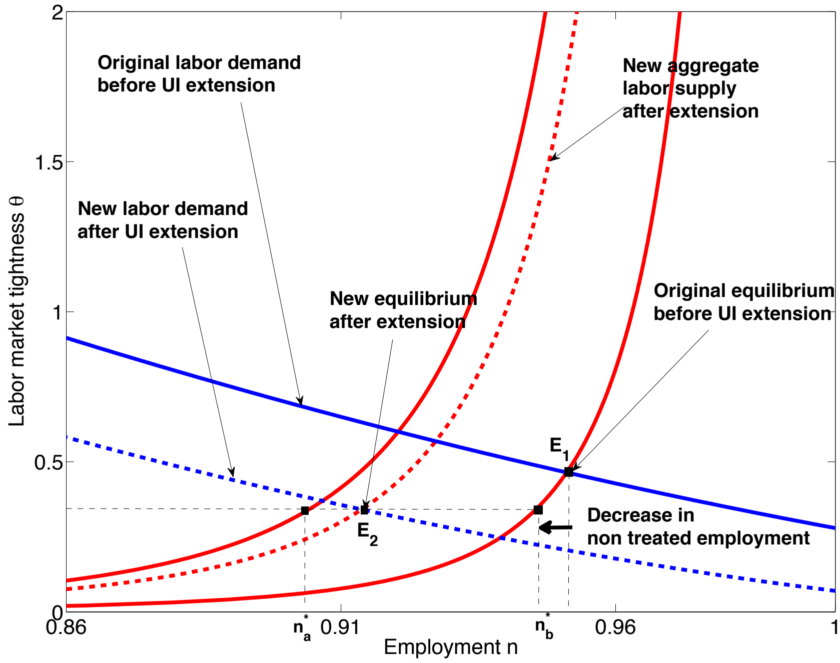
[24]Note that we have assumed that workers were perfectly equivalent so that productivity of labor only depends

Figure 5: MARKET EXTERNALITIES OF UI EXTENSIONS IN AN EQUILIBRIUM SEARCH-AND-MATCHING MODEL WITH TWO GROUPS OF WORKERS:

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 one group of workers (group a), when firms cannot discriminate vacancies between the two groups of workers. 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 market externalities on workers of group b. 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 workers of group b.

market tightness in the two group experiment increase with the size of the treated group. The larger p, the larger market externalities will be. Second, as p tends to 1, $\varepsilon^{\theta}_{B_a}$ tends to ε^{θ}_{B}, so that market externalities identified on group b will tend to capturing the effect of treating the entire labor market. Finally, market externalities identified through the change in the job finding probability of workers of group b still capture the wage effect even if wages are bargained at the individual level. The intuition is that within a labor market, there is random matching. The expected profit of opening vacancies is the weighted average of the profits of opening vacancies for each group of workers. Therefore the increase in bargained wages of workers of group a will reduce the expected profit of opening vacancies and will then affect overall vacancy posting in the market.

Implications for the wedge between micro and macro effects of UI

We are interested in recovering from the two group experiment, the wedge between micro and macro effects of treating the whole labor market. More specifically, starting from equation (10), we are interested in the wedge $W = 1 - e^M/e^m$ where $e^M = \frac{dh}{dB}$ is the total effect on job finding rate of treating the whole market by an increase dB in UI benefits ("macro effect") and e^m is the "micro effect" from equation (10) (*i.e.* the effect of an increase dB in UI benefits on individual job finding rate).

From equation (10) we know that $W = \frac{e^X}{e^m}$, where $e^X = e \cdot f'(\theta) \cdot \frac{\theta}{B} \cdot \varepsilon^{\theta}_{B}$ is the market externality of treating the whole labor market.

From equations (17) and (19), we know that in the two group experiments, starting from a situation where both groups have the same benefits and search effort

$$\frac{dh_b}{dB_a} = p \cdot e^X \tag{20}$$

In other words, the effect of changing benefits for workers of group a on the job finding rates of workers of group b identifies p times the externality of treating all workers, where p is the fraction of workers of group a in the labor market.

In the two group experiment, again starting from a situation where both groups have the same benefits and search effort, we also know that the micro effect e^m will be the same than when treating the whole market. This means that the micro effect $\frac{\partial e}{\partial B} \cdot f(\theta)$ from equation (10) is equal to the micro effect from equation (16): $\frac{\partial e_a}{\partial B_a} \cdot f(\theta)$. And from equations (16) and (17), we know that the micro effect will be identified in the two group experiment as

$$e^m = \frac{dh_a}{dB_a} - \frac{dh_b}{dB_a} \tag{21}$$

In other words, the micro effect is identified by the effect of the change in UI benefits on the job finding rate of workers of group a minus the effect on the job finding rate of workers of group b.

on total employment level and not on the mix of workers. If there is imperfect substitution, total productivity and therefore labor demand now depends on the mix of workers of both types in equilibrium. An extra term kicks in in formula 18. The sign of this extra term will depend on technology.

It follows from equations (20) and (21) that we can identify the wedge W of treating the whole market in the two group experiment:

$$W = \frac{1}{p} \cdot \frac{\frac{dh_b}{dB_a}}{\frac{dh_a}{dB_a} - \frac{dh_b}{dB_a}} \tag{22}$$

Using the fact that we start from a situation where $B_a = B_b$ and $h_a = h_b$, and under the approximation that hazard rates are somewhat constant over a spell so that the duration of unemployment $D \approx 1/h$ we can rewrite equation 22 in terms of responses of unemployment duration:

$$W = \frac{1}{p} \cdot \frac{\frac{dD_b}{dB_a}}{\frac{dD_a}{dB_a} - \frac{dD_b}{dB_a}} \tag{23}$$

A.3 Market externalities across labor markets

In most quasi-experiments involving variations in the generosity of unemployment benefits, treatment is restricted to some but not all labor markets. The REBP program is no exception. The program extended the duration of UI benefits for individuals above age 50 in specific regions meeting specific criteria. From a firm's point of view, this means that the firm can adjust to the policy not only by changing the number of vacancy it opens in the treated labor market, but also by changing the number of vacancies it opens in other labor markets where there exists close substitutes to the treated population. In other words, there exists "non-treated" labor markets that, due to their (geographic or technological) proximity to the treated labor market, are likely to also be affected by the policy in equilibrium. We show here how the existence of other labor markets will affect market externalities. First, we show how and why equilibrium labor market conditions in other markets will be affected. Then, we discuss how the existence of other markets affect the magnitude of market externalities in the treated market.

We start by explaining how and why other labor markets may be affected by a change in UI policy in one labor market. We focus again on a two-group model, but now group a and group b are assumed to be in two different labor markets. This means that firms can perfectly discriminate between the two groups of workers when they open vacancies. In practice, there will be vacancies v_a to which only workers of group a will apply and vacancies v_b to which only workers of group b will apply. The ability of firms to direct their search by tailoring the characteristics of vacancies to each group of workers means that there will be in effect two labor markets with two labor market tightness in equilibrium.

Firms' profits are now equal to:

$$\pi = \phi\Big(p \cdot n_a, (1-p) \cdot n_b\Big) - p \cdot n_a \cdot w_a - (1-p) \cdot n_b \cdot w_b - r \cdot \psi \cdot \left\{ \frac{p \cdot n_a}{q(\theta_a)} + \frac{(1-p) \cdot n_b}{q(\theta_b)} \right\} \tag{24}$$

For the firm, the optimal choice of vacancies to open for group a and group b is equivalent to the optimal choice of n_a and n_b, as v_a vacancies translate into $n_a/q(\theta_a)$ jobs for workers of group a (and v_b vacancies translate into $n_b/q(\theta_b)$ jobs for workers of group b). The optimal labor demand of firms for workers of group a, n_a^d, and for workers of group b, n_b^d, is then implictely defined by the two following first-order conditions:

$$\frac{\partial \phi}{\partial n_a} = \left\{ w_a + \frac{r\psi}{q(\theta_a)} \right\} \tag{25}$$

$$\frac{\partial \phi}{\partial n_b} = \left\{ w_b + \frac{r\psi}{q(\theta_b)} \right\} \tag{26}$$

When technology is such that the marginal product of labor for group a (resp. group b) depends on the level of employment of workers of group b (resp. group a), n_a^d (resp. n_b^d) will be a function of n_b (resp. of n_a). Equilibrium conditions in the two labor markets can therefore be written as: $n_a^d(w_a, \theta_a, n_b) = n_a^s(\theta_a, B_a)$ and $n_b^d(w_b, \theta_b, n_a) = n_b^s(\theta_b, B_b)$ In particular, if n_a and n_b are substitutes and there are diminishing returns to both n_a and n_b, then $\frac{\partial^2 \phi}{\partial n_b \partial n_a}$ will be negative. This means that when the employment of workers of group a decreases (because of REBP for instance), the marginal product of workers of group b, $\frac{\partial \phi}{\partial n_b}$, will increase. Firms will respond by posting more vacancies v_b. This will in turn increase labor market tightness θ_b, bringing up the cost of opening vacancies in the market for group b workers, and decrease the productivity of group b workers, until condition (26) is met again. A decrease in the employment of workers of group a is therefore met by an increase in the employment of workers of group b, when workers are substitutes. The larger the elasticity of substitution σ between group a and group b workers, the larger this substitution effect.

A change in UI benefits B_a for workers of group a in one given market can therefore create market externalities on workers of group b, who are in a separate labor market. These market externalities are given by:

$$\frac{dh_b}{dB_a} = \frac{d(e_b f(\theta_b))}{dB_a} = e_b f'(\theta_b) \frac{d\theta_b}{dB_a} \tag{27}$$

And the equilibrium adjustment in tightness $\frac{d\theta_b}{dB_a}$ is given by implicitly differentiating the equilibrium condition of market for workers of group b, $n_b^d(w_b, \theta_b, n_a) = n_b^s(\theta_b, B_b)$, which gives:

$$\frac{d\theta_b}{dB_a} = \frac{\frac{\partial n_b^d}{\partial n_a} \frac{\partial n_a}{\partial B_a}}{\frac{\partial n_b^s}{\partial \theta_b} - \frac{\partial n_b^d}{\partial \theta_b}} \tag{28}$$

A few points are important to note about equations (27) and (28). First, the existence of market externalities *across* labor markets is entirely driven by the substitution effect. This can easily be seen by noting that $\frac{\partial n_b^d}{\partial n_a}$ on the numerator of the right-hand side of equation (28) is nothing but the marginal rate of technological substitution. When benefits B_a for workers of group a increase, it will lead to a decrease in employment of workers of group a, which will trigger an increase in demand for workers of group b as long as the MRTS is negative. Equilibrium tightness in the market for workers of group b is therefore going to go up, ($\frac{d\theta_b}{dB_a}$ will be positive if $\frac{\partial n_b^d}{\partial \theta_b} > -\infty$) and market externalities on workers of group b will be positive[25]. The higher the elasticity of substitution, the larger $\frac{\partial n_b^d}{\partial n_a}$ and therefore the larger the market externalities on the non-treated labor market.

In terms of empirical identification, the existence of market externalities *across* labor markets through substitution effects means that one needs to be very cautious when choosing the control labor markets for the analysis. The control labor markets must be chosen so as to provide a good counterfactual for what would have happened in the treated labor market in the absence of REBP. At the same time, they must not offer substitution opportunities from the treated labor market.

The second point worth noting is that market externalities on workers of group b, who are now in a separate labor market, are different from market externalities in the treated labor market (workers of group a), contrary to the case where matching was random and the two groups of workers were in the same labor market. This means that in practice, the effect of REBP on the job finding probability of non-treated workers who are not in the same labor market cannot directly identify the market externalities of interest in the treated labor market.

Equation (28) shows that when there are multiple markets, one of them being treated and others not being treated, there will be market externalities in non-treated markets but these externalities cannot directly identify market externalities in the treated market. What can we say then about market externalities in the treated market in this case? How does the existence of substitution opportunities across labor markets affect market externalities in the treated market?

To understand this, let us start by pointing that equation (28) is a partial equilibrium relationship, as n_a is also endogenous to n_b. In general equilibrium, n_a and n_b will be jointly determined, and by Walras law only the ratio of labor market tightness θ_a/θ_b (which is the relative "price" of n_a and n_b in the labor market) will be determined. Using a "Katz & Murphy"-like approach, we can retrieve the relationship between the ratio of labor market tightness θ_a/θ_b and the relative employment level of both groups. From the first order conditions (25) and (26), we have that:

$$\frac{\frac{\partial \phi}{\partial n_a}}{\frac{\partial \phi}{\partial n_b}} = \frac{MC_a}{MC_b} \tag{29}$$

where $MC_a = w_a + \frac{r\psi}{q(\theta_a)}$ is the marginal cost of creating a job of group a and $MC_b = w_b + \frac{r\psi}{q(\theta_b)}$ is the marginal cost of creating a job of group b. If we assume for instance the production function to be CES, $\phi = \left\{(an_a)^\rho + (bn_b)^\rho\right\}^{1/\rho}$, where ρ is the elasticity of substitution between workers

[25]Note, again that with linear technology, $\frac{\partial n_b^d}{\partial n_a^*} = 0$ and we should see no spillover effect across labor markets.

of group a and workers of group b, then it follows that:

$$\frac{\frac{\partial \phi}{\partial n_a}}{\frac{\partial \phi}{\partial n_b}} = (\frac{a}{b})^{\frac{\rho-1}{\rho}} (\frac{n_a}{n_b})^{-\frac{1}{\rho}}$$

Taking logs, we can rewrite equation 29 as:

$$\log \frac{\gamma(\theta_a)}{\gamma(\theta_b)} = \frac{\rho - 1}{\rho} \log \frac{a}{b} - \frac{1}{\rho} \log \frac{n_a}{n_b} - \log \frac{w_a}{w_b} \tag{30}$$

where $\gamma(\theta_a) = 1 + \frac{r\psi}{w_a q(\theta_a)}$ (resp. $\gamma(\theta_b) = 1 + \frac{r\psi}{w_b q(\theta_b)}$) is an increasing function of θ_a (resp. θ_b). Equation 30 shows that an exogenous decrease in the relative labor supply of workers of group a (induced for instance by the REBP policy) will lead to an increase in the relative labor market tightness of workers of group a (the relative "price" of workers of group a in the search market). This effect is a standard substitution mechanism across labor markets. But equation 30 also shows that the larger the elasticity of substitution ρ between the two groups of workers, the smaller the effect of variations in relative labor supply on the relative labor market tightness in the two labor markets. In other words, the existence of substitution opportunities across labor markets will tend to reduce the magnitude of market externalities in the treated market: the more substitutable the two groups of workers are, the lower will be the responsiveness of relative labor market tightness to variations in relative labor supply across markets. The extreme case will be when the treated labor market a is very small and there exists a very large market of perfectly substitutable workers b. In this case, the treated labor market is like a small open economy: labor market tightness in b determines labor market tightness in market a. Labor demand in market a is perfectly elastic at $\theta_a = \theta_b$, and variations in labor supply in market a have no effect on equilibrium labor market tightness θ_a.

The existence of substitution opportunities across labor markets therefore bears important consequences for the interpretation of quasi-experimental results on externalities using variations in unemployment benefits. When the experiment / policy variation is such that the treated population of workers represent a relatively small labor market and there exists non-treated labor markets that offer available substitutes for the treated workers, market externalities in the treated labor market will be relatively small. And estimated equilibrium adjustments in labor market tightness in such a context should be interpreted as a clear lower bound on the equilibrium adjustments in labor market tightness that would occur if the whole population of workers were to be treated.

A.4 Endogenous layoffs

The separation rate ψ as been assumed exogenous. But in practice ψ might be endogenous to UI benefits ($\psi = \psi(B)$) and there is indeed evidence that the separation rate increased for eligible workers during the REBP period (Winter-Ebmer [1996]), implying that $\partial\psi_a/\partial(B_a) > 0$. How will the response of the separation rate to UI benefits affect market externalities of UI? From the definition of labor supply given in equation 7, $n^s = \frac{ef(\theta)}{\psi + ef(\theta)}$, which follows from the equality of flows in and out of unemployment in the steady-state, it appears clearly that an increase in the separation rate ψ will shift labor supply downwards everything else equal. For a given search effort level, and for a given labor market tightness, an increase in the separation rate means that the stock of unemployed will be larger in the steady state and therefore the probability of finding a job (n^s) will be lower. An increase in the separation rate is equivalent to a downward shift in labor supply and its effect on labor supply is comparable to that of a decrease in search effort. If both search effort and the separation rate are responsive to UI benefits, the effect of a change in benefit of workers of group a on labor supply of group a is the sum of a search effort effect ($e'_a \cdot \psi_a$) and of a separation rate effect ($e_a \cdot \psi'_a$):

$$\frac{\partial n^s_a}{\partial B_a} = \frac{[e'_a \cdot \psi_a - e_a \cdot \psi'_a] f(\theta)}{(\psi_a + e_a f(\theta))^2}$$

In the context of REBP, because the separation rate effect $e_a \cdot \psi'_a > 0$ is significantly positive, the downward shift in labor supply of treated workers will be even stronger than if only search effort had reacted to the policy.

But an increase in the separation rate ψ also increases recruiting costs of firms. As new jobs have a higher probability of being terminated, the net present value of a job decreases. This will create a downward shift of n^d that can easily be seen in equation (8) which implicitly determines labor demand of firms n^d as a decreasing function of the layoff rate: $\partial n^d / \partial \psi \leq 0$. So the overall effect on labor market tightness of a change in benefits for workers of group a when layoffs are endogenous is:

$$\frac{d\theta}{dB_a} = p \frac{\frac{\partial n^d_a}{\partial w_a} \frac{\partial w_a}{\partial B_a} + \frac{\partial n^d_a}{\partial \psi_a} \frac{\partial \psi_a}{\partial B_a} - \frac{\partial n^s_a}{\partial B_a}}{\frac{\partial n^s}{\partial \theta} - \frac{\partial n^d}{\partial \theta}} \tag{31}$$

where $\frac{\partial n^d_a}{\partial \psi_a} \frac{\partial \psi_a}{\partial B_a}$ is the layoff rate effect on labor demand. The overall effect of endogenous layoffs on equilibrium adjustments in labor market tightness $\frac{d\theta}{dB_a}$ is therefore ambiguous, as can be seen by comparing equation 31 to equation 18. The presence of endogenous layoffs creates a negative layoff rate effect on labor demand ($\frac{\partial n^d_a}{\partial \psi_a} \frac{\partial \psi_a}{\partial B_a} \leq 0$), which will tend to reduce labor market tightness, but it also increases the magnitude of the shift in labor supply $\frac{\partial n^s_a}{\partial B_a}$ as discussed earlier, which will tend to increase labor market tightness. The relative magnitude of these two effects will therefore determine if endogenous layoffs deepens or attenuates the effect of UI on equilibrium labor market tightness.

B Defining labor markets using vacancy data

Identifying which workers are competing for the same vacancies as REBP eligible workers is critical to determine and define the relevant labor markets that are susceptible of being affected by externalities of the REBP program. As explained in section A.2, when treated and non-treated workers are in the same labor market, i.e. competing for the same vacancies, the effect of the program on non-treated workers can identify equilibrium labor market tightness in the labor market. When treated and non-treated workers are competing for different vacancies, there are in practice two search markets for labor, and the effect of the program on non-treated workers cannot directly identify equilibrium adjustments in the treated market.

To determine which workers are competing for the same vacancies as REBP eligible workers, we use detailed micro data on the universe of job vacancies posted in public employment agencies available for the period 1994-1998. In 2012, vacancies posted in public employment agencies represented 36% of all vacancies posted This data has two important features. First, the data records for each vacancy all the detailed information about the characteristics of the vacancy. This includes the firm identifier of the firm posting the vacancy, the date (in month) at which the vacancy is opened and the date at which it is closed, the reason for closing the vacancy (the vacancy has been filled, search has been abandoned, etc.), the identifier of the public employment service where the vacancy is posted, the industry and job classifications of the job, details on the duration and type of the contract (full-time,/part-time tenured/non-tenured, seasonal job, etc.), the age requirement if any, the education requirement if any, the gender requirement if any, and the posted wage or range of wage if any. Second, the data contains the personal identifier of the person who filled the vacancy if the vacancy is filled. This personal identifier enables us to match this vacancy data to the ASSD and determine the characteristics and REBP eligibility status of the person filling the vacancy.

Our strategy consists in using all the information that we have on each vacancy, and estimate how well the characteristics of each vacancy predicts the REBP eligibility status of the worker who fills the vacancy. If there is perfect discrimination in vacancies between eligible and non-eligible workers, then eligible and non-eligible workers will be competing for two different sets of vacancies and will effectively be in two different labor markets from a search-theoretic perspective. Empirically, this means that characteristics of vacancies for eligible and non-eligible workers are different, and therefore characteristics of vacancies should predict very well whether the individual filling the vacancy is eligible to REBP or not. To the contrary, if eligible and non-eligible workers are in the same job-search market, they will compete for the same vacancies. When opening a vacancy in this market, and conditional on search effort of eligible and non-eligible workers, a firm will be randomly matched to an eligible or to a non-eligible worker. In other words, conditional on search effort of eligible and non-eligible workers, matching is random across eligible and non-eligible workers and vacancies in this market will be filled (randomly) by eligible or non-eligible workers. In this case, the characteristics of a vacancy will have very little predictive power on the eligibility status of the worker who fills it.

To implement this strategy, we take all vacancies opened by firms located in REBP regions that ended up being filled (by REBP eligible or non-eligible male workers) during the REBP period covered in the vacancy data, which means 1994 to 1998. We estimate the following latent variable model:

$$Y_i^* = X_i'\beta + \epsilon_i$$

$$Y_i = \begin{cases} 0 & \text{if} \quad Y_i^* < 0 \\ 1 & \text{if} \quad Y_i^* \geq 0 \end{cases}$$

where Y_i is a dummy variable indicating whether the worker filling vacancy i is eligible to REBP or not, and X_i is a vector of all the characteristics of vacancy i. These characteristics are the two-digit industry code of the firm opening the vacancy, the two-digit occupation code of the job, the duration of the contract (temporary contract, unlimited contract, seasonal job, holiday work, etc.), whether the job is full-time, part-time or flexible hours, whether the job hours are negotiable or not, whether the job implies shift work, whether it implies night or extra hours work, whether the job is an apprenticeship, the size of the firm (in 5 categories), the age required for the job if any, and the level of education required for the job (in 17 categories) if any. We estimate this model using a logit. We run the model separately for various categories of non-eligible workers (30 to 40 years old workers, 40 to 45 years old workers, 45 to 50 years old workers, and 50-54 years old non-eligible workers) in order to compare each of these categories of workers to REBP eligible workers. For each of the categories of non-eligible workers, we then analyze the predictive power of the model using various goodness-of-fit measures.

In figure 2 panel A, we start by plotting the fraction of observations that are incorrectly predicted by the model (*i.e.* the predicted eligibility status to REBP is different from the true eligibility status of the worker filling the vacancy) for all categories of non-eligible workers. The fraction of misclassified observations is less than 7.5% for the model comparing eligible workers to non-eligible workers aged 30 to 40, but increases up to more than 25% for the model comparing eligible workers to non-eligible workers aged 50 to to 54. We also plot in the same figure the p-value from the Pearson's χ^2 test for categories of non-eligible workers. This test is a standard goodness-of-fit test for logistic regressions. A low p-value for the test indicates a poor fit of the data. This test also confirms that the model fits the data very well for comparing eligible workers to non-eligible workers aged 30 to 40, but tend to perform more and more poorly as we use non-eligible workers that are older. When comparing eligible workers to non-eligible workers aged 50 to 54, the p-value is very close to zero, and the goodness-of-fit of the model is extremely poor. This suggests that the predictive power of vacancy characteristics on eligibility is very good when comparing workers that are 30 to 40 to eligible workers, but very low when comparing eligible and non-eligible workers aged 50 to 54. In other words, workers age 30 to 40 seem to fill vacancies that have characteristics that are very different from the vacancies filled by eligible workers. But eligible and non-eligible workers above 50 seem to fill vacancies that have very similar characteristics. This suggests that workers aged 30 to 40 are likely to be in a different job search market than eligible workers, but non-eligible workers aged 50 to 54 are very likely to compete for the same vacancies as eligible workers.

In panel B of figure 2, we also plot the fraction of type I errors, *i.e.* the fraction of true non-eligible workers that are predicted as being eligible to REBP by the model. Type I errors are particularly relevant in our context. They provide information about how likely it is that a non-eligible worker is competing for a vacancy that has been "tailored" to eligible workers based on its characteristics. In this sense, type I errors provide direct information about the intensity of the competition that eligible workers receive from various groups of non-eligible workers when a vacancy is opened in "their" search market. The figure indicates that type I errors seem to be particularly severe when comparing eligible workers to non-eligible workers aged 50 to 54. Because classification is sensitive to the relative sizes of each component group, and always favors classification into the larger group, we also investigate the fraction of type I errors relative to a perfectly random matching. To do so, we run the same model on a sample of random matches where the size of each component group is kept unchanged. In the perfectly random case, type I errors will also vary depending on the relative size of each non-eligible group compared to eligible workers. We then plot the fraction of type I errors of the original model compared to the fraction of type I errors in the perfectly random matching case. This gives us a sense of how close to perfectly random the matching is between eligible workers and the different groups of non-eligible, controlling for the different relative sizes of these groups. Again, we find that the matching process is almost not random at all between eligible workers and workers aged 30 to 40, while it is 50% random between between eligible workers and non-eligible workers aged 50 to 54.

These results help inform our identification strategy and choose the proper groups of non-eligible workers to identify the presence of externalities. The results indicate that it is much more likely for non-eligible workers aged 50 and over to compete for the same vacancies as eligible workers than for non-eligible workers aged below 50. This means that non-eligible workers aged 50 and above are likely to be in the same job-search market as eligible workers, while non-eligible workers aged below 50 tend to compete for different vacancies and are therefore in a different job-search market. This means that the effect of REBP on job-finding probabilities of eligible workers aged 50 and above is more likely to identify variations in labor market tightness in the job-search market of REBP-treated workers. As explained in section A.2, these variations in labor market tightness in the job-search market of REBP-treated workers capture both the rat race effect and the wage effect of UI, and are the relevant variations to consider to identify the equilibrium effect of variations in UI in a given labor market.

Non-eligible workers below 50 years old, to the contrary, seem to be competing for different vacancies than workers eligible to REBP. This means that they are more likely to operate in a different search market than workers eligible to REBP. The effect of REBP on their job finding probability is therefore more likely to identify externalities across search markets. In section A.3, we have shown that such externalities stem from substitution effects, and cannot directly identify the effect of REBP on the labor market tightness in the search market of treated workers.

Table 6: Sensitivity of Baseline Results to Inference Assumptions

	(1) Unemployment duration	(2) Non-employment duration	(3) Spell > 100 wks	(4) Spell > 26 wks
β_0	43.37	29.17	0.240***	0.237***
Baseline cluster	(5.069)***	(5.444)***	(0.0293)***	(0.0240)***
Region cluster	(4.581)***	(4.867)***	(0.0247)***	(0.0278)***
Spatial HAC	(4.319)***	(4.785)***	(0.0230)***	(0.0250)***
Permutation	(1.143)***	(0.930)***	(0.0077)***	(0.0099)***
γ_0	-3.740	-2.327	-0.0130	-0.0165
Baseline cluster	(0.758)***	(0.629)***	(0.00311)***	(0.00660)**
Region cluster	(0.798)***	(1.004)**	(0.00231)***	(0.00585)***
Spatial HAC	(0.862)***	(1.012)**	(0.00287)***	(0.00889)*
Permutation	(1.528)**	(1.124)**	(0.00519)**	(0.00880)*
N	262344	232135	262344	262344

Notes: * p<0.10, ** p<0.05, *** p<0.010. This table reports the main result from Table 2. Numbers in parentheses display standard errors. Baseline standard errors allow for clustering at the region * year level. Region cluster standard errors allow for clustering at the level of the region, i.e. a county × education × industry cell – this is the classification we use to detect market externalities in Table 5 of the paper. Spatial HAC standard errors allow for any correlation in errors in a circle of 33 kilometers around a job seeker's location, and zero correlation beyond that. Spatial HAC standard errors also allow for full correlation between spells starting in the same quarter, one half correlation between spells that start one quarter apart, and no correlation beyond. Permutation standard errors are based on 235 placebo estimates of simulations of the REBP program during non-REBP time periods.

Source: Own calculations, based on ASSD.

Overall, the vacancy data is useful to determine the scope of the different job search markets. This analysis indicates that the externalities that we may find on non-eligible workers may be very different in nature and in magnitude across different groups of non-eligible workers. Non-eligible workers aged 50+ are more likely to experience larger externalities stemming from equilibrium adjustments in labor market tightness in the search market of workers eligible to REBP. Non-eligible workers that are younger than 50 are more likely to experience externalities stemming from substitution effects across search markets.

C Additional tables and figures

Table 7: Sensitivity analysis to sample restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment duration			Non-empl. duration	Spell >100 wks	Spell >26 wks
	A. Men, 46 to 59, excluding steel sector					
β_0	50.20***	44.84***	43.82***	33.60***	0.254***	0.222***
	(3.607)	(3.300)	(3.210)	(5.165)	(0.0192)	(0.0155)
γ_0	-2.680***	-2.133***	-3.222***	-2.514***	-0.00912***	-0.0139**
	(0.782)	(0.657)	(0.608)	(0.527)	(0.00240)	(0.00545)
N	378556	369477	369477	304664	369477	369477
	B. Men and women, 46 to 54, excluding steel sector					
β_0	55.93***	52.28***	51.80***	40.59***	0.297***	0.238***
	(3.549)	(3.472)	(3.319)	(5.147)	(0.0192)	(0.0163)
γ_0	-2.241***	-1.307**	-3.217***	-1.892***	-0.0103***	-0.0106**
	(0.781)	(0.648)	(0.682)	(0.608)	(0.00297)	(0.00522)
N	359901	351433	351433	296768	351433	351433
	C. Men, 46 to 54, including steel sector					
β_0	47.33***	43.82***	43.85***	30.58***	0.242***	0.238***
	(5.534)	(5.108)	(5.045)	(5.603)	(0.0290)	(0.0237)
γ_0	-2.248***	-1.809**	-3.581***	-2.228***	-0.0119***	-0.0158**
	(0.825)	(0.730)	(0.785)	(0.632)	(0.00304)	(0.00700)
N	284099	278021	278021	245621	278021	278021
Educ., industry, citizenship, marital status		×	×	×	×	×
Region-specific trends			×	×	×	×

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 (3) where we explore the sensitivity of our baseline results to various sample restrictions. 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) to (6) we add controls for preexisting trends by region. Column (5) uses as an outcome the duration of total non-employment (conditional on finding employment at the end of the unemployment spell). Columns (6) and (7) use as an outcome the probability of experiencing unemployment spells longer than 100 weeks and 26 weeks respectively. In panel A, the estimation sample includes all men age 46 to 59. In panel B, the sample includes all men and women age 46 to 54. In panel C, the sample is the same as our baseline sample but also includes workers who ever worked in the steel sector.

Table 8: ROBUSTNESS TO REBP-COUNTIES-SPECIFIC SHOCKS: 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	54.32***	50.81***	30.30***	30.29***	0.312***	0.275***
	(7.480)	(6.784)	(7.639)	(7.192)	(0.0432)	(0.0362)
γ_0 (externality)	-7.878**	-6.466*	-7.643***	-6.347**	-0.0742***	-0.0554**
	(3.880)	(3.437)	(2.156)	(2.461)	(0.0222)	(0.0213)
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. We use the same strategy as in table 2 but we use men aged 30 to 39 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 (3) 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 9: Externalities on non-eligible unemployed by initial level of labor market tightness

REBP effect on non-treated	(1) Unemployment duration	(2) Non-empl. duration	(3) Spell >100 wks	(4) Spell >26 wks
	All non-eligible			
$\gamma_0^{High\ \theta}$ ($\theta \geq P50$)	0.728	-1.650	0.00877	-0.0208
	(1.411)	(1.088)	(0.00571)	(0.0125)
$\gamma_0^{Low\ \theta}$ ($\theta < P50$)	-2.250***	-1.809**	-0.00457*	-0.00936
	(0.726)	(0.733)	(0.00255)	(0.00657)
F-Test $\gamma_0^{Low\ \theta} = \gamma_0^{High\ \theta}$	[0.0635]	[0.910]	[0.0530]	[0.422]
N	262109	231940	262109	262109
	Non-eligible 50+			
$\gamma_0^{High\ \theta}$ ($\theta \geq P50$)	-1.317	-2.788	0.00878	-0.0309
	(4.073)	(2.745)	(0.0181)	(0.0204)
$\gamma_0^{Low\ \theta}$ ($\theta < P50$)	-7.539***	-5.999**	-0.0167**	-0.0312*
	(2.334)	(2.407)	(0.00801)	(0.0180)
F-Test $\gamma_0^{Low\ \theta} = \gamma_0^{High\ \theta}$	[0.0530]	[0.320]	[0.114]	[0.992]
N	122174	102598	122174	122174
Educ., marital status, industry, citizenship	×	×	×	×

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 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 the initial level of labor market tightness in county×industry×education cells. Initial labor market tightness is obtained by dividing the average monthly number of vacancies posted in 1990 (the first year for which we have some vacancy information by county) in each county×industry×education cell, by the average monthly number of unemployed in the same county×industry×education cell. $\gamma_0^{High\ \theta}$ identifies externalities of REBP on non-treated workers in REBP county×industry×education cells where labor market tightness was above the median level of tightness in 1990. $\gamma_0^{Low\ \theta}$ identifies externalities of REBP on non-treated workers in REBP county×industry×education cells where labor market tightness was below the median level of tightness in 1990.

D Wages

D.1 Effect of REBP on reemployment wages

As highlighted in section 2 and explained formally in appendix section A, one of the key requirement for externalities to be positive on non-eligible workers 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.[26]

The identification of the effect of REBP on wages is very different from our previous market externality analysis, as we now wish to compare eligible workers to non-eligible workers (rather than non-eligible in treated and non treated markets). The identification of the effect of REBP on wages is difficult for at least three reasons. First, REBP treatment is correlated with longer unemployment duration, which may directly affect wages through duration dependence effects. 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:

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

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.

The second issue is that REBP treatment affects the probability of entering into unemployment and REBP recipients may therefore be selected along unobserved characteristics that are correlated with wages. Treatment is also correlated with the probability of ever reentering the labor force, which creates additional selection issues when looking at reemployment wages.

The third issue is that REBP affects labor market tightness, which will in turn affect the bargaining power of workers. It is thus difficult to separate what is the pure reservation wage effect from other equilibrium effects affecting wages.

We try to address these issues in the following analysis, but we want to stress that our analysis remains tentative. To deal with the first issue, 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 3, and then in a RD setting taking advantage of the age eligibility discontinuity at 50 and experience eligibility discontinuity at 15 years. 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

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

and unemployment benefits is zero.

We plot in appendix figure 6 post-unemployment wages conditional on the duration of the unemployment spell in REBP and non-REBP counties for eligible 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 effect of REBP on reemployment wages conditional on spell duration. This evidence suggests that there was no effect of REBP on reemployment wages.

We formally assess this result in appendix table 10 by running a simple diff-in-diff model where we compare workers eligible to REBP (treatment) to non-eligible workers (control). Each panel uses a different control group. In panel A, we use workers aged 50 to 54 with more than 15 years of experience but residing in non-REBP regions. In panel B we use workers aged 50 to 54 residing in REBP regions but with less than 15 years of experience. In panel C we use workers aged 46 to 49 with 15 years of experience and residing in REBP regions. In column (1), we estimate the model without further controls. In column (2) we add a vector of controls including education, 15 industry codes, family status, citizenship and tenure in previous job. These specifications tend to deliver a negative effect of REBP on reemployment wages. This negative effect may well be driven by selection into unemployment. We know from table 3 that REBP has affected the inflow rate into unemployment of eligible workers. This means that the selection of eligible workers may be different during REBP. We try to control for this using pre-employment wages. In column (3) we add a rich set of pre-unemployment wage dummies to control for potential differential self-selection into unemployment due to REBP. As explained above, the negative effect on reemployment wages found in column (1) and (2) can also be due to duration dependence effects. 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), in column (4), and 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. In this preferred specification of column (4), irrespective of the control group we are using, we always find no significant effect of REBP on reemployment wages.

To complement our diff-in-diff approach, we also 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.

$$E[Y|A=a] = \sum_{p=0}^{\bar{p}} [\gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[A \geq k]] + \sum_{t=0}^{T} \mathbb{1}[D=t] \tag{32}$$

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. We use a third-order polynomial specification. Results are displayed in appendix 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 .14 (.04). This discontinuity then decreases and disappears when REBP is over. This suggests that wages are relatively rigid in the short run, but that in the longer run, wages might adjust to variations in outside options of workers. Note however that the McCrary test rejects continuity of the probability density function of the assignment variable (age) at the cutoff (50 years) during REBP. This implies that the wage effects could also partly be driven by selection (sorting) at the 50 years age cut-off.

We finally exploit the experience eligibility discontinuity in REBP counties using the same methodology. Results are displayed in appendix figure 9. The figure displays for REBP regions the relationship between experience in the past 25 years at the beginning of unemployment spell and reemployment wages for workers aged 50 to 54. We use the discontinuity created by the fact that workers with more than 15 years of experience are eligible for REBP extensions while workers with less than 15 years are not eligible. The graph shows the average reemployment wage for each bin of 6 months of past experience for all non REBP years and for all REBP years. We also estimate a model of the form: $E[Y|E=e] = \sum_{p=0}^{\bar{p}} \gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[E \geq k] + \sum_{t=0}^{T} \mathbb{1}[D=t]$, where Y is real reemployment wage, E is experience at the beginning of the unemployment spell, $k = 15$ is the experience 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 all non REBP years and for all REBP years using a 3rd order polynomial for the regressions. Here, we find no evidence of an effect of REBP on reemployment wages. Note again however that McCrary tests rejects continuity in the probability density function of the assignment variable (experience) at the cutoff (15 years) during REBP.

Overall, this evidence, although tentative, suggests that wages of eligible workers did not strongly respond to REBP, which is in line with the market externalities that we find. Yet, we cannot exclude that these results are confounded by selection, nor can we exclude that wages would have adjusted in the very long run.

D.2 Implications of these results for the wage setting process

What can we learn on the wage setting process from this empirical evidence? Is this evidence, combined with other available evidence, compatible with Nash bargaining?

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.

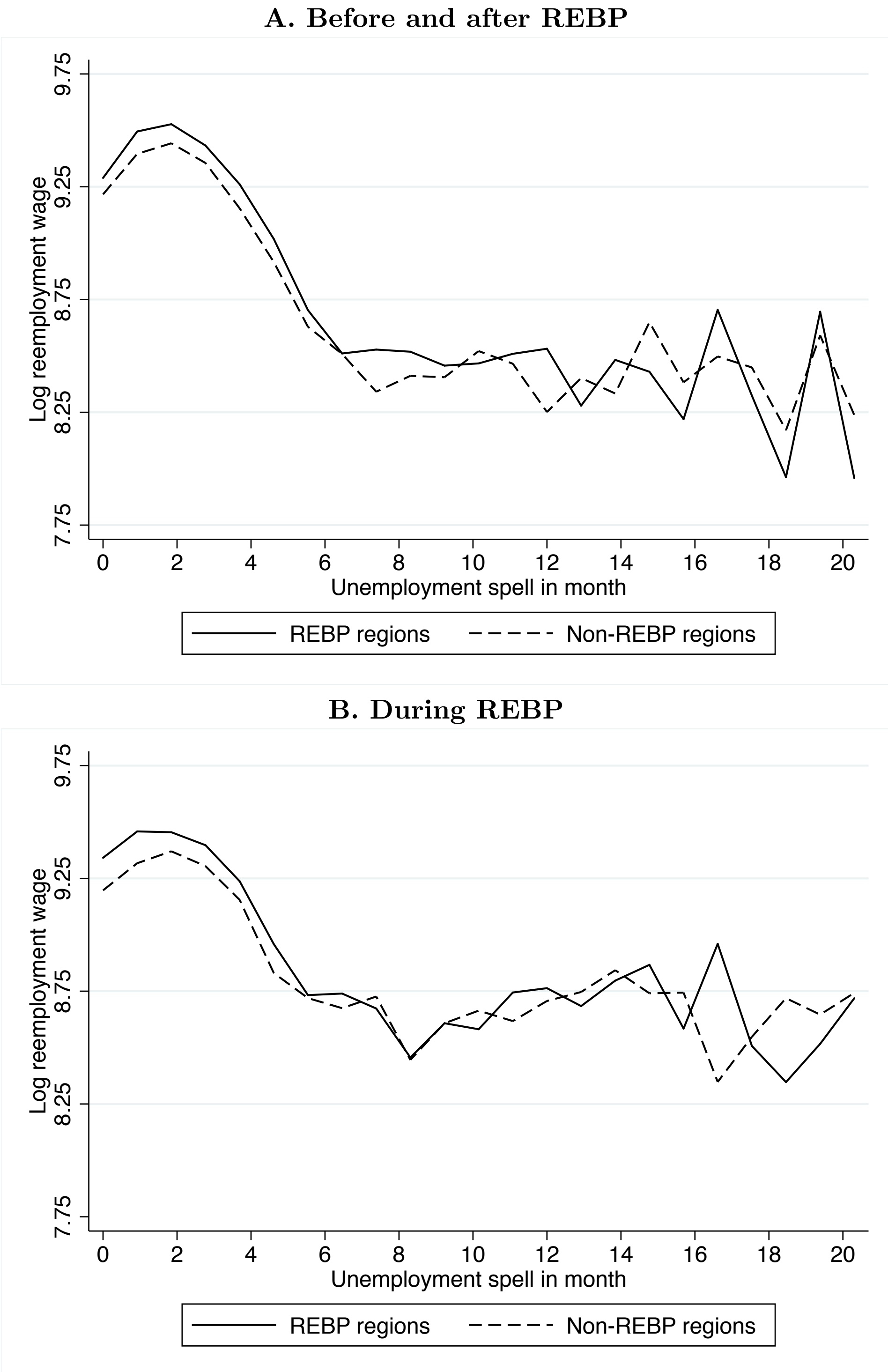
In a standard DMP model with Nash bargaining, the wage w is a weighted average of the productivity of the worker Π (which determines the reservation price of the employer) and of the value of remaining unemployed z (which determines the reservation price of the unemployed):

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

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[27] 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 the "wage externality" would be very large, shocking labor demand down as in figure 5 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.

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

Figure 6: Reemployment wages conditional on duration of unemployment spell in REBP and non-REBP counties



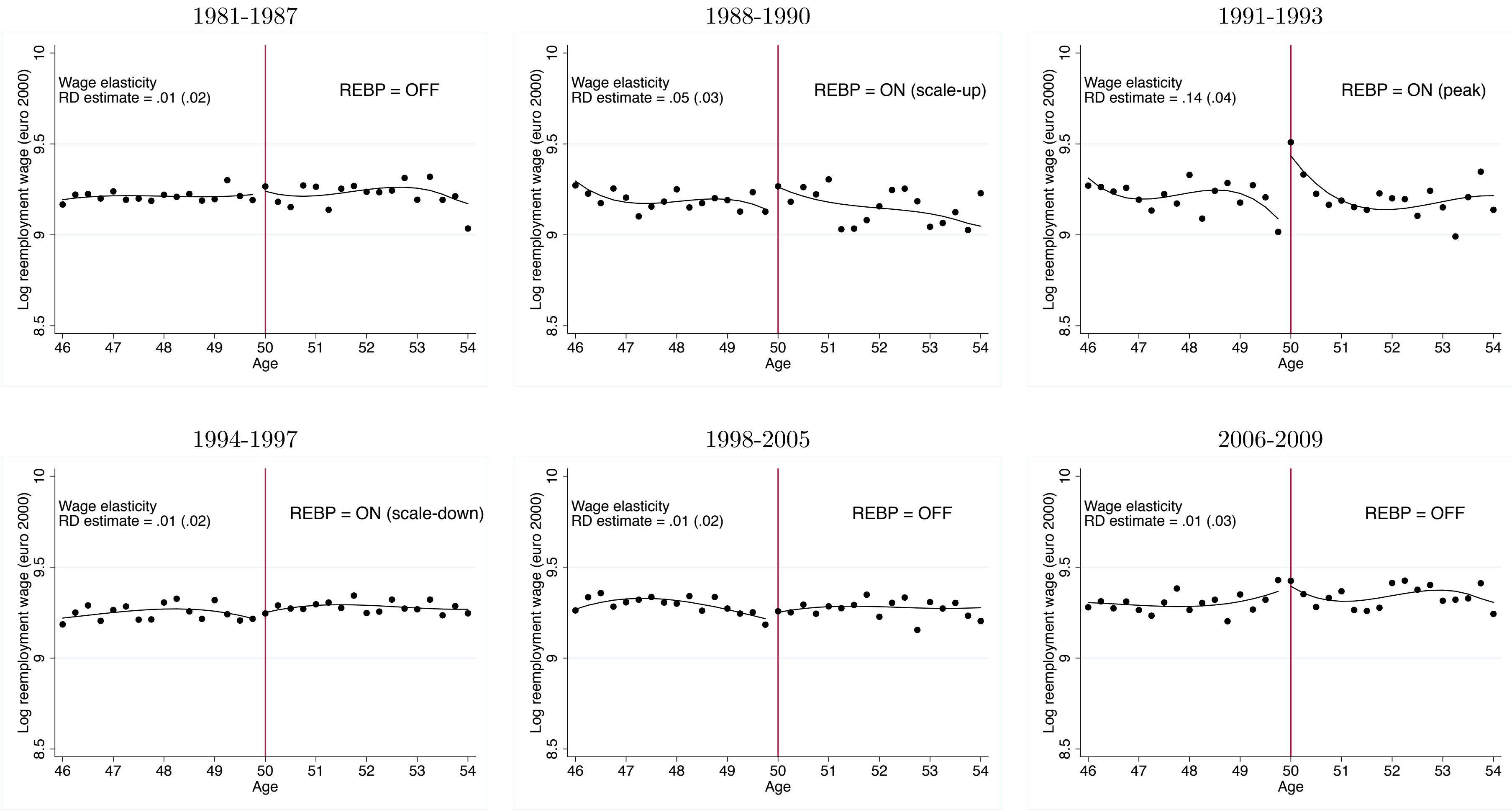
Notes: the figure plots post-unemployment wages conditional on the duration of the unemployment spell in REBP and non-REBP counties for workers aged 50 to 54 with more than 15 years of experience in the past 25 years prior to becoming unemployed. Following the methodology of Schmieder et al. [2012a], 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.

Table 10: Diff-in-diff estimates of the effects of REBP on wages

	(1)	(2)	(3)	(4)
	log reemployment wage			
	A. Control: eligible workers 50-54 in non-REBP regions			
REBP × eligible	-0.0291**	-0.0403**	-0.0589***	-0.00895
	(0.0133)	(0.0153)	(0.0183)	(0.0123)
N	77743	76501	75594	76501
	B. Control: non-eligible workers 50-54 in REBP regions			
REBP × eligible eligible	-0.101	-0.0913	-0.0473	-0.0891
	(0.0820)	(0.0820)	(0.0867)	(0.0591)
N	23278	22996	22781	22996
	C. Control: non-eligible workers 46-50 in REBP regions			
REBP × eligible	0.00550	-0.0144	-0.0313	0.000967
	(0.0228)	(0.0286)	(0.0240)	(0.0242)
N	46701	46251	45826	46227
Educ., marital status, industry, citizenship		×	×	×
Pre-unemployment wage dummies			×	
Set of dummies for duration of U spell				×

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. The specification is a diff-in-diff where we compare workers eligible to REBP (treatment) to non-eligible workers (control). Each panel uses a different control group. In panel A, we use workers aged 50 to 54 with more than 15 years of experience but residing in non-REBP regions. In panel B we use workers aged 50 to 54 residing in REBP regions but with less than 15 years of experience. In panel C we use workers aged 46 to 50 with 15 years of experience and residing in REBP regions. Column (1) runs a basic diff-in-diff specification using log reemployment wages as an outcome with no additional controls. In column (2) we add a vector of controls including education, 15 industry codes, family status, citizenship and tenure in previous job. In column (3) we add a rich set of pre-unemployment wage dummies to control for potential differential self-selection into unemployment due to REBP. In column (4), 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).

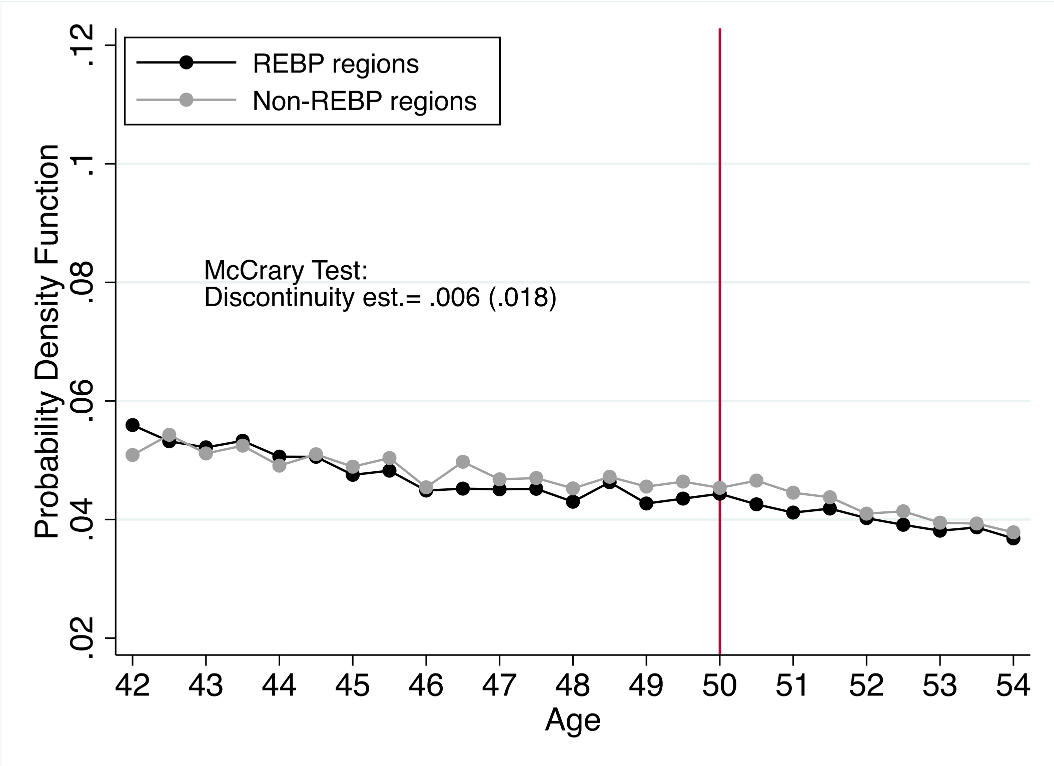
Figure 7: RD evidence on wage bargaining over time: relationship between age and reemployment wages in REBP counties



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 2006-2009). All regressions use a 3rd order polynomial specification. 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.

Figure 8: Probability density function of age at the start of an unemployment spell in REBP and non-REBP counties

A. Before REBP



B. During REBP

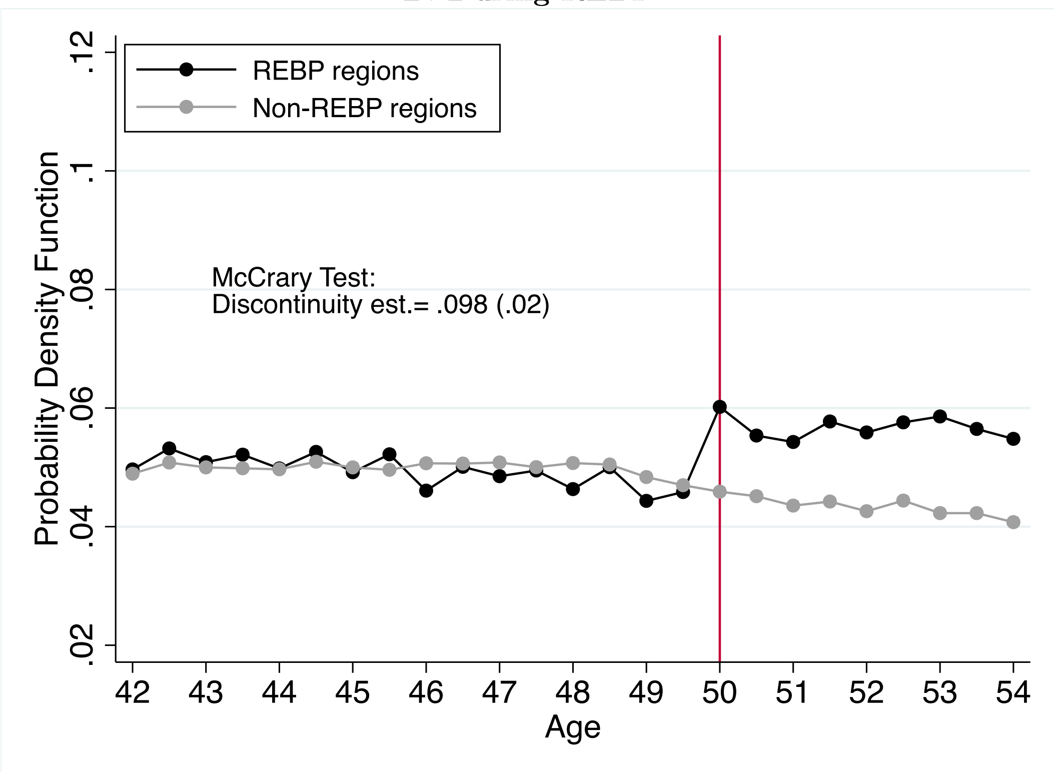
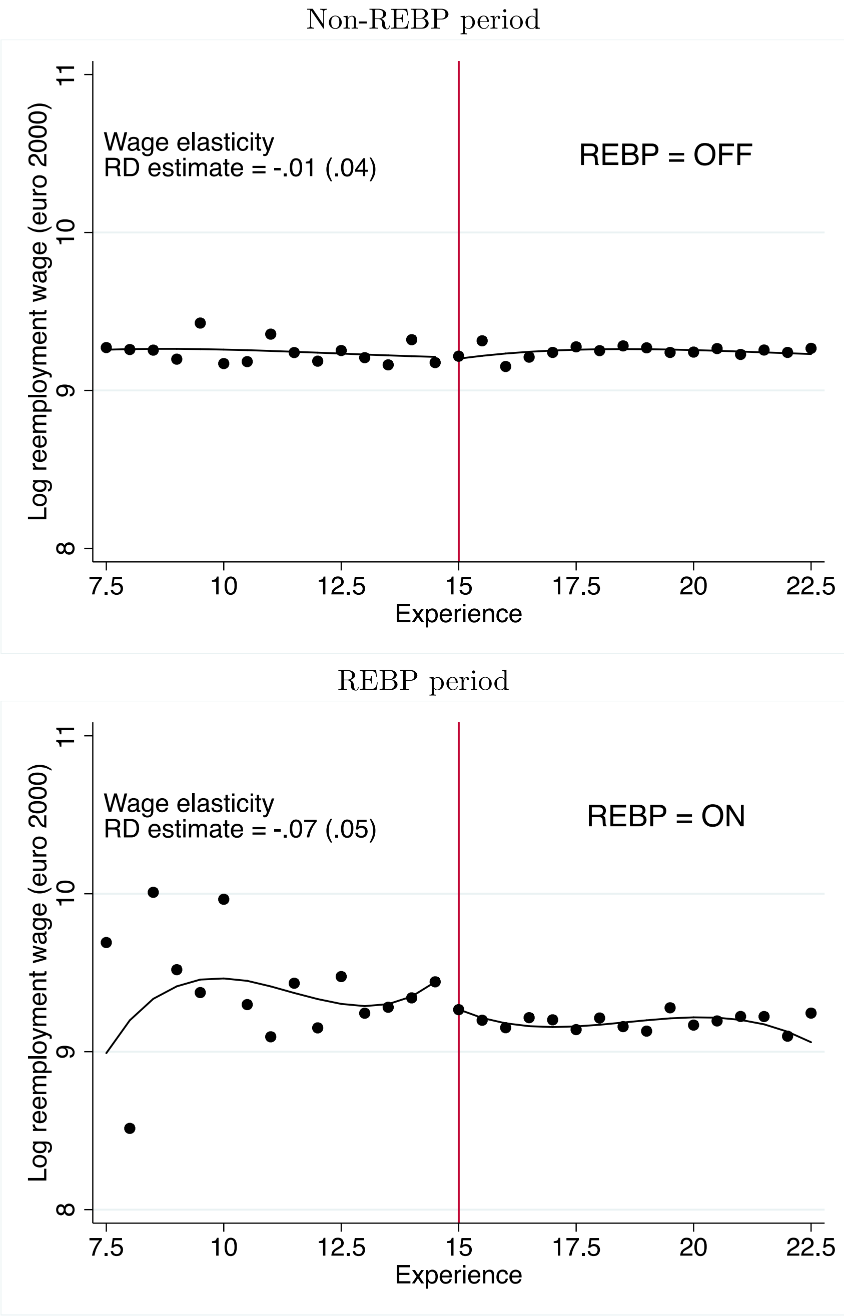


Figure 9: RD evidence on wages using experience cutoff: relationship between experience and reemployment wages in REBP counties



Notes: the figure displays for REBP regions the relationship between experience in the past 25 years at the beginning of unemployment spell and reemployment wages for workers aged 50 to 54. Workers with more than 15 years of experience are eligible for REBP extensions while workers with less than 15 years 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. $E[Y|E=e] = \sum_{p=0}^{\bar{p}} \gamma_p (a-k)^p + \nu_p (a-k)^p \cdot \mathbb{1}[E \geq k] + \sum_{t=0}^{T} \mathbb{1}[D=t]$, where Y is real reemployment wage, E is experience at the beginning of the unemployment spell, $k=15$ is the experience 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 all non REBP years and for all REBP years using a 3rd order polynomial for the regressions.