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Market Externalities of Large Unemployment Insurance Extension Programs[†]

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We provide evidence that unemployment insurance affects equilibrium conditions in the labor market, which creates significant "market externalities." We provide a framework for identification of such equilibrium effects and implement it using the Regional Extension Benefit Program (REBP) in Austria which extended the duration of UI benefits for a large group of eligible workers in selected regions of Austria. We show that non-eligible workers in REBP regions have higher job finding rates, lower unemployment durations, and a lower risk of long-term unemployment. We discuss the implications of our results for optimal UI policy. (*JEL* E24, J64, J65, R23)

The probability that an unemployed individual finds a job depends on her job search strategy and on labor market conditions determining how easy (or difficult) it is to be matched to a potential employer.[1] 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. Changes in UI policies also affect equilibrium labor market conditions which in turn will affect the job finding probability for any given search strategy. We call this second effect *market externalities* of UI.

The micro effect can be identified by comparing two individuals with different levels of UI generosity in the same labor market. A large number of well-identified

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[1]Setting a job search strategy involves decisions such as: how hard to search, what jobs to search for, how to set one's reservation wage, etc. Labor market conditions depend on the number of job searchers (and the intensity with which they search), on the number of available jobs, and on the extent to which labor market frictions inhibit immediate matching of job searchers to open vacancies.

estimates of the micro effect have shown that more generous UI benefits tend to increase unemployment duration.[2] In contrast, evidence on market externalities is scarce. The aim of this paper is to bridge this gap.

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*, consists of both the micro effect and market externalities. Studies comparing individuals subject to differential UI benefit generosity within the same labor market identify the micro effect. These studies cannot shed light on the true effect of UI if externalities are important. Second, market externalities have first order welfare effects, as shown in Landais, Michaillat, and Saez (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 it is empirically challenging to estimate market externalities because general equilibrium effects are typically hard to identify. Recent papers have tried to directly estimate equilibrium effects of active labor market policies such as randomized programs of counseling for job seekers without reaching a clear consensus (Blundell et al. 2004; Ferracci, Jolivet, and van den Berg 2014; Gautier et al. 2012).[3] More recently, Crépon et al. (2013) 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 percent) 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 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 United States 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 significant market externalities.

In this paper we shed new light on market externalities of UI. First, we show how market externalities can be identified in a quasi-experimental setting by looking at

[2] See, for instance, Krueger and Meyer (2002) for a survey of early studies. More recent studies include Landais (forthcoming) for the United States; Schmieder, von Wachter, and Bender (2012b) for Germany; or Winter-Ebmer (1998) and Lalive and Zweimüller (2004a, b) for Austria.

[3] Blundell et al. (2004) study the effect of a counseling program for young unemployed in the United Kingdom and find little evidence of displacement effects. Ferracci, Jolivet, and van den Berg (2014) 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 nonparticipating counties, their results suggest the presence of substantial negative spillovers.

the effect of a UI benefit variation in a given labor market on job search outcomes of workers who are not eligible to the UI benefit variation but who search in the same labor market. We define the relevant 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 benefit 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 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 to meet the eligibility requirements of the REBP program. Using a difference-in-differences 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 a compelling empirical setting to study market externalities of UI. First, treated workers received an *extra three years* of covered unemployment with an unchanged benefit level. This large UI extension generated a strong increase in unemployment duration of treated workers thereby manipulating equilibrium labor market conditions (Lalive and Zweimüller 2004b). Second, REBP was enacted only in a subset of regions (28 of about 100 regions) and, within treated regions, 90 percent of workers above 50 years old were eligible to the program. This allows us to study how ineligible job seekers in REBP regions compare to similar workers in non-REBP regions. 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 the 1980s. By matching data from the unemployment register with social security data on the universe of employment spells in Austria since 1949, we can determine eligibility status for the REBP program along all eligibility dimensions. Our data also enable us to look at many different outcomes, from unemployment and nonemployment durations, to reemployment characteristics and wages. As the data cover sufficiently long periods before and after the REBP program, 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 sizable market externalities of UI. REBP induced a 2 to 4 week *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 for 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 abolished. 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 also 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 finally 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.

Our empirical findings have important policy implications. First, the presence of significant market externalities implies that the micro and the macro effect of UI extensions differ. Our estimates imply a significant wedge between the micro (e^m) and the macro (e^M) effect of UI extensions on the job finding rate of workers in labor markets treated by REBP: $W = 1 - e^M/e^m \approx 0.21$. In the REBP setting, a segment only of the labor force was treated, and substitution opportunities to treated workers were potentially available in non-treated labor markets. We show that our estimated wedge is therefore a lower bound on the magnitude of the wedge when the whole labor force is treated by a change in UI benefits. Second, our results bear important implications for the design of optimal UI policies. Our results imply that more generous UI benefits increase labor market tightness and the job finding rate per unit of search effort. As a consequence, the optimal level of UI will be larger than suggested by the partial equilibrium Baily-Chetty formula (Chetty 2006), as explained in Landais, Michaillat, and Saez (2010). This means that temporary extensions enacted in reaction to business cycles downturns are less socially costly than what a partial equilibrium representation would suggest.

The remainder of the paper is organized as follows. Section I presents our theoretical framework, explains the concept of market externalities and how they can be identified. Section II presents the institutional background of the REBP program. Section III presents the data and our empirical strategy. It also shows how we can use vacancy data to identify groups of non-treated workers competing with treated workers for jobs in the same labor market. Section IV presents the results as well as our robustness and heterogeneity analysis. Section V draws welfare and policy implications.

I. 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.

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

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. However, changes in UI generosity 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 externalities of UI. First, when the generosity of UI varies, for instance, due to UI benefit extensions such as the recent Emergency Unemployment Compensation (EUC) program in the United States, the total effect on unemployment will be the sum of the micro effect and of market externalities. Studies comparing individuals with different UI benefits 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, Michaillat, and Saez (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, Michaillat, and Saez (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. Online 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 when there is a fixed number of jobs, an increase in an individual's search effort will increase her probability of finding a job. However, this must come at the expense of the probability of all other unemployed to find a job as the total number of jobs remains unchanged. Hence an increase in UI generosity, by decreasing aggregate search effort, increases the probability of finding a job per unit of search effort $f(\theta)$. The rat race effect creates a positive market externality.

The wage effect arises when wages are determined through a bargaining process. An increase in UI generosity improves workers' outside options and tends to increase wages. This decreases the return from opening vacancies for firms, leading to a decrease in labor demand. Thus the wage effect creates 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

[5] Diminishing returns is a sufficient but not a necessary condition for the presence of a downward sloping labor demand. Landais, Michaillat, and Saez (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.

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.

To identify market externalities, our strategy compares two groups of workers who are searching for jobs in the same labor market. The first group is "treated" and experiences an exogenous change of UI generosity, while the second group is not treated and does not experience any change in UI benefits. The individual search effort of treated workers will respond, changing their job finding probability. This change in search effort will also affect equilibrium labor market tightness and, therefore, the job finding probability per unit of search effort, creating labor market externalities. The change in the job finding probability of non-treated workers will capture these market externalities.

In online Appendix A.2, we show under which conditions a change in the job finding probability of non-treated workers can identify labor market externalities. 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 same vacancies. From a search-theoretic standpoint, this definition is the most natural: it follows from the law of one price, which defines one equilibrium labor market tightness for each labor market. In practice, 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. In other words, when treated and non-treated workers compete for these vacancies, 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 in online Appendix A.2 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 and that, as 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. Importantly, 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 online Appendix A.3, we also discuss the case when treated and non-treated workers do not compete for the same vacancies, for instance, because firms can discriminate between treated and non-treated workers by offering them different types of vacancies. In that case, non-treated workers will not be in the same labor market as treated workers and 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. Yet, UI variations for treated workers may nevertheless still create externalities for non-treated workers. As shown in online Appendix A.3, such externalities will arise across labor markets due to substitution effects and are different in nature and magnitude from market externalities *within* a labor market. The existence

of externalities across labor markets due to substitution effects bears implications for the interpretation of our results that we discuss in Section V.

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.

II. Austrian Unemployment Insurance and the REBP

Unemployment Insurance and Wage Setting Systems.—The Austrian UI system is more restrictive than many other continental European systems and closer to the US system in terms of generosity. Workers who become unemployed can draw regular unemployment benefits (UB), the amount of which depends on previous earnings. In 1990, the replacement ratio (UB relative to *gross* monthly earnings) was 40.4 percent for the median income earner; 48.2 percent for a worker earning half the median; and 29.6 percent for a worker earning twice the median. UB payments are not taxed, not means-tested, and there is no experience rating.

The maximum number of weeks that one can receive UB (potential duration) depends on work history (the number of weeks worked prior to becoming unemployed) and age. For the age group 50 and older, UB-duration is 52 weeks; and for the age group 40–49, UB-duration is 39 weeks. 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 percent of UB. In 1990, the median post-UB transfer payment was about 70 percent of the median UB. The majority of the unemployed (59 percent) received UB whereas 26 percent received post-UB transfers.

Another relevant feature of the Austrian labor market is its system of wage formation. Almost all workers are covered by collective agreements which take place at the sectoral (or the occupational) level. Collective agreements impose a lower bound on workers' wages. While the Austrian wage setting process is more centralized than in the United States and many European countries (except for Scandinavia), wages are less rigid than one might prima facie think. First, while Austrian wage setting institutions impose a lot of downward rigidity on wages in ongoing employment relationships, wage adjustments take place when workers change jobs or start a new job after an unemployment spell. Second, existing evidence suggests that a substantial fraction of workers are paid above the collectively agreed minimum wage.[6] To the extent that older workers are more experienced and achieve higher wages than

[6]Leoni and Pollan (2011) study "overpayments" (the ratio of effective wages over collectively bargained wages). They find that, in the years when the REBP was in place, effective wages of blue collar workers were, on

the collectively agreed wages, the wage floors of collective agreements are unlikely to contaminate our analysis.

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 REBP that extended UB-entitlement to *209 weeks*. To be eligible to 209 weeks of UB, the worker had to satisfy each of the following criteria at the beginning of his or her unemployment spell: (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 the REBP did not impose any industry requirement. All unemployed who met criteria (i) to (iv) were eligible, irrespective of whether they previously worked in the steel sector or not.

The REBP was in effect until December 1991 before a reform was implemented in January 1992. This reform enacted two changes for new spells. First, the benefit extension was abolished in 6 of the original 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 for extended benefits: new beneficiaries had to be not only residents, but also *previously employed* in a treated region. The program stopped accepting new entrants 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 (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 the foundation. Displaced individuals who decided to join this out-placement center were entitled to regular unemployment benefits for a period of up to three years (later four 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 to make sure that the workers in our sample did not have access to re-training activities provided by the steel foundation. Notice further that no other labor market policies were put in place during the REBP period that may confound the effect of the program. Lalive and Zweimüller (2004b) provide an extensive discussion of the context and institutional background of the REBP and discuss the validity of the REBP as a research design.

average, between 20 to 25 percent above the collectively bargained minimum wages. Hence, a large fraction of workers is paid above the wage floor.

As the REBP was targeted to older workers it could also be used as a pathway to early retirement, the main pathway being retirement via the disability insurance system. The existence of these early retirement programs creates potential complementarities with the REBP program that are susceptible to affect search effort and labor supply in nontrivial ways (Inderbitzin, Staubli, and Zweimüller forthcoming). In order to minimize these complementarity effects and concentrate on the effects of the REBP program alone, our analysis focuses primarily on workers aged 50–54 as they cannot use unemployment benefits as a direct pathway to early retirement.

III. Data and Identification Strategy

Data.—Our dataset covers the universe of UI spells in Austria from 1980 to 2009. In our baseline estimation sample, and for reasons that we explain below, we focus on all unemployed men aged 46 to 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)), 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 complementary information on insurance spells back until 1949 to compute work history in the past 25 years for each individual to precisely determine a worker's REBP eligibility status.[7] We also use social security data to compute wages before and after each unemployment spell, as well as the total duration of nonemployment 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 50 and men above 55 have relaxed access to DI which allows these workers to 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 below age 55 because it is much more difficult for them to 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.

[7] For more information about the ASSD, see Zweimüller et al. (2009). The ASSD covers employment spells from 1972 onwards. To measure worker's experience during the last 25 years (necessary to determine REBP-eligibility), we used complementary data from the Austrian Ministry of Social Affairs on employment spells back to 1949. (The UI administration used a similar source of information on individual experience to determine REBP-eligibility.) As we do not observe final eligibility to REBP, our approach is an intent-to-treat approach. There are 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.

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.[8] This dataset has two important features. First, the data record detailed information about the characteristics of the vacancy.[9] Second, the vacancy data contain 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, $AE = E\big(y_{i1}^{0} - y_{i0}^{0}\big)$.

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}:

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

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 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-differences design. This design is valid if unobserved differences between non-treated workers in markets $M = 0$ and $M = 1$ remain fixed over

[8] We also have some crude vacancy data available for the period 1990–1994 that we use to compute initial labor market tightness in online 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.

[9] 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.

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 workers who are eligible for REBP, based on the three eligibility criteria: age, experience, and geography. To implement our difference-in-differences 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.—Our analysis focuses on *non-eligible workers within REBP counties*, i.e., on workers who both live and had previous employment in REBP counties. However, to properly define treated labor markets, we want to focus on non-eligible workers within REBP counties who actually 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 micro data on job vacancies. The vacancy data contain, for each individual vacancy, detailed information about the characteristics of the vacancy and the personal identifier of the person who filled the vacancy. Our strategy uses all the information on each vacancy, and estimates how well the characteristics of each vacancy predict the REBP eligibility status of the worker who fills the vacancy. (Data and empirical strategy are discussed in detail in online 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.[10]

In Figure 1, panel A, we plot the p-value of two standard goodness-of-fit tests for the logit model, the Pearson's χ^2 goodness-of-fit test and the Hosmer-Lemeshow χ^2 goodness-of-fit test, for different categories of non-eligible workers. A low p-value for the test indicates a poor fit of the data. Both tests suggest that the model fits the data very well for comparing eligible workers to non-eligible workers aged 35 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. In panel B of Figure 1, we plot the fraction of observations that are incorrectly predicted by the model (i.e., the predicted eligibility status to REBP is different

[10] This model aims at testing the ability of firms to direct their search toward different types of workers, who have different search effort due to REBP, by opening different types of vacancies. We therefore estimate it in REBP regions when the REBP was in place. In places or times where REBP is not in place, workers eligible to REBP (would the REBP be in place) and non-eligible workers have the same level of UI benefits, their search effort is likely to be very similar, and firms have therefore much less incentives to direct search differently or to discriminate between these different types of workers.

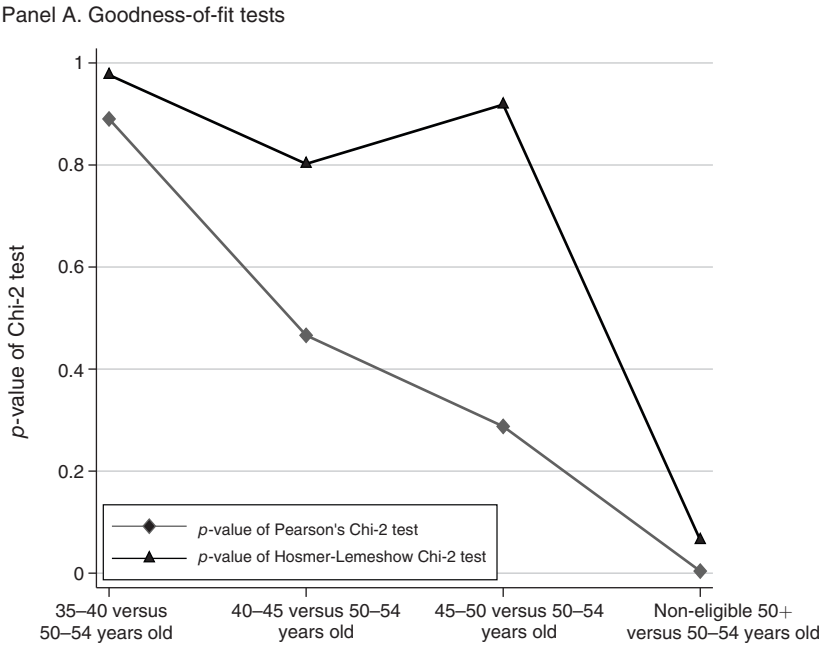


FIGURE 1. EVALUATING THE DEGREE OF COMPETITION FOR IDENTICAL VACANCIES BETWEEN REBP ELIGIBLE WORKERS AND DIFFERENT GROUPS OF NON-ELIGIBLE WORKERS (*Continued*)

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 percent for the model comparing eligible workers to non-eligible workers aged 30 to 40, but increases up to more than 25 percent for the model comparing eligible workers to non-eligible workers aged 50 to 54. 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.[11] The figure indicates that type I errors are very uncommon when comparing eligible workers to non-eligible workers below 50, but they seem to be particularly severe when comparing eligible workers to non-eligible workers aged 50 to 54.[12]

These results are helpful for our identification strategy as they reveal which groups of non-eligible workers are more likely to identify UI market externalities. Workers aged 30 to 40 seem to fill vacancies with characteristics 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

[11] 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.

[12] Because classification is sensitive to the relative sizes of each component group, and always favors classification into the larger group, the classification error measures of panel B should still be interpreted with caution. We therefore tend to prefer goodness-of-fit measures presented in panel A.

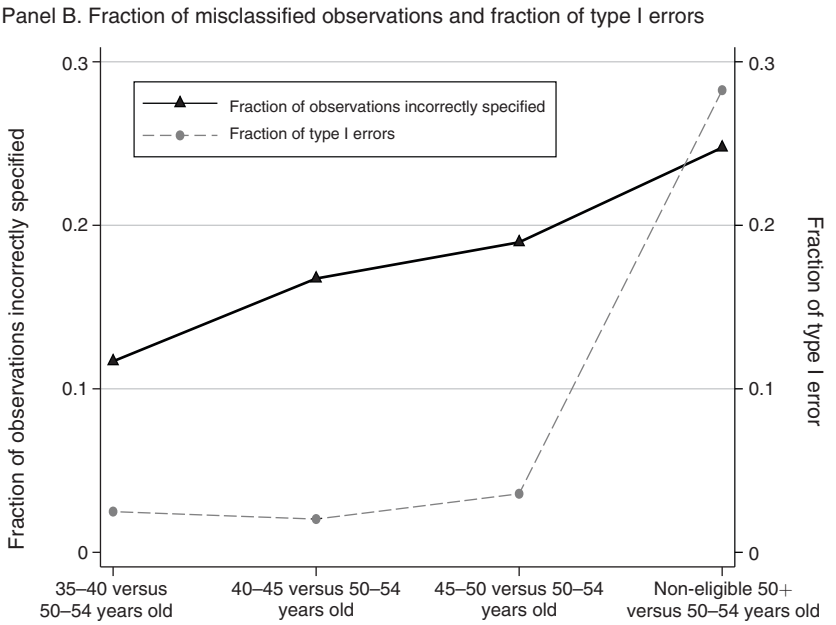


FIGURE 1. EVALUATING THE DEGREE OF COMPETITION FOR IDENTICAL VACANCIES BETWEEN REBP ELIGIBLE WORKERS AND DIFFERENT GROUPS OF NON-ELIGIBLE WORKERS (*Continued*)

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 *p*-value of two standard goodness-of-fit tests for the logit model, the Pearson's χ^2 goodness of fit test and the Hosmer-Lemeshow 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 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 fraction of type I errors of the model. The classification error measures of panel B should be interpreted with caution as classification is sensitive to the relative sizes of each group of workers. We therefore tend to prefer goodness-of-fit measures presented in panel A. All the details are given in online Appendix B.

that workers aged 30 to 40 are likely to be in a different job search market than eligible workers. As we move toward older ages, workers seem to be in closer competition 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 would bias toward 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 the fraction of new hires in non-REBP counties who come from REBP counties. In Figure 2, panel A, we map the average quarterly fraction of men aged 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 few counties where this fraction is above 5 percent and only in a handful of counties is this fraction above 20 percent. 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 percent 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.

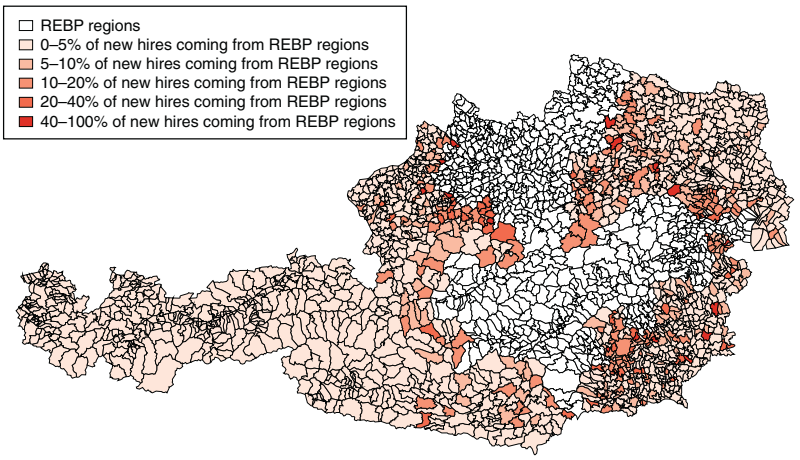
In Figure 2, panel B, we map the average quarterly fraction of men aged 46 to 54 coming from non-REBP regions in the total number of new hires of men aged 46 to 54 in REBP counties for all years when the REBP was not in place. This measures the degree of competition from non-REBP workers faced by workers 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 IV.

Identifying Assumption.—To identify UI externalities, our strategy relies on comparing workers in REBP counties who are non-eligible (because of failing either the age or the experience requirement) to similar workers in non-REBP counties. This difference-in-differences 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 versus non-REBP counties contemporaneous to the REBP program. Indeed, as stated in Section II, 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 the REBP. Because the steel sector only accounts for at most 15 percent of employment in REBP counties, the spillover effects of the restructuring can be assumed to be small on industries not directly related to the steel industry supply chain. We show compelling graphical evidence in favor of our parallel trend assumption in the next section. We also provide in our sensitivity analysis several robustness tests to control for region-specific shocks 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

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



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

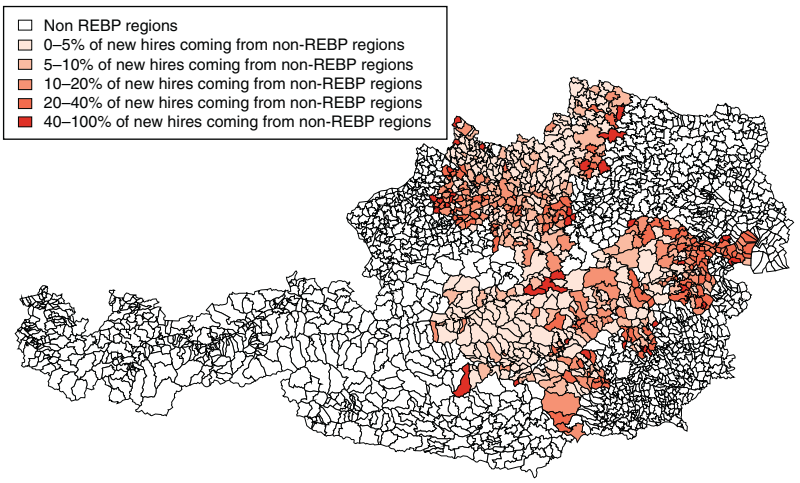


FIGURE 2. REGIONAL DISTRIBUTION OF REBP AND LOCAL LABOR MARKET INTEGRATION DURING NON-REBP YEARS, 1980–1988 AND 1998–2009

Notes: The figure shows the distribution of REBP across the 2,361 communities (counties) in Austria. The treated regions (REBP regions) are all counties with 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 percent 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.

TABLE 1—SUMMARY STATISTICS

Panel A. REBP versus non-REBP counties

	Non-REBP period				REBP period			
	Non-REBP counties (1)	REBP counties (2)	Difference (3)	*p*-value (4)	Non-REBP counties (5)	REBP counties (6)	Difference (7)	*p*-value (8)
Fraction employed in the steel sector	0.055	0.152	−0.097	0	0.057	0.156	−0.099	0
Monthly 46–54 unemployment rate	0.055	0.054	0.001	0.864	0.073	0.113	−0.04	0
Fraction eligible to REBP	0.382	0.396	−0.014	0	0.449	0.533	−0.084	0
Age	49.7	49.7	0	0.343	49.8	50.1	−0.3	0
Unemployment duration	13.6	14.3	−0.7	0	15.9	29	−13.1	0
Nonemployment duration	22.7	21.2	1.4	0	32.9	45.4	−12.4	0
Wage before U spell (€2000)	13,448	14,306	−857	0	13,122	14,498	−1,375	0

Panel B. Eligible versus 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
Nonemployment duration	21.6	24.7	−3.1	0	31.4	99.6	−68.2	0
Wage before U spell (€2000)	14,096	14,623	−527	0	13,316	15,549	−2,232	0
Fraction with compulsory education	0.529	0.501	0.028	0	0.511	0.506	0.005	0.44
Fraction married	0.744	0.751	−0.007	0.076	0.748	0.803	−0.055	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. Nonemployment 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.

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 percent versus 5 percent in non-REBP counties. To control for the potential endogeneity bias in the choice of REBP counties, we remove the steel sector from our baseline estimation sample. More specifically, we get rid of all unemployed who ever worked in the steel sector prior to or after becoming unemployed. The monthly unemployment rate for the 46 to 54 years old was the same on average (5.5 percent) in REBP and non-REBP counties during non-REBP years.

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 percent. 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 nonemployment spells were roughly the same for unemployed in REBP and non-REBP counties.[13] Gross unconditional wages were slightly higher in REBP counties.

In Table 1, panel B, we display descriptive statistics for eligible and non-eligible unemployed workers in REBP counties in our estimation sample of unemployed men, aged 46 to 54 outside 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 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 an equivalent level of education, and were also similar in terms of other sociodemographic characteristics such as education or marital status.

IV. 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 aged 46 to 54 in panel B, and non-eligible workers aged 50 to 54 in panel C), we run the following regression:

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

where $\mathbb{1}[T = \mathrm{t}]$ is an indicator for the start of the unemployment spell being in year t and $\mathbb{1}[T = \mathrm{t}]$ 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 vertical line denotes the beginning of the REBP program, and the two dashed 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.

Figure 3, panel A, plots the estimated difference d_t each year between REBP and non-REBP counties for workers above age 50 with more than 15 years of continuous

[13] All duration outcomes are expressed in weeks. Nonemployment 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.

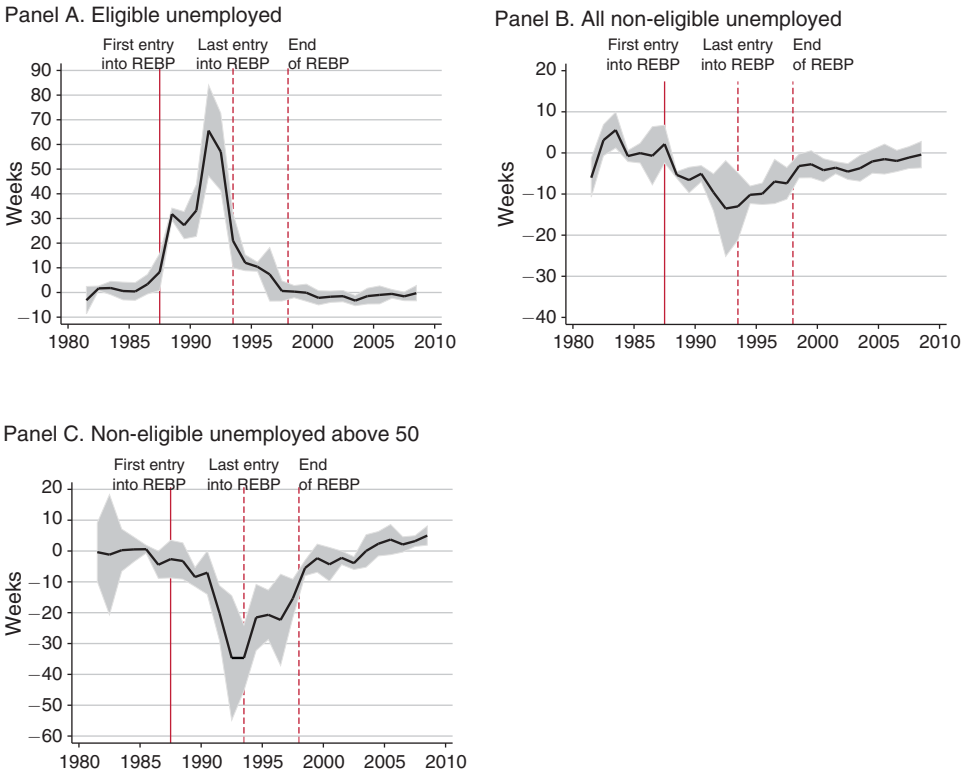


FIGURE 3. DIFFERENCE IN UNEMPLOYMENT DURATIONS BETWEEN REBP AND NON-REBP COUNTIES BY YEAR OF ENTRY INTO UNEMPLOYMENT

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 × 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 of work history). See text for details.

work history, and therefore eligible for the REBP. Figure 3 shows that the introduction of program 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 unemployment duration disappears for workers entering unemployment from 1994 on, when the REBP no longer accepted new entrants. Year 1993 can therefore be seen as the peak of the program effect on aggregate labor supply, since this is the moment where the stock of REBP-eligible unemployed is the highest, and the labor supply of treated workers is the lowest.

Figure 3, 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 the 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 the 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.

Figure 3, 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 the 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.

Figure 4, 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:

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

where Y_{it} are different search outcomes of interest, M is an indicator for residing in a REBP county,[14] 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. $\mathcal{H}$ is an indicator of REBP-eligibility and is equal to one for unemployed

[14] 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 the REBP changed in 1991.

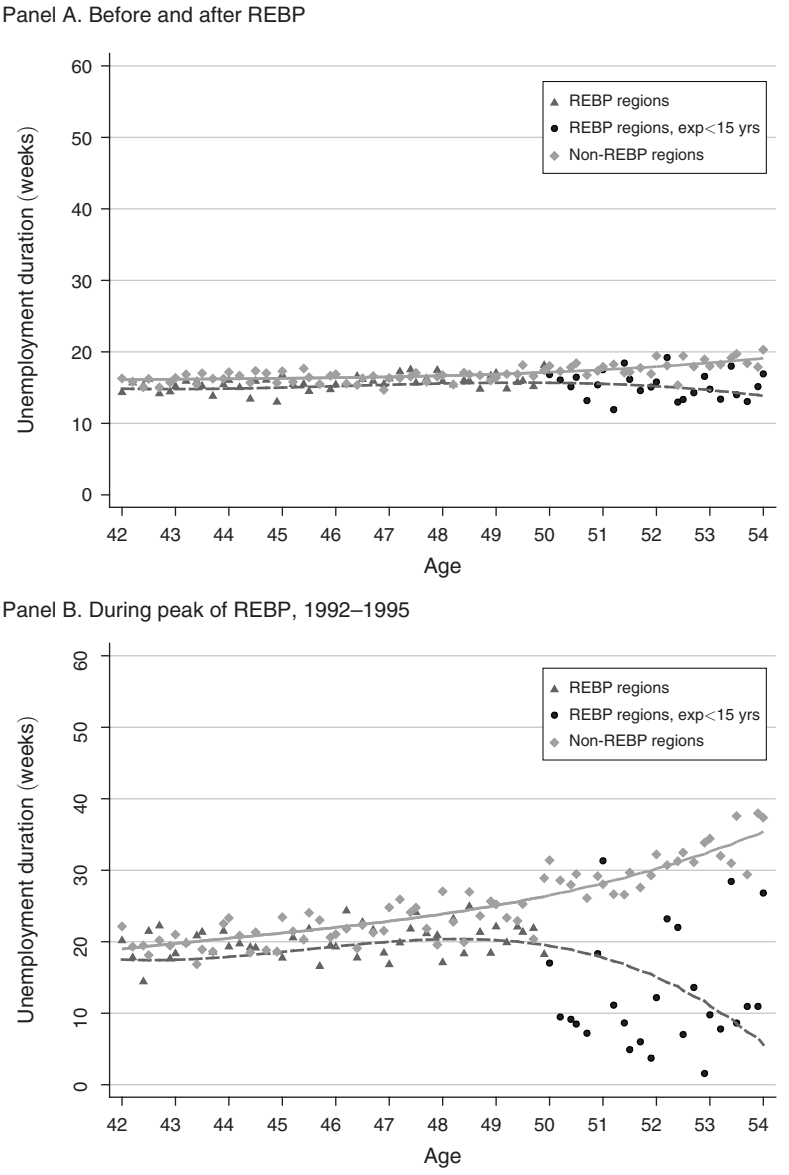


FIGURE 4. UNEMPLOYMENT DURATIONS AS A FUNCTION OF AGE IN REBP AND NON-REBP COUNTIES FOR NON-ELIGIBLE UNEMPLOYED

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 2—Baseline Estimates of the Treatment Effect of REBP on Eligible Unemployed and Non-Eligible Unemployed

	Unemployment duration			Nonemployed duration	Spell $>$ 100 wks	Spell $>$ 26 wks
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 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)
Observations	267,966	262,344	262,344	232,135	262,344	262,344
Panel 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)
Observations	267,966	262,344	262,344	232,135	262,344	262,344
Panel 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)
Observations	254,934	249,894	249,894	220,754	249,894	249,894
Panel 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)
Observations	125,088	122,277	122,277	102,677	122,277	122,277
Education, industry, citizenship, marital status		×	×	×	×	×
Region-specific trends			×	×	×	×

Notes: Standard errors clustered at the year $\times$ region level in parentheses. 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 columns 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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 the REBP on eligible workers, while γ_0 identifies spillovers of the REBP on non-eligible workers in REBP regions. $\sum \nu_t$ is a series of year fixed effects. Because we control for eligibility fixed effects $(\mathcal{H})$ interacted with both the REBP-county indicator (M) and year fixed effects, specification (3) amounts to pooling two difference-in-differences together, one for the REBP effect on eligible unemployed workers and one for the REBP effect 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 columns 3 to 6 we

also add controls for preexisting trends by region. Panel A displays estimates of β_0, the difference-in-differences estimate of the effect of the REBP on eligible workers. Results confirm that the 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 nonemployment (conditional on finding a job at the end of the unemployment spell). The direct effect of the 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 the REBP shifted the whole survival function of unemployed eligible to the REBP.

Table 2, panel B, displays estimates of γ_0, the REBP effect on all non-eligible workers aged 46 to 54 in REBP counties.[15] 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 nonemployment which means that the positive REBP effect 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 III 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 the 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 Table 2, panel C, we focus on the REBP effect for non-eligible workers age 46 to 49 who are non-eligible based on age. Results show that the REBP significantly reduced the duration of unemployment and of total nonemployment of non-eligible workers aged 46 to 49 by 2 to 3 weeks. Panel D shows the REBP effect 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. The REBP significantly reduced the duration of unemployment and of total nonemployment of non-eligible workers above 50 by 6 to 9 weeks.

Robustness.—In online 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

[15] To flexibly correct for the presence of temporary common random shocks that may affect the entire REBP region, or alternatively the entire non-REBP region, we cluster standard errors at the region-year level. In online Appendix Table 6, we also provide evidence of the robustness of our results to various inference strategies. We have checked sensitivity of inference in three ways. First, we allow for clustering by markets defined as county-by-industry-by-education cells. Second, we implement spatial heteroskedasticity-autocorrelation-corrected (HAC) standard errors as in Conley (1999). Finally, we implemented permutation based standard errors as in Chetty et al. (2014); Kline and Moretti (2014); and Lalive, Wuellrich, and Zweimüller (2013). All the details are provided in online Appendix C.

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 in the estimation sample, which had been excluded from the baseline sample to alleviate the concern of nonparallel trends between REBP and non-REBP counties.[16] 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 the 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 previous difference-in-differences model 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 has had a large positive effect on the log separation rate of eligible workers in REBP regions but has not affected the log separation rate of non-eligible workers in REBP regions.[17] 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 columns 2 and 3, we run the difference-in-differences 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

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

[17] Winter-Ebmer (2003) also finds a significant impact of the REBP on the unemployment inflow of eligible workers. We discuss in online Appendix 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.

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

	log separation rate	log real wage in previous job		log tenure in previous job	
	(1)	(2)	(3)	(4)	(5)
Eligible workers	0.279***				
	(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)
Education, marital status, industry, citizenship			×		×
Observations	3,390	240,947	240,923	267,929	267,901

Notes: For columns 2 to 5, standard errors are clustered at the year × region level. 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 difference-in-differences 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 includes 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

sector employees never exceeds 15 percent 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. In this sense, region-specific shocks are likely, if anything, to bias downward the magnitude of 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 below age 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 difference-in-differences specification equivalent to equation (3) where we replace $\mathcal{M}$ by $\mathcal{A} = \mathbb{1}[Age > 50]$. This specification enables us to control for shocks to the labor markets of REBP counties contemporaneous to the REBP that affect all job seekers in the same way. Results are reported in online 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 (online Appendix A.2). To investigate how estimated externalities vary with treatment intensity, we look at different measures of treatment intensity and interact these measures with the REBP effect on non-eligible workers. The estimated specification is

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

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 2, panel B. Counties that, absent REBP, had on average a high fraction of hires coming from non-REBP regions have labor markets that are more integrated to non-REBP regions and the REBP effect 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 percent 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 low level of integration to non-REBP counties. REBP induced a reduction in nonemployment duration of non-eligible workers of only 0.7 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 in the average duration of unemployment of 4 weeks 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. We compute the average yearly fraction of eligible workers among the 50+ for each region $\times$ industry $\times$ education cell during REBP years and define by high treatment intensity a cell where the fraction of eligible 50+ unemployed was more than 90 percent (the median value across all region $\times$ industry $\times$ education cells).[18] 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 2 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, Michaillat, and Saez (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 online Appendix

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

TABLE 4—EXTERNALITIES ON NON-ELIGIBLE UNEMPLOYED BY REBP-TREATMENT INTENSITY

REBP effect on non-treated	Unemployment duration (1)	Nonemployed duration (2)	Spell $>$ 100 wks (3)	Spell $>$ 26 wks (4)
Panel A. Treatment intensity, method 1: county share of hires from non-REBP counties				
All non-eligible				
γ_0^L (share of non-REBP hires $>$ 0.05)	−1.599**	−0.676	−0.00275	−0.00289
	(0.747)	(0.693)	(0.00224)	(0.00661)
γ_0^H (share of non-REBP hires $\leq$ 0.05)	−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 $>$ 0.05)	−4.048**	−4.191*	−0.00300	−0.0119
	(1.894)	(2.309)	(0.00788)	(0.0136)
γ_0^H (share of non-REBP hires $\leq$ 0.05)	−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 $\leq$ 0.9)	−0.849	−1.022	0.00426	−0.00918
	(0.933)	(1.161)	(0.00421)	(0.00886)
γ_0^H (fraction treated $>$ 0.9)	−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 $\leq$ 0.9)	−4.207	−3.661	−0.00126	−0.0351*
	(2.807)	(2.378)	(0.0110)	(0.0188)
γ_0^H (fraction treated $>$ 0.9)	−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]
Education, marital status, industry, citizenship	×	×	×	×

Notes: Standard errors clustered at the year × region level in parentheses. 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 percent, 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 percent 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.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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 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 the REBP. These counties are likely to experience spillover effects from REBP counties and cannot serve as a proper control in our difference-in-differences strategy. We now investigate directly whether we can detect the presence of REBP externalities on unemployed workers residing in these counties. We begin by running a simple difference-in-differences 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.[19] We restrict our sample to male unemployed workers aged 50 to 54 with more than 15 years of experience, who would be eligible to the REBP if residing in REBP counties. Results are reported in panel A of Table 5 and suggest that the 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 percent before the REBP to unemployed in cells where it was lower than 20 percent. Our estimates show that the 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 in unemployment duration of 2.5 to 5 weeks relative to similar workers residing in cells with low competition from REBP workers.

Wages.—The sign and magnitude of our estimated REBP market externalities 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 REBP effect on reemployment wages of eligible workers.

Analyzing the REBP effect on wages is very different from our previous market externality analysis, as we now wish to compare eligible workers to non-eligible

[19] 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 percent for all non-REBP periods.

TABLE 5—GEOGRAPHICAL SPILLOVERS: EFFECT OF REBP ON UNEMPLOYED WORKERS IN NON-REBP COUNTIES WITH HIGH LABOR MARKET INTEGRATION TO REBP COUNTIES

	Unemployment duration		Nonemployment duration	Spell $>$ 100 wks	Spell $>$ 26 wks
	(1)	(2)	(3)	(4)	(5)
Panel A. 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)
Panel B. Labor market integration, measure 2: fraction of hires coming from REBP regions in county × industry × education cell					
γ_0 (geographical spillovers)	−6.373***	−5.166***	−2.515	−0.0141	−0.0169***
	(1.213)	(1.109)	(0.659)	(0.00368)	(0.00603)
Education, marital status, industry, citizenship		×	×	×	×
Observations	104,881	102,840	88,702	102,840	102,840

Notes: Standard errors clustered at the year × region level in parentheses. 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 difference-in-differences 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 a 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 percent 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 percent (for all years when REBP was not in place) to unemployed in cells where it was lower than 20 percent.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

workers. Identification of the effect on wages is difficult for at least three reasons. First, the REBP increases unemployment duration for eligible workers, which may directly affect wages through duration dependence effects. 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, the 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 online Appendix 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 the 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, von Wachter, and Bender (2012a) and estimate the effect of variations in benefits on reemployment wages holding unemployment duration constant. Identification is based on the assumption that there is no correlation between unobserved heterogeneity and unemployment benefits *conditional on unemployment duration*.

We plot, in online 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 difference-in-differences estimate of the REBP effect on reemployment wages conditional on spell duration. This evidence suggests that there was no significant REBP effect on reemployment wages.

We formally assess this result in online Appendix Table 10 by running a simple difference-in-differences model where we compare workers eligible to the 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 REBP effect on reemployment wages.[20]

Overall, this evidence, although tentative, suggests that wages of eligible workers did not strongly respond to the 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.

V. Discussion and Policy Implications

Micro versus Macro Effects of UI Extensions.—Our empirical findings have important policy 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 variation 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

[20] To complement our difference-in-differences approach, in online Appendix D we also exploit the age eligibility discontinuity at 50 and the experience eligibility discontinuity in REBP counties to 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. Results suggest the presence of no wage effect using the experience discontinuity, and a small significant elasticity of wages with respect to UI benefits when using the age discontinuity. Note however that the McCrary test strongly rejects continuity in the probability density function of age at the cutoff (50 years) during the REBP period, which suggests that the estimated wage effects could partly be driven by selection (sorting) at the 50 years age cutoff.

("macro effect") and e^m is the "micro effect." This wedge can be recovered from our two groups quasi-experimental setting (online Appendix A.2):

$$W = \frac{\frac{1}{p}\frac{dD_b}{dB_a}}{\frac{dD_a}{dB_a} - \frac{dD_b}{dB_a}}. \tag{5}$$

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 REBP market externalities. Intuitively, because the effect of REBP on non-treated workers will create externalities that are 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 REBP externalities identified by $\frac{dD_b}{dB_a}$.

We can now calibrate the wedge W of equation (5) 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 ≈ 0.9. This gives us a wedge of $W \approx 0.21$.[21]

To what extent is this wedge informative about the micro and macro effects of treating all labor markets by having a countrywide 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, some untreated labor markets (for workers aged below 50, for instance) are offering substitution opportunities to treated workers. We explain in online Appendix 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

[21] Marinescu (2014) studies the difference between the UI macro and the UI micro effects based on vacancy posting and job application data from a US online job board. Interestingly, she finds that the macro effect is smaller than the micro effect and estimates a wedge which is of a similar order of magnitude than ours.

the market externalities of UI in the treated market. This suggests that the wedge between the micro and macro effects of countrywide 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, Michaillat, and Saez (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 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, 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 online 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, Michaillat, and Saez (2010).

Market externalities are likely to be larger in the short run. There are two potential 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 possible that these externalities would have decreased. First, because, as suggested by online Appendix Figure 7, it seems that wages started to react more to REBP extensions over time. Second, in the long run, labor demand is likely to become more elastic to labor market tightness as returns to labor are more likely to become constant. 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.[22] In terms of policy implications, this means that temporary

[22] This may explain why cross-sectional estimates comparing countries or US states tend to find much larger elasticities than reform-based (short term) estimates. This may also explain why, European countries with generous UI coverage experience high levels of structural long term unemployment despite the fact that most reform-based estimates in Europe find relatively modest elasticities in the short run.

extensions enacted in reaction to business cycle downturns are less socially costly than previously thought. And, when determining the optimal time span of temporary extensions, governments should pay attention to the evolution of market externalities over time.

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