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# Benefit entitlement and unemployment duration The role of policy endogeneity

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**Abstract**

The potential duration of benefits is generally viewed as an important determinant of unemployment duration. This paper evaluates a unique policy change that prolonged entitlement to regular unemployment benefits from 30 weeks to a maximum of 209 weeks for elderly individuals in certain regions of Austria. In the evaluation, we explicitly account for the fact that the program was an endogenous policy response to deteriorating labor market conditions for older workers in certain regions and sectors. The main results are: (i) REBP reduced the transition rate to jobs by 17%; (ii) accounting for endogenous policy adoption is important and quantitatively significant.


*JEL classification:* C41; J64; J65
*Keywords:* Quasi-experiments; Policy endogeneity; Benefit entitlement; Maximum benefit duration; Unemployment duration; Unemployment insurance

## 1. Introduction

Reliable empirical evidence on the incentive effects of unemployment insurance is crucial for designing appropriate labor market policies. Most empirical studies identify such incentive effects from ‘exogenous’ variation in benefit rules. However, changes in such rules are, like any other policy choice, purposeful action. In fact, a frequently adopted policy is to extend the maximum duration of benefit entitlement when labor market conditions are expected to deteriorate. When benefit policy is determined by labor market conditions,

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however, observed changes in unemployment duration are only in part due to changes in entitlement rules. They are also due to changes in labor market conditions that lead to new benefit rules in the first place. Empirical strategies that do not account for the determinants of such policy changes yield biased estimates of the impact of unemployment insurance.

In this paper we evaluate a program that dramatically increased benefit generosity for a specific group of workers: the Austrian Regional Extended Benefits Program (REBP). The REBP was enacted in response to the international steel crisis that hit the traditional iron and steel regions in Austria particularly hard. The explicit intention of this program was to avoid social hardships for the concerned steel workers and to mitigate problems in the concerned regions. Hence rather than being an 'exogenous' policy change, the REBP was adopted because labor market conditions were expected to become worse. Our analysis will take particular care to account for that 'policy endogeneity'.

The present paper goes beyond most of the existing literature in three respects. First, to identify the causal effect of benefit duration on the willingness of individuals to accept jobs, the REBP provides an *unusually rich empirical research design*. Access to the program was contingent upon age and region. The program was primarily targeted towards steel workers but also unemployed workers from other sectors were eligible. Successive policy changes (including a reform that tightened eligibility criteria and the final abolishment of the program) allow us to account for policy endogeneity. Second, the increase in the *benefit extension granted by the REBP was huge*. It extended benefits for workers above age 50 with residence in certain regions to a maximum of 209 weeks (4 years!), up from originally only 30 weeks. In this sense, our paper sheds new light on the importance of benefit eligibility to explain differences in the incidence of long-term unemployment under rules as different as the rules between the US and many European economies. Third, *a unique and extraordinary informative data set* allows us to adopt empirical strategies that are not feasible in other studies. Our data set covers the universe of unemployment spells in Austria that started between January 1986 and December 1995. These unemployment spells can be observed up until December 1998. As the REBP was started in June 1988 and abolished in August 1993, the data cover the inflow of approximately 2 years and 6 months *before*, 5 years *during*, and 2 years and 6 months *after* the program. The data contain all information that is necessary to determine eligibility status, provide important socio-economic characteristics, and report the work history of individuals. The focus of our analysis is on a comparison of age groups 45–49 and 50–54.

Like other extended benefit programs, the Austrian REBP was an endogenous policy response to the expectation of deteriorating labor market conditions. In order to disentangle the causal effect of benefit entitlement from the impact of labor market conditions we adopt four different identification strategies. The *first strategy* performs a simple differences-in-differences-in-differences (DiDiD) analysis exploiting variation in the maximum benefit durations that occurs between regions, time, and age groups. The *second strategy* confines the analysis to the *non-steel* inflow. This analysis should be less afflicted with a policy endogeneity bias as non-steel workers were not directly subject to specific shocks. Our *third strategy* takes the importance of sectoral spillover effects into account by focusing on a subset of regions that lost eligibility status with the reform of the REBP in 1991. For these regions, the expectation of worse labor market conditions had not come true. Our *final strategy* makes use of the changes in the eligibility criteria with the reform of the REBP in 1991. After this reform, eligibility required that an older worker had not only to be a resident in a REBP region, but also the location of the previous employer had to be in such a region. Hence we can exploit variation in employer location among residents in REBP regions to study the benefit entitlement effect.

Previous empirical studies identify the impact of extended benefits from variation in benefit entitlement across time, states, and/or age groups[1]. In the US, for instance, the Extended Benefit Program grants unemployed individuals up to 13 additional weeks of benefits during periods of high unemployment; also in Canada the duration of benefit entitlement to unemployment benefits (UB) depends on the current regional unemployment situation. In many Continental European countries benefit durations have been increased for older individuals in the mid 1980s and/or early 1990s (see OECD, 1999 for an overview)[2]. Many of these changes were motivated by the deteriorating labor market prospects of the respective groups and created an early retirement possibility for older individuals. In principle, these changes in the maximum duration of unemployment benefit receipt are informative on the causal link between benefit duration and unemployment duration. However, we are not aware of an extended benefit program that is as selective with respect to age *and region* as the REBP.

The recent literature dealing with the impact of benefit entitlement rules on unemployment durations has largely ignored the issue of policy endogeneity[3]. An important exception is the paper by Card and Levine (2000) who study the impact of the New Jersey Extended Benefit Program (NJEB). This program was the result of a political compromise rather than a depressed job market and was enacted during a period of improving labor market conditions. Hence, Card and Levine's analysis does not suffer from an endogenous policy bias. In fact, the impact of extended benefits on unemployment durations turns out to be much smaller than in other studies.

Our analysis differs from the study by Card and Levine (2000) in at least three important respects. First, the NJEB program was in place for only 6 months. In contrast, the Austrian REBP was in effect over a period of 5 years and provides an ideal set-up to study the long-term effects of the program. Second, the NJEB program granted a moderate benefit extension of 13 additional weeks of regular UB. This compares to 179 (!) extra benefit weeks granted by the Austrian REBP. Furthermore, Card and Levine (2000) can neglect the policy endogeneity issue as the NJEB program was not targeted towards a

[1] See Bratberg and Vaage (2000); Ham and Rea (1987); Hunt (1995); Katz and Meyer (1990); Moffitt (1985); Moffitt and Nicholson (1982); Steiner (2003); Winter-Ebmer (1998). Other recent studies that shed light on the importance of benefit eligibility compare between receivers and non-receivers of unemployment benefits (Carling et al., 1996 for Sweden and Bover et al., 1998 for Spain); or study major cuts in benefits (e.g. Carling et al., 2001 look at a major benefit cut in Sweden and Abbring et al., 1998; Van den Berg et al., forthcoming look at the impact of major benefit cuts due to sanctions in the Netherlands) or re-employment cash bonuses (see the survey on the US unemployment insurance experiments in Meyer, 1995b). For theoretical analyses see Burdett (1979); Mortensen (1977); Van den Berg (1990) and for an early review of the effect of unemployment insurance on labor market transitions see Atkinson and Micklewright (1991).

[2] For instance, in Germany and Austria unemployment benefit duration became strongly dependent on age in the 1980s. See Hunt (1995) for an evaluation of the former policy change. The benefit system in Slovakia was reformed in 1995 to become strongly dependent on age (see Lubyova and van Ours, 1997).

[3] See Besley and Case (2000) or Meyer (1995a) for a discussion of policy endogeneity in 'natural or quasi experiment' studies.

group with bad labor market prospects. In contrast, the endogenous policy issue is central for the Austrian REBP as this program was intended to help workers confronted with unfavorable conditions on the job market. This allows discussing both, the causal effect of benefit entitlement on unemployment duration and the role of policy endogeneity in unemployment insurance policy changes.

The only previous study that has so far analyzed the impact of the Austrian REBP on the duration of unemployment is a paper by Winter-Ebmer (1998) looking at the early impact of this program and neglecting policy endogeneity. The present paper goes beyond that study in a number of ways. First, and most importantly, we put particular emphasis on the problem of policy endogeneity and provide an empirical test that accounts for this issue. It turns out that accounting for endogenous policy adoption is important. Second, we consider not only the impact of the benefit extension after its introduction, but also the impact of the reform and of the abolishment of the REBP. This is of particular importance as changes in eligibility rules after the reform help us to identify the program-effect. Third, we use a more informative and much larger data set. This allows us to focus on a narrow age group and avoid a possible bias resulting from a misspecification of the effect of age. Fourth, we confine the analysis to male workers and exclude females. The reason is that females had access to early retirement with age 55 and the REBP effectively allowed women to withdraw from the labor force.

The paper is organized as follows. In Section 2 we provide some information on the Austrian labor market, survey the Austrian unemployment insurance system, and give a detailed description of the Austrian REBP. Section 3 describes the data and discusses identification issues. Section 4 presents the econometric model. In Section 5 we present the results and Section 6 concludes.

## 2. Austrian unemployment insurance and the REBP

### *2.1. The unemployment insurance system*

Before August 1989, an unemployed person could draw regular UB for a maximum period of 30 weeks provided that he or she had paid unemployment insurance contributions for at least 156 weeks within the last 5 years[4]. In August 1989 the potential duration of UB payments became dependent not only on previous experience but also on age at the beginning of the unemployment spell. Benefit duration for the age group 40–49 was increased to 39 weeks if the unemployed has been employed 312 weeks within the last 10 years prior to the current spell. For the age group 50 and older, UB-duration was increased to 52 weeks if the unemployed has been employed for at least 468 weeks within the last 15 years. Our empirical analysis below controls for the general change in benefit duration[5].

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

[5] Note that the causal effect of this general increase in benefit eligibility is identified only if the implausible assumption of temporal stability in the unemployment exit rate holds. Thus we do not claim to estimate also the causal effect of this general benefit extension.

Voluntary quitters and workers discharged for misconduct can not claim benefits until a waiting period of 4 weeks has passed. UB recipients are expected to search actively for a new job that should be within the scope of the claimant's qualifications, at least during the first months of the unemployment spell. Non-compliance with the eligibility rules is subject to benefit sanctions that can lead to the withdrawal of benefits for up to 4 weeks.

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

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

### 2.2. Restructuring of the Austrian steel industry and the REBP

To protect its assets after World War II from Soviet appropriation and to provide the capital needed for reconstruction, Austria nationalized its iron, steel, and oil industries, large segments of the heavy engineering and electrical industries, most of the coal mines, and the nonferrous metals industries. Firms in the steel sector were part of a large holding company, the Oesterreichische Industrie AG, OeIAG. By the mid-1970s this holding company was running into serious problems related to shrinking markets, overstaffing, too heavy concentration on outmoded smokestack industries, insufficient research and development, and low productivity. Initially, the Austrian government covered the losses by subsidies. But in 1986, after the steel industry was hit by an oil speculation scandal and failure of a US steel plant project, this protectionist policy was abolished. A new

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

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

management was appointed and a strict restructuring plan was implemented. This plan aimed at focusing on the holding's core competencies. The results were layoffs due to plant closures and downsizing, particularly in the steel industry.

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

The minister for social affairs, a member of the ruling social democratic party (SPÖ), was in charge of selecting those regions that became eligible to the program. It is interesting to look at some characteristics of the chosen regions[8]. On the one hand, the entitled regions were characterized by a strong concentration of employment in the steel sector. In the REBP regions, roughly 17% of workers were employed in the steel industry firm, whereas in the non-REBP regions the corresponding figure was below 5%. On the other hand, it is not possible to detect, *before the REBP starts*, any important differences between treated and non-treated regions in terms of the unemployment rate or the fraction of long-term unemployed.

The REBP was in effect until December 1991 when a reform of these rules took place, which came into effect in January 1992. This 1991-reform left all claims in progress unaffected. The 1991-reform enacted two important changes. First, the reform abolished the benefit extension in six of the originally 28 regions. We label the set of treated regions that were excluded after the reform as 'TR1s', those treated regions that kept entitled after the reform as 'TR2s', and those regions that were never entitled to REBP as 'CRs'. Second, the 1991-reform tightened eligibility criteria to extended benefits: new beneficiaries had to be not only residents, but also *previously employed* in a TR2. The program was abolished in August 1993.

Apart from the REBP, the second important measure to alleviate the problems associated with mass redundancies in the steel sector was the so-called 'steel foundation'. Firms in the steel sector could decide whether to join in order to provide their displaced workers with re-training activities that were organized by the foundation. Member firms were obliged to finance this foundations. Displaced individuals who decided to join this outplacement center were entitled to claim regular UB for a period of up to 3 years (later 4 years) regardless of age. In 1988, the foundation consisted of 22 firms.

In principle, this foundation is a confound to the REBP effect. On one hand, it is possible that individuals below the age 50 are also entitled to long benefits. On the other hand, it may also be the case that REBP-entitled individuals not only have access to prolonged UB but also to (potentially successful) re-training activities (see Winter-Ebmer, 2001, for an evaluation of the steel foundation). However, note that three out of the four

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

identification strategies that will be discussed in the following section do not rely on workers formerly employed in the steel sector. Thus, it is very unlikely that the presence of this additional measure actually biases our estimates of the effects of the REBP on unemployment duration.

## 3. Data and identification strategies

The causal effect of the REBP on unemployment duration is the change in the unemployment exit rate due to the program. We consider the policy as 'exogenous' when the treated individuals are not subject to idiosyncratic shocks during the observation period. 'Policy endogeneity' would arise when policies are introduced exactly because such idiosyncratic shocks are to be expected. In this section we first describe our data. Then, we present our four identification strategies together with some preliminary descriptive evidence that motivates these strategies. In line with Meyer (1995a), the implementation of several such strategies is meaningful in order to shed light on the sensitivity of the estimated effects with respect to key identifying assumptions. The section closes with a discussion of the stable unit treatment value assumption (SUTVA) and potential effects of REBP on wage setting.

### *3.1. Data*

To assess the impact of benefit duration on transition rates to new jobs we use longitudinal individual data from two different sources: (i) the *Austrian social security* database which contains detailed information on the individuals' employment, unemployment and earnings history since the year 1972, and some information on the employer like region and industry affiliation; and (ii) the *Austrian unemployment register* from which we get information on the relevant socio-economic characteristics. The data cover the universe of the unemployment inflow over the period 1986–1995. The corresponding spells are followed up until the end of 1998.

We restrict our sample to male workers aged 45–54 at the beginning of their spell who held a job prior to unemployment. Selecting a narrow age range has two advantages. First, it is reasonable to assume that (non-eligible) workers aged 45–49 are close substitutes to (eligible) workers aged 50–54 in employment. In this sense, the former is a good control group for the latter. Second, age is a dominant predictor of exit rates from unemployment and the age effect may be highly non-linear. By focusing on this narrow age range we avoid any major misspecification bias. Obviously, avoiding such a misspecification of the age-effect is crucial in the present analysis because age $\geq 50$ is an eligibility criterion of the REBP. There are two reasons for excluding women. The first reason is access to early retirement. Old age insurance rules allow women to retire already at age 55, whereas the early retirement age for men is 60[9]. Hence for eligible women the REBP provided a smooth transition to early retirement. The second reason why women were excluded from

[9] Access to early retirement is possible either if the individual has had a long spell of unemployment and/or has paid social security contributions for at least 35 years.

the analysis is that one eligibility criterion is a continuous work history (employment in 15 out of the last 25 years). As we can trace back employment histories only until 1972, classification errors are more likely for women than for men.

To avoid such classification errors for the remaining male sample we include only those males with a 'continuous' work history since 1972. A continuous work history is defined as a career with a ratio of actual to potential work experience since the year 1972 of at least 0.7. This makes sure that only workers who satisfy the work experience requirement with a very high probability are included in the sample. Recall that eligibility requires 780 employment weeks (15 years) within the last 25 years, that is an actual-to-potential experience ratio of at least 0.6. Since we do not observe the entire 25 years-period prior to the spell, we use the more conservative criterion 0.7 to avoid misclassification[10]. Between January 1986 and December 1995, in total 953,478 unemployment spells were started by individuals in the age group 45–54. From these, we excluded all 402,401 female spells and those 239,001 spells by males who did not satisfy the work–experience requirement. We ended up with 312,076 spells of which more than 83% ended in a new job, 15% dropped out of the labor force (due to sickness, retirement, or other reasons), and only 1.9% are censored.

### *3.1.1. Strategy I: the DiDiD estimator*

The fact that the REBP was limited to job seekers aged 50 or more, living in certain regions, during the period from June 1988 until mid 1993 implies that there are many non-entitled workers who may be quite similar to entitled individuals. This first strategy exploits all information provided by potential control groups.

For instance, the simplest estimate of the REBP-effect involves a comparison of the unemployment exit rate of entitled workers during the program to the level before the program. This is problematic because there may have been an economic downturn leading to an overestimation of the effect of the REBP on unemployment duration. In order to account for such a time trend, the unemployment exit rate of individuals slightly younger than 50 living in entitled regions can be used. Still, this may be problematic in the case of a changing age-specific hiring bias: it may be possible that employers become increasingly reluctant to hire individuals older than 50 as opposed to individuals just below the age of 50. This possibility can be controlled for by using data on individuals living in regions that were not entitled to the REBP. In essence, it appears thus to be possible to account for time-trends in the unemployment exit rate as well as age-specific trends in the employment chances of individuals. Section 5 performs this DiDiD analysis.

The central identifying assumption is that the time trend in the unemployment exit rate as well as the age-specific time trend identified in the non-treated group are informative on the counterfactual. This means that there must be *no idiosyncratic shocks to the labor market prospects of the treated individuals during the period when the REBP was in effect*. If this condition holds, the variation in benefit duration produced by the REBP can be relied on in order to estimate the effect of the REBP on unemployment duration. Thus, we

[10] We also excluded those among the remaing individuals who were employed less than 156 weeks within the last 5 years, less than 312 weeks within the last 10 years, and less than 468 weeks within the last 15 years. This guarantees that all individuals in our sample are eligible for at least 30, 39, and 52 weeks of UBs.

Table 1
Unemployment spell characteristics: steel workers vs. non-steel workers

| | All spells | Treated spells | Non-treated spells |
|---|---|---|---|
| *Mean completed duration (days)* | | | |
| Steel workers | 219.92 | 490.14 | 146.05 |
| Non-steel workers | 110.32 | 200.39 | 105.58 |
| *Exit to employment (share of total)* | | | |
| Steel workers | 0.657 | 0.350 | 0.753 |
| Non-steel workers | 0.851 | 0.740 | 0.857 |
| *Observations* | | | |
| Steel workers | 31,205 | 7431 | 23,774 |
| Non-steel workers | 280,871 | 14,479 | 266,392 |

Notes: steel workers: individuals formerly employed in the steel industry.
Source: own calculations, based on Austrian Social Security data.

consider the REBP as an exogenous policy if and only if the 'no idiosyncratic shock condition' holds.

Exogeneity of the REBP in this first identification strategy may hold due to the fact mentioned in the previous section that it is not possible to detect substantial differences in unemployment outcomes across treated and control regions. However, the fact that the REBP was targeted towards the steel industry, which was undergoing major restructuring casts, doubts on the assumption of policy exogeneity in Strategy I.

### *3.1.2. Strategy II: excluding steel workers*

Section 2 shows that the REBP was part of a steel industry restructuring plan. In particular, it became easier to lay off older workers. Thus, one likely reaction of steel firms was to cut back on hiring older workers more strongly than younger workers in the period when the REBP was in effect. Of course, such a hiring policy will directly violate the identifying assumption of Strategy I due to a hiring bias against treated workers during the REBP-period[11]. Moreover, the presence of additional measures targeted at the steel industry implies that, for individuals previously employed in the steel industry, it is not possible to separate the REBP effect from other measures.

To what extent did the steel crisis cause worse labor market prospects for older steel workers in the treated regions? A first look at the data suggests that there are large differences between steel workers and non-steel workers (Table 1). The average duration of a completed unemployment spell in the age group 45–54 is almost twice as long for a steel worker as compared to a worker from a different industry. Steel workers are less likely to reenter employment: only 66% of spells end in a transition to a new job, as

[11] A second reason why the identifying assumption in Strategy I may be violated is that the job separation rate might have been affected by REBP (Winter-Ebmer, 2003). Note, however, that if firms fire the least productive workers first, an increase in the separation rate implies that the average quality of the unemployment pool is higher in the treated regions. As better workers have better chances to get a new job, we underestimate the program effect by not accounting for the selectivity of the unemployment pool. Moreover, the effect of REBP on job separations is greatly reduced by omitting steel workers (Lalive and Zweimüller, 2002b).

opposed to more than 85% of spells by workers from other industries. Moreover, the difference between treated spells and non-treated spells in the percentage of spells ever ending in a regular job is 40% points for steel worker whereas the corresponding gap is only 12% points for non-steel workers.

This evidence suggests that steel workers might have been hit by a severe drop in demand during the period when the REBP was in effect[12]. This violates the identifying assumption of Strategy I. Indeed, the policy was motivated by the fact that one should provide sufficient protection to elderly individuals previously employed in the steel sector (Hesoun, 1988). A possible way to take care for this source of bias is to *exclude* these workers from the analysis. This second strategy identifies the causal effect of the REBP on unemployment duration if and only if there are *no idiosyncratic shocks to treated workers not previously employed in the steel sector.*

### *3.1.3. Strategy III: treated regions with favorable labor market conditions and a small steel sector*

The third identification strategy takes the importance of spillovers into account. The idea of the REBP was to target entire regions with a dominant steel sector, not merely individuals previously employed in the nationalized steel sector of these regions (Hesoun, 1988). Spillovers are to be expected, for instance, in sectors, which are directly linked with the steel sector either via the factor market (steel serving as an input) or via the product market (industries supplying raw materials or services relevant in the steel production). Further indirect effects are to be expected via a reduced purchasing power of the workers employed in the crisis-ridden steel sector.

As mentioned above, the REBP was initially granted to 28 regions but after the reform in 1991 eligibility was restricted to 22 regions (TR2). Unfortunately, the records of the relevant meetings where the decision to exclude these six regions from the REBP was made, are not open to the public. However, aggregate statistics and geography reveal two important clues as to why entitlement to REBP was stopped for these six regions. First, TR1s were characterized by a smaller steel sector than TR2s. In May 1988, the percentage of steel workers was only 8.9% in TR1s, as opposed to 19.2% in TR2. Moreover, all TR1s (but not all TR2s) shared a border with regions that were never entitled to the program. Thus, entitlement to REBP was removed from the regions that had not been suffering from the steel crisis. More important for the purpose of this paper, the unemployed in TR1s are *similarly* affected by the steel crisis as the unemployed in CR.

To shed light on the differences in labor market conditions between CRs, TR1s, and TR2s, we look at relative employment dynamics. Fig. 1 shows the (unweighted) average employment growth rates before, during, and after the REBP. Note that these employment growth rates refer to total employment in the region and not just to older workers. Employment growth in CRs closely reflects the average business cycle conditions over the period 1986–1995: employment rose slightly before the law was enacted, rose strongly during the treatment period, and decreased to pre-treatment levels after abolishment. The

[12] In order to assess the validity of the identifying assumption of Strategy I, we have to assume that the causal effect of extended benefits is identical for steel and non-steel workers. This assumption is likely to hold (conditional on observe characteristics) because unemployment benefits do not depend on previous industry.

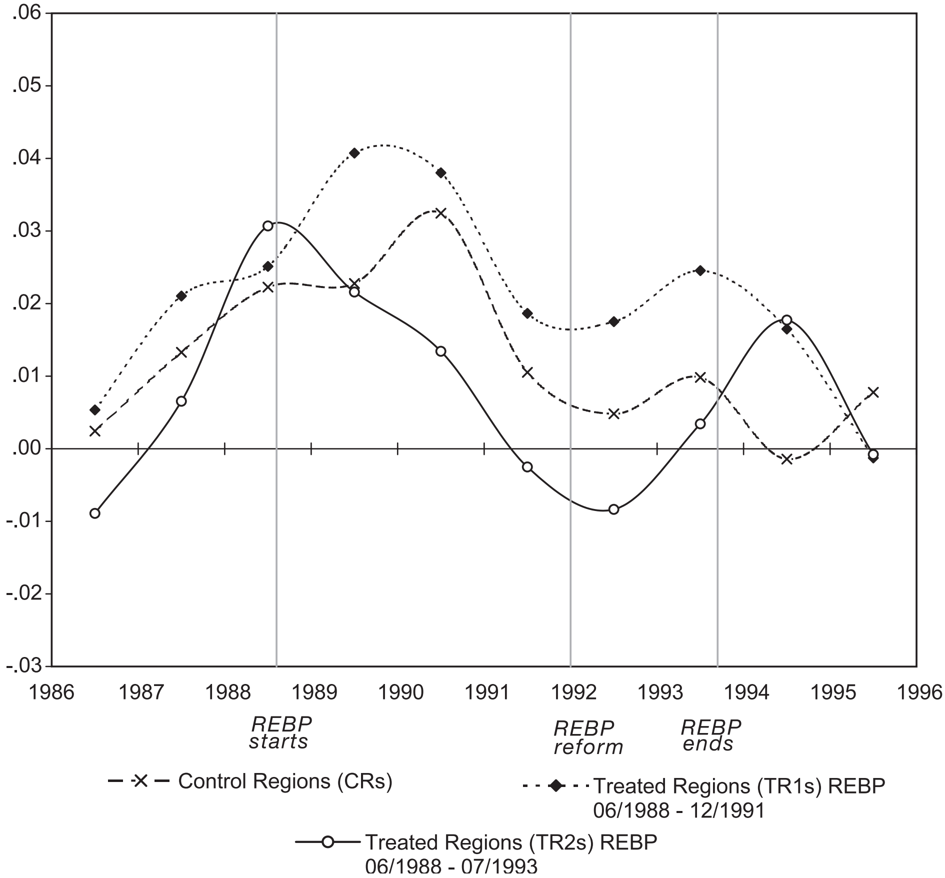


Fig. 1. Employment change per region (unweighted average): 1986–1995.

pattern of fluctuations is similar in the treated regions but their level is quite different. Employment growth rates in TR2s fell short of those in CRs in all years except in 1988 and in 1994. However, employment growth rates in TR1s *exceeded* those in CRs in all years except in 1995[13].

Our third identification strategy relies on non-steel workers and excludes unemployed workers from TR2s. The variation in REBP entitlement in this sub-sample is exogenous due to two reasons. First, in TR1s, the negative spillovers from the problems in the steel sector are as important as in CR. Second, the fact that aggregate employment growth was not significantly different in TR1s than in CRs, again suggests that unemployment outcomes in TR1s may be captured well by these outcomes in CRs. Thus, it can be argued that there was *no idiosyncratic shock* to labor market outcomes of the entitled workers in TR1.

[13] For a more comprehensive analysis of business cycle conditions see Lalive and Zweimüller (2002a,b).

Given that TR1s were not characterized by a dominant steel sector and that labor demand did not differ from the control regions, why were these regions included in the REBP in the first place? The ministry of social affairs had to make the final political decision about the districts to be covered by the program. The government during that period was a coalition among the SPÖ and the conservative party (ÖVP). After the 1986-election, TR2s were dominated by the SPÖ whereas TR1s were dominated by the ÖVP. Furthermore, the 1988 chairman of the ÖVP originally grew up in a TR1. Taken together, these facts provide an explanation for the inclusion of these regions in the first place: to get support for the program, social democrats had to concede REBP-status also to certain labor market districts that were dominated by the conservatives[14].

*3.1.4. Strategy IV: employment and residence as eligibility criteria*

Our fourth identification strategy is based on the second important change in entitlement rules of the 1991 reform. As of January 1992, entitlement to the REBP was restricted to individuals with residence and previous employment in a REBP-region whereas before the reform entitlement to the REBP was solely dependent on residence in such a region. The 'first' group, individuals with residence and previous employment in TR2s, was entitled to the REBP from June 1988 until August 1993. The 'second group', individuals with residence in TR2s and previous employment outside TR2s, was entitled to the REBP from June 1988 only until December 1991. The last identification strategy relies on these two groups (excluding, again, steel workers). Variation in benefit entitlement is exogenous if there is no idiosyncratic shock to unemployment outcomes of the 'first group' in the post-reform period (January 1992–August 1993).

There are essentially three objections against the exogeneity assumption. First, a priori reasoning may lead to expect strong differences between these two groups. Second, the REBP may have altered dramatically the incentives to lay off particular workers or the incentives to live in a particular region. CR firms with location close to TR2s may have increased layoffs among the entitled workers living in TR2s and CR residents knowing they would loose their jobs may have decided to move to TR2s in order to gain eligibility. The third concern relies on forward looking policy. To the extent that policy makers accurately foresaw that workers with previous employment will face a worse labor market in the post-reform period, policy exogeneity is violated.

It is possible to investigate the first two concerns by looking at the data. Table 2 displays the risk of long-term unemployment for individuals aged 50 and older, living in TR2s, by region of previous employer and time period. The first concern implies that there are strong differences in labor market outcomes across the two groups. However, we find that there are only insignificant differences in the risk of long-term unemployment, both, before the REBP was introduced and in the pre-reform period. After the abolishment of the program there is a significant but small (and insignificant relative to the pre-program period) group-difference

[14] This hypothesis is further reinforced by the fact that a change in the leadership of the conservative party that took place in 1991 when a chaiman was appointed that did not come originally from a TR1 region. Thus it seems that TR1 lost entitlement to REBP when the political pressure emanating from the chairman of the coalition party was removed.

Table 2
Does previous employment matter for the risk of long-term unemployment

| Residence/year | Before REBP | Pre-reform period | Post-reform period | After REBP |
|---|---|---|---|---|
| Resident and employed | 5.33 | 16.77 | 25.36 | 11.60 |
| Just resident | 6.74 | 17.27 | 21.05 | 13.87 |
| Employment effect | − 1.41 (0.80) | − 0.50 (0.88) | 4.31 (1.27) | − 2.27 (0.96) |
| Share of 'just resident' | 0.31 | 0.34 | 0.33 | 0.35 |

Notes: Non-steel workers, aged 50 or older, resident in TR2. Notes: share of spells lasting longer than 12 months * 100; standard error in parentheses.
Source: own calculations, based on Austrian Social Security data.

in the risk of long-term unemployment. Note further that, during the *post-reform* period, the first group was entitled to the REBP whereas the second group was not.

The second concern implies that the composition of the inflow was affected by the eligibility status of the two groups. Table 2 shows that there is some evidence of a change in the composition of the inflow pool between the pre-REBP and pre-reform period: the share of the 'just resident' group increases from 31 to 34%. However, there is *no evidence* of such a change between the *post-reform and post-REBP* period. It is the comparison of the latter two periods that allows us to identify the REBP effect. Note further that the change in the composition of the inflow pool between the pre-REBP and the pre-reform period does not affect group-differences in the risk of long-term unemployment. There are several reasons for this low degree of mobility or selective firing. First of all, mobility costs strongly increase with age. As only individuals older than 50 were eligible there is no reason to expect a very strong impact of that program on mobility choices. Second entitlement rules required that an individual had to be resident in a REBP region at least 6 months before entering unemployment. This requirement was even increased to 12 months after the reform. Finally, there is evidence that only steel firms relied upon the REBP in layoff decisions (Lalive and Zweimüller, 2002b).

The third concern against the policy exogeneity assumption concerns the intention behind the REBP-reform. The main reasons were (i) increasing concern about the discriminatory character of the program, (ii) budgetary pressures, and (iii) individual cases of misuse[15]. This suggests that the reform of the REBP *was not driven* by the differences in labor market conditions between the two groups. Moreover, in the previous subsection we have presented evidence that correct anticipation of adverse shocks did not hold for a substantial group of individuals covered by the REBP. Thus, it appears that these three concerns against our fourth identification strategy cannot be substantiated.

REBP differs from conventional programs in scope. This may violate the 'SUTVA' (Rubin, 1974)[16]. This assumption holds that the assignment of a treatment to a unit does not affect other units. For instance, REBP may affect the unemployment exit rate of younger age groups in treated regions due to reduced competition for jobs. We note, however, that the REBP only applied to individuals who spent at least 60% of the previous 25 years in regular employment. Roughly 40% of all unemployment spells suffered by

[15] Personal communication with Roland Sauer and Stefan Potmesil, Ministry for Economic Affairs (BMWA), Vienna.

[16] See Rosenbaum (2002) for a thorough discussion of this assumption.

individuals above 50 were not affected by REBP. We note further that the problem is less prevalent in strategies III and IV. Strategy III relies on TR1s all of which share a common border with a CR. Thus, older individuals living just across the border in a control region may have been competing for the same jobs as the age group just below 50 living within TR1. Strategy IV relies only on individuals aged 50 or older. Thus, it is not possible that potential effects of the program on the age group below 50 are confounded with the REBP effects. Moreover, it is possible to investigate the question whether the unemployment exit rate of the group 45–49 was affected by the REBP by looking at the data in Section 5.

Because the REBP entails such a substantial change in policy, there may also have been effects on wage setting. For instance, more generous unemployment insurance may increase the bargaining strength of workers which will be reflected in higher wages. It is beyond the scope of this paper to investigate this issue in more detail. (For an explorative analysis of this issue see Lalive and Zweimüller, 2002b.)

## 4. Statistical model

The focus of the analysis is on the effect of the REBP on the transition rate from unemployment to regular jobs. We consider spells ending with a transition to retirement (2.2% of all spells), long-term sickness (5.1%), out of labor force (7.7%), and no transition (1.9%) as right-censored. The escape rate from unemployment to regular jobs, denoted by $\theta$, is given by

$$\theta(\tau; A, \Delta_1, \Delta_2, \mathrm{TR}, x) = \lambda(\tau)\exp(\alpha x + \beta_0\Delta_1 + \beta_1\Delta_2 + \beta_2\mathrm{TR} + \beta_3 A + \gamma_1\Delta_1\mathrm{TR} + \gamma_2\Delta_1 A + \gamma_3\Delta_2\mathrm{TR} + \gamma_4\Delta_2 A + \delta_1\Delta_1\mathrm{TR}\cdot A + \delta_2\Delta_2\mathrm{TR}\cdot A)$$

where $\tau$ is the elapsed duration of unemployment; TR is a dummy variable that indicates whether or not an individual lives in a treated region; $\Delta_1$ is an indicator taking the value 1 as of the date when the REBP *started* (June 1988), $\Delta_2$ is the indicator taking the value 1 as of the date when this law was *abolished* (January 1992 if TR = TR1 or August 1993 if TR = TR2)[17]; $A$ is an age dummy that indicates whether or not an individual is 50 years or older; $x$ is a vector of additional control variables and $\alpha$ is the corresponding vector of coefficients; and finally $\lambda$ is the baseline hazard.

The $\beta$-coefficients control, respectively, for differences in unemployment escape rates due to changes in job prospects over time ($\beta_0$ and $\beta_1$), permanent differences across regions ($\beta_2$) and permanent differences between workers under 50 and those 50 and above ($\beta_3$). The $\gamma$-coefficients capture time-series changes in job-chances between regions and between age-groups, respectively, as of the date when the REBP starts ($\gamma_1$ and $\gamma_2$) and as of the date when the law was abolished ($\gamma_3$ and $\gamma_4$).

The interesting coefficients are the two $\delta$-coefficients: these coefficients capture all variation in unemployment durations specific to the older individuals (relative to the younger) in the treated regions (relative to control-regions) in the years when the law was in effect (relative to before the law; $\delta_1$) and in the years after which the law was abolished

[17] These variables vary within a spell if the spell starts in one period and ends in a different period.

(relative to during the law;$\delta_2$). These coefficients are the DiDiD-estimates of, respectively, introducing and abolishing the REBP. Clearly, if the policy effects are symmetric we have $\delta_1 = -\delta_2$. Hence, we can test for *persistence-effects* of the REBP which occur if $\delta_1 > -\delta_2$, when the hazard rate in the treated regions does not return to its pre-introduction level. This statistical model is used in Strategies I through III.

The statistical model used in Strategy IV is based on workers aged 50 or older, living in TR2s. This means that the age dummy $A$ and the treated region dummy TR do not exhibit variation. Let $\Delta_3 = 1$ after the reform was enacted and $E = 1$ if the individual was employed in TR1s prior to the unemployment spell. The statistical model used in Strategy IV is

$$\theta(\tau; \Delta_1, \Delta_2, \Delta_3, x) = \lambda(\tau)\exp(\tilde{\alpha}x + \tilde{\beta}_0\Delta_1 + \tilde{\beta}_1\Delta_2 + \tilde{\beta}_2\Delta_3 + \tilde{\beta}_3E + \tilde{\gamma}_1\Delta_1E + \tilde{\gamma}_2\Delta_2E + \tilde{\gamma}_3\Delta_3E)$$

Again, the parameters $\tilde{\beta}_0$,$\tilde{\beta}_1$,$\tilde{\beta}_2$ capture the time trend in the exit rate of the group 'just resident' in TR2, and $\tilde{\beta}_3$ measures the difference in the exit rate between the group which is 'just resident' and the group 'resident and employed' in TR2 the 'employment effect'. The parameter $\tilde{\gamma}_1$ measures the change in the employment effect because when the REBP was introduced until the reform, both groups were entitled to REBP. The parameters $\tilde{\gamma}_2$,$\tilde{\gamma}_3$ reflect the differences in the exit rate across the two groups due to REBP.

The escape rate from unemployment to a regular job is assumed to be multiplicatively separable in elapsed duration $\tau$ and covariates. This means that we can estimate the model by partial likelihood, as proposed originally by Cox (1972). The advantage of this approach is that duration dependence of the baseline hazard $\lambda(\tau)$ can be left unspecified in the Cox-proportional hazard model. This is important because parameter estimates tend to be biased if the duration dependence pattern is misspecified. In all estimates, inference is based on robust standard errors that take into account the possibility that observations may not be independent within labor market regions[18].

## 5. Results

As outlined in Section 3 the empirical analysis estimating the causal effect of the extended benefits granted by the REBP consists of four strategies that rely on different assumptions. We start with a descriptive analysis and then discuss the results.

### 5.1. Descriptive analysis

Fig. 2 contains first descriptive evidence on the importance of considering different identification strategies. Panel a) displays the risk of long-term unemployment (share of unemployment spells longer than 12 months) of spells started by workers between 1986 and

[18] This problem is discussed in the regression context by Moulton (1990). Lin and Wei (1989) develop the robust estimator for the Cox Proportional Hazard model that is applied in the present context. Note that allowing for possible deviations from independence within labor market regions also addresses the concern with serial correlation in DiDiD analyses (Bertrand et al., 2002). We correct for clustering within the labor market regions of the Austrian unemployment insurance administration (AMS). There are roughly 100 of these regions.

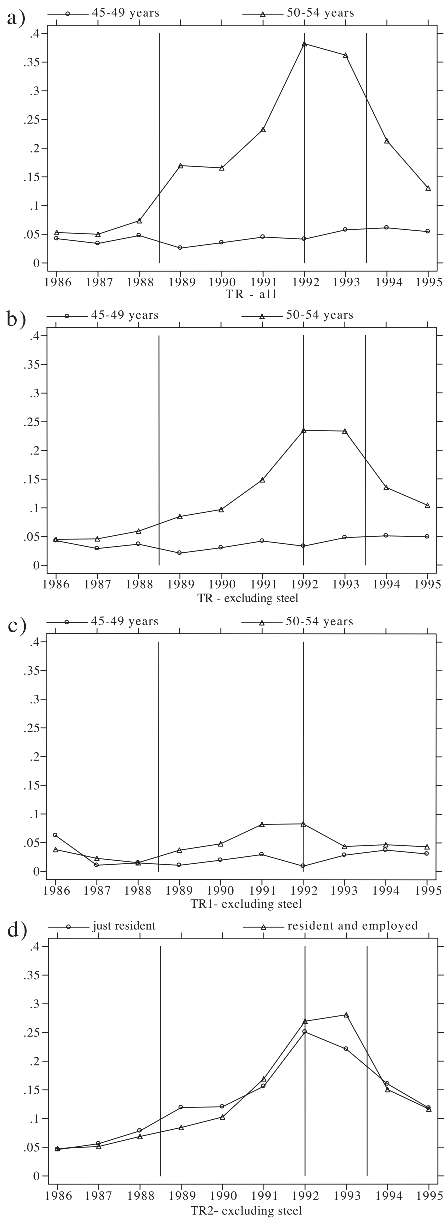


Fig. 2. Risk of long-term unemployment.
Notes: Each point in the graph represents the risk of long-term unemployment (share of spells lasting longer than 12 months) observed in the inflow of 6 months before and 6 months after January 1 of the respective year (with the exception of 1986 and 1995). Vertical bars indicate changes in REBP. These are REBP start (June 1988); REBP reform excluding TR1 and the group “just resident” (December 1991); and REBP end (August 1993).
Source: Own calculations based on Austrian social security data.

1995, living in regions covered by the REBP, separately for the (non-eligible) age group 45–49 and for the (eligible) age group 50–54. The vertical bars indicate the period during which this latter group was entitled to extended benefits. Clearly, the risk of long-term unemployment increases very strongly for individuals aged 50 or older, from about 0.07 in 1987 to almost 0.40 in 1991. As of 1992, there is a strong decline in the risk of long-term unemployment. However, the risk of long-term unemployment of older workers in treated regions remains above that of younger workers even after REBP was abolished in August 1993.

Panel b) shows that excluding steel workers from the analysis changes the picture: unemployment outcomes of older individuals do not deteriorate as strongly in the period from 1988 until 1991 as in the sample that includes also steel workers. The highest risk of long-term unemployment is about 0.25. Again, as of 1992, the risk of remaining unemployed for longer than 1 year decreases in the group aged 50 or older but remains substantially higher than in the group below age 50. In sum, excluding steel workers appears to be important *quantitatively*. The remaining sample, however, shows a *qualitatively* similar pattern as above.

Panel c) shows that focussing on TR1s has important effects on the difference in the risk of long-term unemployment between the entitled and the control group. The increase in the risk of long-term unemployment associated with the REBP is much weaker than in the previous two panels of Fig. 1; the highest long-term unemployment risk is below 0.10. Also, the risk of long-term unemployment declines when the REBP is abolished and reaches about the same level as the one in the age group 45–49. Hence, a first descriptive look at the data suggests that the indirect spillover effects may be quantitatively important.

Finally, panel d) reports the risk of long-term unemployment in TR2 for workers aged 50 or older, by location of the previous employer. This panel shows that—with the exception of 1989—the dynamic pattern of the risk of long-term unemployment is very similar across both groups in all periods with identical eligibility status. In the post-reform period (January 1992 until August 1993) there is a strong difference in the risk of long-term unemployment between the two groups. This difference is in line with the different eligibility status. Note that both, panel c) and panel d) attribute an equal difference of slightly less than 10% points in the risk of long-term unemployment to REBP. Also, in both panels, REBP introduction and abolishment has a symmetric effect on the outcome.

#### *5.1.1. Strategy I: the DiDiD estimator*

Table 3 shows the results of implementing Strategy I through IV in a Cox-proportional hazard model for transitions to a regular job. In the estimates, we control for marital status, education, skill level, nationality, type of job, recall status, previous industry, quarter of inflow, unemployment history, work experience, and the regional unemployment rate in the age bracket 45–54. This last variable is measured *at the beginning* of the second quarter of each year and is time-varying. This variable is a measure for competition for jobs. Table A1 reports results on the effect of all these covariates.

Column 'Strategy I' in Table 3 shows results regarding the first identification strategy. These indicate that the effect of introducing the REBP *is very large*[19]. The log hazard rate

[19] See Table 4 for simulations showing the effect on expected duration.

Table 3
The causal effect of REBP on unemployment duration: accounting for policy endogeneity

| | Strategy I The DiDiD estimator | Strategy II Excluding steel workers | Strategy III Favorable labor market and small steel sector in TR1 | Strategy IV Employment and residence in TR2 |
|---|---|---|---|---|
| REBP introduced | − 0.477*** (0.064) | − 0.294*** (0.039) | − 0.185*** (0.062) | |
| REBP abolished | 0.390*** (0.060) | 0.255*** (0.040) | 0.185*** (0.045) | − 0.186** (0.085) |
| REBP abolished[a] | | | | 0.185*** (0.067) |
| Change in employment effect[b] | | | | 0.034 (0.062) |
| Log likelihood | − 3,045,483.1 | − 2,774,739.5 | − 2,277,674.8 | − 154,249.7 |
| Number of spells | 312,076 | 280,871 | 233,223 | 22,091 |

Notes: asymptotic standard errors in parentheses. Standard errors adjusted for clustering within labor market regions (Lin and Wei, 1989). ***, **. * denotes 1%, 5%, and 10% level of significance. Controls: marital status, education, skill level, nationality, hours in new job, recall, previous industry, unemployment, employment history, and regional unemployment rate in 45–54 age bracket (time-varying).

[a] Strategy IV: First 'REBP abolished' effect refers to group that is only resident but not previously employed in TR2. Second 'REBP abolished' effect refers to the group that is resident and previously employed in TR2.

[b] Change in employment effect measures the change in the ratio of the transition rate to regular jobs of individuals who are resident and have been previously employed in TR2 to the same transition rate of those who are just resident in TR2.

See Table A1 for complete results.

Source: own calculations, based on Austrian Social Security data.

decreases by 0.477 which means that the transition rate from unemployment to a regular job for the treated individuals with 209 week of benefit duration is about 37.9% $(1-\exp(-0.477)=0.379)$ lower than in the counterfactual state of a potential benefit duration of 30 weeks.

Does abolishing the benefit program lead to a reversal of this picture? Table 3 shows that it does, but it also shows that there is an asymmetry between the effect of the introduction and the effect of the abolishment of the REBP. The log hazard rate increases by 0.390 after abolishment of the law which is quantitatively large but significantly lower in size than the effect of introduction.

There are basically two possible reasons for this asymmetry. First, a long-term program such as the REBP causes a persistent change in behavior and escape rates may not return to pre-program levels after the law has been abolished. The second possible reason is that the estimated effects do not only measure effects caused by extended benefits, but also group specific differences in labor market shocks. In the latter case, the estimated asymmetry is due to larger differences in the labor market conditions *before and during* the program, as compared to the differences *during and after* the program.

Clearly, the conclusion that the REBP has caused strong effects on escape rates from unemployment to a regular job and that there are asymmetric effects between the introduction and the abolishment of the program depends on the validity of our identifying assumption. The DiDiD estimator relies upon a strong assumption: namely that there are no shocks idiosyncratic to the group of treated individuals.

*5.1.2. Strategy II: excluding steel workers*

The second identification strategy consists of a DiDiD analysis *excluding* steel workers. If steel workers suffered from specific shocks this identification strategy is expected to deliver different results from the first one (DiDiD on the entire sample). If such adverse shocks to labor market prospects were concentrated in the steel sector, leaving other sectors unaffected, excluding steel workers is a valid strategy to eliminate the policy endogeneity bias.

Table 3 presents the results from this analysis (Column 'Strategy II'). The number of spells used for this analysis is 280,871 because data on 31,205 individual spells with previous employment in the steel industry have been omitted[20]. The treatment effects, both the effect of introduction and the effect of abolishment, are considerably lower in absolute value. The quantitative impact of the REBP-introduction reduces by more than a third (in absolute value), from $-0.477$ to $-0.294$. Similarly, the estimated effect of the abolishment of the program decreases from 0.390 to 0.255. However, the effect of introducing the REBP is estimated to be stronger than the effect of abolishing the REBP, i.e. the asymmetry is still apparent.

*5.1.3. Strategy III: treated regions with favorable labor market conditions and a small steel sector*

Column 'Strategy III' in Table 3 presents the results from the identification strategy that relies on variation in REBP that was argued to be exogenous in Section 3. The idea is to identify the causal effect of the REBP by using data on treated individuals within the set of TR1s which, are characterized by a small steel sector and did not experience a severe downturn in labor demand.

The estimated REBP coefficients are substantially smaller in absolute value[21]. The quantitative impact of the benefit extension is highly significant, but quantitatively not particularly strong. Introducing or removing the REBP-status changes the log-hazard rate by 0.185. Compared to the results of the second identification strategy, the effect of introduction reduces more strongly than the effect of abolishment of the REBP. Interestingly, the effects of introduction and abolishment are now symmetric.

A potential explanation is that negative spillover effects that were transmitted by the steel crises were strongest when this crises was at its peak. The peak occurred when the REBP was in effect, hence the introduction effect is large. A procedure that removes those indirect effects should estimate a considerably lower introduction effect. Indirect effects are of lower importance at the end of the REBP period and differences in labor market conditions have become less pronounced. Hence an estimate that is not afflicted with these indirect effects should not be very much smaller than an estimate that does not account for these indirect effects.

20 An alternative, albeit more restrictive, approach to assess the stability of the REBP-effect would be to add a complete set of interactions for job seekers that were previously employed in the steel sector. Results based on that procedure are very similar to those in Table 1.

21 The number of spells used in this analysis is 233,223 consisting of all non-steel, non-TR2 residence spells. Thus, information on 47,648 unemployment spells of non-steel workers with residence in TR2 is discarded.

#### 5.1.4. Strategy IV: employment and residence as eligibility criteria

In the last column, Table 3 presents results regarding Strategy IV. For the second group (just resident in TR2) covered in our sample, abolishment of the REBP leads to an increase in the log hazard rate of 0.186[22]. There is a corresponding decrease in the hazard rate when the REBP was also abolished for the first group (the effect on the log hazard being 0.185)[23]. Thus, the results are almost identical to those obtained in the third identification strategy.

An additional advantage of our fourth identification procedure is that one can test whether the second group is an appropriate comparison group to the first one. In the pre-reform period both groups had the same eligibility status and any differences in the exit behavior between them should be entirely due to labor market conditions. The results in Table 3 suggest that, in fact, there were no significant differences in escape rates between the two groups in the pre-reform period (coefficient 'Change in Employment effect', $\tilde{\gamma}_1$).

### 5.2. Discussion of benefit entitlement results and SUTVA

The fact that the previous two 'robust' identification strategies produce identical results is remarkable because these two procedures are quite different. First, Strategy III relies on the identifying assumption, that eligible workers in TR1s face similar labor market conditions as non-eligible workers in CRs. In contrast, the present Strategy IV assumes that there are no idiosyncratic shocks to labor market prospects *with respect to employment in TR2 for those who live in TR2*. Moreover, the results are based on entirely different data in the last as compared to the previous strategy. No spell that is used in the previous procedure is used in the final procedure.

The fact that Strategy III and Strategy IV deliver identical results suggests that both strategies identify the causal effect of the REBP on unemployment duration. Confidence in the estimated effect is further supported by the fact that the estimated effects are similar when introducing the program as when abolishing the program (Strategy III), and that abolishing the program has the same effect for two different groups of workers (Strategy IV). Such a result is not likely to arise if the identifying assumptions are violated.

To what extent may the stable unit treatment value assumption (SUTVA) have been violated? If we are willing to assume that, without the program, the outflow rate of the

[22] The parameter estimate shows the change in the hazard rate for the first group compared to the second group. The abolishment effect is thus $-1$ times this parameter estimate.

[23] Note that allowing for the fact that the unemployment exit rate tends to increase strongly when unemployment benefits are exhausted (Meyer, 1990) does not alter the results reported in Table 1. When we allow for changes in the hazard rate as a function of time until benefit exhaustion, the estimated REBP effects are $-0.178$ ("REBP introduced"; Strategy III), 0.176 ("REBP abolished; Strategy III), $-0.179$ ("REBP abolished, employed and resident"; Strategy IV), 0.179 ("REBP abolished, resident"; Strategy IV). All REBP effects are significantly different from zero (5% level). The "change in employment effect" is not significantly different from zero; this indicates that there were no differences in the transition rate to regular jobs with respect to previous employment in TR2. The main reason for this minor change in results is that benefit exhaustion effects are not quantitatively important.

group aged 45–49 in TRs would have been identical to the exit rate of the same age group in CRs, one can investigate this question. Table A1 reports all DiDiD coefficients. The coefficient 'During * TR' reflects the regional difference in the outflow rate for the age group 45–49 associated with REBP. Results suggest that the outflow rate in treated regions did increase when the REBP was introduced, significantly (Strategies I and II) and insignificantly (Strategy III). This finding is in line with the concern raised in Section 3. However, if the change in the outflow rate has indeed been brought about by the REBP, the coefficient 'After * TR' should be negative and of about the same magnitude as the 'During * TR' coefficient. Table A1 shows that the 'After * TR' effect is positive, significant (Strategy II) and insignificant (Strategy I and III). This is in contrast to the interpretation that the outflow rate of younger individuals in treated regions was affected by the REBP. Thus, violations of SUTVA do not seem to be important in the present application.

### *5.3. Socio-economic and other characteristics*

All results presented in Table 3 are based on hazard rate models that include a large number of covariates (Table A1 reports full results for all four identification strategies). Here we mention the importance of the various determinants only briefly. Married males have higher escape rates from unemployment than unmarried workers. A medium level of education (apprenticeship) decreases the escape rate slightly (relative to low education), but the exit rate of highly educated workers is well below the one of the lower educated[24]. Blue collar workers have much higher transition rates to job than white collar workers and immigrants have a higher transition rate than natives. Previous industry turns out very important. On the one side, seasonal industries (tourism and construction) have shorter unemployment spells which is in part the result of recalls by previous employers. On the other side, workers from the steel industry face job chances significantly lower than in other industries (based on column 'Strategy I'). Moreover, the previous unemployment and employment history is an important determinant of the duration of unemployment spells. Unemployment in the past is associated with a higher escape rate, but the cumulative duration of past unemployment decreases the chance to get a job. A continuous working career in the past decreases the exit rate from unemployment. Finally, the regional labor market conditions are of high importance. This variable is approximated by the regional unemployment rate of males aged 45–54 measured *at the beginning* of the second quarter of each year[25]. Hence, this variable is a measure for competition for jobs and a predetermined variable. The higher the regional unemployment rate the lower is the individual unemployment exit rate.

[24] One reason for this result may be that the higher educated individuals are more specialised and thus the job offer rate may be lower for individuals with higher as opposed to lower education.

[25] This covariate is predetermined and varies each year. This means that the information on the regional unemployment rate is updated for all spells that are in progress at that time. This information is predetermined because we only use historical information in explaining the transition rate in the year that follows.

Table 4
Simulations of the effect of REBP on expected unemployment duration (measured in weeks)

| | Estimated effect on log hazard | Expected unemployment duration | Effect on unemployment duration | Increase per week | Increase in percent of control |
|---|---|---|---|---|---|
| Reference[a] | | 18.9 | | | |
| *REBP* | | | | | |
| Strategy I | − 0.477 | 52.7 | 33.8 | 0.189 | 178.9 |
| Strategy II | − 0.294 | 36.4 | 17.5 | 0.098 | 92.8 |
| Strategy III | − 0.185 | 28.7 | 9.8 | 0.055 | 52.1 |
| Strategy IV | − 0.186 | 28.8 | 9.9 | 0.055 | 52.4 |

Notes: Strategy I: entire sample; Strategy II: excluding steel workers; Strategy III: excluding TR2 residents and steel workers; Strategy IV: TR2 residents, aged 50 or older, non-steel workers.

[a] Married, low education, blue collar worker, Austrian, looking for part-time or unknown type of job, not recall, inflow in first quarter, from other industry, unemployed at least once, and with average values of the continuous covariates (unemployment duration in the past, work experience, and regional unemployment rate). Source: own calculations, based on Austrian Social Security data.

### *5.4. Simulations*

We can use the results from the various procedures to simulate the impact of the REBP on the duration of unemployment of a typical worker (Table 4)[26]. Expected unemployment duration for this worker is almost 19 weeks. How much does the REBP affect this expected duration? According to the first identification strategy, the benefit extension will lead to a 3-fold increase in the expected unemployment duration, which amounts to almost 53 weeks. The results imply an increase of 0.189 weeks of unemployment per additional week of extended benefits. The corresponding simulation from the second identification strategy yields less dramatic results, but also predicts a doubling of the duration of unemployment and an increase in 0.098 weeks per additional week of unemployment. Finally, the 'exogenous' identification strategies predict that the REBP increases unemployment duration by 50% and this increase amounts to 0.055 per additional week of extended benefits.

How do these estimates from the robust identification strategies compare to other studies?[27] Our result is well below the range of the estimates found in studies concerning the US system. For instance, Katz and Meyer (1990) identify an increase of 0.16–0.20

[26] The reference individual is married, has low education, is a blue collar worker, Austrian, looking for part-time or unknown type of job, not on recall, became unemployed in the first quarter, from other industry, was unemployed at least once, and has average values of the continuous covariates (unemployment duration in the past, work experience, and regional unemployment rate). Expected duration of unemployment until a regular job starts is $E(T\mid x)=\lim_{U\to\infty}\int_0^U\exp(-\int_0^t\hat{\theta}(z)\mathrm{d}z)\mathrm{d}t$ where $\hat{\theta}(z)$ is the estimated transition rate to jobs for the reference worker (Lancaster, 1990). $U$ was chosen such that simulated expected duration converges. Note that there is a highly non-linear relationship between the transition rate to jobs and expected duration.

[27] It is difficult to compare the increase in unemployment duration per additional weeks of unemployment benefits to standard estimates of the labor supply elasticity (Blundell and MaCurdy, 1999). Whereas the numerator is similar in both measures of the change in labor supply due to a change in transfer payments, the denominator is different. In order to make this comparison, one would have to calculate the expected increase in transfers due to REBP. This is difficult in the present setting because the payoff to unemployment also depends on the availability of unemployment assistance. We do not have data on unemployment assistance that allows us to perform this calculation.

weeks per additional week of benefit entitlement. The first reason for the fact that the estimates identified in the present study are substantially smaller could be that Katz and Meyer (1990) do not account for the endogeneity of extended benefits, which for the case of Austria turns out very important. Indeed, the study by Card and Levine (2000) that accounts for policy endogeneity finds a substantially smaller effect per week of about 0.08[28]. Thus, policy endogeneity is an important problem also in US studies. A second reason is that the end of entitlement to regular UB entails a smaller drop in income in the case of Austria compared to the US Individuals whose benefits have run out may rely on "transfer payments for those in need" ('Notstandshilfe') if they pass the means-test[29]. A final reason for the comparably low REBP-impact may simply come from the fact that the benefit extension was so large. An increase in benefit entitlement from 26 to 39 weeks (the US case) will have a stronger impact than the same 13 weeks-extension if it extends benefits from 196 to 209 weeks.

The results in Katz and Meyer (1990) are close to the effects reported in other US studies (Moffitt, 1985; Moffitt and Nicholson, 1982). Ham and Rea (1987) report a somewhat larger impact of extended benefits on unemployment duration for Canada; Bratberg and Vaage (2000); Hunt (1995) find a similar effect, respectively, for Germany and for Norway.

Table 4 suggests that it is possible to decompose policy endogeneity into severe labor market problems for steel workers ('idiosyncratic steel shock') and in spillovers affecting the entire region ('steel spillover'). The change in the estimated REBP-effect when going from Strategy I to Strategy II is about 16.3 weeks (= 33.8 − 17.5). Recall, that Strategy II differs from Strategy I merely due to the exclusion of steel workers. Hence, the difference in the 'REBP-effect' between these two strategies is an estimate of the 'idiosyncratic steel shock' affecting older workers in the steel sector during the period when the REBP was in effect. The 'idiosyncratic steel shock' seems to have increased unemployment duration by about 16 weeks. This is a substantial effect compared to the causal effect of the REBP. The size of the 'steel spillover' effect can be gauged by comparing Strategy III with Strategy II: The reduction in the implied effect of the REBP is about 7.7 weeks (= 17.5 − 9.8). Recall that Strategy III concentrates on individuals residing in TR1, a set of regions with a steel sector that is comparable in size to the rest of Austria. Thus, the negative spillovers emanating from the steel sector may have prolonged individual unemployment duration by almost 8 weeks. This general equilibrium effect is substantially smaller than the idiosyncratic steel shock, and comparable in magnitude to the REBP-effect.

## 6. Conclusions

In June 1988 the Austrian government enacted, in 1991 reformed, and in August 1993 abolished the REBP. This program extended entitlement to regular UB from 30 weeks to a

[28] Note that the change in the per week effect of benefit entitlement due to policy endogeneity is almost the same as that reported in Table 4.

[29] Note that also in the US some groups among the unemployed could also rely on rather generous welfare provisions. For instance, income from unemployment insurance and income from welfare programs is quite similar for households with two children. The situation has changed due to the 1996 Welfare Reform Act that has introduced strong work requirements making it harder for unemployed individuals to draw cash benefits (Moffit, 2002).

maximum of 209 weeks for elderly individuals in certain regions. In this paper, we exploit the successive changes in unemployment insurance rules provided by the REBP and use a large and informative data set to evaluate the impact of the program on the incentives to take up a regular job.

A central problem of previous studies is that the reasons for changes in benefit rules are typically not accounted for. However, when benefit policy is endogenously determined by labor market conditions, observed changes in unemployment duration are partly due to worse job market conditions and the estimates of the impact of benefit generosity on unemployment durations will be biased. The Austrian REBP offers the unique possibility to discuss the empirical relevance of policy endogeneity in the econometric evaluation of policy measures. Moreover, the Austrian REBP offers the unique opportunity to investigate how a policy change as large as the maximum US–European differential in benefit eligibility duration might affect the duration of unemployment.

We address the endogenous policy issue by applying two different identification strategies. First, we exploit the fact that a subset of entitled regions faced employment conditions very similar to regions that were not covered by the REBP. Second, the 1991-reform of this program changed eligibility criteria such that claimants had to be not only resident but also previously employed in a treated region. This policy reform created variation in the REBP-status for workers of identical age living in the same region. Such variation is useful in discussing identification of the causal effect of benefit entitlement on unemployment duration. Both of these strategies allow a comparison between individuals that had rather similar labor market conditions but a different eligibility status. These two completely different identification strategies yield almost identical results. This can be taken as evidence that these strategies succeed in addressing policy endogeneity.

The main results are: (i) The increase in UB-entitlement from 30 to 209 weeks reduces the transition rate to jobs by 17%. The program increased expected unemployment duration by about 9 weeks, leading to an increase in unemployment duration per week of additional benefits of 0.055. (ii) The effect of introducing and the effect of abolishing the program are of the same magnitude. Hence, the behavior of individuals is symmetric with respect to the direction of the benefit change. (iii) Accounting for policy endogeneity is important. Estimates which fail to do so suggest that the transition rate to regular jobs reduces by 40% (instead of 17%) due to REBP.

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## Appendix A

Table A1
Complete results

| | Strategy I | | Strategy II | | Strategy III | | Strategy IV | |
|---|---|---|---|---|---|---|---|---|
| | Coefficient | *Z*-value | Coefficient | *Z*-value | Coefficient | *Z*-value | Coefficient | *Z*-value |
| REBP introduced (during * TR * Age 50+) | − 0.477 | ( − 7.45) | − 0.294 | ( − 7.47) | − 0.185 | ( − 2.99) | | |
| REBP abolished (After * TR * Age 50+) | 0.390 | (6.53) | 0.255 | (6.35) | 0.185 | (4.06) | | |
| During | 0.138 | (7.73) | 0.132 | (7.18) | 0.139 | (7.04) | | |
| After | 0.011 | (0.82) | 0.006 | (0.42) | − 0.001 | ( − 0.05) | | |
| TR | 0.005 | (0.20) | − 0.056 | ( − 2.67) | − 0.090 | ( − 2.31) | | |
| Age 50 + | − 0.109 | ( − 9.27) | − 0.105 | ( − 9.02) | − 0.107 | ( − 9.09) | | |
| TR * Age 50 + | − 0.012 | ( − 0.43) | 0.018 | (0.73) | 0.119 | (3.55) | | |
| During * TR | 0.059 | (2.20) | 0.069 | (2.60) | 0.065 | (1.23) | | |
| During * Age 50 + | 0.005 | (0.25) | − 0.009 | ( − 0.55) | − 0.036 | ( − 2.20) | | |
| After * TR | 0.016 | (0.64) | 0.045 | (2.08) | 0.023 | (1.53) | | |
| After * Age 50 + | 0.001 | (0.03) | − 0.009 | ( − 0.48) | − 0.051 | ( − 2.64) | | |
| During I | | | | | | | 0.019 | (0.39) |
| During II | | | | | | | 0.172 | (3.23) |
| After | | | | | | | 0.128 | (2.51) |
| Employment effect | | | | | | | − 0.025 | ( − 0.54) |
| Change in 'Employment effect' | | | | | | | 0.034 | (0.55) |
| REBP abolished for group 'just resident' | | | | | | | − 0.186 | ( − 2.18) |
| REBP abolished for group 'resident and employed' | | | | | | | 0.185 | (2.78) |
| *Change in 1989* | | | | | | | | |
| Age 45 – 49 | 0.004 | (0.34) | 0.010 | (0.74) | − 0.003 | ( − 0.23) | | |
| Age 50 – 54 | − 0.050 | ( − 2.26) | − 0.027 | ( − 1.63) | 0.015 | (0.89) | | |
| *Marital status* (*single*) | | | | | | | | |
| Married or cohabitant | 0.185 | (18.63) | 0.190 | (18.46) | 0.197 | (17.09) | 0.154 | (4.26) |
| Separated | 0.020 | (1.66) | 0.007 | (0.57) | 0.004 | (0.25) | 0.090 | (2.46) |
| *Education* (*low*) | | | | | | | | |
| Medium | − 0.020 | ( − 2.00) | − 0.019 | ( − 1.70) | − 0.018 | ( − 1.39) | − 0.016 | ( − 0.80) |
| High | − 0.162 | ( − 6.51) | − 0.161 | ( − 6.55) | − 0.173 | ( − 6.04) | 0.027 | (0.36) |

*(continued on next page)*

Table A1 (*continued*)

| | Strategy I | | Strategy II | | Strategy III | | Strategy IV | |
|---|---|---|---|---|---|---|---|---|
| | Coefficient | *Z*-value | Coefficient | *Z*-value | Coefficient | *Z*-value | Coefficient | *Z*-value |
| *Education* (*low*) | | | | | | | | |
| Blue collar | 0.682 | (33.23) | 0.706 | (33.65) | 0.715 | (28.95) | 0.695 | (13.48) |
| Foreign | 0.021 | (1.44) | 0.004 | (0.23) | 0.006 | (0.33) | 0.003 | (0.06) |
| Looking for a full time position | − 0.043 | ( − 2.66) | − 0.063 | ( − 3.83) | − 0.075 | ( − 3.98) | − 0.059 | ( − 2.21) |
| Recall | 0.267 | (17.11) | 0.272 | (17.25) | 0.265 | (14.59) | 0.375 | (11.59) |
| *Industry* (*other*) | | | | | | | | |
| Steel | − 0.131 | ( − 3.50) | | | | | | |
| Electronic machines | − 0.125 | ( − 4.92) | − 0.155 | ( − 5.79) | − 0.135 | ( − 4.89) | − 0.302 | ( − 3.03) |
| Small machines | − 0.166 | ( − 2.67) | − 0.180 | ( − 2.86) | − 0.185 | ( − 2.63) | − 0.064 | ( − 0.17) |
| Other manufacturing | − 0.021 | ( − 0.96) | − 0.033 | ( − 1.38) | − 0.026 | ( − 0.92) | − 0.116 | ( − 1.84) |
| Mining | 0.087 | (1.93) | 0.083 | (1.64) | 0.040 | (0.74) | 0.158 | (1.35) |
| Construction | 0.222 | (14.41) | 0.238 | (15.53) | 0.245 | (13.72) | 0.215 | (7.07) |
| Tourism | − 0.041 | ( − 2.31) | − 0.042 | ( − 2.22) | − 0.030 | ( − 1.40) | − 0.161 | ( − 3.08) |
| *Inflow quarter* (*I*) | | | | | | | | |
| II | − 0.020 | ( − 1.27) | − 0.031 | ( − 1.72) | − 0.019 | ( − 0.97) | − 0.113 | ( − 1.85) |
| III | − 0.064 | ( − 4.24) | − 0.093 | ( − 5.42) | − 0.093 | ( − 4.92) | − 0.061 | ( − 1.21) |
| IV | − 0.350 | ( − 15.45) | − 0.367 | ( − 15.81) | − 0.361 | ( − 13.27) | − 0.402 | ( − 10.59) |
| *Unemployment history* | | | | | | | | |
| Not unemployed | − 0.436 | ( − 27.82) | − 0.427 | ( − 24.86) | − 0.400 | ( − 23.23) | − 0.804 | ( − 12.78) |
| Unemployment duration (years) | − 0.106 | ( − 29.52) | − 0.109 | ( − 27.71) | − 0.103 | ( − 24.72) | − 0.146 | ( − 15.26) |
| Work experience (years) | − 0.017 | ( − 13.44) | − 0.015 | ( − 11.55) | − 0.012 | ( − 10.07) | − 0.034 | ( − 8.64) |
| Unemployment rate in region (%) | − 6.406 | ( − 15.65) | − 5.872 | ( − 13.33) | − 6.082 | ( − 10.69) | − 5.530 | ( − 9.36) |
| Log likelihood | − 3,045,483.10 | | − 2,774,739.50 | | − 2,277,674.80 | | − 154,249.67 | |
| Number of spells | 312,076 | | 280,871 | | 233,223 | | 22,091 | |

Notes: (a) Benefit extension coefficients are not identified because the sample is restricted to those living in entitled regions, aged 50 or older. (b) Sample restricted to non-steel individuals.

Table A2
Means and standard deviations

| | Strategy I Mean S.D. | Strategy II Mean S.D. | Strategy III Mean S.D. | Strategy IV Mean S.D. |
|---|---|---|---|---|
| *Change in 1989* | | | | |
| Age 45–49 | 0.338 | 0.344 | 0.344 | 0.000 |
| Age 50–54 | 0.323 | 0.312 | 0.307 | 0.000 |
| *Marital status* (*Single*) | | | | |
| Married or cohabitant | 0.789 | 0.790 | 0.788 | 0.827 |
| Separated | 0.111 | 0.109 | 0.019 | 0.092 |
| *Education* (*low*) | | | | |
| Medium | 0.412 | 0.403 | 0.403 | 0.375 |
| High | 0.011 | 0.011 | 0.012 | 0.009 |
| Blue collar | 0.849 | 0.856 | 0.852 | 0.870 |
| Foreign | 0.110 | 0.110 | 0.112 | 0.095 |
| Looking for a full time position | 0.267 | 0.261 | 0.245 | 0.314 |
| Recall | 0.391 | 0.422 | 0.421 | 0.413 |
| *Industry* (*other*) | | | | |
| Steel | 0.100 | 0.000 | 0.000 | 0.000 |
| Electronics machines | 0.033 | 0.037 | 0.031 | 0.071 |
| Small machines | 0.002 | 0.002 | 0.003 | 0.001 |
| Other manufacutring | 0.063 | 0.070 | 0.064 | 0.109 |
| Mining | 0.015 | 0.017 | 0.014 | 0.030 |
| Construction | 0.439 | 0.487 | 0.496 | 0.428 |
| Tourism | 0.120 | 0.133 | 0.140 | 0.089 |
| *Inflow quarter* (*I*) | | | | |
| II | 0.139 | 0.128 | 0.129 | 0.122 |
| III | 0.132 | 0.125 | 0.124 | 0.125 |
| IV | 0.421 | 0.432 | 0.432 | 0.435 |
| *Unemployment history* | | | | |
| Not unemployed | 0.220 | 0.188 | 0.181 | 0.250 |
| Unemployment duration (years) | 1.224 (1.284) | 1.304 (1.299) | 1.325 (1.305) | 1.139 (1.266) |
| Work experience (years) | 16.225 (4.706) | 16.173 (4.641) | 16.121 (4.630) | 16.832 (4.665) |
| Unemployment rate in region (%) | 4.294 (2.398) | 4.212 (2.299) | 4.054 (2.161) | 5.099 (2.740) |
| Number of spells | 312,076 | 280,871 | 233,223 | 22,091 |

Table A3
Definition of variables

| | |
|---|---|
| REBP | Regional extended benefit program: benefits extended from 30 to 209 weeks for individuals age 50 or older, living in TR, from 1988.06 to 1993.07 |
| REBP introduced (During * TR * Age 50+) | |
| REBP abolished (After * TR * Age 50+) | |
| During | 1 after 1988.06 |
| During I | 1 after 1998.06 |

(continued on next page)

Table A3 (*continued*)

| | |
|---|---|
| REBP | Regional extended benefit program: benefits extended from 30 to 209 weeks for individuals age 50 or older, living in TR, from 1988.06 to 1993.07 |
| During II | 1 after 1992.01 (reform) |
| After | 1 after 1993.08 |
| TR | 1 if Treated region 1 or Treated region 2 (see text for details). |
| TR1 | 1 if Treated region 1 |
| TR2 | 1 if Treated region 2 |
| TR2 Residence | 1 if individual lives in TR2 |
| TR2 Employment | 1 if individual was previously employed in TR2 |
| Age 50+ | a |
| TR * Age 50+ | a |
| During * TR | a |
| During * Age 50+ | a |
| After * TR | a |
| After * Age 50+ | a |
| *Change in 1989* | |
| Age 45–49 | 1 after 1989.08 and age $<$ 50 |
| Age 50–54 | 1 after 1989.08 and age $\geq$ 50 not in TR during REBP |
| *Marital status* | |
| Single | a |
| Married | a |
| Separated | a |
| *Education* | |
| Low | 9 years of schooling (mandatory) |
| Medium | apprenticeship |
| High | vocational, university degree |
| Blue collar | a |
| Foreign | a |
| Looking for a full time position | 1 if individual indicates that she or he is looking for a 100% position |
| Recall | 1 if employer will re-hire the person |
| *Industry* | |
| Other | a / agriculture, mining, food, textiles, wood, services (all except tourism) |
| Steel | a |
| Electronic machines | a |
| Small machines | a |
| Other steel | a |
| Mining | a |
| Construction | a |
| Tourism | a |
| *Unemployment history* | |
| Not unemployed | a |
| Unemployment duration (years) | Unemployment duration since January 1972, at start of unemployment spell |

Table A3 (*continued*)

| | |
|---|---|
| REBP | Regional extended benefit program: benefits extended from 30 to 209 weeks for individuals age 50 or older, living in TR, from 1988.06 to 1993.07 |
| *Unemployment history* | |
| Work experience (years) | Years spent in regular employment since January 1972, at the start of the unemployment spell |
| Unemployment rate in region (%) | Regional unemployment rate, ages 45–54, on May 10 of respective year, used as time-varying covariate |

Note: *a*, Variable is a dummy that takes the value 1 if the condition is fulfilled.

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