Marginal Jobs and Job Surplus: Evidence from Separations and Unemployment Insurance*

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Abstract

We study the role of marginal jobs in employment adjustment, in three steps. *First*, we provide evidence on job destruction in response to reductions in job surplus from improved worker outside options. Our design exploits a sharp quasi-experimental increase in unemployment benefits for older workers in Austria. The treatment effect is larger for workers with larger outside option increases, proxied for with their ex-ante risk of exhausting the pre-reform maximum benefit duration when unemployed. *Second*, we isolate and characterize the marginal matches driving this separation response, extending complier analysis to difference-in-difference settings. We find that marginal jobs originate from blue-collar occupations in industries with a high incidence of sickness and disability among older workers. Compared to surviving jobs, marginal jobs had lower earnings and lower worker fixed effects and were more prevalent in shrinking industries and firms. Taken together, our findings indicate that increasing workers' outside options destroys low-surplus jobs. *Third*, one direct implication is that outside options shift the composition of *surviving* jobs towards higher-surplus jobs. To test this prediction, we exploit the abolition of the reform to show that the formerly-treated cohorts indeed exhibit lower extensive-margin aggregate elasticities to subsequent labor demand shocks – due to the missing mass of marginal matches the reform had previously destroyed.

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1 Introduction

Employment consists of jobs with positive surplus: these jobs leave both the firm and the worker better off in the job than would their outside options. Employment falls if shocks and policies shift the surplus distribution to the left, sweeping up the mass of marginal jobs. Viable job opportunities for unemployed job seekers shrink, and marginal existing matches separate. In this paper, we shed direct empirical light on the characteristics of marginal jobs, and their role in driving extensive-margin employment adjustment. In our setting, sharp declines in job surplus arise from an increase in workers' non-employment values, brought about by a large increase in the generosity of unemployment insurance. How many employment relationships separate in response to these sharp declines in surplus? What are the characteristics of these marginal jobs, and do they indeed carry markers of low surplus? Finally, does the mass of marginal matches indeed mediate aggregate employment sensitivity to shocks?

Our empirical analysis answers these three questions guided by a basic conceptual framework. Our setting studies a simple discrete-choice framework in which the allocative concept that drives job continuation and destruction is job surplus: that is, the employer and the employee maintain a match as long as its internal value exceeds that of the two parties' outside options combined. The Coasean cause of this result is a bilaterally efficient, bargained wage that is found to leave each party to strictly prefer her share of the pie to her outside option.[1] A shift in the worker's outside option – as induced by the policy we study – then sweeps up all matches between those with zero surplus during the original policy regime up until the match with pre-reform surplus equal to the surplus reduction at hand. The latter match becomes the new marginal match, now with zero surplus under the new regime. Separation responses quantify the mass of pre-reform matches between the old and the new marginal match.[2]

To investigate the separation effect of job-specific surplus arising from workers' nonemployment value, we exploit a large increase in the generosity of unemployment insurance (UI). Specifically, we exploit a reform that extended the maximum duration of UI benefits from one year to four years, which was enacted in Austria during the period from 1988 to 1993.[3] The Austrian system featured a replacement rate of around 40% for most workers, very similar to the U.S. rate, and no experience rating, as in most OECD countries but unlike in the United States. Crucially and unlike in the U.S., Austrian quitters were eligible for UI, which ensures a reduction of surplus across *all* matches rather than in those at risk of layoff, and justifies our

[1] While we cast our conceptual framework in a general and abstract form in the main part of the paper, we develop a full dynamic equilibrium model in the Appendix with heterogeneous productivity, job amenities, endogenous vacancy values and heterogeneous labor disutility. That model features labor demand, idiosyncratic shocks in matches, as well as long-term employment relationships with workers transitioning between unemployment and employment.

[2] With frictions, inefficient separations may emerge among positive-surplus jobs as unilateral layoffs or quits.

[3] The reform was politically motivated and originally meant to cushion restructuring in the steel sector (observations from which we drop), but covered all non-steel workers in the treated regions (whom we study).

Coasean framework. The quasi-experimental features of the reform allow for comparisons along three dimensions: (i) *calendar time* (the program was in place from June 1988 until July 1993); (ii) *region* (only residents from 28 out of roughly 100 regions could get extended UI); (iii) *age* (only workers aged 50 or older were eligible).[4] In a sharp difference-in-difference design, we compare these older eligible workers to slightly younger, ineligible control cohorts within the treatment regions, as well as to workers of the same age yet ineligible due to their residence in control regions.[5] We use population administrative data with workers' daily employment and earnings biographies and month-year of birth.

In a *first* step, we test whether higher outside options, which reduce job surplus, indeed lead to separations. We present causal evidence on the effect of UI generosity on separations by plotting raw data on job survival for the reform period by birth cohort. From the jobs active at the onset of the reform, among the eligible age groups, 50% in treated regions were destroyed through the five-year policy period, compared to only 39% in the control regions. This 11ppt effect amounts to a 28% policy-induced increase in job destruction over the 39ppt base. By contrast, for the slightly younger – ineligible – control cohorts, we see no differential separation behavior before, during or after the reform. Importantly, despite the fact that Austria has no experience rating, these separations are not temporary layoffs (e.g. Feldstein (1976)). Instead, we find that the separators move into long-term non-employment, almost entirely accounted for by the reduction of employment duration with the initial employer.[6] When we investigate heterogenous responses by the predicted probability of the worker's unemployment spell to exhaust the one-year duration (which was extended to four), we find that the two top quintiles exhibit a 20ppt separation rate compared to a 11pt mean, confirming that separations arose from those workers for whom the outside option was improved the most. Finally, we exploit a retrospective survey of the reform cohorts asking workers to self-classify separations. Another difference-in-difference cohort design reveals that the incremental separations largely came from

[4]The reform applied to workers with at least 15 years of employment in the last 25 years, which is our sample restriction for control groups as well.

[5]The reform applied to workers aged 50 and above, so we study workers in the top quarter of their prime working age, among whom the Frisch elasticity may be high and may even be used as a bridge into early retirement. In the model of Prescott et al. (2009), extensive-margin Frisch elasticities play out among young and older workers. Hutchens (1999) illustrates interactions of UI and early retirement. Our design provides evidence for these predictions. For other evidence on retirement behavior, see Kyyrä and Wilke (2007) and Gelber et al. (2017), and in particular Manoli and Weber (2016) and Inderbitzin et al. (2016) for the Austrian setting. Other studies of older workers show that a higher UI benefit duration increases the inflow into unemployment, discourages job search and provides a bridge until retirement Tuit and van Ours (2010); Hunt (1995); Kyyrä and Ollikainen (2008); Tatsiramos (2010). Similarly, stricter search requirements and entrance requirement for unemployed workers lead to a significant increase in the transition rate to employment and a lower hazard rate out of employment, respectively Lammers et al. (2013); Christofides and McKenna (1996); Green and Riddell (1997); Baker and Rea Jr. (1998). Using Spanish data, Rebollo-Sanz (2012) provides evidence for a strong relationship between UI benefits and job turnover.

[6]A consistent picture of this separation effect emerges from various other angles. For instance, in terms of quarterly transition probabilities, the probability of moving from employment into unemployment increased by 50%, from 2ppt to 3ppt, only for the eligible age groups in the treated regions.

quits, bilaterally agreed upon terminations, early retirement including that motivated by limited employability, and sickness-related separations.

In the Austrian context, Lalive et al. (2011) find small inflow effects of a different, national reform, using regression-based difference-in-difference methods, and comparing inflow and outflow effects in a steady-state analysis. In the REBP context, Winter-Ebmer (2003) pioneers the study of inflow effects, distinguishing two worker groups (all below 50 vs. 50-65) using a 2% sample of our population data. These studies have in common a focus on the quantity of separations, not the quality and neither paper uses the separation responses to study marginal jobs, or the compositional consequences after the abolition.We revisit and extend this previous work in our population data and provide transparent graphical evidence for separation effects that includes parallel pre-trends.[7]

In a *second step*, we use the predictions of the model to characterize marginal jobs dissolved by the reform and the workers and firms that occupy them. Beyond the novel characterization of marginal jobs and workers, we are particularly interested in whether marginal jobs carry markers of low surplus compared to surviving jobs, in line with our conceptual framework as well as canonical models of endogenous (efficient) separations (e.g. Mortensen and Pissarides (1994)). To that end, we extend the complier analysis methodology from Imbens and Rubin (1997) and Abadie (2003) to difference-in-difference designs. We compare the distribution of *pre*-separation attributes of dissolved matches in the treated group (comprising always-separators and compliers) with dissolved matched in the control group (comprising always-separators). Under an additive separability assumption common in difference-in-differences designs, we can identify the distribution of attributes of the compliers, marginal jobs dissolved in response to the policy-induced surplus reduction that otherwise would have been preserved in the absence of the policy.

Analyzing the occupations from which marginal jobs originate, we find that marginal jobs are overwhelmingly manual, blue-collar jobs (87.2%), while a smaller fraction (58%) of never-separators stems from that segment of the labor market. These results are mirrored by the higher fraction of marginal jobs in mining and manufacturing, which are characterized by high manual labor intensity. We also find that marginal jobs are more likely to stem from shrinking industries and, within those industries, from shrinking firms (87.8% compared to 44.6% among never-separators), i.e. marginal jobs occur where labor demand falls. Moreover, marginal jobs were disproportionate in industries with a high incidence of sickness and disability among older workers at baseline (note that the reform applied to workers aged 50 and above), suggesting

[7]Inderbitzin et al. (2016) study the interaction of the REBP UI reform with the disability insurance system. Lalive et al. (2015) examine the search spillovers between unemployed job seekers because of moral hazard among the treated older workers in REBP. We net out this spillover effect in our within-region differences. We find that the separation rate only can account for the full amount of the policy-induced unemployment increase, which doubled (from 4ppt to 8ppt). In contrast to these studies, which look at job search behavior of unemployed workers, we focus on the impact of UI on employed workers.

lower surplus through high disutility of work or lower productivity. We further find that average wages in marginal jobs are slightly lower compared to those of never-separators. Dissecting wage differences between marginal jobs and never-separators, we find that marginal jobs are characterized by particularly low worker fixed effects (from wage regressions following Abowd et al. (1999)). These results lend support to a long-standing hypothesis that surplus shocks will lead to more separations among lower-wage workers (see, e.g., Oi (1962)).[8]

Our strategy to study the impact of job surplus variation on separations from *existing matches* provides new insights to the canonical focus on unemployed job seekers in the large existing literature.[9] In principle, the economics of separations and job formation are two sides of the same job surplus coin: the marginal matches that separate in response to our instrument are those that also fall beneath the reservation value of match formation (absent e.g. adjustment costs). However, the complier analysis we conduct would be impossible if we attempted to infer marginal jobs from new matches entered by previously unemployed workers. Our strategy would need to characterize *post*-treatment outcomes as job characteristics become only observable conditional on a newly formed match. The exclusion restriction associated with such a design would almost certainly be violated by all interesting instruments affecting the surplus across all matches: inframarginal job finders and the composition of jobs will be affected too, along with dynamic selection among the unemployed. [10] Our strategy, in contrast, does not rely on such an exclusion restriction by focusing on pre-reform, pre-separation attributes. Moreover, since our compliers are observed among pre-treatment cross-section of active jobs, we resolve a basic measurement challenge: for the unemployed, only the average accepted job ends up being directly observed, whereas inferring the attributes of rejected offers would require worker surveys or strong parametric assumptions. Finally, studying the quality and process of job separations as such is substantively important, given their role in business cycles, reallocation, and households' exposure to aggregate fluctuations and idiosyncratic lifetime earnings risk.[11]

In a final *third step*, we zoom out to document the aggregate, almost tautological, role of marginal matches in driving employment adjustment. An abstract thought experiment would simply conduct this analysis by removing a clean set of marginal matches from a set of jobs,

[8]We find higher firm fixed effects, possibly mirroring the aforementioned industrial composition in high wage, yet declining, industries. Another interpretation is that the firm effects capture rents rather than surplus differences.

[9]Katz and Meyer (1990), Krueger and Meyer (2002), Card et al. (2015), for business cycles see Shimer (2012).

[10]For instance Nekoei and Weber (2017) document wage increases and dynamic selection in response to UI reforms.

[11]For worker survey evidence on reservation wages, see Krueger and Mueller (2016) and Le Barbanchon et al. (2017) and for discussion of the econometrics of censored reservation wages, see Heckman (1979). The role of separations in aggregate employment fluctuations is explored in Elsby et al. (2013) for a cross section of OECD countries. Davis and von Wachter (2011) measure a drop in lifecycle income following displacement and its cyclicality; Berger et al. (2016) draw implications for monetary policy. Gruber (1997) measures drop in consumption upon separations; Kolsrud et al. (2015) and Ganong and Noel (2015) provide administrative measures of the path of consumption for the unemployed.

and examine the aggregate behavior of their remaining peers to surplus shifts, compared to the counterfactual in which those marginal jobs were still present.[12] We implement this design in our Austrian context. Specifically, our quasi-experiment exploits the *abolition of the reform* in 1993, as well as our panel data with which we can track job survivors from 1988, the onset of the reform, to 1993. That is, we then "switch off' the instrument, which restores the previous, gross-of-instrument surplus for each individual *surviving* job. The formerly-treated group then mirrors the control group, except for the missing mass of marginal matches, which should attenuate its aggregate, group-level sensitivity to subsequent shocks. We investigate this hypothesis by testing for heterogeneous group-level elasticities to industry shocks (leave-out-mean industry shocks). We first show that in the control group industry shocks reduce separations in the post-REBP period. We then document that the effect of industry shocks in the formerly treated groups is completely muted and separations respond much less to industry shocks. Interpreted through the lens of our model, these results show that breaking up low-surplus matches through outside option increases shifts the composition of employment towards higher-surplus jobs, that are less sensitive to subsequent shocks after the reform.

In this third step we thereby have documented the role of the density of marginal matches and low job surplus in driving aggregate extensive-margin elasticities, the elementary mechanism in employment adjustment. In consequence, those aggregate elasticities are not fixed, but can be history-dependent, endogenous to institutional or other shocks. This last part also provides a direct micro-macro evidence for theories related to the "cleansing effect of recessions" hypothesized in the macroeconomic literature (Caballero and Hammour (1994), Barlevy (2002)). Finally, we have shed light on the consequences of higher outside options for the composition of job quality.

Finally, in this paper we attempt to empirically trace and make concrete the role of job surplus in extensive-margin adjustment, which usually remains empirically elusive. Versions of job surplus is the core allocative concept in many labor market models. Low average surplus is the fundamental factor for high equilibrium unemployment in *steady state* (e.g. Pissarides (2000)). Low job surplus is the fundamental ingredient that underlies successful *cyclical* models of the labor market (Ljungqvist and Sargent (2017)). Here, we document *direct* micro-macro evidence on job-level choices and aggregate elasticities for the link between (low) job surplus and job destruction. Relatedly, our design documents clean evidence for the effect of workers' UI-induced outside options on separations, lending sharp micro-empirical support for the mechanisms underlying the canonical model of endogenous job destruction (Mortensen and Pissarides (1994)). Our documentation of the channel is non-trivial because much of the literature assumes that

[12]Importantly, we have no prediction for the during-reform distribution of job survivors net of the outside option increase, since we obtain local information about marginal jobs. The behavior of the survivors during the reform could be more or less elastic to shocks, and is entirely driven by the shape of the density function of job surplus rather than by marginal matches in particular.

separations are exogenous, in effect that most matches carry sufficient surplus to withstand a wide range of shocks.

Section 2 develops a conceptual framework to study the separation process and the characteristics of marginal jobs. In Section 3, we describe the institutional context, the reform we study, as well as the administrative micro data sets used in the empirical analysis. Section 4 provides causal evidence of UI on job separations and Section 5 non-parametrically characterizes the marginal jobs along a rich set of worker-, firm- and job-level attributes, all measured before the separation. In Section 6, we trace the effect of the surplus selection of jobs on the composition of surviving matches post-reform-abolition, and their sensitivity to subsequent shocks.

2 Conceptual Framework: Jobs and Surplus

The conceptual framework formalizes three predictions of the effect of higher outside options on incumbent matches driven by marginal matches' surplus thresholds. The basis is a job-level bilateral discrete choice problem with bargaining, in which separations are bilaterally efficient and job surplus is the sole allocative concept. First, higher outside options lead to separations by lowering job surplus, sweeping up low-surplus – marginal – matches. Section 4 presents reduced-form evidence for separation effects on outside option increases. Second, within the model, we derive our complier analysis methodology to directly trace and characterize these marginal jobs in the data, with respect to low-surplus proxies. We conduct this analysis in Section 5. Third, we discuss the consequences of higher outside options for the set of surviving jobs, where a composition shift towards higher surplus attenuates its responsiveness to subsequent shocks, due to the missing mass of marginal matches. We derive a testable prediction and document evidence for it in Section 6.

2.1 Jobs: Definitions and Assumptions

We define jobs and their attributes. We then clarify an assumption, efficient bargaining, in our benchmark model to derive job surplus as the single allocative concept for match formation, separation and continuation. We then partition the set of jobs – which may be very heterogeneous in their attributes – solely by the surplus these attributes generate and their resulting extensive-margin behavior. Here, we refer to abstract match attributes in a bilateral setting, while Appendix Section B presents a full equilibrium model with job finding and separation margins and also considers concrete attributes such as productivity, amenity or disutility of labor.

Jobs. Jobs present discrete choice problems between employment in the match at hand and match resolution: leisure (or the option of searching for another job) for the worker, and a vacant

position (and thus either destroying the job or filling it with a replacement hire, the change in the marginal product of all other workers,...) for the firm. Jobs, workers and firms face value-relevant factors subsumed in vector $\mathbf{x}$, such as job amenities, productivity, disutility of labor, firing costs or vacancy values. Other factors are institutional such as unemployment insurance generosity b – from where our shift in outside options will arise. The participation constraint demands that for each party $i \in \{W, F\}$, her job value $V^i_{\text{Stay}}(b, \mathbf{x})$ amount to at least her (separation) outside value $V^i_{\text{Separate}}(b, \mathbf{x})$. Each actor solves $\max_{\text{Stay,Separate}}\{V^W_{\text{Stay}}(b, \mathbf{x}), V^W_{\text{Stay}}(b, \mathbf{x})\}$. Moreover, we assume that the parties can transfer utility (value) by means of wage w, where w may encompass deferred compensation, non-monetary benefits, or any other complicated compensation schedules. For any given $\mathbf{x}, b$ and w, the conditions for a viable match then are:

$$V^W_{\text{Stay}}(b, \mathbf{x}) + w \geq V^W_{\text{Separate}}(b, \mathbf{x}) \tag{1}$$

$$V^F_{\text{Stay}}(b, \mathbf{x}) - w \geq V^F_{\text{Separate}}(b, \mathbf{x}) \tag{2}$$

Job attribute distribution. The realized and observed distribution of employment relationships is described by distribution $F(\mathbf{x})$ of matches with non-negative surplus. Match attributes $\mathbf{x}$ may evolve, for matches to separate without aggregate shocks. Markov process $g(\mathbf{x}'|\mathbf{x})$ denotes the stationary and homogeneous law of motion of attributes.[13] These movements may reflect aging, human capital growth on the job, experience effects, or idiosyncratic shocks; besides b we do not explicitly consider aggregate shocks. In this paper, we follow a single cohort of initially viable jobs and study their survival and separation; a stationary distribution would arise with inflow of new jobs from some distribution.[14]

Definition: job surplus. Job surplus is the difference between the sum of the inside values minus the sum of the outside values, i.e. the sum of the net values of each party:

$$S(b, \mathbf{x}) = V^W_{\text{Stay}}(b, \mathbf{x}) + V^F_{\text{Stay}}(b, \mathbf{x}) - V^W_{\text{Separate}}(b, \mathbf{x}) - V^F_{\text{Separate}}(b, \mathbf{x}) \tag{3}$$

Assumption: transferable utility, bargaining and bilateral efficiency. In our Coasean benchmark, the parties find a wage within the *bargaining set* of reservation wages $w \in [\underline{w}^W, \overline{w}^F]$, any of which implements the *bilaterally efficient* allocation: forming and maintaining matches

[13] For one single cross-sectional sorting of pre-reform jobs, the stationarity of the Markov process is not important, i.e. it could be time varying $g_t(\mathbf{x}'|\mathbf{x})$. Homogeneity between treatment and control groups is necessary.

[14] where total jobs N consist of new hires H and legacy jobs $N - H$:

$$F^{\text{all}}(\mathbf{x}) = \frac{N-H}{N} F^{\text{old}}(\mathbf{x}) + \frac{H}{N} F^{\text{new}}(\mathbf{x})$$

that carry non-negative surplus:[15]

$$V^F_{\text{Stay}}(b,\mathbf{x}) - V^F_{\text{Separate}}(b,\mathbf{x}) \geq V^W_{\text{Separate}}(b,\mathbf{x}) - V^W_{\text{Stay}}(b,\mathbf{x}) \tag{4}$$

$$\Leftrightarrow S(b,\mathbf{x}) \geq 0 \tag{5}$$

Implication: job surplus defines match viability. Surplus is the allocative concept in this benchmark model, in that it subsumes a broad collection of elementary factors such as attributes $\mathbf{x}$, outside options and inside options. Such factors are allocative to the degree to which they affect surplus. This basic result allows us to partition the set of jobs (defined by their attributes $\mathbf{x}$) into viable, nonviable and marginal jobs, solely along the surplus (scalar) dimension. We discuss deviations from this Coasean set-up in Appendix Section D arising from wage rigidity that prevents bilaterally efficient wage bargaining.

Partitioning the set of jobs (attribute vectors x) by surplus. $\mathbf{X}^V$ is the set of attribute vectors that render a match viable:

$$\mathbf{X}^V_b = \{\mathbf{x} \quad \text{s.t.} \quad S(\mathbf{x};b) \geq 0\} \tag{6}$$

$\mathbf{X}^N$ is the set of attribute vectors that render a match nonviable:

$$\mathbf{X}^N_b = \{\mathbf{x} \quad \text{s.t.} \quad S(\mathbf{x};b) < 0\} \tag{7}$$

These jobs are not observed – their attributes are latent because they would have separated/not been formed. They could possibly be recovered by looking at rejected matching opportunities, such as survey questions on received yet rejected job offers (worker perspective) or rejected applications (firm perspective).

[15] Reservation wages are the wage thresholds that barely keep the respective party prefer the match to the outside option:

$$\underline{w}^W(b,\mathbf{x}) = V^W_{\text{Separate}}(b,\mathbf{x}) - V^W_{\text{Stay}}(b,\mathbf{x})$$
$$\overline{w}^F(b,\mathbf{x}) = V^F_{\text{Stay}}(b,\mathbf{x}) - V^F_{\text{Separate}}(b,\mathbf{x})$$

Bargaining protocols can implement this allocation. Nash bargain wage w gives the worker (firm) their outside option, plus fraction β (resp. $1-\beta$), the party's bargaining power, of the surplus:

$$\max_w \left([V^W_{\text{Stay}}(b,\mathbf{x}) + w] - V^W_{\text{Separate}}(b,\mathbf{x})\right)^{\beta} \cdot \left([V^F_{\text{Stay}}(b,\mathbf{x}) - w] - V^F_{\text{Separate}}(b,\mathbf{x})\right)^{1-\beta}$$
$$\Rightarrow w^N = [V^W_{\text{Separate}}(b,\mathbf{x}) - V^W_{\text{Stay}}(b,\mathbf{x})] + \beta \cdot S(b,\mathbf{x})$$
$$= \underline{w}^W + \beta \cdot [\overline{w}^F - \underline{w}^W]$$

$\mathbf{X}^M$ is the set of attribute vectors that render a match (precisely) marginal:

$$\mathbf{X}_b^M = \{\mathbf{x} \quad \text{s.t.} \quad S(\mathbf{x}; b) = 0\} \tag{8}$$

2.2 The Effect of Better Worker Outside Options on Destruction of Marginal, Low-Surplus Jobs

We describe the effect of outside options on separations next. In our model, besides truly marginal jobs (a knife-edge set that will remain empirically elusive and the relevant category for non-infinitesimal surplus pertubations), we define a more empirically tractable set of nearly-marginal jobs. These are the matches that drive incremental separation responses to decreases in surplus such as those arising from an outside option, the focus of our paper. In Section 4, we test whether outside option increases indeed entail separations by lowering job surplus and sweeping up nearly-marginal matches. Figure 1 illustrates the mechanisms.

Our instrument: increase in worker's non-employment value Δb from unemployment insurance. Our surplus shifter is a binary instrument Z that increases workers' outside value[16] through higher unemployment insurance generosity $b = b_0 + Z \times \Delta b$. We define a useful auxiliary $\Delta^S(\Delta b, S(\mathbf{x}; b))$ as the surplus shift induced by Δb for match $S(\mathbf{x}; b)$:[17]

$$\Delta^S(\Delta b, S(\mathbf{x}; b)) = S(b_0, \mathbf{x}) - S(b_0 + \Delta b, \mathbf{x}) > 0 \tag{9}$$

Effects of instrument on the set of viable maches. The instrument shifts the treatment group's surplus distribution to the left. First, the shift destroys *marginal jobs* for whom the

[16] The most plausible channel of surplus reduction from Δb works through the worker's non-employment value. Accordingly, in the empirical context of our application (Austria), even worker-sided quits receive full benefits (after a brief waiting period). Alternative channels include efficiency-wage stories, by which the worker's improved outside option may lower productivity ((Shapiro and Stiglitz (1984), Akerlof and Yellen (1986), Katz (1986)). Indirectly, surplus may fall if implicit firing costs are lowered from reputational or morale concerns on part of employers (Bewley (2002), Hall and Lazear (1984), Lazear (1981). Lazear (1979)). Still, those proximate feedback mechanisms all ultimately arise from the worker's improved non-employment value. Finally, b may enter the firm's separation value through shifts in recruitment costs or quality of replacement hires. However, in our empirical design we net out this mechanism with our similarly aged yet ineligible control group.

[17] where the shifts are:

$$\begin{aligned}
S(b_0 + \Delta b, \mathbf{x}) - S(b_0, \mathbf{x}) &= \Delta V^W_{\text{Stay}}(b_0 + Z \times \Delta b, \mathbf{x}) - \Delta V^W_{\text{Separate}}(b_0 + Z \times \Delta b, \mathbf{x}) \\
&\quad + \Delta V^F_{\text{Stay}}(b_0 + Z \times \Delta b, \mathbf{x}) - \Delta V^F_{\text{Separate}}(b_0 + Z \times \Delta b, \mathbf{x}) \\
\Delta V^W_{\text{Stay}}(b_0 + Z \times \Delta b, \mathbf{x}) &= V^W_{\text{Stay}}(b_0 + \Delta b, \mathbf{x}) - V^W_{\text{Stay}}(b_0, \mathbf{x}) \\
\Delta V^W_{\text{Separate}}(b_0 + Z \times \Delta b, \mathbf{x}) &= V^W_{\text{Separate}}(b_0 + \Delta b, \mathbf{x}) - V^W_{\text{Separate}}(b_0, \mathbf{x})
\end{aligned}$$

weak inequality just holds absent the instrument:

$$S(b_0, \mathbf{x}') = 0 \tag{10}$$

Second, Δb *sweeps up* nearly-marginal jobs, for whom the surplus inequality only barely held, up until the condition that defines the marginal job at the new regime. Δb characterizes that new, net-of-instrument marginal match by its gross-of-instrument counterfactual surplus:

$$S(b_0, \mathbf{x}') - \Delta^S(\Delta b, S(\mathbf{x}'; b)) = 0 \tag{11}$$

$$\Leftrightarrow S(b_0, \mathbf{x}') = \Delta^S(\Delta b, S(\mathbf{x}'; b)) \tag{12}$$

Δb therefore triggers separation of matches with initial, gross-of-reform surplus $[0, \Delta^S(\Delta b, S(\mathbf{x}'; b))$. $\Delta^S(\Delta b, S(\mathbf{x}'; b))$ acts as an *upper bound* on the gross-of-reform surplus of the matches destroyed in response to the policy. We next formally define this set of jobs that are "nearly-marginal" with respect to Δb.

Nearly-marginal matches. We call the set of matches swept up by the instrument nearly-marginal matches. They are viable under regime b but non-viable under $b + \Delta b$ (i.e. they separate in the transition). Note that for $\frac{\partial S}{\partial b} \leq 0$, $S(\mathbf{x}; b) > S(\mathbf{x}; b + \Delta b)$ if $\Delta b > 0$, and thus:

$$\mathbf{X}^V_{b+\Delta b} \subseteq \mathbf{X}^V_b \quad \text{if} \quad \Delta b \geq 0 \tag{13}$$

That is, attributes of nearly-marginal matches are the relative complement of $\mathbf{X}^V_{b+\Delta b}$ in $\mathbf{X}^V_b$:

$$\begin{aligned} \mathbf{X}^C_{b;b+\Delta b} &= \mathbf{X}^V_b \setminus \mathbf{X}^V_{b+\Delta b} \\ &= \{\mathbf{x} \in \mathbf{X}^V_b | \mathbf{x} \notin \mathbf{X}^V_{b+\Delta b}\} \\ &= \{\mathbf{x} \quad \text{s.t.} \quad S(\mathbf{x}; b) \geq 0 \wedge S(\mathbf{x}; b + \Delta b) < 0\} \\ &= \{\mathbf{x} \quad \text{s.t.} \quad 0 \leq S(\mathbf{x}; b) < \Delta^S(\Delta b, S(\mathbf{x}; b))\} \end{aligned} \tag{14}$$

Compliers' no-instrument counterfactual surplus is in $(0, \Delta^S(\Delta b, S(\mathbf{x}; b))]$. For small Δb, $\lim_{\Delta b \to 0} \Delta^S(\Delta b, S(\mathbf{x}; b)) = 0$, we approximate the marginal – i.e. zero-surplus – match absent the reform. Going forward, we will refer to nearly-marginal jobs simply as marginal jobs.

Separations. In Section 4, we provide reduced-form causal evidence for the effect of outside options on separations. In our model, the probability of separation for a currently viable match $\mathbf{x}$ (i.e. $S(\mathbf{x}, b) \geq 0$) in regime b is the probability that its next-period surplus is to turn negative:

$$\delta(b, \mathbf{x}) = \text{Prob}\left[S(\mathbf{x}', b) < 0 | \mathbf{x}, b\right] = \int_{\mathbf{x}'} \mathbb{1}(S(\mathbf{x}', b) < 0) g(\mathbf{x}' | \mathbf{x}) d\mathbf{x}' \tag{15}$$

The average separation rate is a function of b and the distribution of match attributes:

$$\overline{\delta}(b, F(\mathbf{x})) = \int_{\mathbf{x}} \text{Prob}\left[S(\mathbf{x}', b) < 0 | \mathbf{x}\right] dF(\mathbf{x}) = \int_{\mathbf{x}} \int_{\mathbf{x}'} \mathbb{1}(S(\mathbf{x}', b) < 0) g(\mathbf{x}'|\mathbf{x}) d\mathbf{x}' dF(\mathbf{x}) \tag{16}$$

For our instrument, we have the following two separation rates for a control and treatment group, where we assume that their distributions $F(\mathbf{x})$ are initially identical. For the control group in regime b_0 we have:

$$\overline{\delta}^{Z=0} = \overline{\delta}(b_0, F(\mathbf{x})) = \int_{\mathbf{x}} \text{Prob}\left[S(\mathbf{x}', b_0) < 0 | \mathbf{x}\right] dF(\mathbf{x}) \tag{17}$$

For the treatment group in regime $b_0 + \Delta b$ we have:

$$\begin{aligned} \overline{\delta}^{Z=1} &= \overline{\delta}(b_0 + \Delta b, F(\mathbf{x})) = \int_{\mathbf{x}} \text{Prob}\left[S(\mathbf{x}', b_0) < \Delta^S(\Delta b, S(\mathbf{x}'; b)) | \mathbf{x}\right] dF(\mathbf{x}) \\ &= \overline{\delta}(b_0, F(\mathbf{x})) + \int_{\mathbf{x}} \text{Prob}\left[0 \leq S(\mathbf{x}', b_0) < \Delta^S(\Delta b, S(\mathbf{x}'; b)) | \mathbf{x}\right] dF(\mathbf{x}) \end{aligned} \tag{18}$$

The *treatment effect* of the surplus shifter (here: from outside options) on separations is driven by the nearly-marginal matches (those that would not have separated absent the reform) with gross-of-instrument surplus between zero and $\Delta^S(\Delta b, S(\mathbf{x}'; b))$:

$$\overline{\delta}^{Z=1} - \overline{\delta}^{Z=0} = \int_{\mathbf{x}} \text{Prob}\left[0 \leq S(\mathbf{x}', b_0) < \Delta^S(\Delta b, S(\mathbf{x}')) | \mathbf{x}\right] dF(\mathbf{x}) \tag{19}$$

The size of the response will depend on the mass of marginal workers with respect to our instrument. In Section 4, we test whether outside option increases indeed entail separations by lowering job surplus, and which fraction of the jobs is swept up by the higher outside option.

Graphical illustration. Figure 1 illustrates the mechanisms at play. Figure 1 (a), we plot a hypothetical distribution of job surplus $S(\mathbf{x}', b_0)$, where the mass of negative-surplus realizations of $\mathbf{x}'$ render the margin non-viable, leading to separations. Our control group remains in regime b_0. Figure 1 (b) illustrates the left-ward shift in surplus induced by the outside option increase. Figure 1 (c) shows the partitioning of the set of potential jobs into Always-Separators, Compliers and Never-Separators. The dotted red mass of marginal matches are those that drive the separation response in response to the treatment Z. Figure 1 (e) shows the match viability cutoff under the instrumented regime $b_0 + \Delta b$, expressed in gross-of-instrument, pre-reform surplus S.

2.3 The Characteristics of the Marginal Jobs Destroyed by Improved Outside Options

Our model has a clear prediction: the set of marginal jobs, which drive the quantity (separation) response to the outside option improvement, carry *low surplus*. Next, we extend our conceptual model to sketch an empirical strategy to nonparametrically back out marginal jobs' characteristics $\mathbf{x}$ in the data, drawing from tools of complier analysis. This allows us to investigate the surplus channel in two ways: first, an agnostic approach will directly reveal low surplus markers (assuming the model to be correct). Second, we can posit plausible surplus proxies and check whether compliers are more likely to carry them than surviving matches in the data. The full econometric model, which extends complier analysis (Imbens and Rubin (1997); Abadie (2003)) to diff-in-diff designs, and our application to an Austrian extension in UI are presented in Section 5.

Strategy: revealed-preference identification of marginal matches with binary surplus shift instrument. Next, we sketch our methodology to isolate the set of nearly-marginal matches, which carry around zero surplus, to then measure this job set's attribute vectors as an empirically feasible approximation of the ideal identification of exactly-marginal jobs. Nearly-marginal and their attributes are informative about the elusive set of marginal jobs because for small pertubations in surplus, they will pass the separation threshold and *by their very separation* reveal their identity of carrying nearly-zero gross-of-instrument surplus. They, and their attributes $\mathbf{X}^C$, can be measured as follows: the instrumented distribution is an average (weighted by the number of incremental separations, our "first stage" defined in Equation (19) and estimated in Section 4) of the uninstrumented distribution and the incremental marginal separations due to the policy. Here we sketch this methodology, which can be applied to any moment, for the mean of the attributes.

Observed jobs and attributes. Without an instrument in regime b, a single set of viable, active matches merely provides a moment inequality, since $S(\mathbf{x}', b) \geq 0$. Similarly, a single set of separations also does not convey sharp information about marginal attributes, since they reveal the complementary inequality of negative surplus $S(\mathbf{x}, b) < 0$; it may largely include big inframarginal shocks such as health shocks. Instead, we would like to isolate attributes associated with nearly zero surplus, carried by matches *at the verge of separation*. However, our instrumental variable strategy with difference-in-difference approaches helps us distill marginal matches' attributes from treatment and control groups' distributions of separators.

Partitioning matches by treatment effect terminology. We now switch gears to the terminology of the treatment effects literature to illustrate how we can nonparametrically identify

marginal matches' attributes following complier analysis (Imbens and Rubin (1997); Abadie (2003)). Consider first the control group, which stays under regime $b = b_0$. Here, we observe *Always-Separators*, who separate even in the regime b and therefore also in regime $b + \Delta b$, and have surplus $\in (-\infty, 0]$:

$$\bar{\mathbf{x}}^{\text{AS}} = \mathbb{E}\left[\mathbf{x} \middle| S(\mathbf{x}', b_0) < 0\right] \tag{20}$$

Never-Separators are stayers in the treatment group, who remain viable under b_0 as well as $b_0 + \Delta b$, carrying surplus $\in [0, \infty)$:

$$\bar{\mathbf{x}}^{\text{NS}} = \mathbb{E}\left[\mathbf{x} \middle| S(\mathbf{x}', b_0) \geq \Delta^S(\Delta b, S(\mathbf{x}'; b))\right] \tag{21}$$

Treatment Separators are even less informative about marginal workers as those matches are associated with surplus of $(-\infty, \Delta^S(\Delta b, S(\mathbf{x}'; b))]$, an even larger set that spans a wider surplus range than the Always-Separators' surplus $(-\infty, 0]$. Their attributes are $\mathbf{x}^{\text{AS}\cup\text{C}}$:

$$\bar{\mathbf{x}}^{\text{AS}\cup\text{C}} = \mathbb{E}\left[\mathbf{x} \middle| S(\mathbf{x}', b_0) < \Delta^S(\Delta b, S(\mathbf{x}'; b))\right] \tag{22}$$

Importantly, the Treatment Separators are the *union* of Always-Separators and Compliers. Specifically, the observed average $\mathbf{x}$ is just the average of Always-Separators' $\bar{\mathbf{x}}^{\text{AS}}$ and Compliers' $\bar{\mathbf{x}}^{\text{C}}$, *weighted by their respective shares*. These shares are estimated off the treatment effect on the separation rate: the average separation rates in the control group $\overline{\delta}^{Z=0}$ and treatment group $\overline{\delta}^{Z=1}$, which also defines the first stage $\overline{\delta}^{Z=1} - \overline{\delta}^{Z=0}$:

$$\bar{\mathbf{x}}^{\text{AS}\cup\text{C}} = \frac{\overline{\delta}^{Z=0}}{\overline{\delta}^{Z=1}} \times \bar{\mathbf{x}}^{\text{AS}} + \frac{\overline{\delta}^{Z=1} - \overline{\delta}^{Z=0}}{\overline{\delta}^{Z=1}} \times \bar{\mathbf{x}}^{\text{C}} \tag{23}$$

Complier attributes. A simple subtraction of the sets of Treatment-Separators and Always-Separators (controls) allows us to distill Compliers' attributes. Rearranging Equation (23) returns compliers' attributes:

$$\bar{\mathbf{x}}^{\text{C}} = \frac{\overline{\delta}^{Z=1}}{\overline{\delta}^{Z=1} - \overline{\delta}^{Z=0}} \times \bar{\mathbf{x}}^{\text{AS}\cup\text{C}} - \frac{\overline{\delta}^{Z=0}}{\overline{\delta}^{Z=1} - \overline{\delta}^{Z=0}} \times \bar{\mathbf{x}}^{\text{AS}} \tag{24}$$

$$= \mathbb{E}\left[\mathbf{x} \middle| 0 \leq S(\mathbf{x}', b_0) < \Delta^S(\Delta b, S(\mathbf{x}'; b))\right] \tag{25}$$

Our revealed preference argument is that the incremental separations among the compliers in response to Δb reveal those matches with surplus gross-of-Δb (i.e. absent the reform) in the interval $(0, \Delta^S(\Delta b, S(\mathbf{x}'; b))]$. For small changes in b, $\lim_{\Delta b \to 0} \Delta^S(\Delta b, S(\mathbf{x}'; b)) = 0$, we approximate the precisely marginal – i.e. zero-surplus – match absent the reform. Complier characteristics are associated with surplus in this range. This procedure for the mean can be

generalized to nonparametrically back out the full distribution (thus any of its moments) of any complier characteristic $s(\mathbf{x})$, the share of $\mathbf{x}$-attribute-bearers among the compliers.[18]

2.4 The Effect of Outside Options on the Composition of Jobs: Implications for Aggregate Elasticities to Subsequent Shocks

Having identified the role of and characterized the attributes of marginal matches, we now discuss the implications of outside options for the *composition* of surviving (instrument-selected) jobs with respect to surplus. We zoom out from job-level discrete choices to the resulting truncation of group-level distributions, and the aggregate, group-level behavior. We derive an important and empirically testable implication of the surplus selection mechanism and the role of marginal matches within our conceptual framework: when outside options remove marginal matches, the sensitivity of the jobs surviving the treatment will – if it were subsequently undone – more resilient to subsequent shocks to surplus. The comparison is the control group's set of jobs that still include the marginal jobs. This prediction is empirically testable: in Section 6, we study the abolition of the reform (our instrument) and the group-level sensitivity of the formerly-treated, positively selected jobs to subsequent labor demand shocks.

Beyond our particular case study of outside options and separations, this empirical test directly documents the role of the mass of marginal matches and low job surplus in driving aggregate extensive-margin elasticities, the elementary mechanism underlying extensive-margin employment adjustment. It moreover shows that those elasticities are not fixed, are history-dependent, and can be affected by institutional or other surplus-related shocks.

Net-of-instrument surplus distribution. We now zoom out to examine the match surplus distributions $F^Z(\sigma)$ that arise from the job-level discrete choice problems discussed in the previous sections, comparing treatment and control groups. The treatment group's surplus distribution is a shifted and truncated version of the control group's surplus distribution $F^{Z=0}(\sigma)$

[18] The expression for the density $\mathbf{x}$ among the compliers is:

$$s^C(\mathbf{x}) = \frac{\bar{\delta}^{Z=1}}{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}} \times s(\mathbf{x}, Z=1) - \frac{\bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}} \times s(\mathbf{x}, Z=0)$$

Alternatively, $s(\mathbf{x})$ is the product of the relative first stage among $\mathbf{x}$-bearers compared to the average first stage, times the share of $\mathbf{x}$-bearers in the initial sample $f(\mathbf{x})$:

$$s^C(\mathbf{x}) = \frac{[\delta(\mathbf{x}; b+\Delta b) - \delta(\mathbf{x}; b)]}{\int_{\mathbf{x}} [\delta(\mathbf{x}; b+\Delta b) - \delta(\mathbf{x}; b)]\, dF(\mathbf{x})} \cdot f(\mathbf{x})$$

due to surplus shifter $\Delta^S(\Delta b, S(\mathbf{x};b))$:

$$F^{Z=1}(\sigma) = \begin{cases} \frac{F^{Z=0}(\sigma+\Delta^S(\Delta b,S(\mathbf{x};b)))-F^{Z=0}(\Delta^S(\Delta b,S(\mathbf{x};b)))}{1-F^{Z=0}(\Delta^S(\Delta b,S(\mathbf{x};b))} & \text{if} \quad \sigma \geq 0 \\ 0 & \text{if} \quad \sigma < 0 \end{cases} \tag{26}$$

The instrument reshapes the distribution of surplus in the treatment group $Z = 1$ in two ways vis-à-vis the control group $Z = 0$. First, inframarginally, the treatment group distribution shifts to the left, by $\Delta^S(\Delta b, S(\mathbf{x};b))$ for all matches. Second, the instrument induces a *compositional* change by sweeping up all marginal matches with *gross*-of-instrument surplus between 0 and $\Delta^S(\Delta b, S(\mathbf{x};b))$.

Empirical challenge. While the treatment group typically contains matches with higher hypothetical *gross*-of-instrument surplus by virtue of having shed the marginal matches, it is *net* surplus that drives actual choices. A naive analysis examining the sensitivity *during* the instrumental period is therefore not informative and depends on the net surplus distribution of Never-Separators, particularly the – unknown – relative mass of nearly-marginal workers at the new gross surplus threshold, compared to the control group's mass.[19] In principle, the gross surplus distribution would therefore lends itself to a reduced-form test of the role of marginal matches in driving group-level elasticities, but it is not allocative with the instrument switched on.

Gross-of-instrument surplus distribution. Gross surplus captures the job-level counterfactual surplus level absent the instrument. The gross-of-instrument surplus distribution is defined over those jobs surviving the instrument. The gross-of-surplus distribution $H(\sigma)$ is a rightward-shifted version of the actual treatment distribution $F^{Z=1}(\sigma)$ – or equivalently a version of the control group truncated at $\Delta^S(\Delta b, S(\mathbf{x};b))$:

$$H^{Z=1}(\sigma) = \begin{cases} \frac{F^{Z=0}(\sigma)-F^{Z=0}(\Delta^S(\Delta b,S(\mathbf{x};b))}{1-F^{Z=0}(\Delta^S(\Delta b,S(\mathbf{x};b))} & \text{if} \quad \sigma \geq \Delta^S(\Delta b, S(\mathbf{x};b)) \\ 0 & \text{if} \quad \sigma < \Delta^S(\Delta b, S(\mathbf{x};b)) \end{cases} \tag{27}$$

The gross-of-instrument surplus distribution equals the control distribution minus the marginal matches, which have previously separated in response to the instrument. In the gross surplus distribution, each non-marginal match in the control group has a peer match in the treatment group, except for the marginal matches, which are missing in the treatment group. Next, we describe a strategy to have that gross-surplus distribution become allocative – simply by switching off the instrument on the formerly-treated group.

[19] Our instrumental variable strategy only reveals the nearly marginal matches with gross-of-instrument surplus between 0 and $\Delta^S(\Delta b, S(\mathbf{x};b))$; we have not learned anything about the Never-Separators except for the moment inequality of their gross-of-instrument surplus being at least $\Delta^S(\Delta b, S(\mathbf{x};b))$.

Strategy: panel variation instruments. We exploit the insight that the gross-surplus distribution would also arise in the treatment group if we "switched off" the instrument , and followed the surviving matches in the initial experimental cohorts. Our empirical strategy therefore uses panel variation in the instrument. Specifically, the sensitivity of the jobs surviving the initial instrument $Z_1 = 1$ will, if Z were subsequently removed $Z_2 = 0$, be more resilient to subsequent shocks to surplus – compared to the control group's set of jobs, in which sequence $(Z_1 = 0, Z_2 = 0)$ did not previously extract marginal jobs. This prediction is empirically testable and documents the role of density of marginal matches and low job surplus in driving aggregate extensive-margin elasticities.

Post-instrument separation elasticity to surplus shocks. Namely, the separation response to a given surplus shock of size $-\widetilde{\Delta S} < 0$ will be as follows:

$$\delta^{Z_1=1,Z_2=0}(\widetilde{\Delta S}) = H^{Z=1}(\widetilde{\Delta S}) \tag{28}$$

$$= \begin{cases} \frac{F^{Z=0}(\widetilde{\Delta S}) - F^{Z=0}(\Delta^S(\Delta b, S(\mathbf{x};b)))}{1 - F^{Z=0}(\Delta^S(\Delta b, S(\mathbf{x};b)))} & \text{if} \quad \widetilde{\Delta S} \geq \Delta^S(\Delta b, S(\mathbf{x};b)) \\ 0 & \text{if} \quad \widetilde{\Delta S} < \Delta^S(\Delta b, S(\mathbf{x};b)) \end{cases} \tag{29}$$

Compared to the control group's response, where the "old" marginal jobs are still present and will drive the aggregate, group-level employment effect:

$$\delta^{Z_1=0,Z_2=0}(\widetilde{\Delta S}) = F^{Z=0}(\widetilde{\Delta S}) \tag{30}$$

The difference in separation responses between the never-treated $(Z_1 = 0, Z_2 = 0)$ and formerly-treated $(Z_1 = 1, Z_2 = 0)$ groups is:

$$\delta^{Z_1=0,Z_2=0}(\widetilde{\Delta S}) - \delta^{Z_1=1,Z_2=0}(\widetilde{\Delta S}) \tag{31}$$

$$= \begin{cases} \frac{F^{Z=0}(\Delta^S(\Delta b, S(\mathbf{x};b)))}{1 - F^{Z=0}(\Delta^S(\Delta b, S(\mathbf{x};b)))} \cdot \left[1 - F^{Z=0}(\widetilde{\Delta S})\right] & \text{if} \quad \widetilde{\Delta S} \geq \Delta^S(\Delta b, S(\mathbf{x};b)) \\ F^{Z=0}(\widetilde{\Delta S}) & \text{if} \quad \widetilde{\Delta S} < \Delta^S(\Delta b, S(\mathbf{x};b)) \end{cases} \tag{32}$$

Most obviously, for $\widetilde{\Delta S} < \Delta^S(\Delta b, S(\mathbf{x};b))$, there are no incremental separations in the formerly-treated group triggered by the second surplus shifter, while a subset of the marginal jobs in the former control group separate: for shocks smaller than the surplus threshold of the initial instrument, no matches remain at the margin in the treatment group. For larger shocks, the initial "missing matches" among the separators simply become a smaller fraction of the total separations.The difference is therefore concave in shock $\widetilde{\Delta S}$. Finally, the difference is increasing in the surplus value of the initial surplus instrument $\Delta^S(\Delta b, S(\mathbf{x};b))$.[20]

[20]This mechanism can be thought of as a test of theories related to the "cleansing effect of recessions" (Caballero and Hammour (1994), Barlevy (2002)), by which negative business cycle shocks may destroy low-

Implications for measured elasticities with or without marginal jobs. We empirically test for the differential separation responses between treatment and control group absent the instrument in Section 6, where we exploit the abolition of our UI reform instrument. The above conceptual framework and our empirical design document that shocks and their sequences, or also institutions, can change aggregate employment elasticities by shaping the mass of marginal actors.[21] Extensive margin behavior therefore reflects highly local densities (rather than constant structural elasticities such as "the" Frisch elasticity), and the position and density of the marginal matches can vary between contexts – but also in predictable ways as in our case.[22]

Graphical illustration. Figure 1 (f) illustrates the removal of the instrument (reform abolition). Figure 1 (g) shows the truncated post-abolition surplus distribution and the missing mass of marginal matches in the treatment group. Figure 1 (h) shows the post-abolition separation rate as a function of the subsequent surplus shock for the control group and the formerly-treated group, under the simplifying assumption of no (aggregate or idiosyncratic) shifts in surplus in the treatment group except for the initial outside option instrument. Under this assumption, the initial separation rate is zero, and remains precisely zero up until the size of the subsequent surplus shock exceeds the surplus shock of the initial instrument.

3 Institutional Context, Reform, and Data

In order to study whether and, if so, how reductions in job surplus from improved worker outside options lead to job destruction, we analyze a large extension of unemployment benefits in Austria which sharply increased the attractiveness of becoming and staying unemployed for older workers. Specifically, we analyze the job destruction effects of the Regional Extended Benefit Program (REBP), a large unemployment benefit extension that was implemented between June 1988 and July 1993. The REBP increased the potential UI benefit duration from 1 year to 4 years for treated workers above age 50. The design of the reform allows for sharp comparisons among three dimensions – region, time, and age – allowing us to isolate the causal effect of surplus reductions on existing employment relationships. This section describes the Austrian labor market and the relevant institutions at the time, as well as the reform and our administrative data sources.

productivity jobs and may subsequently yield improved stayers in the selected sample; similarly, our selection-based model speaks to the view that selection of jobs by cyclical variation in outside options in terms of job-to-job switching opportunities may generate composition bias and thereby help explain the cyclical behavior and cohort patterns of stayers' wages (Hagedorn and Manovskii (2013)).

[21] See e.g. Schoefer (2010) for the case of regulations affecting labor supply elasticities.

[22] Most obviously, if one ran the same instrument on the treatment group (e.g. in a stationary environment with idiosyncratic shifts due to Markov process $g(\mathbf{x}'|\mathbf{x})$), then the first treatment effect (and associated elasticity measure) would mirror the control group's. However, if one ran the experiment again, then the effect may be much attenuated due to the elimination of the marginal matches.

3.1 Institutional Context: The Austrian Labor Market and Unemployment Insurance System of the 1980s and 1990s

The unemployment insurance system and labor market. The Austrian labor market is characterized by relatively low levels of fluidity, i.e. low inflow and outflow rates (for a cross-country comparison of worker transition rates in OECD countries, see Appendix Figure 17), much like other Continental European countries such as Germany, Italy, or France. UI is funded through employer- and employee-contributions proportional to the employees' wages up to a social security earnings maximum. Similar to most other European countries, the Austrian system does not feature experience rating. During our study period until 1989, the gross replacement rate was around 41% for most employees and capped below and above at a minimum and maximum amount.[23]

Crucially, individuals are eligible for unemployment benefits upon quitting after fulfilling a four-week waiting period.

Potential benefit duration before the reform. Figure 3 shows an overview of potential benefits. Before the REBP reforms that we study were enacted, an unemployed individual with sufficient work experience — having worked three out of the last five years — could claim benefits for up to 30 weeks.[24] When benefits run out, individuals can receive means-tested welfare-like benefits that are substantially lower than unemployment benefits.

Employment protection and layoffs. While employment protection legislation is not as stringent as in many other (particularly in Southern European) countries, an Austrian firm firing a worker has to obey a set of rules. At the time of the REBP, the firm could lay off a worker but had to give advance notice, which was a step function of the workers tenure. The advance notice period was 5 (4, 3, 2, 1.5) months for workers with thresholds 25 (15, 5, 2) years of tenure. An advance notice obligation applies also for quits. The worker can leave the firm at the end of the month, with one-month advance notice period.

The Austrian labor law provides a particular role for works councils in the firing process. In firms with 5 employees or more, workers can organize within a works council. The firm has to inform and consult the works council when a layoff is planned. If the firms fails to do so, a layoff is void. If a layoff violates substantial interests of the worker, the firm has to prove that the layoff is economically necessary for the survival of the firm. The works council must also be consulted when choosing the particular worker to be fired. The role of the works council is to make sure that the firm takes into account potential social hardships of the candidate

[23] UI benefits are not taxed. The net replacement ratio, UI benefits over the wage net of social security contributions and income taxes is 55%, slightly higher than in the US.

[24] For individuals with less work experience, the maximum potential benefit duration during that time was 20 weeks.

considered for a layoff, which among other factors provides some employment protection for older and longer-tenured workers.

Mass layoffs in larger firms are subject to specific further rules. Firms with more than 100 employees that reduce employment by more than 5 percent (or more than 50 employees) within one month have to give written notice to the regional employment agency, one month before the mass layoff is implemented, where failure to notify renders the mass layoff void.

A further important set of regulations refers to severance payments in cases of layoffs, again a step function of tenure. A worker with less than 3 years of tenure is not eligible for a severance payment. Workers with more than 3 (5, 10, 15, 20, 25) years of tenure are eligible for a severance payment of at least 2 (3, 4, 6, 9, 12) monthly salaries. Severance payments are only due for the following separation types: layoffs, job terminations upon mutual agreement (between firm and worker), and after the end of a temporary contract. In contrast, worker-induced quits and dismissals for cause were exempt from the severance-pay rule, except if the worker had been employed with the firm for at least 10 years.

3.2 The Regional Extended Benefit Program (REBP)

In 1988, the Austrian government enacted a large expansion of potential benefit duration for workers aged 50 and above in certain regions.[25] Figure 2 provides a map of the affected regions. The reform induces variation in three dimensions: first, over time as we can compare the reform period, 1988 to 1993, to the time period before and after; second, across age as we can compare workers aged 50 and above to their younger peers; and third, across regions comparing the REBP regions to regions not affected by the reform. The confluence of these three factors along with the large expansion in potential benefit duration makes the REBP reform an ideal setting to study which workers and matches separate in response to improved outside options.

Background and context of reform Austria nationalized its iron, steel, and oil industries and other segments of the heavy industries after World War II. Firms in the steel sector belonged to a large state-owned holding company, the Oesterreichische Industrie AG (OeIAG). By the mid-1970s, the company suffered from low commodity prices, shrinking markets, overstaffing, and low productivity. After the steel industry was hit by an oil speculation scandal and a failure of a US steel plant project, a new management implemented a restructuring plan that led to plant closures and downsizing in the steel industry. For the purpose of our analysis, we exclude workers employed in the steel sector.

Reform: extension of unemployment benefits In 1988, the REBP was introduced to mitigate the labor market consequences in the concerned regions and raised potential benefit

[25] See also Lalive et al. (2015) for a detailed description of the reform.

duration for older workers in reform regions (see Figure 3 for a visualization). It extended potential benefit duration effectively from 52 to 209 weeks for individuals who met all four eligibility criteria at the beginning of their unemployment spell: (i) age 50 or older; (ii) a continuous work history, which is defined as at least 780 employment weeks during the last 25 years prior to the claim; (iii) residence in one of 28 selected labor market districts since at least 6 months prior to the current unemployment spell (see Figure 2); and (iv) start of the new unemployment spell after June 1988 or spell in progress in June 1988.[26] Although the program was intended to support the steel industry (which we exclude in our analysis), it did not impose an industry requirement, which is why beneficiaries could have worked in any sector. The eligible labor market districts were selected by having a large share of employment in the steel sector: in the REBP-regions, about 17% of workers were employed in the steel industry, compared to less than 5% in the Non-REBP-regions. Note that before the start of the reform, treated and non-treated regions did not differ in terms of the unemployment rate or the fraction of long-term unemployed. In January 1992, a reform of the law enacted two important changes: First, the benefit extension was abolished in 6 regions. Figure 2 shows treatment regions that were treated until 1991 (TR1) and regions that were treated until 1993 (TR2). For our analysis, we only focus on TR2 and drop regions only treated until 1991 (TR1). The reform also prescribed that new beneficiaries had to be not only residents of the relevant regions, but to have also been previously employed in a REBP-region. Finally, the program accepted new beneficiaries until July 31, 1993, before it was abolished.

Additional reform An additional reform changed potential benefit duration for different groups of workers in August 1989 based on age and experience.[27] Specifically, potential benefit receipt for workers aged 40 to 49 was increased to 39 weeks with an experience requirement of 312 weeks of employment in the last 10 years before the current spell. For workers aged 50 and above, potential benefit duration was raised to 52 weeks with an experience requirement of at least 468 weeks of employment within the last 15 years. Figure 3 visualizes these additional changes along with the ones brought about by the REBP. For the purpose of our analysis the additional reform does not confound the effects of the REBP reform. First, it applied uniformly across the REBP and the control region. Secondly, our econometric strategy absorbs age or cohort effects so that comparisons are always within the same age group or cohort.

Interactions with disability and retirement program The REBP acted partly as an early retirement program since it changed the incentives for men aged 50 and above to leave the labor force. In the absence of the REBP, unemployed men could effectively retire early at age 54

[26] In our analysis, we only focus on workers who meet the experience requirement.

[27] The reform also increased the replacement rate from 41% to 47% in the monthly income bracket from 5,000 to 10,000 ATS, roughly US$ 400 to US$ 800 at the time.

by claiming unemployment benefits followed by a disability pension at age 55.[28] The extended benefits under the REBP made this option available to unemployed men aged 51 or above, while younger unemployed workers or older workers in the control regions did not have this option. Thus, REBP-eligibility gives 3 (2/1/0) additional years of coverage before reaching age 55 when a worker loses their job at age 51 (52/53/54). We therefore expect increases in permanent exits from the labor force and regard the REBP in part as a pathway to early retirement.

Abolition of the reform in 1993. REBP was completely abolished in 1993. The abolition decision was formally announced in June 1993, and already became effective in August 1993, an implementation gap of few months. A detailed newspaper analysis reveals the surprise nature of the 1993 abolition. In fact, the Austrian government had come up with a plan in January 1993 to *expand* access to longer benefit duration to older unemployed workers in *all* Austrian regions from one to four years along with changes in the eligibility requirements.[29] In the followings weeks and months, the government reversed course completely and eventually decided to end the REBP program.

3.3 Data and Sample

Administrative social security data on population of Austrian private-sector employees. Our main data come from the Austrian Social Security Database (ASSD), which is described in detail in Zweimuller et al. (2009). The ASSD gives detailed monthly information on the labor market and earnings histories of private-sector workers in Austria since 1978. We use this data on employment, pension and retirement to determine if the work history made an individual eligible for REBP. That allows us to compare individuals whose potential benefit duration only differs due to their place of residence. The data also connects the employment spells to the firms the individuals were working for, which allows us to determine their industry and location. We draw on this information to distinguish between firms in REBP- and Non-REBP-Regions as well as different industries (classification of 15 industries and NACE Rev. 2, respectively).

Sample restrictions. We drop all individuals working in the steel sector because the reform was targeted on these workers who presumably face worse labor market prospects. Likewise, we drop the 6 regions that were REBP-eligible only until 1991. In our analysis on the individual level we exclude women for two reasons. First, whereas old age insurance rules allow men to

[28] Regular retirement with an old-age pension was feasible at age 58.

[29] See "Longer benefits for all unemployed workers" (*Laenger Geld fuer alle Altersarbeitslosen*). Der Standard, 9 January 1993: "All older unemployed workers throughout Austria - and not only in [REBP regions] as in the past - will be eligible for unemployment benefits of four years instead of one. Minister of Social Affairs, Josef Hesoun, and the social partners have agreed in principle on this [...]." [translation by the authors].

retire at age 60, women can already retire at age 55. The second reason is that individuals must have been employed in 15 out of the last 25 years in order to be eligible for REBP. Since we cannot observe all 25 years prior to the reform, it is likely that classification errors arise for women, who due to childcare typically have a less continuous work history than men. However, women are included in our sample when we look at the macroeconomic outcomes.

Additional data sources. We supplement our analysis with limited use of three additional data sets. First, we draw from the Austrian Labor Force Survey to trace the nature of separations by type. Second, we draw from the unemployment registry to measure the duration of nonemployment that is due to registered unemployment, as well as to construct our administrative quit proxy, for which we exploit the wait period between employment spell end and associated unemployment registration, and benefit receipt. Third, to determine eligibility for REBP by employment experience in the last 25 years, we draw from supplementary data from the Austrian Ministry of Social Affairs; in practice the experience test was conducted on the basis of similar data.

4 Quasi-Experimental Evidence of Separation Effects from UI Benefit Extension

In this section, we leverage the variation brought about by the REBP reform to estimate whether and by how much improved worker outside options brought about by extended unemployment benefits affect job separations. We estimate that the extended benefits destroyed about 11 percent of initial matches. We further show that the separations led to long-term unemployment spells, rather than to employment with other firms. The effect of the reform is larger among workers whose outside options were effectively increased more strongly, as indicated by characteristics predisposing them to long-term unemployment above one year, which would have led them to exhaust the pre-reform benefit duration (one year). Finally, we document that the separation effect that we estimate is large in the sense of accounting for the majority of the effect of the REBP reform on employment.

4.1 Separation Effects

Separation effect among 1988 job holders. In a first step, we test whether outside option increases through benefit extensions affect separations by considering the population of workers holding a job in 1988 before the onset of the reform and show sizable separation effects in affected cohorts. Showing outcomes across cohorts and across regions, Figure 4 plots whether workers employed in 1988 before the reform were still employed with the same employer in 1993

when the REBP treatment was over.

The right y-axis shows the exposure to the REBP extensions across cohorts in REBP regions, showing sharp variation in exposure with older and younger cohorts effectively never covered by the REBP. As the green dashed line shows, cohorts born before 1933 or after 1943 were effectively not covered by the policy. Younger cohorts born after 1943 turned 50 after the REBP was abolished in 1993 so they could never claim extended benefits under the program. Older cohorts born before 1933 were effectively also not covered because they were older than 55 at the time the REBP was initiated in 1988 and, at that age, also had access to more generous disability/early retirement benefits with relaxed entry conditions. The intermediate cohorts, born between 1933 and 1943, were exposed to the reform in REBP regions. Exposure to extended benefits was maximal for the cohort born in 1938 who turned 50 at the onset of the reform in 1988 and was then exposed to the reform until it was abolished in 1993 when the 1938-cohort turned 55.

The red and blue lines of Figure 4.(a) show the share of workers in the REBP and control region, respectively, who in 1993 were still employed with their 1988 employer and document a clear decline for affected cohorts in REBP regions. By 1993, only a small share of workers in the older, non-treated cohorts born before 1933 were still employed with their 1988 employer. For the younger, non-treated cohorts born after 1943 the share of workers still employed by their 1988 employer is at roughly 60% both in the REBP and the control regions and differences between REBP and control regions are flat. Strikingly, the share of workers still employed with their 1988 employer drops visibly among affected cohorts in REBP regions. At its peak (trough) the difference in the share is about 20 percentage points relative to a control region share of about 50 percent.

Several potential confounds can already be dismissed as a consequence of the flat pre- and post-trends among older and younger cohorts and the small differences in levels between REBP and control regions among those cohorts. For instance, the fact that level differences for younger and older cohorts are small suggests that labor market conditions were comparable in REBP and control regions. Most importantly, the flat pre- and post-trends address potential concerns that the REBP and control regions were affected by different shocks in the time period from 1988 to 1993.

Separation effect among age-49 job holders. A potential confounder that is not ruled out by the graphical evidence in Figure 4 are region-by-age shocks in separation behavior. For instance, pathways to retirement could differ between regions as a consequence of potentially different industry structures. We tackle this potential concern by comparing the share of workers holding the same job at age 54 as at age 49 across cohorts and regions. If labor market trajectories of workers in this age range differed between REBP and control regions, one would expect to see differences for the younger and older non-treated cohorts.

Figure 5 shows that separations between the ages of 49 and 54 increased sizably in REBP-cohorts exposed to the reform relative to older and younger non-exposed cohorts in comparison to the pattern across cohorts in the control region. Specifically, the share of workers who at age 54 still work for the employer they had at age 49 falls steadily from around 70% in older cohorts to just above 50% in younger cohorts in the control region (blue line, panel (a) of Figure 5). While the pattern for the pre-1933 and post-1943 cohorts is similar in the REBP regions with slightly higher shares throughout, the share of retained workers falls sharply for the treated, intermediate cohorts born between 1933 and 1943. As panel (b) shows, the magnitude of the drop at peak (trough) is around 20 percentage points.

The fact that the drop in REBP regions relative to control regions in affected cohorts is substantially larger than the same difference in the non-affected cohorts provides strong evidence against region-by-age shocks driving the results in Figure 4. Taken together, Figures 4 and 5 document a significant increase in separations as a consequence of the REBP benefit extensions that is not driven by region-by-age or region-by-year confounds. Quantitatively, the reform increased separation among the most-exposed cohort by 300%, from one to four years, over a period of five years by 20 percentage points.

Employment outcomes. Figures 4 and 5 show clear visual evidence of separations from the initial employer induced by the REBP benefit extensions. However, separations could be separations into employment with other employers or into unemployment or other types of non-employment. In order to distinguish between these two mechanisms and, moreover, understand the employment consequences of the reform among initially employed workers, we plot employment outcomes between 50 and 55 analogous to the way we plotted outcomes in Figure 4. Again, the sample is confined to age-49 job holders.

The y-axes in panels (a) and (b) of Figure 6 plot the quarters employed and unemployed between the ages of 50 and 55 by cohort and region.[30]For the non-exposed younger and older cohorts, pre- and post-trends are flat and even the levels are remarkably similar. For the cohorts exposed to the reform, the Figure reveals an economically significant decrease of employment and a tantamount increase in unemployment of almost two quarters at peak. The evidence therefore rules out that the separations were simply transfers to different employers. Instead, the reform increased separations and reduced employment for a sample of initially employed workers.

Employment outcomes in initial job. In a final step, we ask whether the reduction in employment was due to shorter employment with the initial employer or due to shorter employ-

[30]We also produce analogous figures using other employment statuses, in particular disability, as outcomes as well as the number of quarters that an individual is observed in the social security data between the ages of 50 and 55. We do not find effects of the reform on the prevalence of these additional labor market statuses.

ment spells with potential future employers. In order to investigate these two channels, we plot an outcome variable measuring the quarters of continuous employment with the initial employer (Figure 7). While somewhat noisier than the previous employment measures, we find a sizable drop in continuous employment with the initial employer. As panel (b) shows, the decrease in continuous employment with the initial employer is about two quarters at peak and therefore quantitatively very similar to the overall reduction in employment between the ages 50 and 55 (Figure 6 (a.ii)). Taken together, the evidence therefore cleanly shows that benefit extensions lead to more separations, shorter employment with the initial employer and a tantamount increase in unemployment in a sample of initially employed workers.

Regression Specification. While the previous results provided visual evidence on the magnitude and the consequences of the separation effect and allowed for multiple, direct assessments of the parallel trends assumptions underlying our research design, we next estimate difference-in-differences specifications to quantify the average effect of the reform. To do so, we estimate regression specifications of the following type on the population of workers holding a job in 1988 before the onset of the reform:

$$Y_{irc} = \beta + \phi_r + \psi_c + \nu Z_{rc} + \chi_{irc}, \tag{33}$$

where Y_{irc} denotes an outcome for worker i in region r born in cohort c and the coefficient of interest, ν, captures the effect of REBP eligibility between 1988 and 1993. The model includes a region effect, ϕ_r, so that the comparison we make is within a given region and unobserved heterogeneity between regions is absorbed. The model also includes cohort effects, ψ_c, which account both for differences across cohorts as well as for lifecycle patterns in Y since we are considering the cross-section of workers employed in 1988. The specification thus exploits within-region, within-cohort variation.

Table 3 reports results of the specification in 33. Column (1) documents a 10.9 percentage point increase in separations among initially employed workers. Relative to the control group mean of 51.7% this represents about a 21% increase in separations. Column (2) through (4) document that the reform also led to substantial employment effects among initially employed workers. The reform induced months of employment to drop by 16.5 months among initially employed workers. We similarly see a large drop in continuous employment with the initial employer of about 6.9 months and a comparable increase in unemployment of 6.9 months. As the effects on unemployment and continuous employment with the initial employer are similar in magnitude, the evidence thus shows that the outside option increases due to REBP led to exits from the initial employer into long-term non-employment. For all outcome variables, the effects of the REBP reform are precisely estimated and differ significantly from zero ($p < 0.001$). In the specifications reported in Table 3, we cluster standard errors at the establishment level.

We find similar results in specifications with standard errors clustered at the 2-digit industry or at the district (*Bezirk*) level.

4.2 Adverse Selection: Heterogeneous Separation Responses by Predicted Risk of Long-Term Unemployment

To shed further light on whether benefit extensions led to separations by increasing workers' outside options, we assess whether the effect of the reform is larger among workers whose outside options effectively increased more strongly as indicated by having characteristics disposing them to unemployment durations of more than one year. For workers who are at risk of long-term unemployment beyond one year, the REBP was particularly relevant as it increased benefit duration from one to four years.

To assess workers' risk of long-term unemployment, we estimate a model based on observations from the pre-reform period and create long-term unemployment risk scores for workers employed at the onset of the REBP reform in 1988. For all workers observed in 1982, we regress an indicator for whether they had an unemployment spell of one year or longer on a rich set of covariates measured in 1982: industry fixed effects, an indicator for working in a white collar occupation, the local unemployment rate, and third degree polynomials in tenure and experience. We then take the estimated coefficients and predict the probability of having an unemployment spell of one year or longer than among the sample of workers employed in 1988 and create a predicted long-term unemployment risk score. We then split workers into quintiles based on their long-term unemployment risk score and estimate the effect of the REBP reform within each quintile in a fully interacted difference-in-differences regression taking out the baseline effects of being in an REBP-eligible cohort and residing in an REBP region both interacted with indicator variables for each risk quintile.[31]

Figure 8 shows that the separation effect is substantially stronger among workers with a higher predicted risk of long-term unemployment. In the three lowest-risk quintiles, the separation effects are relatively small and statistically indistinguishable from zero in two out of three quintiles. The separation effects are markedly larger, around 20 percentage points, in the two quintiles with the highest risk of long-term unemployment. An F-test rejects that the treatment effects are equal in the five quintiles with $p < 0.001$. Overall, the analysis shows that workers whose outside option effectively increased by more were more likely to separate in response to the reform

[31] Nekoei and Weber (2017) construct a related heterogeneity measure, in form of the probability of exhausting the pre-reform benefit level with a focus on unemployed job seekers' search behavior, in the context of different reform.

4.3 Tracing the Nature of Separations: A Retrospective Household Survey

Our discrete choice framework takes as a point of departure the most general case of efficient separations and wage bargaining. As a result, the relevant job value measure is the joint surplus. However, in the presence of frictions such as wage rigidity, separations might not be bilaterally efficient but one-sided, in form of firm-initiated layoffs or worker-initiated quits. We formalize these alternative separation scenarios first. Then, we trace the nature of the separations in the data. The key challenge we face is general to administrative data: while providing high-quality data on worker transitions and employment biographies, the spell data are agnostic to the particular nature of a separation. Given the limited knowledge about the nature and process of separations, let alone in response to shifts in the worker's non-employment value, we attempt to shed light on this blackbox in two ways. First, we do draw from a worker level survey that retroactively asks workers to classify the type of separation of the worker's last job. Second, related to Appendix Section E, we exploit an administrative wait period for UI eligibility for quits that we can trace in the administrative data.

Allowing for inefficient separations: layoffs and quits. In Appendix Section D, we consider deviations from the surplus-focused setting of bilateral efficiency that may arise from frictions, e.g. in form of ad-hoc wage rigidity. With wages not necessarily set to achieve bilateral efficiency, there will be a range of wages that will leave one party prefer the outside option to the inside option at that wage, although the match carries positive surplus – i.e. wages could be set to fulfill the participation constraint of both parties. In the Coasean environment, classifying separations into quits and layoffs is meaningless because separations are bilaterally efficient: wages will adjust to preserve positive-surplus matches, but wage adjustments cannot prevent destruction of jobs that lost their surplus. With inefficient wages, quits and layoffs become meaningful.

In order to further disentangle the types of REBP-induced separations, we leverage rich microdata based on the Austrian Microcensus. The analysis based on the survey documents that the REBP led to additional quits and, to a lesser extent, layoffs for economic reasons. Further evidence shows that these incremental quits were in part retrospectively classified by respondents as having been due to sickness or disability.

Survey: Mikrozensus. The Austrian Microcensus is the largest, continuous survey of the Austrian population and is conducted by Statistics Austria. Sampling is based on administrative population registries and the Microcensus follows a rotating panel of households. Participation in the survey is mandatory by law, thus ensuring high participation rates and representative samples.

We leverage information on non-employed respondents' last employment spell to classify excess separations due to the REBP. Specifically, starting in 1995, the survey asked respondents retrospectively about their last employment spell. Crucially, for employment spells that ended within the last eight years, respondents also report the reason for the end of the last employment spell and, in particular, whether the last employment spell ended due to quit – initiated by the worker or mutually agreed upon – or a layoff due to establishment closure, for economic reasons, or for other reasons.[32] If the last employment spell ended due to a quit, respondents can also list reasons for the quit, including sickness or inability to work and personal or family circumstances.

Replicating the effect of REBP on separations in the Mikrozensus survey. Our analysis first revisits whether the REBP led to excess separations. Figure 9 plots an indicator for whether the last employment spell of a non-employed respondent ended when the respondent was between 50 and 54 years old, i.e. the effective age range when extended benefits were available under the REBP program. The Figure shows averages of the indicator by cohort and region. As in the analysis based on administrative data, the analysis based on the Microcensus survey data clearly documents excess separations in the REBP regions for cohorts affected by the reform, i.e. those born between 1933 and 1943. The lines of the REBP and the control regions are parallel and in fact almost lie on top of one another outside of the treatment cohorts.

Composition shifts in separation types. Having replicated the separation effects of REBP, we next turn to the causes of excess separations due to the REBP by again focusing on the sample of respondents whose job ended when they were between 50 and 54 years old and analyzing indicator variables for the different types of separations between the ages of 50 and 54. We estimate the following model:

$$y_{rci} = \alpha + \gamma_r + \kappa_c + \beta \ \text{REBP}_r \times \text{Treated Cohort}_c + \epsilon_{rci},$$

where y_{rci} is a variable indicating a separation type of respondent i from region r born in cohort c. The model includes baseline effects γ_r and κ_c for region and cohort. The coefficient on the interaction between an indicator for the REBP region, REBP_r, and cohorts born between 1933 and 1943, Treated Cohort_c, captures the effect of the REBP reform on the types of separations of affected workers. For y_{rci}, we consider several indicator variables that are equal to one if the last employment spell ended between the ages of 50 and 54 and was either:

1. a one-sided or amicable quit,

2. a layoff due to establishment closure,

[32] The remaining categories of reasons for employment spell endings are the end of a fixed term contract, early retirement, regular retirement, and civil service.

3. a layoff due to economic reasons,

4. early retirement *incl. limited employable and health*,

5. regular retirement, or

6. other reasons.[33]

Importantly, the questionnaire and the survey manual clarify that "early retirement" encompasses separations leading into permanent exits from the labor force due to limited employability, such as labor market conditions or idiosyncratic factors such as health.

Figure 10, top panel, plots the difference-in-difference treatment effects of the REBP program on these four types of separation indicators and shows a statistically highly significant, positive effect of the REBP program on quits among older workers of about 0.7ppt, which is a roughly 50% increase off a baseline probability of 1.4ppt for older workers in the control region. The Figure also provides some evidence for additional layoffs due to economic reason with an effect size of about 0.5ppt, also corresponding to about a 50% increase. The effects on layoffs due to closure or for other reasons are much smaller, and statistically not significant. In addition, the analysis also reveals increases in early retirement as well as retirement for other reasons. (We report relative effects in companion graph in the bottom panel of Figure 10.)

Further disaggregation of quits. Finally, we dissect the reasons for the additional quits induced by the REBP and analyze outcome variables that measure whether respondents retrospectively classify a quit as having been due to (i) personal or family circumstances, (ii) sickness or disability, or (iii) other reasons. Strikingly, we find that almost half of additional quits induced by the REBP were classified as having been due to sickness or disability. This result has two core implications: first, it provides additional support suggesting that increasing workers' outside options destroys low surplus jobs, in this case of individuals who perceived themselves to be sick or unable to work. Second, the analysis provides a direct example of post hoc rationalization: while our analysis strongly suggests that the additional separations and quits under scrutiny were attributable to the REBP program and would not have occurred had benefits not been extended, individuals retrospectively attribute causality to their own sickness or disability. Third and alternatively, households may report the largest detractor in surplus rather than considering the incidental surplus reduction from our instrument.

[33]Other reasons include ends of fixed term contracts, civil service, and a residual, unclassified category.

5 Complier Characterization: Marginal Jobs and Workers

We next shed light on the economic mechanisms underlying endogenous job destruction and employment adjustment more generally. In order to do so, we extend complier analysis (Imbens and Rubin (1997); Abadie (2003)) to difference-in-difference settings under suitable parallel trends assumptions. This methodological advancement allows us to directly characterize the marginal jobs destroyed by the reform, in particular among markers of job surplus. More broadly, our methodology can be applied in other difference-in-differences IV settings to characterize compliers.

Our analysis shows that marginal jobs originate from blue-collar occupations in industries with a high incidence of sickness and disability among older workers. Compared to surviving jobs, marginal jobs had lower earnings and were more prevalent in shrinking industries and firms. In turn, our findings imply that increasing workers' outside options shifts the composition of employment towards jobs with higher earnings and higher initial surplus.

5.1 Econometric Strategy: Identification and Estimation

To characterize the marginal jobs dissolved in response to the surplus reduction from the UI extension, we cast our analysis as an instrumental variable problem and then rely on and extend results that identify characteristics of compliers (see Imbens and Rubin (1997); Abadie (2003)) to difference-in-differences settings. Specifically, we extend the methodology from Abadie (2003) to a difference-in-differences framework, thereby complementing recent advances to extend instrumental variable approaches to the difference-in-differences framework (De Chaisemartin and D'Haultfoeuille (ming), Hudson and Liebersohn (2017)). This formulation and the associated assumptions correspond tightly to the discrete choice problem from the previous Section.

Instrument: an age- and region-specific unemployment insurance extension. For each individual, we first define a binary variable Z:

$$Z = \begin{cases} 0 & \text{ineligible for unemployment insurance extension } \Delta b \\ 1 & \text{eligible for unemployment insurance extension } \Delta b \end{cases}$$

The particular reform and difference-in-differences setup. To conduct the complier analysis, we examine a quasi-experimental reform, described in detail in Section 3, that was introduced in certain regions but not in others. Within the treatment regions, the extension only applied to workers older than 50 at some point during the reform period. Older workers in the treatment region were eligible for extended benefits ($Z = 1$), whereas younger workers

as well as older workers in the control region are not eligible ($Z = 0$). Interpreted through the lens of the discrete choice problem introduced in this Section, one can think of Z as modifying a worker's outside option $b + Z \cdot \Delta b$.

Given the variation across regions and age groups, we cast our analysis as a difference-in-differences setup and analyze specifications with fixed effects for region r and age a. We let (r, a) represent a region and age such that $Z = 1$, and (r', a') represent other regions and ages (i.e. $Z = 0$ for$(r', a), (r, a')$ or (r', a')).

Treatment variable: separation. We next define a binary variable, D, indicating whether a worker separates from their initial, pre-reform job by the end of the reform:

$$D = \begin{cases} 0 & \text{no separation} \\ 1 & \text{separation} \end{cases}$$

We let D_0 and D_1 denote the value that D takes for $Z = 0$ and $Z = 1$, respectively ($D = ZD_1 + (1 - Z)D_0$).

Partitioning the set of pre-reform matches. Based on the discrete choice framework and following Imbens and Rubin (1997) and Abadie (2003), we define the following three groups:

$$\begin{aligned} D_0 = D_1 = 1\text{: Always-Separators} &\quad \Leftrightarrow S(\mathbf{x}', b_0) < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') \\ D_0 = D_1 = 0\text{: Never-Separators} &\quad \Leftrightarrow S(\mathbf{x}', b_0) \geq \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') \\ D_0 = 0, D_1 = 1\text{: Compliers (marginal matches)} &\quad \Leftrightarrow 0 \leq S(\mathbf{x}', b_0) < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') \end{aligned}$$

Compliers, stated alternatively, separate if the reform is in place but do not otherwise.

Attributes. We define X to be attributes that we characterize for compliers and the other two groups. Our goal is to characterize the mean for compliers, i.e., $E[X|a, r, D_0 = 0, D_1 = 1]$.

Assumptions. We make the following four assumptions in order to characterize marginal matches.

Assumption 1. First stage: For all (r, a), $P(D_1 = 1|r, a) > P(D_0 = 1|r, a)$.

Intuitively, assumption 1 posits that more separations take place under the reform and ensures the existence of compliers.

Assumption 2. Monotonicity: $D_1 - D_0 \geq 0$.

Importantly, assumption 2 rules out defiers, i.e. individuals that would separate if benefits are not extended but would not separate if unemployment benefits are more generous.

Assumption 3. Independence: $(X, D_0, D_1) \perp Z|(r, a)$.

The independence assumption posits that conditional on r and a, the instrument Z is orthogonal to X, D_0, and D_1.

Assumption 4. Additive separability:
(a) For all (a, a') and $d, d' \in \{0, 1\}$,
$E[X|a, r, D_0 = d, D_1 = d'] - E[X|a', r, D_0 = d, D_1 = d']$ does not depend on r.
(b) For all (a, a') and $d, d' \in \{0, 1\}$,
$P(D_0 = d, D_1 = d'|a, r)$-$P(D_0 = d, D_1 = d'|a', r)$ does not depend on r.

Assumption 4 implies parallel trends assumptions both for attributes X as well as for the vector (D_0, D_1). Specifically, we assume that the difference across age groups in attribute X does not differ across regions. Our additive separability assumption mirrors analogous assumptions for parallel trends in outcomes Y in recent work extending the instrumental variables setup to difference-in-differences settings (De Chaisemartin and D'Haultfoeuille (ming), Hudson and Liebersohn (2017)).

Identification and estimation of complier means. Under assumptions 1 through 4, we can express the characteristics of compliers, $E[X|r, a, D_1 = 1, D_0 = 0]$, in terms of estimable quantities:

$$\begin{aligned} E[X|r, a, D_1 = 1, D_0 = 0] \;=\; & \tfrac{\pi^C_{ra}+\pi^A_{ra}}{\pi^C_{ra}} \cdot E[X|r, a, D_1 = 1] \\ & -\tfrac{\pi^A_{ra}}{\pi^C_{ra}} E[X|r, a, D_0 = 1], \end{aligned} \tag{34}$$

where $\pi^A_{ra} = P(D_0 = 1, D_1 = 1|r, a)$ denotes the share of always-separators, $\pi^C_{ra} = P(D_0 = 0, D_1 = 1|r, a) = 1 - \pi^A_{ra} - \pi^N_{ra}$ the share of compliers, and $\pi^N_{ra} = P(D_0 = 0, D_1 = 0|r, a)$ the share of never-separators. Appendix F documents the proof following arguments in Abadie (2003).

Equation 34 shows that under assumptions 1 through 4, complier characteristics are identified in a difference-in-difference IV setting as we can construct sample analogues to each of the terms on the right-hand side as follows. By independence, we have that:

$$E[X|r, a, D_1 = 1] = E[X|r, a, D = 1, Z = 1]. \tag{35}$$

By independence and parallel trends in X, we have:

$$\begin{aligned} E[X|r, a, D_0 = 1] =& E[X|r, a', D_0 = 1] + (E[X|r', a, D_0 = 1] - E[X|r', a', D_0 = 1]) \\ =& E[X|r, a', D = 1, Z = 0] + (E[X|r', a, D = 1, Z = 0] \\ & - E[X|r', a', D = 1, Z = 0]). \end{aligned} \tag{36}$$

Sample analogues exist for each of the right-hand side terms in 35 and 36. For example, we can estimate a sample analogue for $E[X|r, a', D = 1, Z = 0]$ as:

$$E[X|r, a', D = 1, Z = 0] = \frac{1}{N_{r,a'}} \sum_{i \in (r,a')} X_i D_i (1 - Z_i), \tag{37}$$

where $N_{r,a'}$is the number of observations in (r, a').

Finally, under independence and parallel trends in (D_0, D_1), we can express the conditional probabilities π^A_{ra} and π^N_{ra} as follows:

$$\begin{aligned}
\pi^A_{ra} = P(D_0 = 1, D_1 = 1|r, a) &= P(D_0 = 1|r, a) \\
&= P(D_0 = 1|r, a') + P(D_0 = 1|r', a) - P(D_0 = 1|r', a') \qquad (38) \\
&= E(D|Z = 0, r, a') + E(D|Z = 0, r', a) - E(D|Z = 0, r', a'), \\
\pi^N_{ra} = P(D_0 = 0, D_1 = 0|r, a) &= P(D_1 = 0|r, a) \\
&= P(D = 0|Z = 1, r, a) \qquad (39) \\
&= 1 - E(D|Z = 1, r, a).
\end{aligned}$$

These quantities can be estimated in the regression:

$$D_{ira} = \beta + \phi_r + \psi_a + \nu Z_{ra} + \chi_{ira}. \tag{40}$$

The sample estimators are then given by $\pi^A_{ra} = \hat{\beta} + \hat{\phi}_r + \hat{\psi}_a$, $\pi^N_{ra} = 1 - \hat{\beta} - \hat{\phi}_r - \hat{\psi}_a - \hat{\nu}$ and $\pi^C_{ra} = 1 - \pi^N_{ra} - \pi^A_{ra} = \hat{\nu}$.

Under assumptions 1 through 4, our framework therefore allows us to directly estimate any complier characteristic X in a difference-in-differences IV setup. Importantly, we can also estimate distributions of complier characteristics $\tilde{X}$ using indicator functions $X = 1(\tilde{X} < c)$. For inference, we use the non-parametric bootstrap to arrive at a sampling distribution of 34.

5.2 Characterizing Marginal Matches: Results

Which are the marginal jobs and who are the marginal workers? Drawing on the discrete choice framework and the econometric method laid out in Section 2, we next dissect the characteristics of the jobs that are marginal. We classify those jobs as marginal that would have survived in the absence of benefit extensions but did not survive when benefits are extended.

At a broad level, our results suggest that marginal jobs are characterized by low and declining match surplus. In particular, we find that compliers are primarily blue-collar workers in declining firms from manual labor-intensive industries with higher shares of disability or sickness among older workers. Table 7 provides an overview of means for compliers, never-separators, and

always-separators, based on pre-reform data from 1988, along with p-values for mean differences.

Occupational proxy. In a first step to shed light on the characteristics of marginal jobs, we investigate the occupational structure of marginal jobs based on a classification into blue- and white-collar occupations. Strikingly, 87.2% of marginal jobs are in blue-collar occupations while the share of blue-collar workers among *both* always- and never-separators are lower at 77% and 58%, respectively.

Industry composition. Considering the share of marginal jobs across industries also reveals a concentration in manual labor-intensive sectors as Figure 13 shows. Specifically, manual labor-intensive sectors such as mining or manufacturing have relatively high shares of marginal jobs and of always-separators while virtually no marginal jobs exist in high-skilled, white collar-intensive sectors such as health or banking.

Why are marginal jobs concentrated in blue-collar occupations and manual labor-intensive sectors? We provide evidence on shifts in both labor supply and labor demand which suggest that low and declining surplus in these jobs rendered them marginal.

Pre-separation employment growth: firm and industry level. In a first step, we provide evidence on the labor demand side and show that marginal jobs stem from industries or firms with stagnating or declining labor demand. In order to do so, we calculate industry and firm employment growth rates in the pre-period from 1982 to 1987 and consider this as a complier attribute. The analysis reveals that marginal jobs stem from declining industries which had a *negative* growth rate of -1.8ppt in the pre-period while both always- and never-separators come from moderately growing industries with positive growth rates of around 0.9 and 2.9ppt in the same time frame. The pattern is more pronounced at the firm level, where we find that compliers stem from firms with a negative average growth of -19.2ppt while never-separators stem from firms with positive growth of 8.1ppt and always-separators stem, on average, from stagnating firms with pre-period growth of -0.8ppt (see also Figure **??**, which shows a histogram of firm growth rates in the three respective groups). Relatedly, only 12.2% of marginal jobs stem from firms with positive growth while the shares of both always- and never-separators in growing firms are substantially higher at 35.5% and 53.6%, respectively. We also consider the full time series of firms' employment in Figure **??**, which reveals that marginal jobs stem from initially substantially larger firms that exhibited strong, secular declines in the 1980s. Overall, the evidence documents that marginal jobs occurred in sectors and firms with declining labor demand and thus a low or decreasing match surplus.

Wage earnings. The previous section established that marginal jobs originated primarily from firms in declining industries. We next investigate the wages of compliers as economists

have long hypothesized that surplus shocks will lead to more separations among lower-wage workers (see, e.g., Oi (1962)).[34] Our analysis reveals that relative to never-separators (EUR 28,252, CPI-deflated to 2003), marginal matches have slightly lower average earnings at EUR 27,500 ($p < 0.1$). The wage earnings of marginal jobs and always-separators are statistically indistinguishable. Interpreted in the context of our model, the results suggest that low-surplus matches of compliers and always-separators carry lower wages than high-surplus matches of never-separators.

AKM decomposition of wages. We next dissect the differences in wages between the group further by estimating an AKM-specification in the pre-period before 1988 and show that the relatively modest average wage differences between marginal jobs and never-separators shroud important heterogeneity in worker and firm effects. Following Abowd et al. (1999), we estimate the following model:

$$\ln w_{it} = \alpha_i + \psi_{J(i,t)} + X'_{it}\phi + \nu_{it},$$

where α_i and $\psi_{J(i,t)}$ denote worker and firm fixed effects, respectively. X_{it} denotes a vector of control variables that include a third-order polynomial in tenure. Worker effects α_i can be interpreted as the permanent component of wages, an effect workers command irrespectively of their employer. Firm effects $\psi_{J(i,t)}$ capture the wage premium or discount a given employer pays to a worker controlling for the worker effect. As in the AKM literature, the terms are identified through movers between different employers, in the largest connected set.[35]

Our analysis reveals that worker effects are substantially *lower* among marginal workers compared to never-separators ($\Delta = 9.71$ppt, $p < 0.001$). The results document that marginal matches are among workers that have permanently lower earnings compared to their peers. One possible interpretation is in line with the hypothesis and important facts documented by Oi (1962), who argued that shocks will lead to more separations among workers with lower wages, his proxy for occupation differences in the quasi-fixedness of labor – surplus in our terminology – that would arise from specific investments.

We next turn to an analysis of firm effects and find that marginal jobs originate from firms with, on average, slightly *higher* firm effects ($\Delta = 0.64$ppt, $p < 0.1$). Since firm effects have been shown to correlate positively with measures of value added per worker (see Card et al.

[34]Specifically, Oi (1962) writes: "Consider a firm faced by a decline in product demand. The adjustment process involves a reduction in output accompanied by a decline in the demand for each variable factor. There is no reason to expect that the demands for all variable factors will be decreased by the same proportion. [...] A direct test of the theory would require estimates of the degree of fixity for different grades of labor. Since such data are not readily available, an auxiliary variable that is closely correlated with the degree of fixity must be employed as a proxy. The use of an occupation's wage rate as the proxy seems justifiable and derives support from the empirical evidence presented in this section."

[35]For estimation, we follow the procedure in Correia et al. (2016).

(2017)), this result is puzzling at face value. We offer two explanations: first, recent work (Sorkin (ming)) has documented that firm effects may reflect in part compensating differentials such that firms with lower amenities need to compensate workers such that higher firm effects may correlate with disamenities. A second potential explanation is that we estimate firm effects in the pre-reform period when, e.g., manufacturing firms paid wage premia but then experienced a stronger labor demand decline in the late 1980s and early 1990s.

Tenure. We next turn to the analysis of tenure and document that tenure of workers in marginal jobs is *higher* compared to the whole sample. Specifically, the complier mean for tenure in 1988 is 13.3 years compared to 10.6 and 11.6 years for always-separators and never-separators, respectively. Similar to the results for wages, we find that characteristics of marginal jobs fall between those of always- and never-separators.

Worker sickness. Finally, we investigate to what extent surplus variation due to labor supply-based factors matters for rendering matches marginal. To do so, we investigate the age patterns of sickness and disability across sectors in the pre-reform period. Figure 14 shows the lifecycle incidence of sickness and disability and shows that the incidence is small and close to zero across all industries and increases sharply in some industries starting around age 50. At age 55, the highest incidence of sickness and disability can be found in mining, construction, and manufacturing, sectors dominated by blue-collar labor that also showed a high share of marginal jobs (see Figure 13). We next show that the incidence of sickness among older workers (in the pre-period) in a given sector predicts the share of jobs that are marginal in that sector (see scatter plot in Figure 15, which compares the complier share of the industry against the share sick at age 50). Table 7 also provides the industry-level average shares of sickness spells among older workers and shows that marginal jobs come from sectors with an average prevalence of sickness spells of 2.47% compared to 2.30% for never-separators. While the mean difference is seemingly small, it corresponds to 29% of a standard deviation in the outcome variable and is statistically highly significant ($p < 0.001$). Moreover, sickness and disability spells for workers younger than 55 are an indicator of severe health problems as disability at this age is formally only granted if a medical impairment reduces the capacity to work by at least 50 percent in *any* occupation. Overall, the results pertaining sickness indicate either disutility of working increases or worker productivity decreases sharply with age in these manual labor-intensive sectors, i.e. when surplus is low and shrinking with age.

6 Marginal Jobs and Aggregate Employment Adjustment: Evidence from the Abolition of the Reform in 1993

In a final step, we test whether increasing worker outside options indeed shifted the composition of employment toward jobs that had higher surplus and were, in turn, more resilient. The intuition of our model is simple: the cleansing effect of the outside option increase swept up low-surplus matches. If outside options were subsequently reduced again, the remaining jobs would have higher surplus on average; more specifically, the resulting distribution would feature a missing mass of exactly the marginal matches that would drive aggregate extensive-margin elasticities (and drove the employment reduction in response to the initial surplus shock from the REBP instrument). The direct prediction is that subsequent negative surplus shocks will have a smaller effect on average job destruction in that formerly-treated sample. period,

To directly test whether the set of jobs surviving increased outside options are subsequently less sensitive to negative shocks to surplus, we zoom in on the subset of workers whose 1988 job had survived the REBP reform until its abandonment in 1993 and then estimate whether subsequent labor demand shocks lead to separations in the post-reform period. Moreover, by conducting this test with non-UI shocks to surplus (but with reduced-form labor demand shocks), the test also comprises the allocative concept of joint job surplus.

Experiment: abolition of the reform. A unique feature of our reform allows us to implement this thought experiment directly. Specifically, we exploit the unique feature of the sharp abandonment of the REBP in August 1993, which generated the following paths of benefits in the treatment group (see also Figure 3 for the reform timeline):

1. Pre-reform (until 1988): b_0
2. Reform (1988–1993): $b_0 + \Delta b$
3. Post-reform (after 1993): b_0

We discuss the timing and context of the abolition in 1993 in Section 3. In the conceptual framework in Section 2.4 and in this empirical test, we treat the regime shift as unexpected as well as approximate the expectations of the agents to reflect a nearly permanent installation of the reform up until 1993.

Sample: 1988–1993 job holders. This Section switches samples to May 1988–August 1993 job stayers. Our intention is to trace the consequences of the selection into separation by the outside option increase active from 1988 to 1993 by conditioning on 1988–1993 job stayers. The formerly-treated jobs shed their marginal jobs, whereas the always-control group still contains these compliers. We therefore examine post–1993 employment biographies of this sample.

Null hypothesis. Our null hypothesis is that post-abolition separation and shock sensitivity is similar between both groups. This effect would emerge if selection into separation had been orthogonal to surplus, or if job surplus is not persistent within a match. By contrast, we expect longer post-1993 duration of formerly-treated jobs. Importantly, thanks to the switching-off of the reform and the return to an homogeneous incentive environment between former-REBP and control groups, the differences in outcome behavior will be entirely driven by compositional effects.

Assumptions. Our empirical strategy recognizes that REBP has had great effect not only on the composition of job stayers, but might have affected REBP and control regions through many other channels, such as investment, migration, demand for non-tradables or the quality of the labor pool. Our difference-in-difference design aims to nets out region-and-time-specific shocks by first taking the within-region difference in job duration below and above the 1943 eligibility threshold. Our identifying assumption is therefore that those other REBP channels do not have a differential effect on slightly older workers. A violation of this assumption would exist if, for instance, workers in slightly older cohorts in REBP regions might have developed stronger tastes for early retirement. However, since we consider a sample of workers still employed in 1993 and analyze sharp differences between cohorts that *all* expected to eventually be eligible for the REBP, we do not expect discreet shifts between cohorts in such taste parameters and rather assume that observed differences stem from the composition change among those cohorts that had actually been eligible for REBP by the time abolished.

Labor demand shocks at industry level. We identify a labor demand shock for worker i employed by firm j in industry k by constructing the leave-out mean industry growth rates between time period t and t' in the workers' industry k:

$$\widetilde{\Delta S_{ijk}} = \frac{\sum_{j' \in J \setminus j} \mathbb{1}(Industry_{j'} = k) \cdot (Employment_{j't'} - Employment_{j't})}{\sum_{j' \in J \setminus j} \mathbb{1}(Industry_{j'} = k) \cdot Employment_{j't}}. \tag{41}$$

Stated differently, the labor demand shock corresponds to the employment growth rate in industry j leaving out the observation of worker i's own firm to prevent a mechanical relationship between employment outcomes at firm j and $\widetilde{\Delta S_{ijk}}$. In our main specifications, we calculate the shocks at the four-digit industry level (NACE) over a period of five years. As an alternative to relying on leave-out industry growth rates as labor demand shocks, we also construct analogous industry growth rates based on data from Germany. In Figure 16, we document — using data from the control region among cohorts that would have been eligible for the REBP had they lived in REBP regions — that the cross-sectional effect of industry shocks on separations is negative.

Econometric specification. Letting $\widetilde{D_{ijrc}}$ denote a separation from the 1993 employer, we estimate the following model:

$$\widetilde{D_{ijrc}} = \alpha_1 + \kappa_r + \lambda_c + \delta_k + \phi_{kr} + \rho_{kc} + \gamma Z_{rc} \tag{42}$$

$$+ \gamma_{zs}(Z_{ra} \cdot \widetilde{\Delta S_{ijk}}) + \epsilon_{ijra}. \tag{43}$$

The coefficient of interest is γ_{zs} , capturing the excess sensitivity of separations among jobs that survived the REBP reform to surplus shocks in the post-REBP period. Since the cross-sectional effect of industry growth rates on separations is negative (Figure 16), a positive coefficient γ_{zs} indicates smaller sensitivity of separations to surplus shocks among cohorts treated by the REBP program. The model includes baseline effects for region r (REBP versus control region), cohort c (cohorts affected by the reform versus older and younger cohorts), and the interaction of the two, Z (varying at the region-by-cohort level). Importantly, the model also includes industry-by-region effects, ϕ_{kr}, and industry-by-cohort effects, ρ_{cr}, as control variables. The two sets of fixed effects absorb a large class of potential confounders. For example, one might worry that firms in certain industries in REBP regions might be more responsive to industry shocks; by including industry-by-region effects, ϕ_{kr}, the econometric specification accounts for such a potential confounder as the comparison is across older and younger cohorts within the same industry and region cell. An alternative confounder might be a differential responsiveness of older cohorts in certain industries to labor demand shocks. By including industry-by-cohort effects, ρ_{kc}, as control variables, our identification strategy accounts for such a potential confounder as our comparison is across the REBP and control regions among individuals in the same cohort and industry cell.

Empirical results. Table 5 presents empirical results of the econometric model (43) and provides support for the prediction of our model: cohorts in REBP regions treated by the REBP reform subsequently exhibit less sensitivity to industry shocks and the remaining matches are thus more resilient. We present specifications at time horizons ranging from one to five years after the abolition of the REBP reform and find positive effects of the industry growth rate, $\widetilde{\Delta S_{ijk}}$, on the probability of a job surviving among treated cohorts in the REBP region. As discussed above and visualized in Figure 16, the baseline effect of industry growth on the separation probability is *negative*. Therefore, the positive coefficients on $\widetilde{\Delta S_{ijk}}$ in Table 5 indicate that industry shocks had *smaller* effects among groups of workers treated by the REBP reform. The magnitude is substantial: while higher industry growth rates have a strong negative effect on the probability of a separation in the cross-section (e.g., a 10% increase in the 4-digit industry growth rate is associated with a 3.5 ppt increase in separations), the effect is strongly attenuated among cohorts that had undergone the REBP program before 1993 (a decrease by 2.7 ppt, standard error of 1.1 ppt). We find stronger results in the years immediately after

the REBP program ended and smaller but still statistically significant effects at a five year horizon. In terms of economic magnitude, we find an attenuation of a similar magnitude to the benchmark effect in specifications using 4-digit industry growth and a reduction of about 78% of the benchmark effect in specifications with industry growth calculated at the 2-digit level.

Robustness checks and analysis of mechanisms. We provide several robustness checks, changing outcome variables, functional form restrictions, samples, and winsorization, and throughout all specifications find less sensitivity among REBP-treated cohorts. First, we assess the age patterns of effects focusing on outcome variables measuring whether workers had separated by a certain age a, with a ranging from 53 to 57. The analyses, reported in Table 6, documents strong attenuation effects for separations by 53 and 54 and smaller and statistically insignificant attenuation at higher ages. Second, we allow for different effects of negative industry shocks on separations. baseline effects of industry growth are substantially larger for negative industry growth rates than for positive industry growth rates, thereby confirming patterns documented in Davis et al. (2006). Our analysis of attenuation effects also reveals substantially larger point estimates for attenuation effects of negative industry growth rates. However, the effects are too imprecisely estimated in most specifications to reject an equality of effects of negative and positive industry growth rates. Third, we also confine the sample to younger cohorts born after 1939 who were 54 or younger at the time of the abolition of the REBP program. We focus on younger cohorts as workers above 55 had relaxed access to disability insurance so that employment rates are relatively low and adjustment responses might be limited. We find a similar pattern of results when focusing the analysis on this sample of younger cohorts. Finally, we explore sensitivity of our results to winsorization. Our main table had analyzed industry shocks winsorized at the 5% level. In additional specifications, we analyze different levels of winsorization ranging from 1% to 10% winsorization. Across specification, we find that point estimates for the attenuation effects are larger in the winsorized specifications even though all specifications show attenuation effects. One interpretation of the winsorization effects is that extreme industry growth rates are particularly noisy and thus lead to measurement error in specifications without winsorization. A second interpretation, borne out of our model in Section 2.4, is that the separation effects from surplus shocks are concave. Small shocks remove marginal matches still present in the control region, of which the formerly-treated group has fewer left. Larger, negative surplus shocks entail more and more separations even in the treatment group, finally resolving the relatively more resilient matches that survived the REBP reform into separations.

7 Conclusion

When labor market quantities adjust, they largely do so along the extensive margin (individuals at work or not) rather than the intensive margin (hours worked per individual).[36] Employment adjusts through the separation margin and the job finding/hiring margin. At the micro level, such extensive-margin adjustments play out as discrete choice problems within employment relationships of individual workers and firms with heterogeneous job surplus values, that are destroyed, maintained or formed. The reservation strategy of the parties requires that the inside value of the job exceed each party's outside options, i.e. that job surplus be positive.

In this paper, we have documented *direct* micro-evidence for the link between job surplus and job destruction. We have identified and characterized marginal matches to shed light on how the basic mechanism underlying extensive-margin employment adjustment plays out at the job level. Taken together, our findings indicate that increasing workers' outside options destroys marginal, low-surplus jobs and shifts the composition of employment towards jobs with higher earnings and higher gross surplus, and shown that these composition shifts have consequences for measured employment elasticities, which are lower when the mass of marginal matches is reduced.

We exploited a quasi-experimental and large extension of unemployment insurance benefits in Austria between 1988 and 1993. When workers' non-employment value increases, job surplus shrinks, and marginal matches are dissolved. While the literature has primarily focused on understanding decentralized bilateral transactions by studying the job finding margin, that margin is by nature empirically elusive: dynamic selection, unobserved reservation wages, and hard to measure job offers restrict observation to the average accepted job. We also provide new evidence on the effect of specifically unemployment insurance as an institution, documenting jobs separation as well as compositional effects.

The empirical picture we paint of the marginal jobs and marginal workers is broadly consistent with the theoretical prediction of low surplus jobs being dissolved first, in response to the increase in non-employment value of the worker. We view this finding as a sharp test of the theory of endogenous job destruction (Mortensen and Pissarides (1994)). The notion of endogenous separations may be self-evident, but our documentation of the channel is non-trivial because much of the literature assumes that separations are exogenous, in effect that most matches carry sufficient surplus to withstand a wide range of shocks. Documenting the marginal matches directly as well as proxies for low surplus is possible to the complier characterization. While on average low-surplus proxies are associated with marginal matches, we also find dispersion in a variety of job, firm and worker attributes, which reveals that UI-induced separations affected a broad cross-section of job types. Many different pockets of the Austrian labor market contained

[36] For business cycle adjustment, see Hansen (1985). For cross-country facts, see Prescott et al. (2009) and Bick et al. (2014).

marginal matches. Finally, the link between job surplus and employment sensitivity is a fundamental ingredient that underlies successful *cyclical* models of the labor market (Ljungqvist and Sargent (2017)). It also is the fundamental factor for high unemployment in *steady state* (Pissarides (2000)).

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8 Tables

Table 1: Summary Statistics (Sample of Job Holders in 1988)

	no rebp	rebp until 1993	Total
Age	35.95	35.38	35.82
	(11.88)	(11.64)	(11.83)
White Collar	0.397	0.326	0.381
	(0.489)	(0.469)	(0.486)
Experience	14.32	14.85	14.44
	(8.204)	(8.204)	(8.207)
Tenure	7.418	8.229	7.605
	(5.707)	(5.850)	(5.750)
Earnings	23854.8	23775.8	23836.6
	(8896.3)	(8538.6)	(8815.3)
Ln Earnings	9.987	9.990	9.987
	(0.478)	(0.464)	(0.475)
Local Unemp. Rate	0.0258	0.0395	0.0289
	(0.0254)	(0.0330)	(0.0279)
Local URate (50+)	0.0451	0.134	0.0656
	(0.0440)	(0.128)	(0.0816)
Firm Size	445.5	1577.7	706.0
	(1078.7)	(3889.3)	(2145.8)
Emp. Growth Firm	0.391	0.479	0.412
	(3.204)	(5.741)	(3.936)
Emp. Growth Ind.	0.0331	0.0127	0.0284
	(0.0801)	(0.0743)	(0.0793)
1(Growing Firm)	0.576	0.522	0.564
	(0.494)	(0.500)	(0.496)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Summary Statistics (Sample of Job Holders at Age 49)

	REBP		
	no rebp	rebp until 1993	Total
Age	49.83	49.83	49.83
	(0.0684)	(0.0683)	(0.0684)
White Collar	0.458	0.352	0.433
	(0.498)	(0.478)	(0.496)
Experience	21.89	22.72	22.08
	(5.450)	(4.861)	(5.331)
Tenure	11.30	11.75	11.40
	(7.332)	(7.010)	(7.261)
Earnings	28195.7	27716.5	28084.8
	(8415.5)	(7628.8)	(8242.5)
Ln Earnings	10.19	10.19	10.19
	(0.346)	(0.306)	(0.337)
Local Unemp. Rate	0.0259	0.0456	0.0305
	(0.0256)	(0.0394)	(0.0306)
Local URate (50+)	0.0466	0.159	0.0726
	(0.0501)	(0.146)	(0.0956)
Firm Size	347.9	920.2	480.4
	(745.6)	(1741.4)	(1089.7)
Emp. Growth Firm	0.215	0.205	0.212
	(2.664)	(4.524)	(3.192)
Emp. Growth Ind.	0.0319	0.00653	0.0260
	(0.0807)	(0.0749)	(0.0801)
1(Growing Firm)	0.512	0.442	0.496
	(0.500)	(0.497)	(0.500)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Difference-in-Differences Effects of the REBP on Outcomes of Initially Employed Workers

	(1) Separation	(2) Employment (Months)	(3) Unemployment (Months)
REBP Eligibility	0.114***	-13.00***	7.233***
	(0.0131)	(1.726)	(0.930)
REBP Region	0.0363**	-1.349**	0.612
	(0.0172)	(0.673)	(0.456)
Constant	0.517***	297.4***	17.35***
	(0.0117)	(2.078)	(1.143)
Observations	481,294	481,294	481,294 4
R-squared	0.143	0.340	0.022
Clusters	71,471	71,471	71,471

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The table reports results of the econometric specification in 33. $REBP$ captures the effect of REBP-eligibility on the outcomes listed in columns (1) through (4) on a sample of workers employed at the onset of the reform (1988 Q.2). The regression specification includes region and cohort effects. *Separation* denotes an indicator function that is 1 if a worker is not employed by their 1988-employer by the end of the reform (1993 Q.3). $Employment\,(Months)$, $Unemployment\,(Months)$ and $Continuous\,Employment\,(Months)$ denote the months of employment, unemployment, and continuous employment with the initial employer between 1988 Q.2 and 1993 Q.3. Standard errors clustered at the firm level are reported in parentheses. Levels of significance: * 10%, ** 5%, and *** 1%.

Table 4: Complier Characteristics: Means

Attribute	Complier	Never-Sep.	p-v. (C-NS)	Always-Sep.	p-v. (C-AS)	SD
White Collar	0.129	0.472	<0.001	0.337	<0.001	0.484
	(0.02424)	(0.00388)		(0.00448)		
Emp. Growth Ind.	-0.0675	0.0696	<0.001	0.0287	<0.001	0.140
	(0.00702)	(0.00095)		(0.00123)		
Emp. Growth Firm	-0.192	0.0815	<0.001	-0.00820	<0.001	0.311
	(0.01867)	(0.00236)		(0.00377)		
1(Gr. Firm)	0.123	0.536	<0.001	0.355	<0.001	0.492
	(0.02328)	(0.00383)		(0.00468)		
Earnings	27498	28252	0.100	26856	0.820	6983
	(354.50)	(52.261)		(72.343)		
Worker FE	0.0384	0.136	0.0400	0.0658	<0.001	0.259
	(0.01150)	(0.00201)		(0.00222)		
Firm FE	0.0119	0.00585	0.0700	0.0362	0.580	0.198
	(0.01272)	(0.00191)		(0.00237)		
Tenure	13.23	11.56	<0.001	10.66	<0.001	5.812
	(0.29653)	(0.03915)		(0.05256)		
Share Sickness in Ind.	0.0247	0.0230	<0.001	0.0226	<0.001	0.00594
	(0.00026)	(0.00004)		(0.00006)		

Note: This table reports characteristics of compliers, always-separators, never-separators as well as the three groups combined (overall) based on the methodology in Section 5.1 and the methodology in Abadie (2003). Compliers are those workers who are employed in 1988 and whose job would have survived in the absence of the REBP reform. For each of the variables and groups, the table reports means as well as standard errors (in parentheses) based on 100 bootstrap replications. See the Section 5 for more details on how the variables are constructed. See Section 5.1 for the methodology underlying the decomposition into the groups.

Table 5: Separation Probability and Leave-Out-Mean Industry Growth: Calendar Year Intervals

	Separation Probability				
	1993–1994	1993–1995	1993–1996	1993–1997	1993–1998
A. 4-digit NACE 08					
Industry Growth x Previous REBP Eligibility	0.621***	0.317***	0.272***	0.215***	0.154***
	(0.206)	(0.117)	(0.087)	(0.062)	(0.055)
Benchmark Size Effect	-0.401	-0.198	-0.211	-0.210	-0.150
Adjusted R^2	0.067	0.085	0.110	0.134	0.161
Observations	284321	284321	284321	284321	284321
No of Clusters	511	511	511	511	511
Mean of Industry Growth	0.015	0.027	0.035	0.049	0.059
Sd of Industry Growth	0.038	0.070	0.101	0.136	0.163
B. 2-digit NACE 08					
Industry Growth x Previous REBP Eligibility	0.439***	0.230	0.255**	0.238**	0.195***
	(0.160)	(0.195)	(0.127)	(0.092)	(0.073)
Benchmark Size Effect	-0.362	-0.270	-0.350	-0.269	-0.174
Adjusted R^2	0.051	0.066	0.090	0.115	0.141
Observations	284841	284841	284841	284841	284841
No of Clusters	87	87	87	87	87
Mean of Industry Growth	0.015	0.028	0.037	0.050	0.059
Sd of Industry Growth	0.021	0.038	0.059	0.080	0.103
Mean of Dependant Variable	0.145	0.266	0.374	0.468	0.556
Sd of Dependant Variable	0.352	0.442	0.484	0.499	0.497
Sample	No Steel	No Steel	No Steel	No Steel	No Steel
Industry x Region FE	Yes	Yes	Yes	Yes	Yes
Industry x Cohort FE	Yes	Yes	Yes	Yes	Yes
Clustering	NACE 08	NACE 08	NACE 08	NACE 08	NACE 08

Note: Winsoring at 5%, 1928–1948 cohorts.

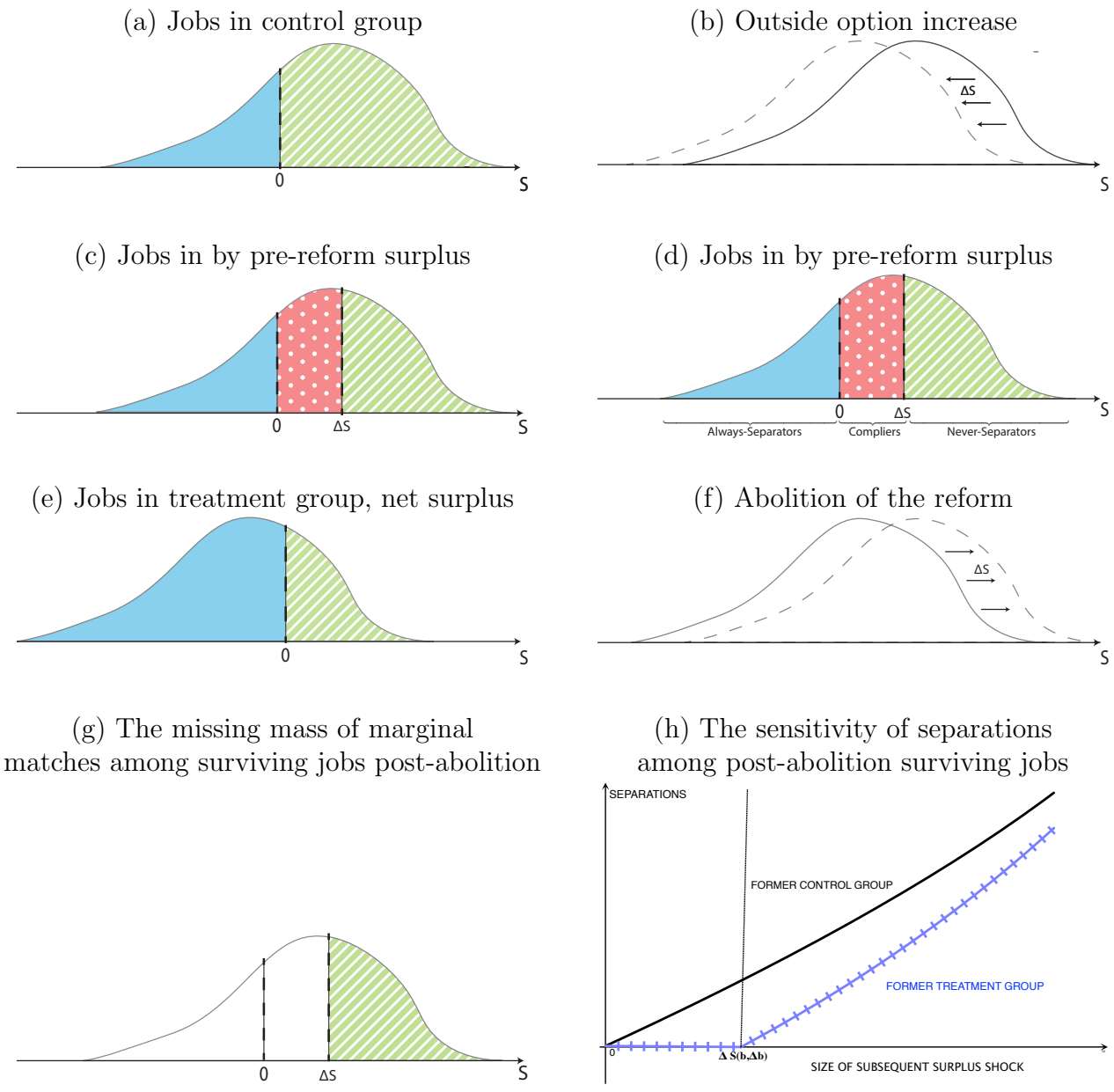
Table 6: Separation Probability and Leave-Out-Mean Industry Growth: Age Intervals

	Separation Probability				
	Until 53	Until 54	Until 55	Until 56	Until 57
A. 4-digit NACE 08					
Industry Growth x Previous REBP Eligibility	0.106**	0.072	0.037	0.008	0.018
	(0.047)	(0.048)	(0.047)	(0.042)	(0.040)
Benchmark Size Effect	-0.101	-0.147	-0.182	-0.113	-0.125
Adjusted R^2	0.142	0.140	0.140	0.133	0.167
Observations	176698	204439	229223	243495	254066
No of Clusters	497	500	506	508	508
Mean of Industry Growth	0.060	0.060	0.059	0.059	0.059
Sd of Industry Growth	0.165	0.165	0.165	0.165	0.164
B. 2-digit NACE 08					
Industry Growth x Previous REBP Eligibility	0.157***	0.126**	0.053	0.009	-0.057
	(0.049)	(0.063)	(0.066)	(0.055)	(0.052)
Benchmark Size Effect	-0.207	-0.282	-0.310	-0.102	-0.093
Adjusted R^2	0.111	0.111	0.115	0.116	0.151
Observations	177123	204901	229706	243991	254568
No of Clusters	86	86	87	87	87
Mean of Industry Growth	0.059	0.059	0.059	0.059	0.059
Sd of Industry Growth	0.102	0.103	0.103	0.103	0.103
Mean of Dependant Variable	0.260	0.297	0.366	0.563	0.649
Sd of Dependant Variable	0.439	0.457	0.482	0.496	0.477
Sample	No Steel	No Steel	No Steel	No Steel	No Steel
Industry x Region FE	Yes	Yes	Yes	Yes	Yes
Industry x Cohort FE	Yes	Yes	Yes	Yes	Yes
Clustering	NACE 08	NACE 08	NACE 08	NACE 08	NACE 08

Note: Winsoring at 5%, 1928–1948 cohorts.

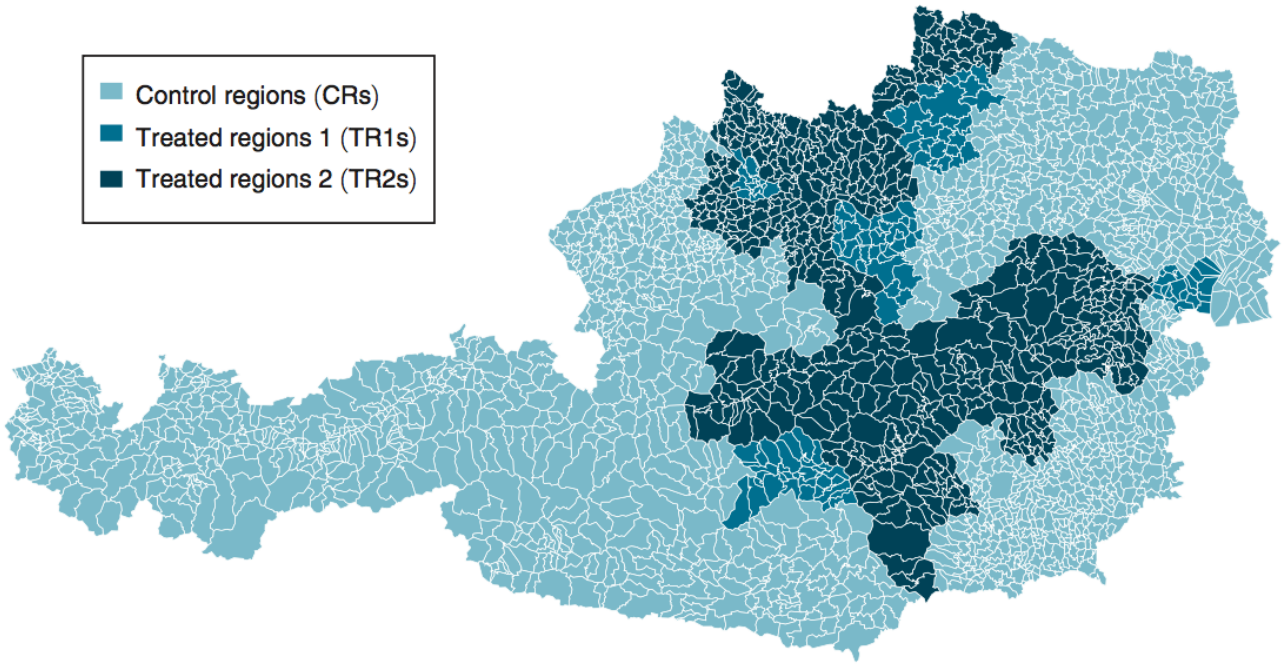
9 Figures

Figure 1: Conceptual Framework: The Consequences of Outside Options Increases



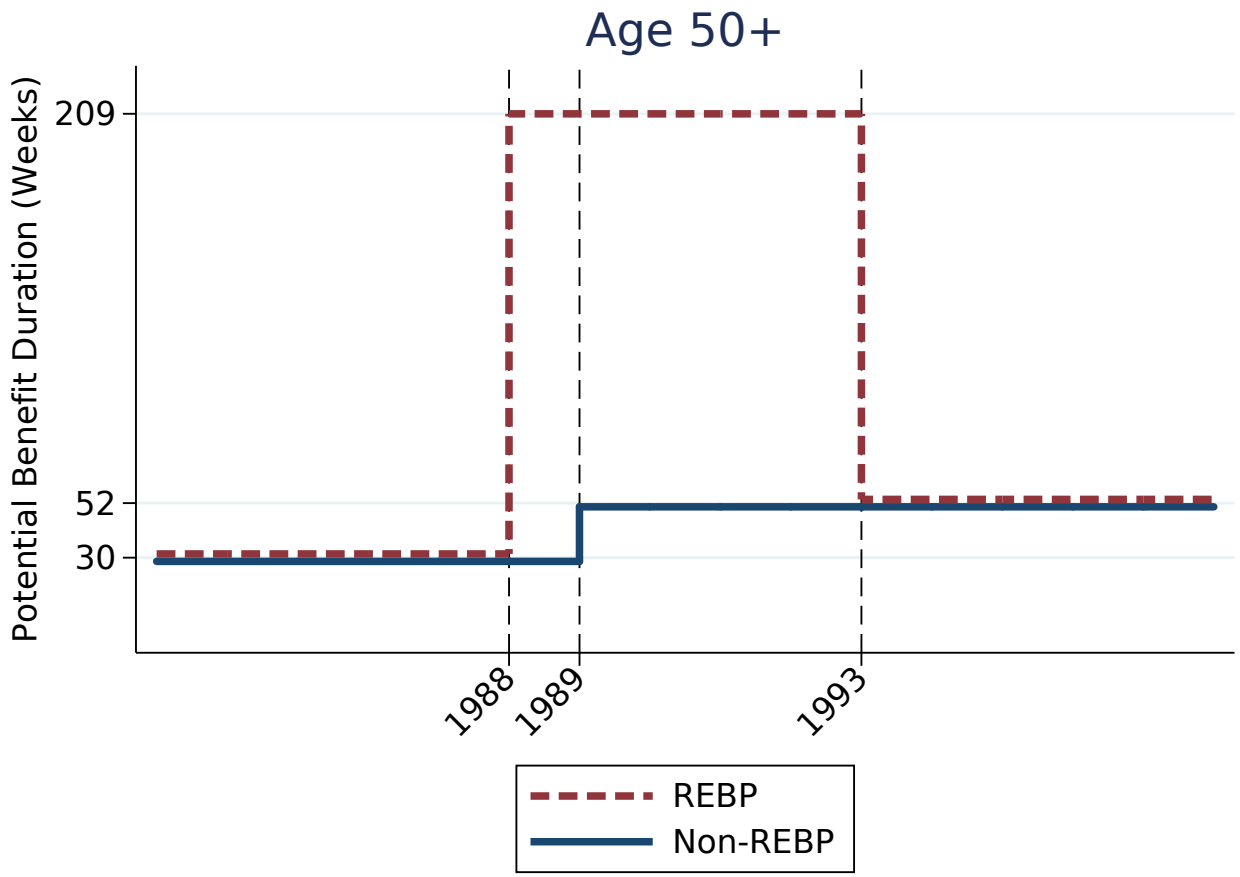
Note: Figure 1 illustrates the conceptual framework presented in Section 2. Panel (a) plots a distribution of job surplus S, where negative-surplus realizations entail separations. Panel (b) illustrates the left-ward shift in surplus induced by the outside option increase. Panel (c) and (e) shows surplus cutoff instrumented regime $b_0 + \Delta b$, expressed in gross-of-instrument, pre-reform surplus S, Panel (c) partitions the jobs into Always-Separators, Compliers and Never-Separators. Panel (f) illustrates the removal of the instrument (reform abolition). Panel (g) shows the truncated post-abolition surplus distribution and the missing mass of marginal matches in the formerly-treated group. Panel (h) shows the post-abolition separation rate as a function of the subsequent surplus shocks for the control and formerly-treated group, under the simplifying assumption of no interim (aggregate or idiosyncratic) shifts in surplus.

Figure 2: Map of REBP Treatment and Control Regions



Note: This map of Austrian municipalities depicts the REBP regions. REBP was introduced in TR1 and TR2 in 1988. TR2, REBP was in place until the end of 1991. In TR2, REBP was in place until July 31, 1993.

Figure 3: Timeline of Potential Benefit Duration in Age Group 50+ and 40-49



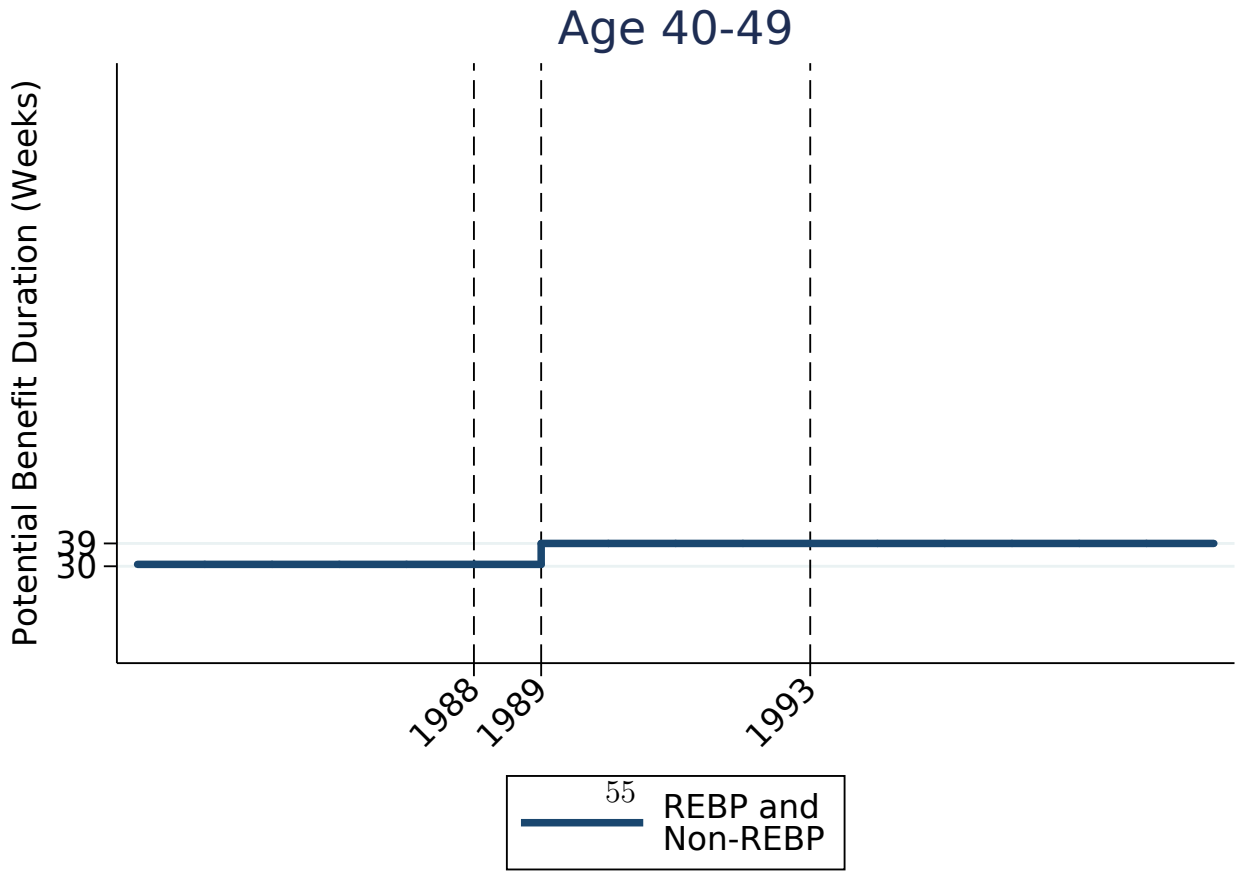
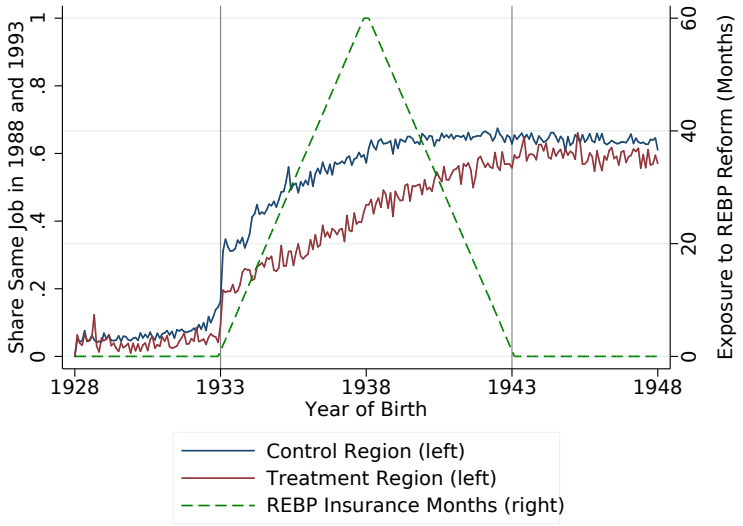
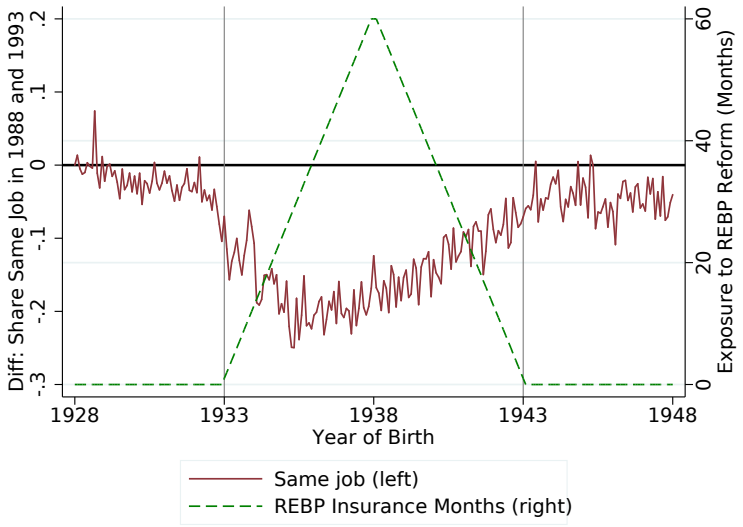


Figure 4: Benefit Extensions and Separations – Share of Workers With Same Job in 1988 and 1993

(a) REBP and Control Region



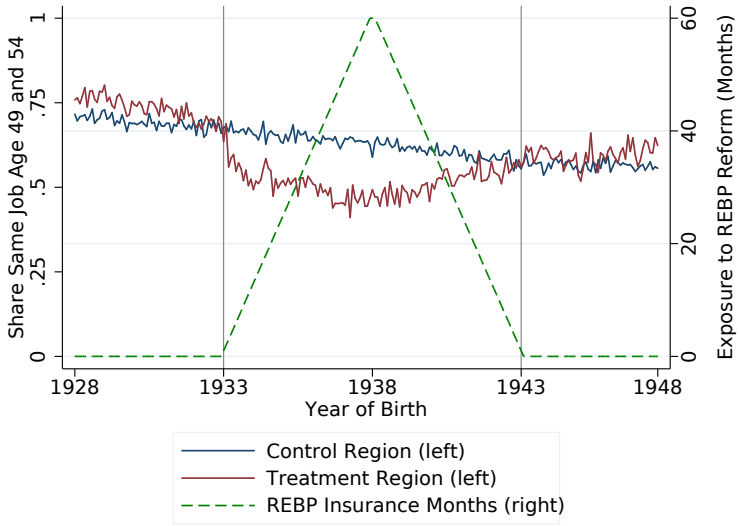
(b) Difference (REBP - Control Region)



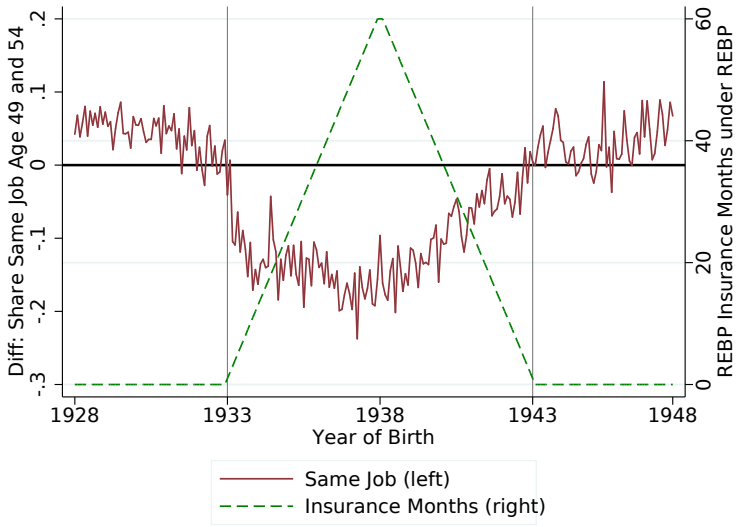
Note: The figures show the share of workers who hold the same job in Q.3 of 1993 as in Q.2 of 1988 across birth cohorts. The sample is confined to workers who are employed in Q.2 of 1988 ($N = 537,899$). Holding the same job is defined as being with the same employer. Panel (a) shows the share by month of birth in the REBP and in the control region, respectively, indicated on the left y-axis. Panel (b) shows the difference between the REBP and the control region by cohort, indicated on the left y-axis. The dashed green line, associated with the right y-axes, shows the number of months that REBP-region workers in each cohort are eligible for extended unemployment benefits under the REBP, i.e. benefits of 4 years rather than 1 year, if they enter unemployment at age 50 or above. Cohorts born before 1933 or after 1943 are effectively not covered by the policy.

Figure 5: Benefit Extensions and Separations – Share of Workers With Same Job At Age 49 and Age 54

(a) REBP and Control Region

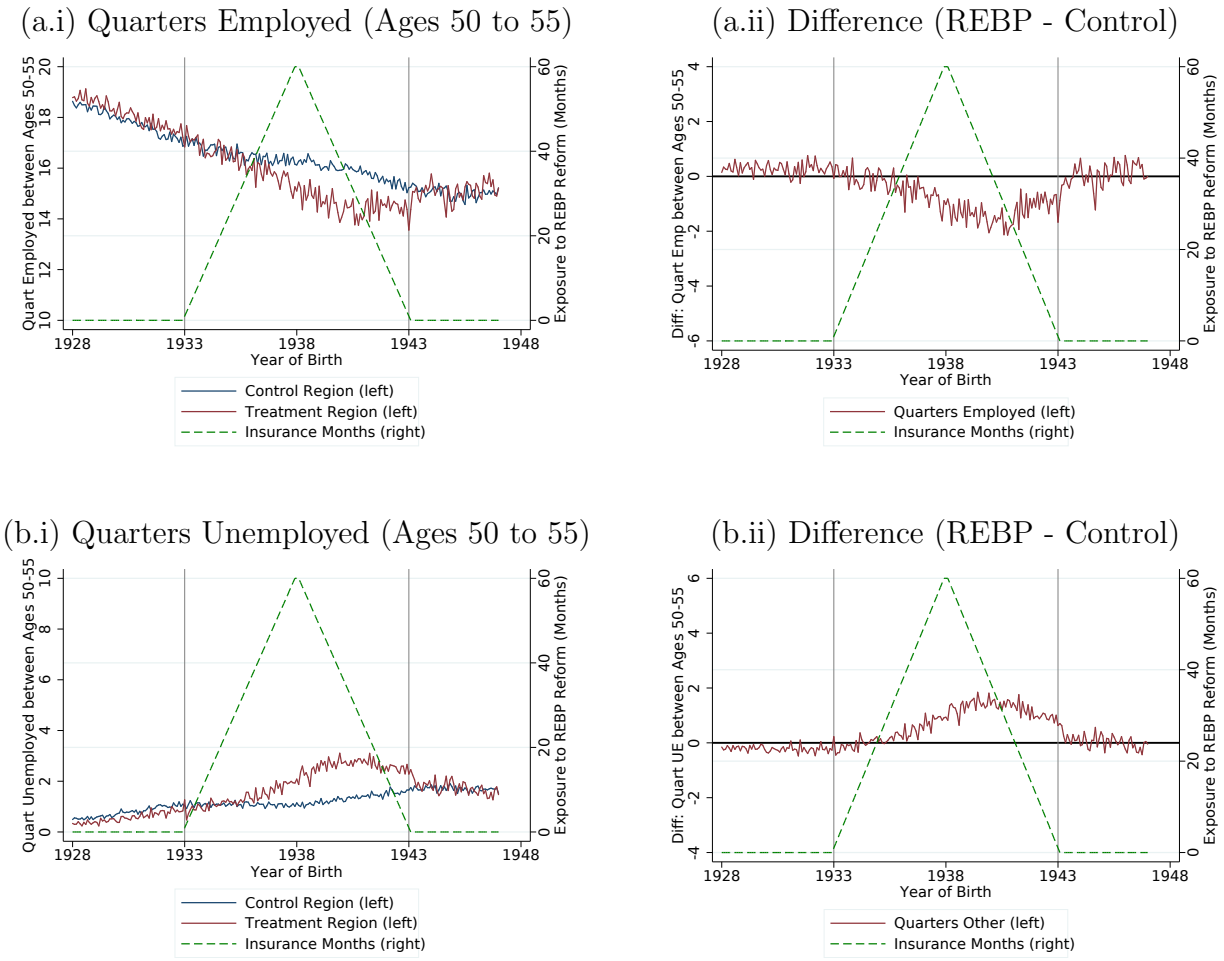


(b) Difference (REBP - Control Region)



Note: The figures show the share of workers who hold the same job at age 49 as at age 54 across birth cohorts. The sample is confined to workers who are employed at age 49 ($N = 537,899$). Holding the same job is defined as being with the same employer. Panel (a) shows the share by month of birth in the REBP and in the control region, respectively, indicated on the left y-axis. Panel (b) shows the difference between the REBP and the control region by cohort, indicated on the left y-axis. The dashed green line, associated with the right y-axes, shows the number of months that REBP-region workers in each cohort are eligible for extended unemployment benefits under the REBP, i.e. benefits of 4 years rather than 1 year, if they enter unemployment at age 50 or above. Cohorts born before 1933 or after 1943 are effectively not covered by the policy.

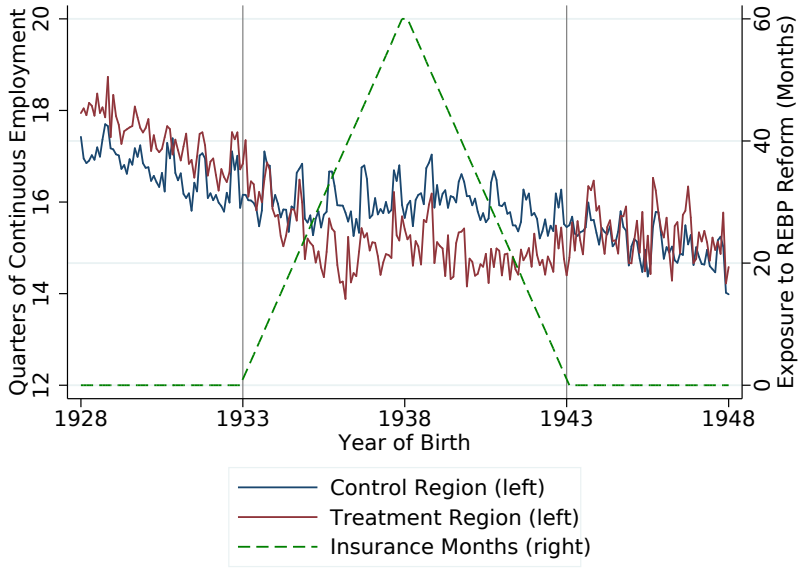
Figure 6: Benefit Extensions and Employment Outcomes



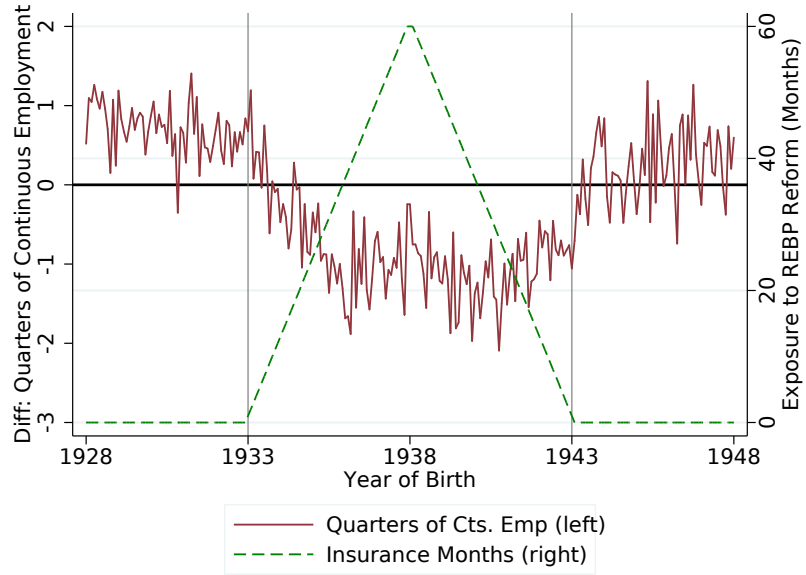
Note: The figures show the quarters between the ages of 50 and 55 in employment and unemployment across birth cohorts. The sample is confined to workers who are employed at age 49 ($N = 537,899$). The dashed green line, associated with the right y-axes, shows the number of months that REBP-region workers in each cohort are eligible for extended unemployment benefits under the REBP, i.e. 4 rather than 1 year, if they enter unemployment at age 50 or above.

Figure 7: Benefit Extensions and Employment Outcomes: Overall Employment and Continuous Employment With Initial Employer

(a) Continuous Employment With Initial Employer

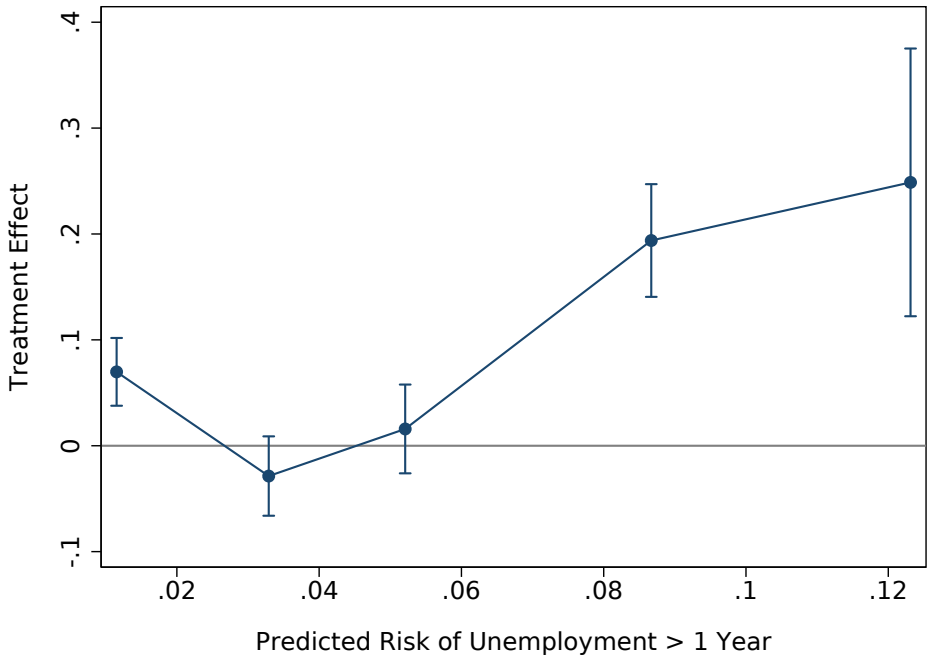


(b) Continuous Employment With Initial Employer (Difference REBP - Control Regions)



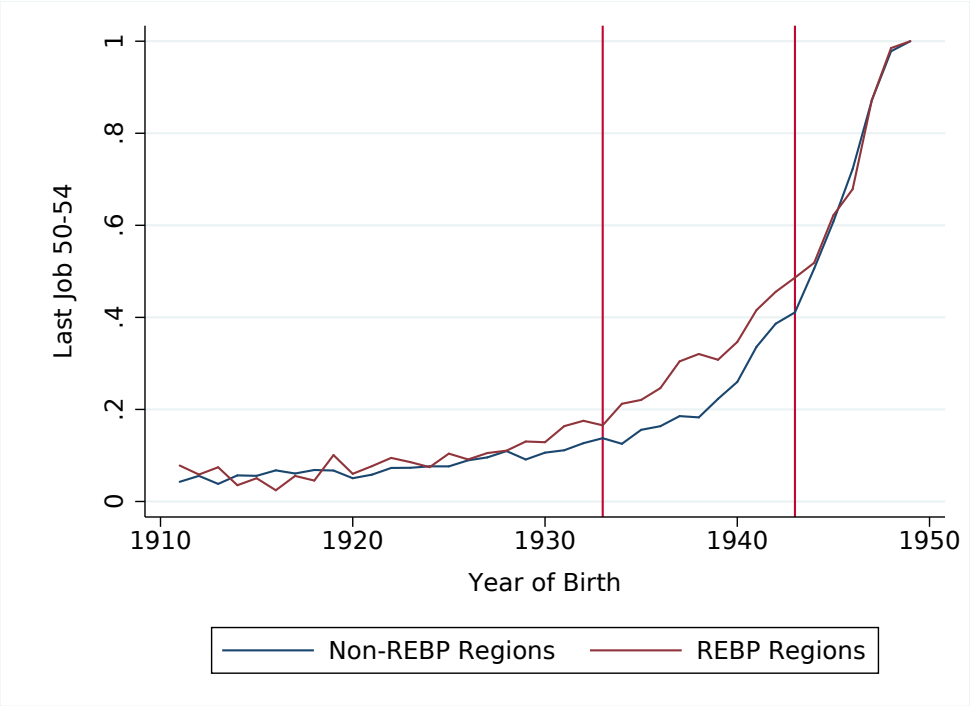
Note: The figures show difference in employment outcomes across birth cohorts. The outcome variable shown on the y-axis are the quarters of continuous employment with the initial employer at age 49. The sample is confined to workers who are employed at age 49 ($N = 537,899$). The dashed green line, associated with the right y-axes, shows the number of months that REBP-region workers in each cohort are eligible for extended unemployment benefits under the REBP, i.e. 4 rather than 1 year, if they enter unemployment at age 50 or above.

Figure 8



Note: The figure shows estimates for the treatment effect of the REBP extension on separations for workers sorted into quintiles by their risk of long-term unemployment. The estimates are obtained in a fully interacted difference-in-differences regression taking out the baseline effects of being in an REBP-eligible cohort and residing in an REBP region both interacted with indicator variables for each risk quintile. Risk of long-term unemployment, we estimate a model for workers observed in 1982, regressing an indicator for having a pre-reform unemployment spell of one year or longer on a rich set of covariates measured in 1982: industry fixed effects, an indicator for working in a white collar occupation, the local unemployment rate, and third degree polynomials in tenure and experience. We use the coefficients to predict the risk for 1988 workers, and split them into risk quintiles.

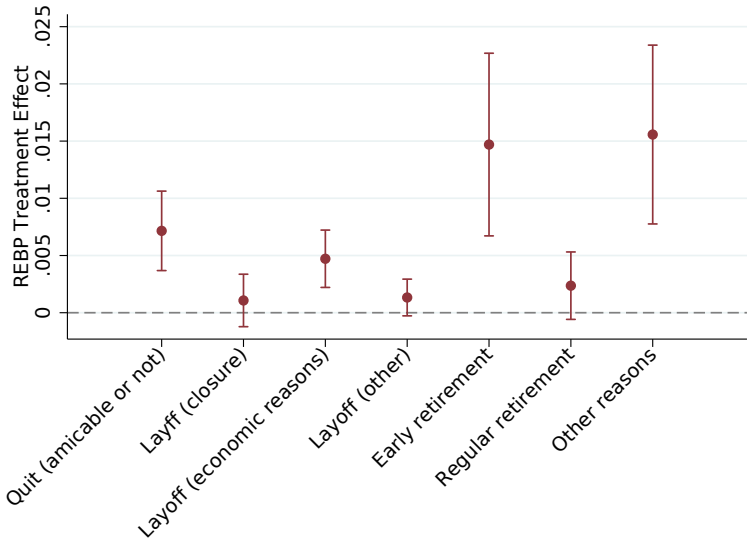
Figure 9: Microcensus: Effect of REBP on Separations between Age 50 and 54



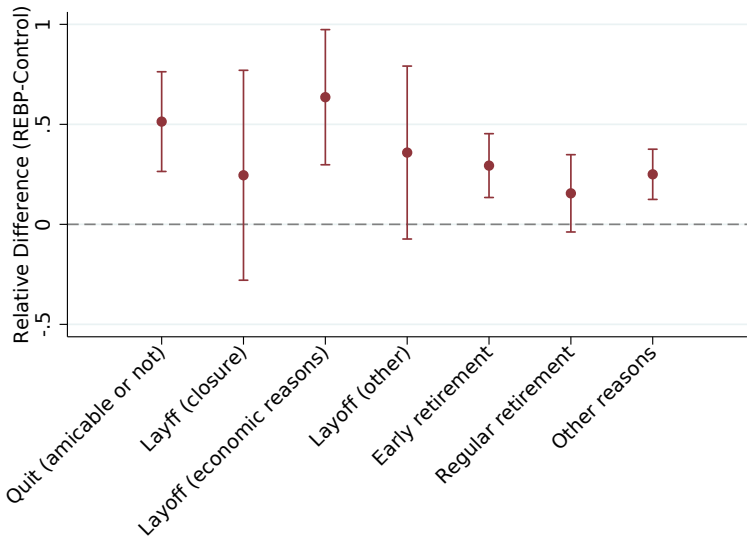
Note: The figure plots data based on the Austrian Microcensus. Across cohorts and regions, it plots an indicator variable for whether a respondent's last employment spell ended in the time when a respondent was between 50 and 54 years old. The two red vertical lines denote the oldest and youngest cohorts, respectively, who were eligible for the REBP program between 1988 and 1993 and were aged between 50 and 54 at some point in that time range.

Figure 10: Microcensus: Effect of REBP on Type of Separations between Age 50 and 54.

(a) Absolute Effects (Percentage Points).

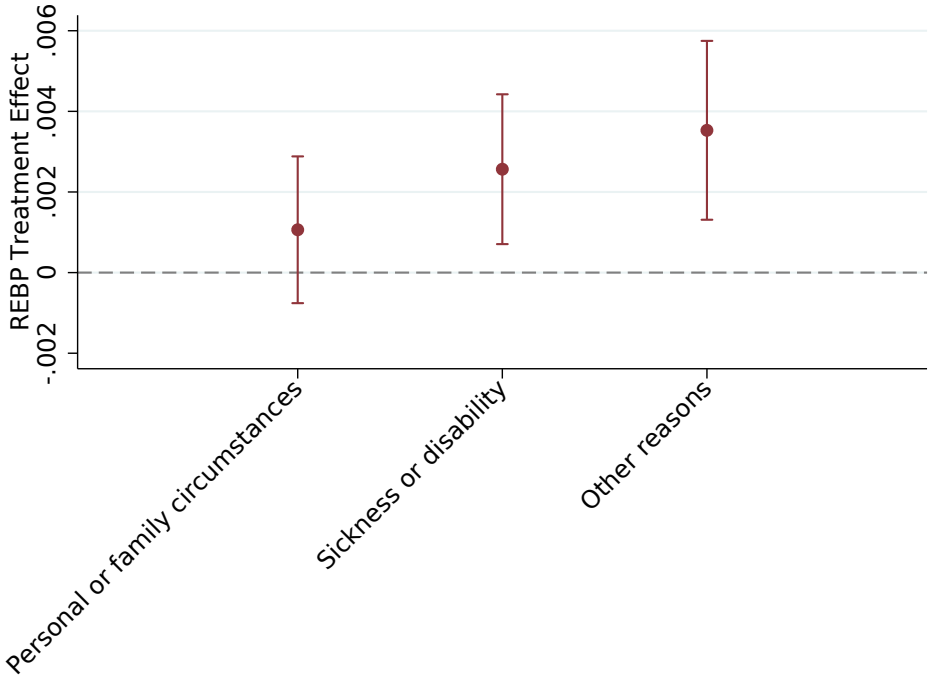


(b) Relative Effects (Percent) Relative to Control Mean.



Note: The figure plots data based on the Austrian Microcensus and shows a treatment effect for the REBP program in a difference-in-difference specification controlling for region and cohort fixed effects. The treatment effect is relative to the base effect (the control mean). The four outcome variables that are considered are indicators that equal one if a respondent's last employment spell ended between the ages of 50 and 54 and was either a one-sided or amicable quit, a layoff due to establishment closure, a layoff due to economic reasons, a layoff for other reasons, early retirement, regular retirement, or for other reasons. Each estimate stems from a separate regression based on 180,137 observations. The red vertical lines indicate 95% confidence intervals based on standard errors based on 88 clusters at the administrative region level.

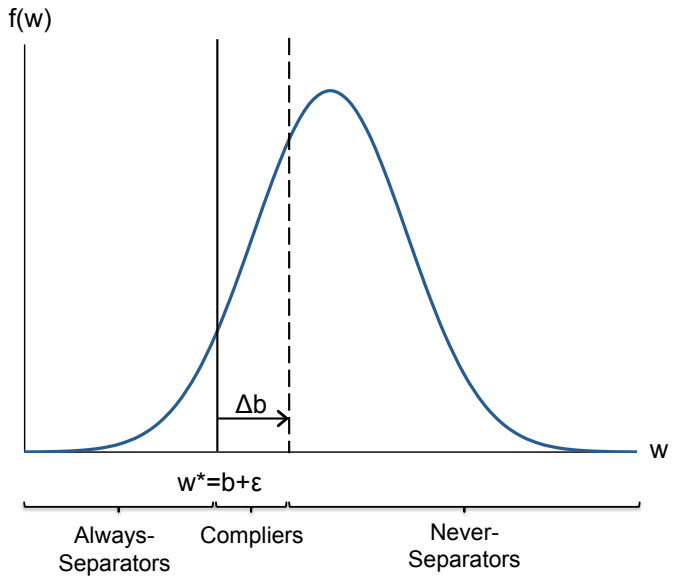
Figure 11: Microcensus: Effect of REBP on Types of Quits and Mutually Agreed-Upon Separations between Age 50 and 54



Note: The figure plots data based on the Austrian Microcensus and shows a treatment effect for the REBP program in a difference-in-difference specification controlling for region and cohort fixed effects. The three outcome variables that are considered are indicators that equal one if a respondent's last employment spell ended between the ages of 50 and 54 and was a quit and was either (i) due to personal or family circumstances, (ii) sickness or disability, or (iii) other reasons. Each estimate stems from a separate regression based on 180,137 observations. The red vertical lines indicate 95% confidence intervals based on standard errors based on 88 clusters at the administrative region level.

Figure 12: Partitioning the 1988 Job Holders into Never-Separators, Compliers and Always-Separators.

(a) McCall: Homogeneous Workers



(b) Distribution of 1988 Job Holders: Compliers, Always Separators, Never Separators

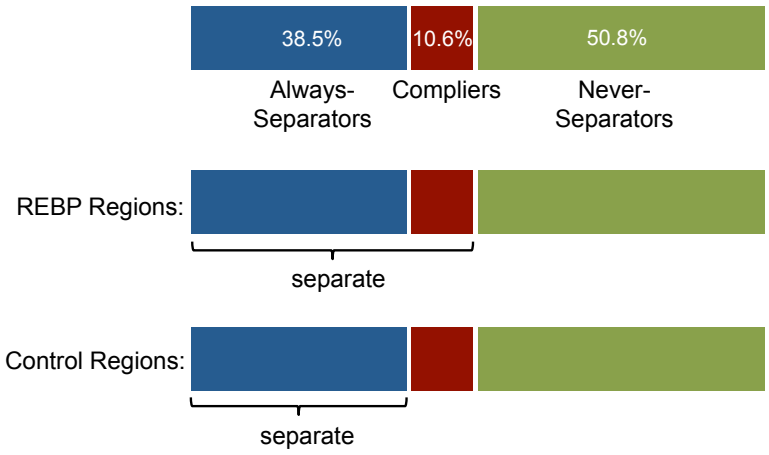
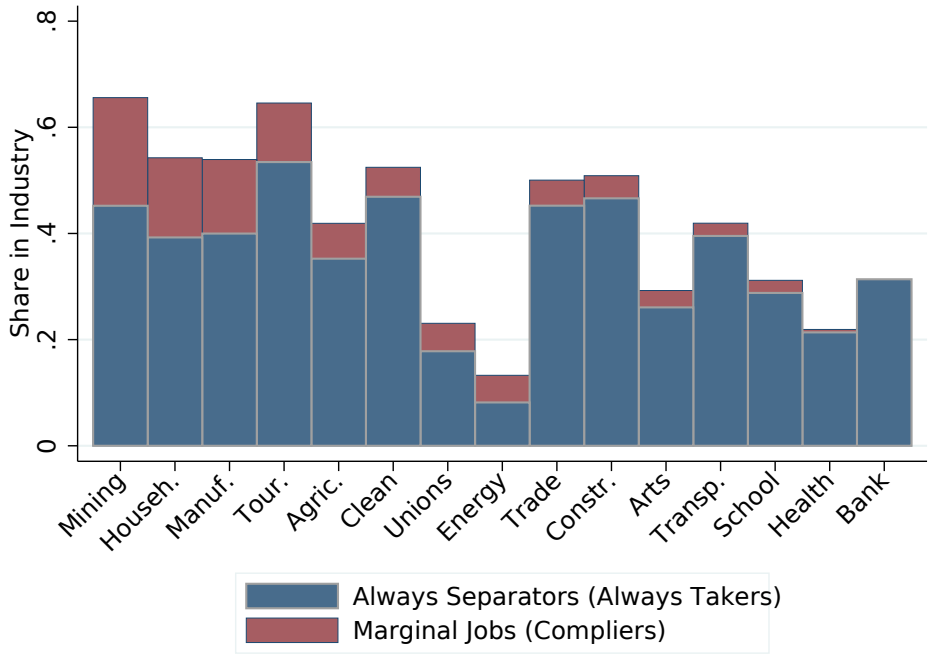


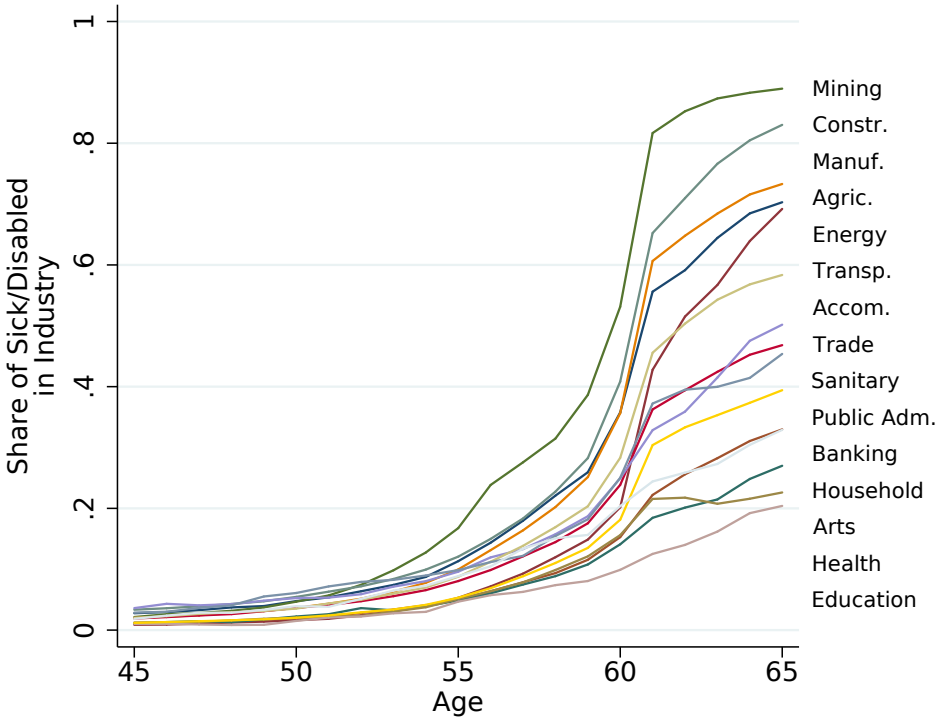
Figure 13: Share of Compliers By Industry



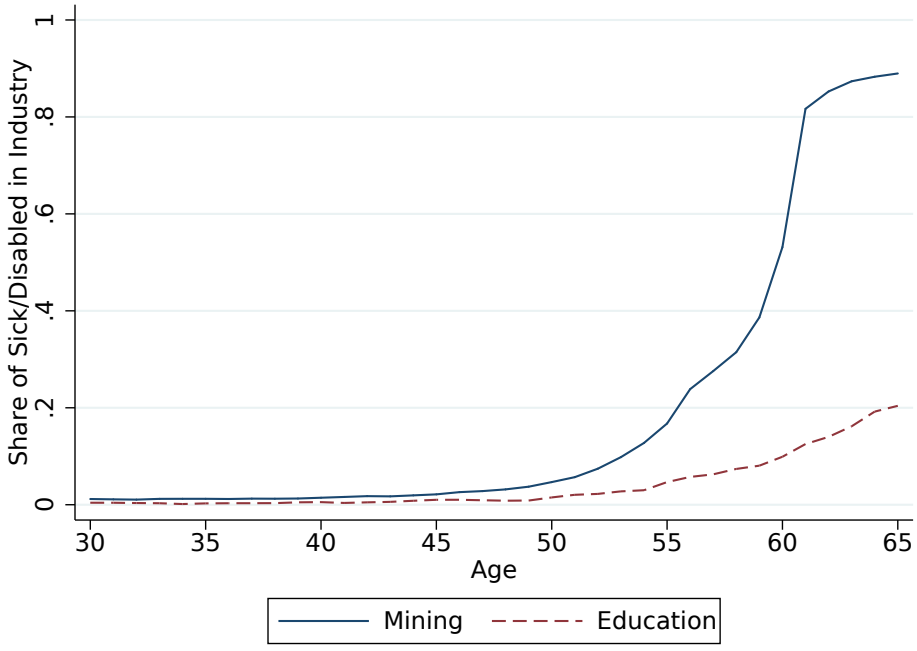
Note: The figure plots the share of always-separators and marginal jobs by industry. Always-separators are defined as workers whose job would have led to a separation between 1988 and 1993 even in the absence of the reform. Marginal jobs or compliers are defined as jobs that would survive between 1988 and 1993 in the absence of the REBP reform but resulted in a separation under REBP.

Figure 14: Lifecycle Sickness Incidence By Industry

(a) All Industries

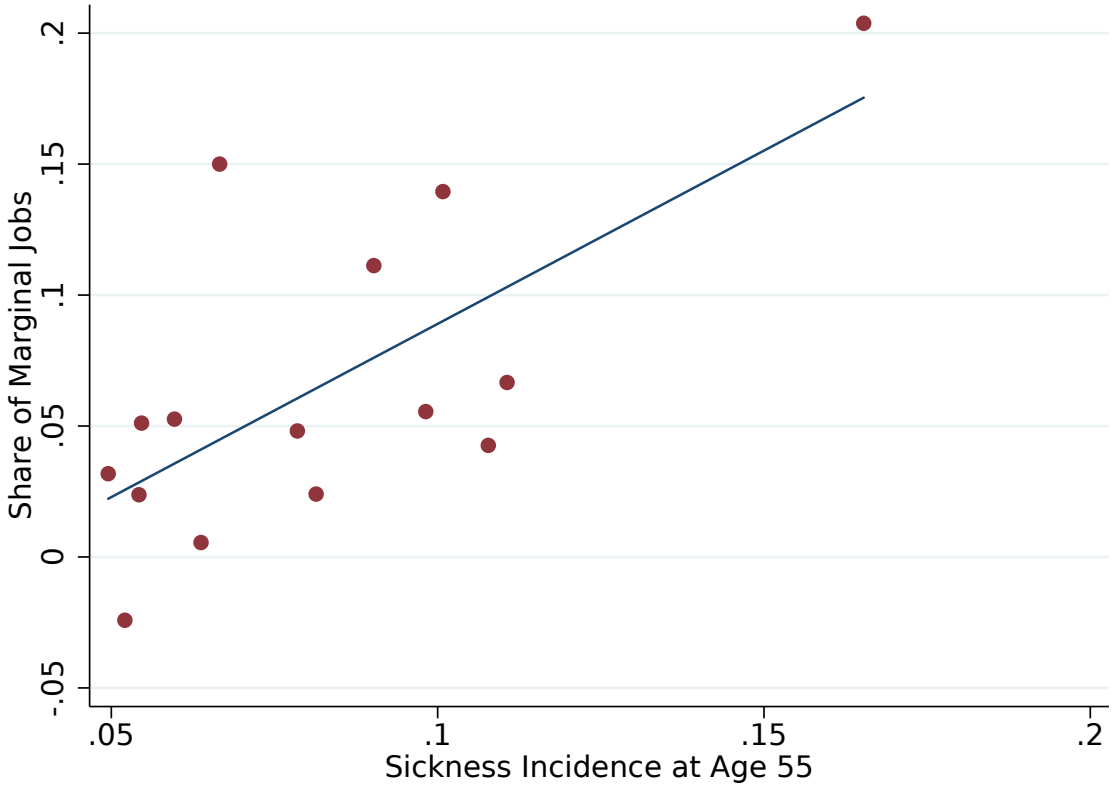


(b) Mining and Education



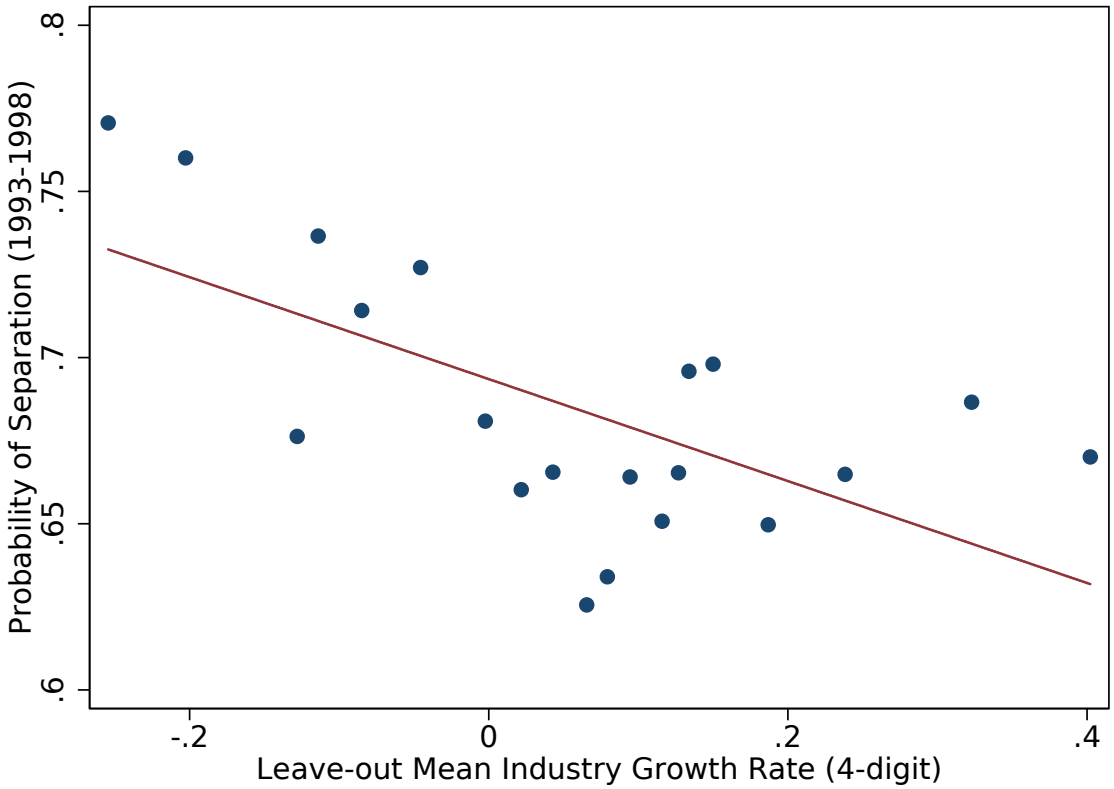
Note: The figures plot the incidence of sickness and disability spells over the lifecycle by industry based on quarterly data. Each sickness or disability spell gets assigned to the industry of a worker's last employer.

Figure 15: Share of Compliers Against Share Sick at Age 55



Note: This graph compares the industry share of compliers against the share of workers with a sickness or disability spell by age 55.

Figure 16: Separations and Industry Shocks in Control Group



Note: The figure shows the relationship between a separation from the 1993-employer by 1998, plotted on the y-axis, and industry growth, plotted on the x-axis. The sample is confined to workers who reside in the control region and are in cohorts that would have been eligible for extended benefits in the REBP region. Moreover, the sample only includes workers whose 1988 job had survived the REBP reform. Industry growth rates are calculated as leave-out means, leaving out a worker's own firm, of employment in the firm's four-digit industry.

Appendix

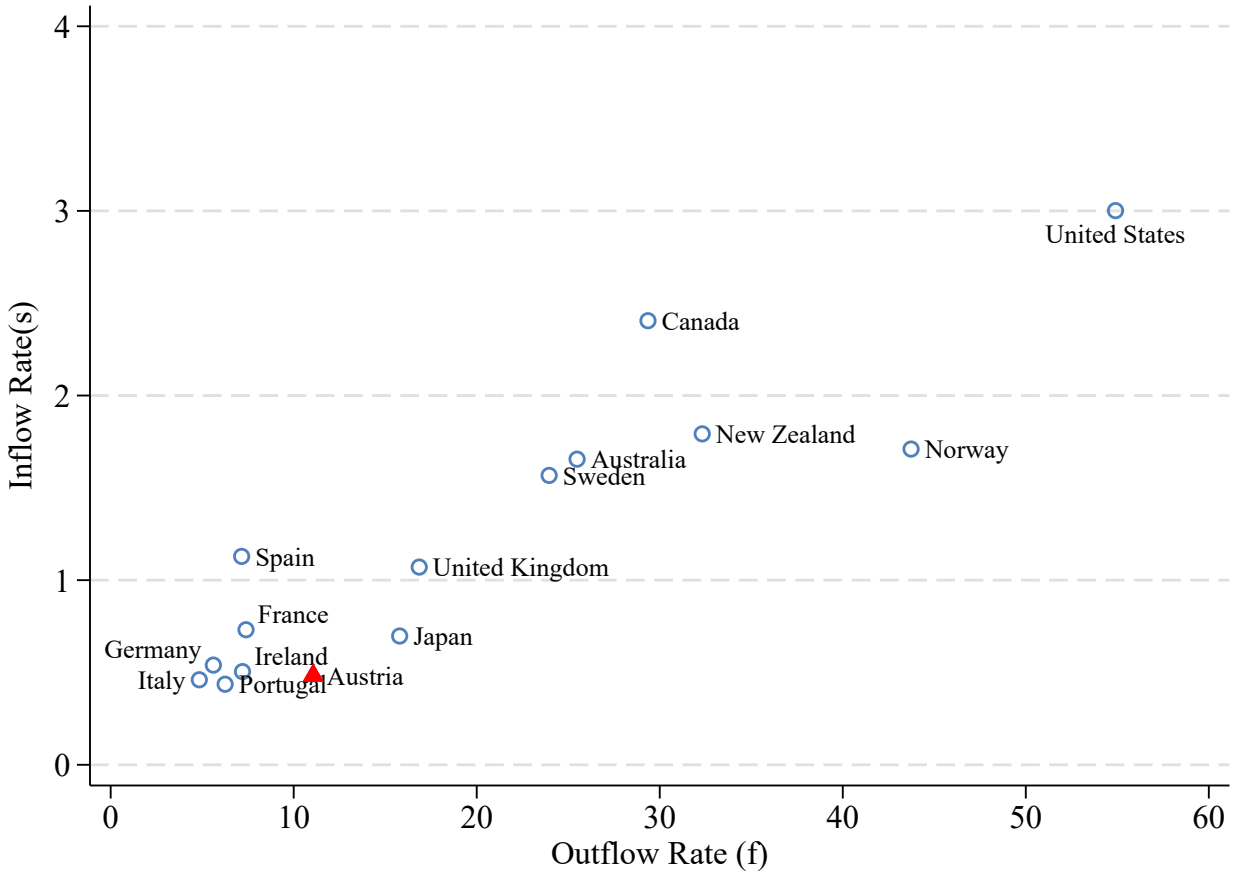
A Institutional Details

A.1 Sources and Institutional Review: Separations in Austria between 1988 and 1998

A.2 The Abolition of the Reform in 1993: Newspaper Analysis

A.3 Appendix Figures

Figure 17: Inflow and Outflow Rates in Austria and other OECD Countries



Note: The figure shows inflow and outflow rates into and out of unemployment in Austria and other OECD countries for the time period from 1994 to 2009. It extends the analysis in Elsby et al. (2013) to include Austria.

B An Equilibrium Model with Endogenous Separations

The fuller model incorporates dynamics explicitly, as well as equilibrium considerations from labor demand. Jobs are heterogeneous in a McCall setup, but heterogeneity is with respect to match quality only. Search is random on both sides. Wages are Nash bargained. Next, we describe set of attributes $\mathbf{x}$, considering worker, firm and match-specific attributes. Attributes are stable within workers and firms, but match attributes are stochastic and can be redrawn.

Worker attributes. The employed worker consumes wage w and job amenity a. Her outside option, as an unemployed job seeker, is unemployment benefit b, which includes amenities such as unemployment insurance but also the net value of leisure over labor. ξ captures workers' net value of non-employment (disutility of labor).[37]

Firm attributes. ε_i captures the idiosyncratic value of the job to the firm v_f captures the value of the vacant job. ϕ_f captures potential firing costs in form of regulations or reputational costs. The firm reaps profits p_{fw} from the match.

Job attributes. Jobs are drawn from a distribution $G(a, p)$, where a denotes the amenity and p denotes the productivity. For the purposes of worker search, we suppose a relationship between productivity, amenity and wages such that jobs can be described by the worker-facing payoff $a+w$. For a given linear wage rule such as the to-be-derived Nash wage, we can therefore transform $G(a, p)$ into $\tilde{G}(a + w)$, and similarly for the firm's payoff distribution $\hat{G}(p - w)$. Let $\overline{a + p}$ denote the reservation match quality – productivity plus amenity – to be derived explicitly below.

Idiosyncratic shocks. Idiosyncratic match quality shifts with probability λ, forcing the match to take a new draw from the match quality distribution over a and p. δ describes a reduced-form shock that forces a separation. $(1-\delta)\lambda$ is therefore the probability that the match does not exogenously separate and receives a new match quality draw. Fraction $(1 - \delta)(1 - \lambda)$ of matches are not exogenenously separated and do not see their match quality change.

[37] With heterogeneity with respect to ξ, firms would take into account the distribution of searchers w.r.t. that parameter in the ZPC. With random search, this is only relevant through the final $f/q = \theta$ in the wage. Directed search w.r.t. ξ would replicate submarkets mirroring this aggregate economy.

Value function: unemployed job seekers. looking for jobs characterized by (w, a) – later on, we will derive w from the fundamentals (productivity and amenity, and bargaining terms):

$$U = b + \xi + \beta(1-f)U + \beta f \cdot \int_{-\infty}^{\infty} \max\{W(a+w), U\} \mathrm{d}\tilde{G}(a+w) \tag{44}$$

$$= b + \xi + \beta U + \beta f \cdot \int_{\overline{a+w}}^{\infty} (W(a+w) - U)\, \mathrm{d}\tilde{G}(a+w) \tag{45}$$

Value function: employed worker in a job characterized by (w, a):

$$W(a,w) = a + w + \beta(1-\delta)(1-\lambda)W(a,w) + \beta(1-\delta)\lambda \cdot \int_{-\infty}^{\infty} \max\{W(a+w), U\} d\tilde{G}(a+w) + \beta\delta U \tag{46}$$

$$= a + w + \beta(1-\delta)(1-\lambda)(W(a,w) - U) + \beta U \tag{47}$$

$$+ \beta(1-\delta)\lambda \int_{\overline{a+w}}^{\infty} (W(a+w) - U)\, d\tilde{G}(a+w) \tag{48}$$

Worker's value of reservation job. It's useful to consider the following value difference:

$$W(a,w) - U = a + w - (b+\xi) + \beta(1-\delta)(1-\lambda)\,(W(a,w) - U) \tag{49}$$

$$+ \beta((1-\delta)\lambda - f) \cdot \int_{\overline{a+w}}^{\infty} (W(a+w) - U)\, d\tilde{G}(a+w) \tag{50}$$

Worker's reservation match quality is defined by set of (a, w) jobs such that $W(a,w) - U = 0$:

$$\overline{a+w} = (b+\xi) + \beta(f - (1-\delta)\lambda) \cdot \int_{\overline{a+w}}^{\infty} (W(a+w) - U)\, d\tilde{G}(a+w) \tag{51}$$

The McCall searcher therefore applied a simple reservation wage strategy that equals the flow benefit of being nonemployed, plus an option-value term that increases selectivity that arises from upgrading to a higher quality job. That consideration arises from the opportunity cost of accepting a job in form of future draws, assuming no on the job search.

Note that the reservation-wage job has the following value:

$$W(\overline{a+w}) = \frac{\overline{a+w} + \beta(1-\delta)\lambda \int_{\overline{a+w}}^{\infty} (W(a+w) - U)\, d\tilde{G}(a+w)}{1-\beta} = U \tag{52}$$

Firm's value functions. J is the value of the job, V is the vacant job's value. As in the worker's case, we transform the distribution of match quality fundamentals of jobs $G(a,p)$ into a firm-facing payoff distribution $\hat{G}(p,w)$, where w will be determined later on. In fact, we will

assume $\hat{G}(p-w)$ and presuppose that $J(p,w)$ will depend only on $p-w$ as $J(p-w)$.

$$J(p,w) = p - w + \beta(1-\delta)(1-\lambda)J(p,w) + \beta(1-\delta)\lambda \int_{-\infty}^{\infty} \max\{J(p,w), V\} d\hat{G}(p,w) + \beta\delta V \tag{53}$$

Vacancy value is posting cost k plus filling payoff:

$$V = -k + \beta q \int_{-\infty}^{\infty} \max\{J(p,w), V\} d\hat{G}(p,w) + \beta(1-q)V \tag{54}$$

Free entry in job creation implies $V = 0$:

$$\Rightarrow \frac{k}{q} = \beta \int_{-\infty}^{\infty} \max\{J(p,w), 0\} d\hat{G}(p,w) = \beta \int_{0}^{\infty} J(p,w) d\hat{G}(p,w) \tag{55}$$

Plugging $V = 0$ and this expression into $J(p,w)$:

$$J(p,w) = p - w + \beta(1-\delta)(1-\lambda)J(p,w) + \beta(1-\delta)\lambda \int_{0}^{\infty} J(p,w) d\hat{G}(p,w) \tag{56}$$

$$\Leftrightarrow J(p,w) = \frac{p - w + (1-\delta)\lambda\frac{k}{q}}{1 - \beta(1-\delta)(1-\lambda)} \tag{57}$$

Wage bargaining. Upon matching, the parties bargain over the formation of the match and the surplus split, where we assume period-by-period Nash bargaining with a flow wage:

$$\max_{w} \left(W(a,w) - U\right)^{\phi} \cdot \left(J(p,w) - V\right)^{1-\phi} \tag{58}$$

$$\Rightarrow \phi(J(p,w) - V) = (1-\phi)(W(a,w) - U) \tag{59}$$

$$\Leftrightarrow w = \phi f \cdot \frac{k}{q} + \phi p + (1-\phi)((b+\xi) - a) \tag{60}$$

where $f/q = \theta$, labor market tightness, with a CRS matching function.

Flow payoffs. Flow payoffs are linear in idiosyncratic job fundamentals (a, p):

$$a + w = \phi(p + \theta k + a) + (1-\phi)((b+\xi)) = (b+\xi) + \phi(p + a - (b+\xi)) + \phi\theta k \tag{61}$$

$$p - w = p - \phi(p + \theta k) - (1-\phi)((b+\xi) - a) = (1-\phi)(p + a - (b+\xi)) - \phi\theta k \tag{62}$$

Workers' reservation value. Remember that worker's reservation value (51). Now consider the job types that generate this wage and amenity combination as a function of job fundamentals a and p and Nash bargaining:

$$\overline{a + w} = \phi(p + \theta k + a) + (1-\phi)((b+\xi)) = (b+\xi) + \phi(p + a - (b+\xi)) + \phi\theta k \tag{63}$$

So the premium over $b + \xi$ must equal the search option value for the marginal job:

$$\beta(f - (1-\delta)\lambda) \cdot \int_{\underline{a+w}}^{\infty} (W(a+w) - U)\, d\tilde{G}(a+w) = \phi(p + a - (b+\xi)) + \phi\theta k \tag{64}$$

$$\Leftrightarrow p + a - (b+\xi) = \frac{\phi}{1-\phi}\theta k - \frac{1}{1-\phi}(1-\delta)\lambda\frac{k}{q} \tag{65}$$

Firms' reservation value.

$$0 = J(\overline{p-w}) \tag{66}$$

$$\Leftrightarrow 0 = \overline{p-w} + \beta(1-\delta)(1-\lambda)0 + \beta(1-\delta)\lambda\int_0^{\infty} J(p,w)d\hat{G}(p,w) \tag{67}$$

$$\Leftrightarrow \overline{p-w} = \boxed{-}(1-\delta)\lambda\frac{k}{q} \tag{68}$$

which can be negative due to the option value of returning to the mean job quality, which in equilibrium (ZPC) is strictly positive due to search frictions.

Due to efficient separations, the same job fundamentals (p, a) that define the worker's reservation payoff $a + w(a, p)$ also define the firm's reservation flow value $p - w(p, a)$:

$$\overline{p-w} = (1-\phi)(p + a - (b+\xi)) - \phi\theta k = \boxed{-}(1-\delta)\lambda\frac{k}{q} \tag{69}$$

$$\Leftrightarrow p + a - (b+\xi) = \frac{\phi}{1-\phi}\theta k - \frac{1}{1-\phi}(1-\delta)\lambda\frac{k}{q} \tag{70}$$

This bilateral result in turn justifies describing the reservation values with respect to the same distribution of fundamentals transformed into a wage-based function. Moreover, quits and layoffs coincide as efficient separations since the quit and layoff thresholds of match quality coincide.

Fraction of Always-Separators.

$$s^{AS} = \delta + (1-\delta)\lambda G(\overline{a+p}(b)) \tag{71}$$

Fraction of Never-Separators.

$$s^{NS} = 1 - [\delta + (1-\delta)\lambda G(\overline{a+p}(b))] \tag{72}$$

Fraction of Compliers.

$$s^{C} = (1-\delta) \cdot [\lambda G(\overline{a+p}(b + Z \times \Delta b)) - \lambda G(\overline{a+p}(b))] \tag{73}$$

Shift in Δb. A shift in b from b to $b + \Delta b$ has two effects. First, it will shift the reservation value of jobs, holding aggregate conditions constant. Second, Δb is expected to change labor

market tightness θ endogenously, through free entry in vacancies (zero profit condition). This will affect not only wages, but also the ex-post thresholds the bargaining parties apply. However, as Lalive, Landais and Zweimuller (2015) have shown, a suitable control group are 45-49 year old workers, who appear to be engaged in random search in the same labor market as the slightly older treated workers. Since we apply a difference-in-difference design, we therefore "net out" these market-level effects on θ.

C Employment Effects: Disentangling the Separation and Job Finding Margins

Section 4.1 provided direct graphical evidence of separation effects for a sample of initially *employed* workers. In doing so, it mirrored the approach in the job search literature which focuses on samples of initially *unemployed* workers. In this section, we combine both analyses in difference-in-discontinuities specifications to estimate the effect of benefit extensions on the separation, the job finding, and the unemployment rate. We estimate an elasticity of the quarterly separation rate to potential benefit duration of .17 (arc: 0.33), very close to our estimate of the job finding rate response (.17, arc: .55), on the lower end of existing estimates. We further calculate the contributions of the separation and the job finding margin on the employment effect of the REBP reform and find that the separation margin can account for 70% of the overall employment effect.

Difference-in-discontinuity specification. Specifically, we exploit differences between REBP and control regions and sharp discontinuities at age 50 using the following regression framework:

$$y_{rait} = \gamma_t + \alpha_r + \sum_{a=45}^{55} \beta_a \times \text{Age}_a + \sum_{a=45}^{55} \beta_a^{\text{REBP}} \times \text{Age}_a \times \text{REBP}_r + \epsilon_{rait},$$

where y_{rait} indicates the outcome of individual i from region r, at age a in time period t. The specification includes region fixed effects, α_r, which absorb permanent differences between regions. It includes fine-grained age indicators, Age_a, that non-parametrically control for an individual's age. The coefficients of interest, the β_a^{REBP}, capture the interaction between a dummy indicating residence in the REBP region and age. As the model includes baseline region effects, α_r, one of the interaction terms is normalized to zero and we set the interaction term in the last quarter before an individual turns 50 to zero. In the main specifications, we estimate the model during the treatment period, from 1988 to 1993, so that the β_a^{REBP} capture the treatment effect for individuals aged 50 and above. The model allows for a visual representation of treatment effects and a direct inspection of the common trends assumption for individuals younger than 50.

Identification assumption and potential threats to identification. The core identification assumption is that the policy variation is exogenous conditional on the covariates included in the model, implying that workers above age 50 in the REBP and the control region would have had parallel trends of the separation, job finding, and unemployment rate had the policy counterfactually not been implemented. The plausibility of this assumption can be tested by comparing trends before age 50 during the treatment period. Potential threats to identification would be region-by-age confounds that lead to non-parallel trends. For instance, older workers'

labor force participation between the ages of 50 and 55 could differ between the REBP and the control regions as a consequence of different industry structures. As in section 4.1, the structure of the research design allows us to test for such a violation of the identification assumption by estimating placebo specifications in the pre- and the post-reform periods. We implement such placebo exercises focusing on the pre-reform period from 1983 to 1988 and on the post-reform period from 1998 to 2003.[38] Several other classes of potential violations of the identification assumption, e.g., age-by-time period specific confounds are already accounted for by including suitable fixed effects.

Separation rate. We first estimate the effect of benefit extensions on the separation rate and document a 50% increase from two to three percentage points per quarter in response to the REBP reform. As panel (a) of Figure 18 shows, the pre-trend of the separation rate for workers younger than 50 is flat. At 50, there is a sharp increase by almost one percentage point followed by a steady increase to about 1.4 percentage points at age 55. The control mean in the 50-plus age group is two percentage points so that the treatment effect represents, on average, a roughly 50% increase in the separation rate. We also estimate placebo difference-in-discontinuities specifications in pre-period before the onset of the REBP reform in 1988 and in a post-period after 1998 (see panels (b) and (c)). Relative to workers just below age 50, both placebo specifications show no significant effects on the separation rate of workers aged 50 and above. For the pre-period placebo specification, a slight pre-trend can be discerned but it goes into the opposite direction. Overall, the difference-in-discontinuities analysis confirms the results of the graphical analysis in section 4.1 and provide a precise estimate of the separation rate increase.

Job finding rate. We next provide an analysis of the effect of benefit extensions on the job finding, thereby taking the more common perspective in the literature and replicating previous work on the REBP reform (Lalive and Zweimüller (2004), and Lalive (2008)). Panel (a) of Figure 19 shows flat pre-trends for workers below age 50 and a sizable drop in the job finding rate from a level of 43 percent percent per quarter by about 25 percentage points. Again, placebo estimates in the pre- and post-period reveal that the drop at age 50 during the treatment period is unusually large compared to the change in the job finding rate when no differential policy discontinuity at age 50 exists.

[38] We omit the post-period from 1993 to 1998 from the analyses as older workers who had became unemployed before the end of the REBP program still had access to longer benefits and so the time period does not constitute a placebo for the job finding and unemployment rate. For the sake of consistency across exercises, we therefore only report placebo exercises for time periods when the program did not have a direct effect on any of the relevant outcome variables. A placebo analysis of the separation rate between 1993 and 1998 nevertheless shows no discernible placebo effects on separation rates for workers above age 50.

Unemployment rate. Finally, we provide a difference-in-discontinuities estimates to provide analogous estimates for the effect of the benefit extensions on older workers' employment prospects (see also). The analysis documented in Figure 20 shows flat pre-trends and unemployment rates rising with age for REBP-region workers older than 50 during the treatment period. A steady increase in the unemployment rate level rather than a discrete increase at age 50 was to be expected based on the evidence on the shifts in the separation and job finding rates. Quantitatively, the unemployment increases by around 4 percentage points for workers in their mid-fifties compared to a control mean of 3.4%. As in the previous exercises, the placebo exercises show no comparable effects in the pre- and post-period when the REBP reform was not in place.

Decomposing the employment effect: f vs. s. As the benefit extensions affected the separation and the job finding rate in our context, we next propose and implement a methodology that isolates the contribution of each margin. The employment/population ratio shifts by a combination of the EN (separation) and the NE (job finding) margin, for each birth cohort:[39]

$$e_{b,t} = e_{b,t-1} - \rho^{EN}_{b,t-1} \cdot e_{b,t-1} + \rho^{NE}_{b,t-1} \cdot (1 - e_{b,t-1}) \tag{74}$$

To assess the role of each margin, we conduct counterfactuals with this law of motion.[40] For treatment group T, we impose control group C's transition rate for the given cohort b and time period t for the reference transition NE, but for the rate of interest NE, we feed in treatment group T's actual transition probability:[41]

$$\tilde{e}^{EN}_{T,b,t} = \tilde{e}^{EN}_{T,b,t-1} - \rho^{EN}_{T,b,t-1} \cdot \tilde{e}^{EN}_{T,b,t-1} + \boxed{\rho^{NE}_{C,b,t-1}} \cdot \left(1 - \tilde{e}^{EN}_{T,b,t-1}\right) \tag{75}$$

[39] In the main part of this paper, we impose a balanced panel of workers in that workers that leave our data set for reasons such as death, international migration or other attrition out of the administrative system are counted as nonemployed. Our most general definition of non-employment includes both unemployment job search and labor force abstention.

[40] Shimer (2012) and Elsby et al. (2013) assess the relevance of separations and job finding margins for aggregate fluctuations. They use either steady-state approximations or respectively deviations from steady state using a partial derivative approach, by setting the reference rate to its long-run average, and letting only the rate of interest vary. The resulting employment fluctuations then trace out a counterfactual employment series that can be compared to the actual empirical employment series. Our approach builds on these steady-state approximations but differs in three important ways. First, we do not use any steady-state approximation but trace out the full dynamic law of motion. As a result, we precisely match actual employment rates when feeding in actual transition probabilities. Second, the existing literature assess the contribution of each given rate by shutting off *any* variation in the other rate, i.e. setting that rate constant. We allow the reference rate to vary dynamically, by extrapolating the control group's rate. Third, the macroeconomic applications have so far been restricted to reduced-form decompositions of cyclical fluctuations. Our methodology assess the transmission of a particular shock (shift in UI generosity) into labor market net quantitites. Fourth and crucially, we adopt the accounting framework to a quasi-experimental design with difference-in-difference estimates.

[41] We measure the transition rates as the fraction of workers in state $s \in \{E, N\}$ out of a given state, in each month. To avoid seasonal adjustment, we average the transition rates by calendar year.

To assess the role of the treatment group's job finding rate, we feed in the control group's separation rate but impute employment evolution using the treatment group's job finding rate:

$$\tilde{e}^{NE}_{T,b,t} = \tilde{e}^{EN}_{T,b,t-1} - \boxed{\rho^{EN}_{C,b,t-1}} \cdot \tilde{e}^{NE}_{T,b,t-1} + \rho^{NE}_{T,b,t-1} \cdot \left(1 - \tilde{e}^{NE}_{T,b,t-1}\right) \tag{76}$$

For $X \in \{EN, NE\}$, we then aggregate across cohorts to obtain macro employment rates:

$$\tilde{E}^{X}_{T,b\in(b_{\min},b_{\max}),t} = \sum_{b_{\min}}^{b_{\max}} \omega_{T,b,t} \cdot \tilde{e}^{X}_{T,b,t} \tag{77}$$

where weight $\omega_{T,b,t} = \frac{P_{T,b}}{\sum_{b'=b_{\min}}^{b_{\max}} P_{T,b'}}$ reflects the counts of each cohort in the cohort interval. Figure 22 plots the underlying population-weighted average transition rate for the REBP and the control regions.

Figure 21 plots the time series of the potentially-treated cohorts' employment rate difference between the REBP and control regions. There is a substantial drop in employment, by around 6.6ppt. The counterfactual time series that allows uses the treatment effect on the canonical job finding rate channel matches the initial, small, employment drop well. But it completely misses the substantial employment drop later on, where it, on its own, matches only 30% of the full drop in employment at trough. By contrast, the counterfactual employment rate that uses the separation rate channel only almost perfectly traces out the empirical treatment effect on net employment. Therefore, the focus on the job finding rate channel only misses the majority of the dramatic employment effects from the benefit extension in our analysis of the REBP effects.

C.1 Appendix Tables

Table 7: Summary Statistics (Difference-in-Differences Analysis)

	no rebp	rebp until 1993	Total
White Collar	0.505	0.399	0.468
	(0.500)	(0.490)	(0.499)
Experience	23.57	24.03	23.73
	(2.313)	(1.928)	(2.198)
Tenure	9.875	10.30	10.02
	(7.687)	(7.531)	(7.635)
Earnings	30088.9	29858.5	30008.4
	(8785.1)	(7940.4)	(8500.4)
Age	49.53	49.49	49.51
	(2.924)	(2.907)	(2.918)

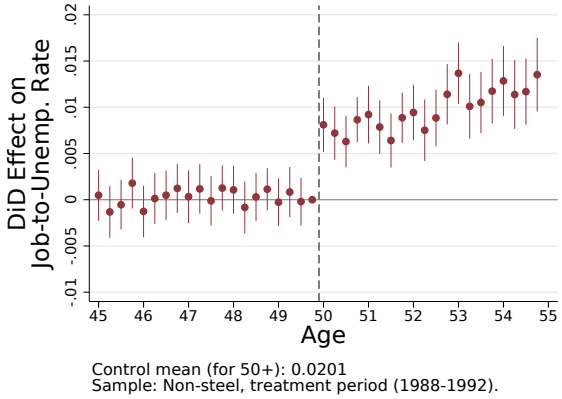
mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C.2 Appendix Figures

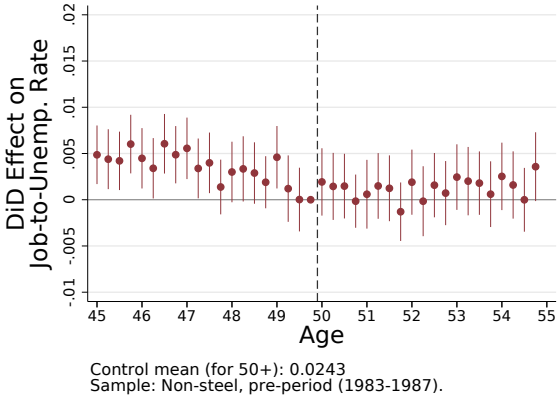
Figure 18: The Effect of Benefit Extensions on the Separation Rate

(a) Treatment Effect



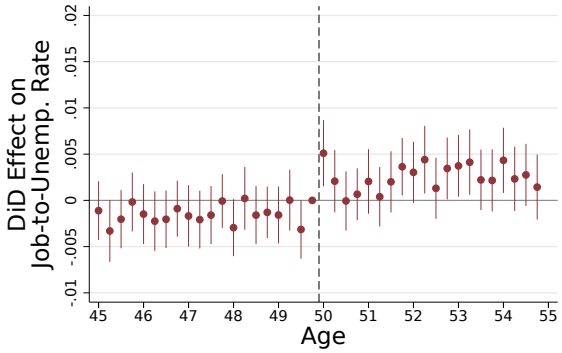
Control mean (for 50+): 0.0201
Sample: Non-steel, treatment period (1988-1992).

(b) Placebo I (Pre-Period)



Control mean (for 50+): 0.0243
Sample: Non-steel, pre-period (1983-1987).

(c) Placebo II (Post-Period I)

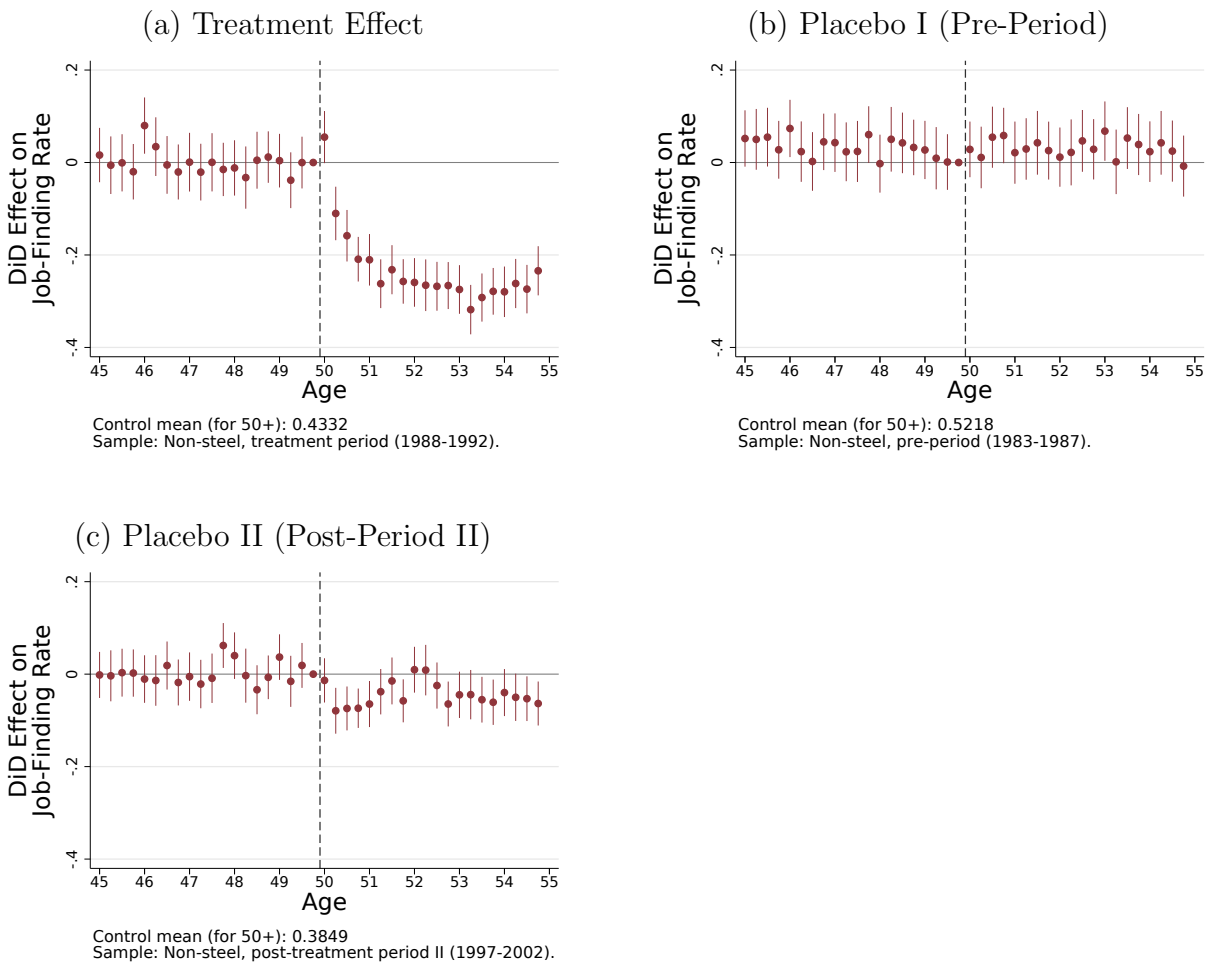


Control mean (for 50+): 0.0255
Sample: Non-steel, post-treatment period I (1993-1997).

(d) Placebo II (Post-Period II)

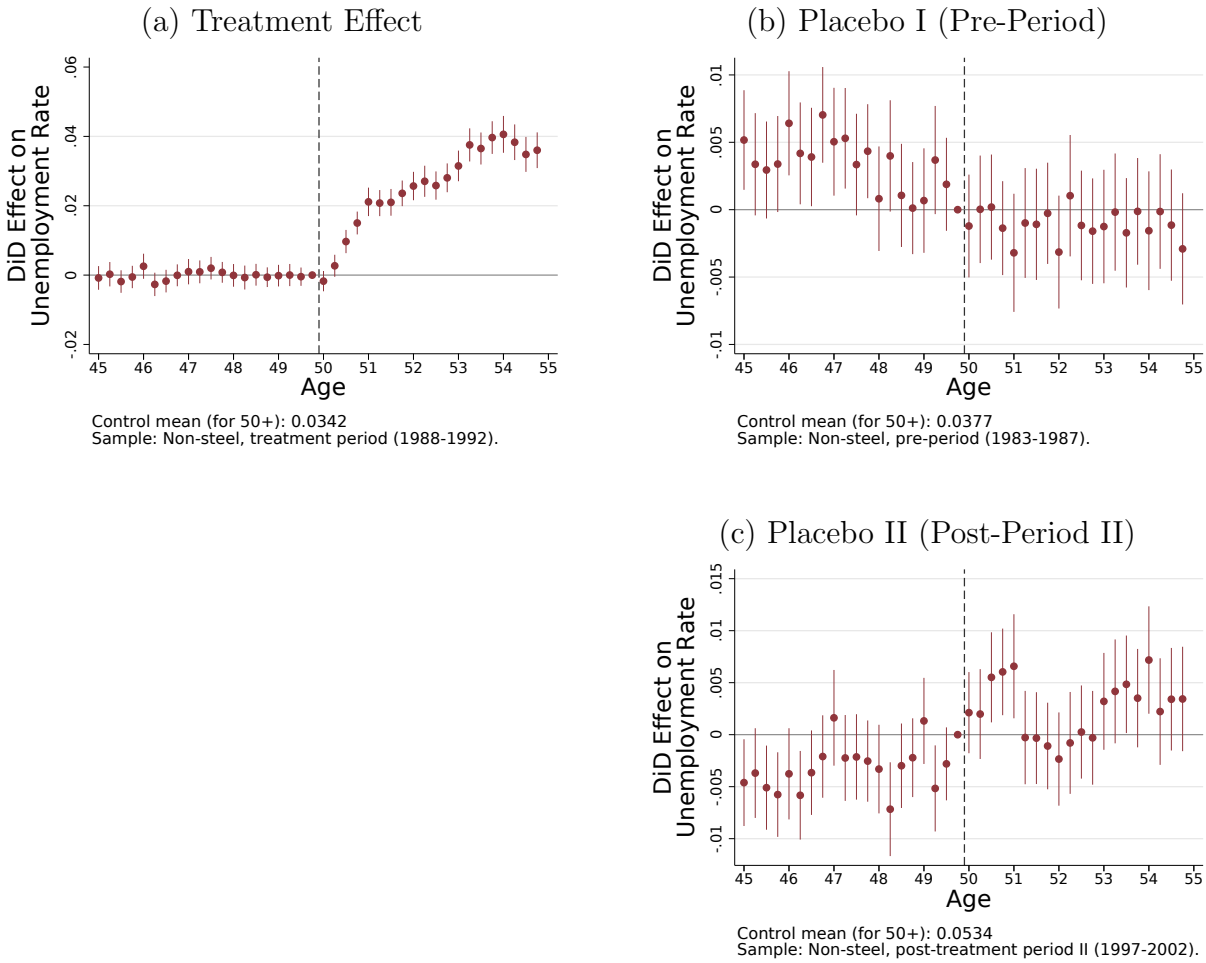
Note: The figures show difference-in-differences specifications for the interaction between age and REBP region in specifications controlling for the base effects of REBP and age. During the treatment period (Panel (a)), the benefit extension from 1 to 4 years affected eligible workers in REBP regions above age 50. For the placebo specifications in the pre- and post-treatment period (Panels (b) through (d)), there was no differential policy discontinuity at age 50 differing between REBP and control regions. The vertical maroon lines are 95% confidence intervals based on standard errors clustered at the individual level.

Figure 19: The Effect of Benefit Extensions on the Job Finding Rate



Note: The figures show difference-in-differences specifications for the interaction between age and REBP region in specifications controlling for the base effects of REBP and age. During the treatment period (Panel (a)), the benefit extension from 1 to 4 years affected eligible workers in REBP regions above age 50. For the placebo specifications in the pre- and post-treatment period (Panels (b) through (c)), there was no differential policy discontinuity at age 50 differing between REBP and control regions. The vertical maroon lines are 95% confidence intervals based on standard errors clustered at the individual level.

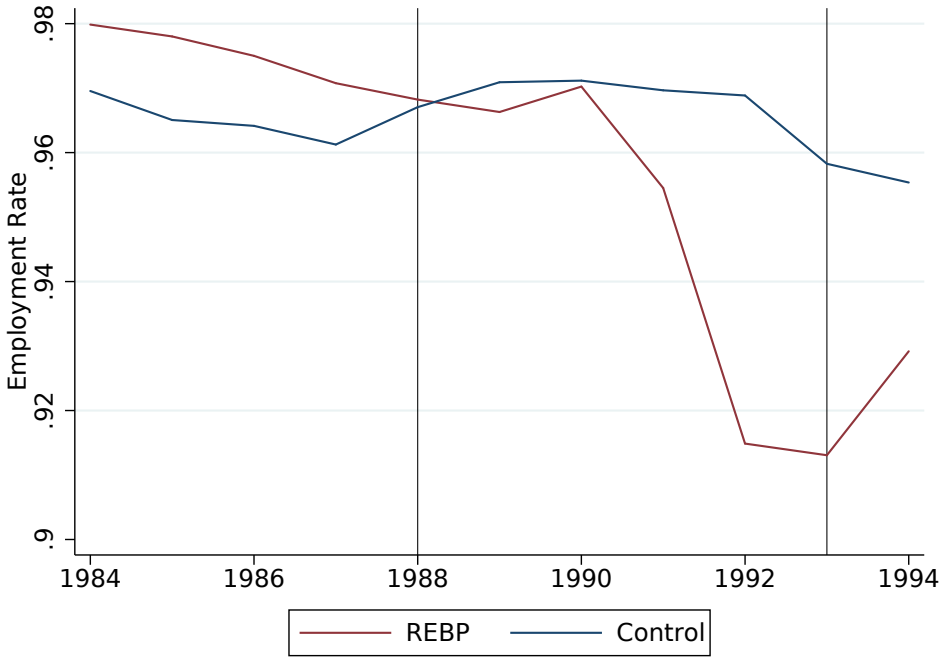
Figure 20: The Effect of Benefit Extensions on the Unemployment Rate



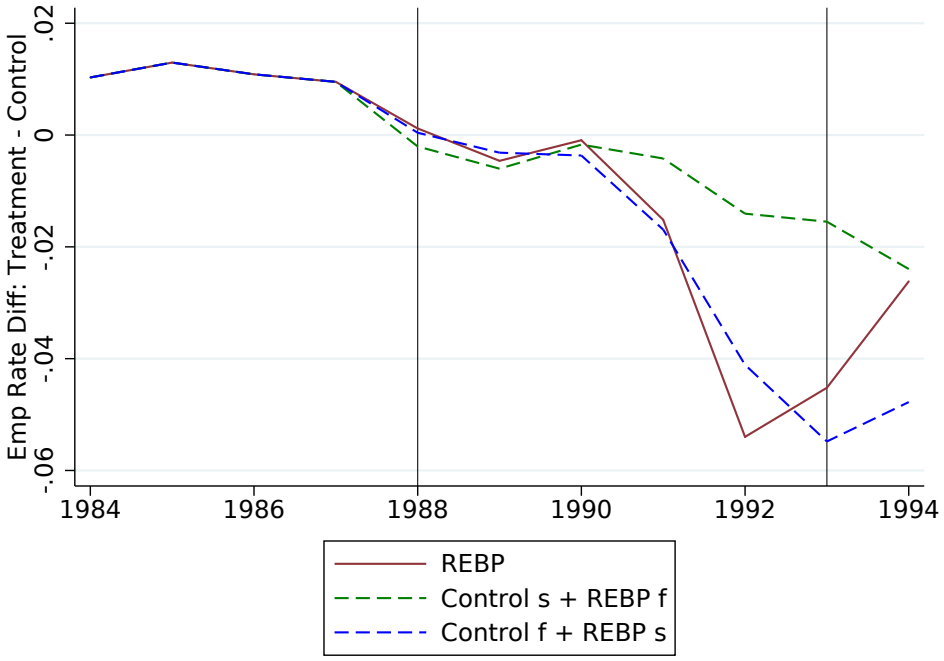
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Figure 21: Actual and Counterfactual Employment Rate Time Series

(a) Employment Rate Time Series

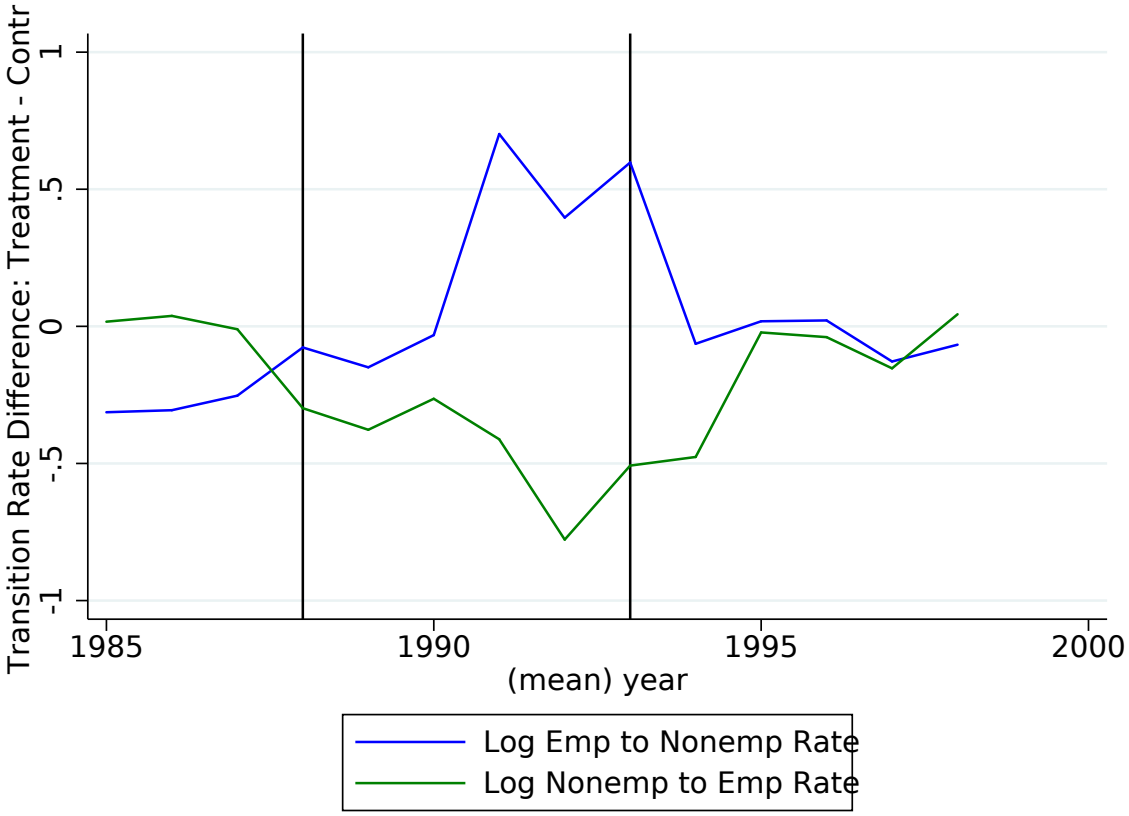


(b) Difference in Employment Rate (REBP - Control Region) and Counterfactual Employment Rate Time Series



Note: See Appendix Section C.

Figure 22: Transition Rate Differences: f vs. s



Note: See Appendix Section C.

D Non-Coasean Settings: Inefficient Separations of Positive-Surplus Matches

Inefficient separations. Next, we consider deviations from the surplus-focused setting of bilateral efficiency that may arise from frictions, e.g. in form of ad-hoc form *wage rigidity*. With wages not necessarily set to achieve bilateral efficiency, there will be a range of wages that will leave one party prefer the outside option to the inside option at that wage, although the match carries positive surplus – i.e. wages could be set to fulfill the participation constraint of both parties. Not all efficient matches are jointly viable with wage rigidity. Bilaterally inefficient separations concern positive-surplus matches that lead one party to prefer to separate. Such separations (or rejected offers) are inefficient because they forego positive surplus.

In the Coasean environment, classifying separations into quits and layoffs is meaningless because separations are bilaterally efficient: wages will adjust to preserve positive-surplus matches, but wage adjustments cannot prevent destruction of jobs that lost their surplus. With inefficient wages, quits and layoffs become meaningful.

Quits: worker-initiated inefficient separations. Quits emerge when a given wage w falls short of the worker's reservation wage but in principle the firm's willingness to pay exceeds the worker's reservation wage – that is, at wage w here, the worker's employment value incl. the wage falls short of the outside option:

$$w < \underline{w}^w(b,\mathbf{x}) \quad \wedge \quad \overline{w}^w(b,\mathbf{x}) < \overline{w}^f(b,\mathbf{x}) \tag{78}$$

$$\Leftrightarrow \quad V^W_{\text{Stay}}(b,\mathbf{x}) + w < V^W_{\text{Separate}}(b,\mathbf{x}) \quad \wedge \quad V^F_{\text{Stay}}(b,\mathbf{x}) - w \geq V^F_{\text{Separate}}(b,\mathbf{x}) \tag{79}$$

Layoffs: firm-initiated inefficient separations. emerge when a given wage w exceeds the firm's reservation wage but in principle the firm's willingness to pay exceeds the worker's reservation wage – that is, the firm's value of the employment relationship incl. the wage cost falls short of its vacancy value:

$$w > \overline{w}^f(b,\mathbf{x}) \quad \wedge \quad \overline{w}^w(b,\mathbf{x}) < \overline{w}^f(b,\mathbf{x}) \tag{80}$$

$$\Leftrightarrow \quad V^F_{\text{Stay}}(b,\mathbf{x}) - w < V^F_{\text{Separate}}(b,\mathbf{x}) \quad \wedge \quad V^W_{\text{Stay}}(b,\mathbf{x}) + w \geq V^W_{\text{Separate}}(b,\mathbf{x}) \tag{81}$$

The incidence of shocks on layoffs and quits. Given w, the side of the relationship whose value is affected by a given shift in the environment or the match payoff, will play a role in whether a separation manifests itself as a quit or a layoff. This is because the surplus is no longer the sole determinant of the separation decision.

Marginal jobs: quits. With wages not set to ensure bilateral efficiency at any point, the marginal match for the quit margin has the prevailing wage w equal the worker's reservation wage $\underline{w}^w(b, \mathbf{x})$:

$$w \boxed{=} \underline{w}^w(b, \mathbf{x}) \quad \wedge \quad \overline{w}^f(b, \mathbf{x}) > \overline{w}^f(b, \mathbf{x}) \tag{82}$$

$$\Leftrightarrow \quad V^W_{\text{Stay}}(b, \mathbf{x}) + w \boxed{=} V^W_{\text{Separate}}(b, \mathbf{x}) \quad \wedge \quad V^F_{\text{Stay}}(b, \mathbf{x}) - w > V^F_{\text{Separate}}(b, \mathbf{x}) \tag{83}$$

When a shift in fundamentals affects the worker's inside or outside value (or, equivalently, the worker's reservation wage) while leaving the wage w unchanged, quits emerge among marginal jobs – despite non-negative surplus, which here pre-separation fully accrued to the firm.

In this case, the wage of the match identifies the worker's net benefit of working (gross of the wage), and therefore continues to equal the worker's reservation wage. However, it will not reflect the firm's reservation wage, as in the Coasean case. Moreover, the match carries positive surplus.

When a negative shock affects the firm's values only, then it will not entail separations since the firm is inframarginal.

Marginal jobs: layoffs. Similarly, the marginal match for the layoff margin has the prevailing wage w equal the firm's reservation wage $\overline{w}^f(b, \mathbf{x})$:

$$w \boxed{=} \overline{w}^f(b, \mathbf{x}) \quad \wedge \quad \overline{w}^w(b, \mathbf{x}) < \overline{w}^f(b, \mathbf{x}) \tag{84}$$

$$\Leftrightarrow \quad V^F_{\text{Stay}}(b, \mathbf{x}) - w \boxed{=} V^F_{\text{Separate}}(b, \mathbf{x}) \quad \wedge \quad V^W_{\text{Stay}}(b, \mathbf{x}) + w \geq V^W_{\text{Separate}}(b, \mathbf{x}) \tag{85}$$

A negative shift in the firm's inside or outside value (or, equivalently, the firm's reservation wage) triggers layoffs among these jobs, which may carry surplus but where wage w had fully allocated it to the worker.

Reservation wage coincidence for marginal matches. With wage frictions that prevent the bargain from ensuring the efficient outcome, the wage still capture reservation wages – albeit these no longer coincide between both parties as in the efficient case. In our design, we consider a labor supply shock Δb that shifts the outside (non-employment) value of the workers. Therefore, we now conjecture that if some matches dissolve, the most likely candidate is either efficient separations or the quit decisions. In that case, the measured pre-separation wage will precisely capture the reservation wage of the worker and therefore the wage-metric net benefit to non-employment (net of the amenity).

E Administrative Quit Proxy: Wait Period of Benefit Receipt

By exploiting a retrospective household survey, we were able to classify the agnostic separation measure into quits and layoffs. This analysis revealed a mixed increase in quits and layoffs. Next, we further trace the incidence of separations into quits and layoffs by constructing a direct administrative quit proxy. In the subsequent Section, we construct an indirect layoff market.

Quits and UI eligibility. Importantly for our analysis, workers are eligible for UI even if they initiate the separation in form of a quit without mutual agreement with the employer and firms do not face experience rating in Austria. While quits are formally not eligible for UI and experience ratings may lead firms to classify quits as layoffs, eligibility for UI may still extend to quits de-facto even for the United States.[42]

The 28 wait rule for quits. We exploit an administrative wait period for UI eligibility for quits that we can trace in the administrative data. Specifically, worker-initiated quits come with a 28-day wait period between registration with the government employment agency, and eligibility for receipt of unemployment insurance benefits. In our administrative data, we see the registration on a day-specific basis, as well as the first receipt of benefits. We classify a separation as a worker-initiated quit if the delay is at least 28 days.

In practice, workers are a strong incentive to immediately register at with the employment agency to avoid losing health insurance coverage. Importantly, this incentive extends to the quitters not yet eligible for actual UI benefits. In our data, we find that the vast majority of of workers register with the employment agency within little more than a month of the end of an employment spell.

Treatment effect on the fraction of quits in total separations. We conduct a version of our difference-in-discontinuity specification in two ways:

Specifically, we exploit differences between REBP and control regions and sharp discontinuities at age 50 using the following regression framework:

$$y_{rait} = \gamma_t + \alpha_r + \sum_{a=45}^{55} \beta_a \times \text{Age}_a + \sum_{a=45}^{55} \beta_a^{\text{REBP}} \times \text{Age}_a \times \text{REBP}_r + \epsilon_{rait},$$

where y_{rait} indicates the outcome of individual i from region r, at age a in time period t.

[42]For a discussion, see Hagedorn et al. (2013) in response to Jesse Rothstein. The latter group of authors argue that while US firms have incentives to classify firms as quits rather than layoffs, the institutional features make it difficult for firms to enforce correct classification in practice.

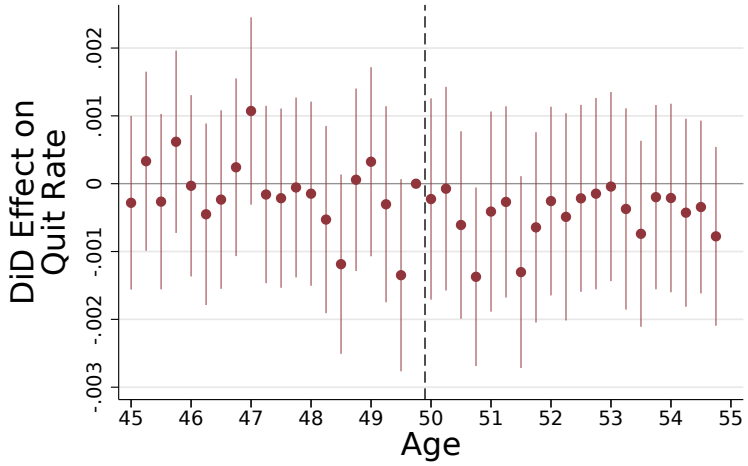
The specification includes region fixed effects, α_r, which absorb permanent differences between regions. It includes fine-grained age indicators, Age_a, that non-parametrically control for an individual's age. The coefficients of interest, the β_a^{REBP}, capture the interaction between a dummy indicating residence in the REBP region and age. As the model includes baseline region effects, α_r, one of the interaction terms is normalized to zero and we set the interaction term in the last quarter before an individual turns 50 to zero. In the main specifications, we estimate the model during the treatment period, from 1988 to 1993, so that the β_a^{REBP} capture the treatment effect for individuals aged 50 and above. The model allows for a visual representation of treatment effects and a direct inspection of the common trends assumption for individuals younger than 50.

We replace the separation indicator by a quit indicator, estimating quarterly transition rates. In Figure 23, we graphically plot the age-specific treatment effects for the policy period, as well as the placebo years when the policy was not active. The treatment effect is significantly positive, for exactly the treated age groups. That is, the quarterly probability of transitioning from employment into non-employment through a quit increases in response to the UI extension. By contrast, there is no age discontinuity in the quit probability during pre-reform years.

E.1 Appendix Figures

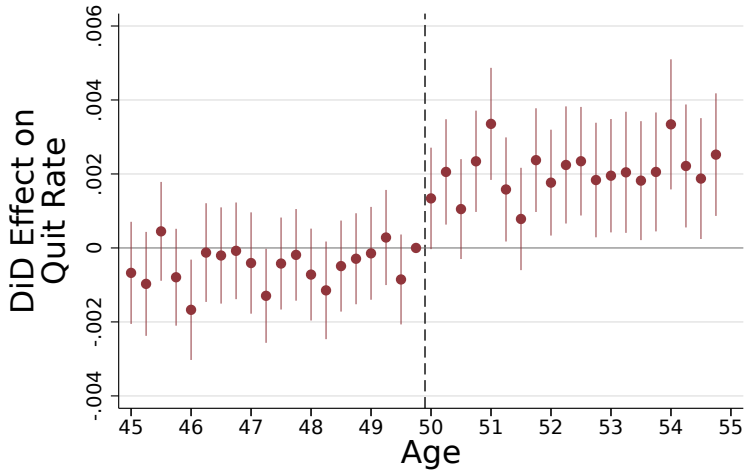
Figure 23: The Effect of Extensions on Non-Amicable Quits: Administration Quit/Layoff Proxy from 28-Day Wait Period

(a) Pre-Period 1982–1987



Control mean (for 50+): 0.0040
Sample: Non-steel, pre-period (1983-1987).

(b) Reform Period 1988–1993



Control mean (for 50+): 0.0050
Sample: Non-steel, treatment period (1988-1992).

Note: The figures show difference-in-differences specifications for the interaction between age and REBP region in specifications controlling for the base effects of REBP and age. Non-amicable quits are defined as ends of employment spells with a lag between the end of the employment spell and the onset of an unemployment spell of more than 4 weeks. During the treatment period, the benefit extension from 1 to 4 years affected eligible workers in REBP regions above age 50. The vertical maroon lines are 95% confidence intervals based on standard errors clustered at the individual level.

F Identification of Complier Characteristics in Difference-in-Differences IV Settings

In this appendix section, we show how complier characteristics are identified in difference-in-differences settings under assumptions 1 through 4 from section 5:

Assumption 1. First stage: For all (r, a), $P(D_1 = 1|r, a) > P(D_0 = 1|r, a)$.

Intuitively, assumption 1 posits that more separations take place under the reform and ensures the existence of compliers.

Assumption 2. Monotonicity: $D_1 - D_0 \geq 0$.

Importantly, assumption 2 rules out defiers, i.e. individuals that would separate if benefits are not extended but would not separate if unemployment benefits are more generous.

Assumption 3. Independence: $(X, D_0, D_1) \perp Z|(r, a)$.

The independence assumption posits that conditional on r and a, the instrument Z is orthogonal to X, D_0, and D_1.

Assumption 4. Additive separability:
(a) For all (a, a') and $d, d' \in \{0, 1\}$,
$E[X|a, r, D_0 = d, D_1 = d'] - E[X|a', r, D_0 = d, D_1 = d']$ does not depend on r.
(b) For all (a, a') and $d, d' \in \{0, 1\}$,
$P(D_0 = d, D_1 = d'|a, r)$-$P(D_0 = d, D_1 = d'|a', r)$ does not depend on r.

Since Z does not vary conditional on region and age, we only observe either $Z = 0$, or $Z = 1$, for a given region and age cell. Under assumptions 1 through 4, the expected value of complier characteristics can be represented as a function of observable quantities for all region/age combinations. In the expressions below, let (r, a) represent a region and age combination such that $Z = 1$, and (r', a') represent other regions and ages (i.e. $Z = 0$ for$(r', a), (r, a')$ or (r', a')).

We can rewrite the conditional expectation of individuals with $D_1 = 1$ in region r and of age a as follows:

$$\begin{aligned} E[X|D_1 = 1, r, a] \quad = \quad & E[X|r, a, D_1 = 1, D_0 = 1] \cdot P(D_0 = 1|r, a, D_1 = 1) \\ & + E[X|r, a, D_1 = 1, D_0 = 0] \cdot P(D_0 = 0|r, a, D_1 = 1) \end{aligned} \tag{86}$$

Rearranging yields

$$\begin{aligned} E[X|r, a, D_1 = 1, D_0 = 0] \quad = \quad & \tfrac{1}{P(D_0=0|r,a,D_1=1)} \cdot E[X|r, a, D_1 = 1] \\ & - \tfrac{P(D_0=1|r,a,D_1=1)}{P(D_0=0|r,a,D_1=1)} E[X|r, a, D_1 = 1, D_0 = 1] \end{aligned} \tag{87}$$

By monotonicity $(D_1 - D_0 \geq 0)$, we have that $E[X|r, a, D_1 = 1, D_0 = 1] = E[X|r, a, D_0 = 1]$, which implies:

$$E[X|r,a,D_1=1,D_0=0] = \begin{array}{l} \frac{1}{P(D_0=0|r,a,D_1=1)} \cdot E[X|r,a,D_1=1] \\ -\frac{P(D_0=1|r,a,D_1=1)}{P(D_0=0|r,a,D_1=1)} E[X|r,a,D_0=1] \end{array} \tag{88}$$

Using the definition of conditional probabilities, $P(D_0=1|r,a,D_1=1) = \frac{P(D_0=1,D_1=1|r,a)}{P(D=1|r,a)}$ and $P(D_0=0|r,a,D_1=1) = \frac{P(D=0,D_1=1|r,a)}{P(D_1=1|r,a)}$. Define the (conditional on region and age) probability of always-separators as $\pi_{ra}^A = P(D_0=1,D_1=1|r,a)$, of never-separators as $\pi_{ra}^N = P(D_0=0,D_1=0|r,a)$ and by monotonicity of compliers as $\pi_{ra}^C = P(D_0=0,D_1=1|r,a) = 1-\pi_{ra}^A-\pi_{ra}^N$. The conditional expectation term above can then be expressed as follows:

$$E[X|r,a,D_1=1,D_0=0] = \begin{array}{l} \frac{\pi_{ra}^C+\pi_{ra}^A}{\pi_{ra}^C} \cdot E[X|r,a,D_1=1] \\ -\frac{\pi_{ra}^A}{\pi_{ra}^C} E[X|r,a,D_0=1]. \end{array} \tag{89}$$

Equation 89 shows that under assumptions 1 through 4, complier characteristics are identified in a difference-in-difference IV setting as we can construct sample analogues to each of the terms on the right-hand side as follows. By independence, we have that:

$$E[X|r,a,D_1=1] = E[X|r,a,D=1,Z=1]. \tag{90}$$

By independence and additive separability in X, we have:

$$\begin{aligned} E[X|r,a,D_0=1] &= E[X|r,a',D_0=1] + (E[X|r',a,D_0=1] - E[X|r',a',D_0=1]) \\ &= E[X|r,a',D=1,Z=0] \qquad (91) \\ &+ E[X|r',a,D=1,Z=0] \\ &- E[X|r',a',D=1,Z=0]. \qquad (92) \end{aligned}$$

Sample analogues exist for each of the right-hand side terms in 90 and 91:

$$E[X|r,a,D=1,Z=1] = \frac{1}{N_{r,a}} \sum_{i\in(r,a)} X_i D_i Z_i, \tag{93}$$

$$E[X|r,a',D=1,Z=0] = \frac{1}{N_{r,a'}} \sum_{i\in(r,a')} X_i D_i (1-Z_i), \tag{94}$$

$$E[X|r',a,D=1,Z=0] = \frac{1}{N_{r',a}} \sum_{i\in(r',a)} X_i D_i (1-Z_i), \tag{95}$$

$$E[X|r',a',D=1,Z=0] = \frac{1}{N_{r',a'}} \sum_{i\in(r',a')} X_i D_i (1-Z_i), \tag{96}$$

where $N_{r,a'}$is the number of observations in (r,a') and so forth.

For the conditional probabilities in 89note that (using independence and the parallel trends

in D assumption):

$$\begin{aligned}
\pi^A_{ra} = P(D_0 = 1, D_1 = 1|r, a) &= P(D_0 = 1|r, a) \\
&= P(D_0 = 1|r, a') + P(D_0 = 1|r', a) - P(D_0 = 1|r', a') \qquad (97) \\
&= E(D|Z = 0, r, a') + E(D|Z = 0, r', a) - E(D|Z = 0, r', a') \\
\pi^N_{ra} = P(D_0 = 0, D_1 = 0|r, a) &= P(D_1 = 0|r, a) \\
&= P(D = 0|Z = 1, r, a) \qquad (98) \\
&= 1 - E(D|Z = 1, r, a).
\end{aligned}$$

These quantities can be estimated in the regression:

$$D_{ira} = \beta + \phi_r + \psi_a + \nu Z_{ra} + \chi_{ira}. \qquad (99)$$

The sample estimators are then given by $\pi^A_{ra} = \hat{\beta} + \hat{\phi}_r + \hat{\psi}_a$, $\pi^N_{ra} = 1 - \hat{\beta} - \hat{\phi}_r - \hat{\psi}_a - \hat{\nu}$ and $\pi^C_{ra} = 1 - \pi^N_{ra} - \pi^A_{ra} = \hat{\nu}$. All objects on the right-hand side of 89 thus have estimable sample counterparts.

Extensions. Under additional assumptions, we can alternatively estimate the conditional expectations in 89 in a regression framework. Specifically, if trends in X are the same for always-separators, always-separators and compliers, never-separators, and never-separators and compliers, the conditional expectations of characteristics can be estimated from the regression below:

$$X_{ira} = \alpha + \kappa_r + \lambda_a + \delta D_{ira} + \gamma Z_{ra} + \varphi D_{ira} \times Z_{ra} + \epsilon_{ira}. \qquad (100)$$

This regression implies common trends across the four identified groups since the values of D and Z do not affect the trends κ, λ. The sample estimators are $E[X|r, a, D_1 = 1] = \hat{\alpha} + \hat{\kappa_r} + \hat{\lambda_a} + \hat{\delta} + \hat{\gamma} + \hat{\varphi}$, and $E[X|r, a, D_0 = 1] = \hat{\alpha} + \hat{\kappa_r} + \hat{\lambda_a} + \hat{\delta}$.

Under slightly weaker assumptions, not requiring us to assume parallel trends for never-separators and compliers, we can use the following regression to estimate the required conditional expectations in equation 34 by interacting the trend variables with D so that we can estimate separate trends for (1) always-separators and (2) never-separators and compliers:

$$X_{ira} = \alpha + \kappa_r + \lambda_a + \delta D_{ira} + \tilde{\kappa_r} \times D_{ira} + \tilde{\lambda_a} \times D_{ira} + \gamma Z_{ra} + \varphi D_{ira} \times Z_{ra} + \epsilon_{ira} \qquad (101)$$

We then have $E[X|r, a, D_0 = 1] = \alpha + \kappa_r + \lambda_a + \delta + \tilde{\kappa_r} + \tilde{\lambda_a}$ and $E[X|r, a, D_1 = 1] = \alpha + \kappa_r + \lambda_a + \delta + \tilde{\kappa_r} + \tilde{\lambda_a} + \gamma + \varphi$.

Our approach can also be extended to estimate complier characteristics in the $Z = 0$ cells if one of the following assumptions holds:

Assumption 6 (a). Age and region trends are the same for always-separators and compliers, or

Assumption 6 (b). Age and region trends are the same for always-separators and never-separators and either the proportion of compliers or never-separators is constant across age and region.