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

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Abstract

We provide direct micro-evidence on how workers' improved outside option leads to job destruction. We then shed light on the *characteristics* of these marginal jobs that are resolved, tracing a range of proxies for low job surplus. Our laboratory is an increase in unemployment benefit duration by 300% (from 1 to 4 years). The reform increased separations by 50% (from 2ppt per quarter to 3ppt). As a result, at the end of the five-year reform period, an additional 25% of initial jobs separated (a 11ppt increase over a 39ppt baseline). While the employment relationships that selected into separation were very heterogeneous, they were on average indeed more likely to exhibit low-surplus markers than the surviving jobs. These jobs had lower than average earnings but higher tenure, and were concentrated in stagnating industries such as manufacturing and mining, and shrinking firms. Workers also exhibited idiosyncratic recent sickness and disability, and worked in industries with steeper sickness-age profiles, compressing surplus by either reducing productivity on the job or increasing disutility of performing the job. The marginal workers that separated had a higher propensity to experience longer unemployment spell duration (predicted by pre-reform attributes). Finally, our discrete-choice model clarifies that the pre-separation wage is the net benefit of working and also the reservation wages of those marginal workers, also in their capacity as unemployed job seekers.

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1 Introduction

Employment is the sum of jobs with positive surplus: they leave both the firm and the worker better off in the job than their outside options. Employment falls if shocks and policies shift the surplus distribution to the left, sweeping up the mass of marginal matches. Viable job opportunities for unemployed job seekers shrink, and marginal matches separate. While prominent pivots in theory, marginal workers, firms and matches usually remain anonymous in empirical work, subsumed in average actors' adjustment. In this paper, we shed direct empirical light on the identity and characteristics of these marginal jobs. Our laboratory is the separation response to sharp declines in job surplus arising from workers' nonemployment values, where our instrument draws from an is unemployment insurance reform. How many employment relationships are at the margin and separate in response to reductions in surplus? Which are the marginal jobs, and who are the marginal workers? Do they indeed carry markers of low surplus, or might frictions entail inefficient destruction of high surplus jobs?

To identify and nonparametrically characterize the set of marginal jobs, we adapt tools from the treatment effects literature. Our focus is on separations from existing matches rather than unemployed job seekers – two sides of the same job surplus coin. Our strategy requires two steps and builds on the complier characterization method by Imbens and Rubin (1997) and Abadie (2003). In the first stage, we measure the separation effects of an instrument that reduces surplus in some existing matches (our treatment group) but does not affect other matches (our control group). Second, we compare the distribution of pre-separation attributes of surviving treated matches (which comprise always-separators and compliers) with the control group (which comprises always-takers). Weighting these to distributions by the first stage separation effect, we can nonparametrically back out the distribution of attributes of the compliers: of the marginal jobs and workers that separated due to the surplus shift but would have been preserved otherwise.

The upside of our approach becomes clear when contrasted with the large existing literature that has focused on the unemployed job searcher.[1] If we attempted infer marginal jobs from the jobs unemployed jobs seekers enter, our strategy would need to characterize post-treatment *outcomes*: job characteristics become only observable conditional on a match. The exclusion restriction associated with such an analysis would almost certainly be violated by all interesting instruments affecting surplus across all matches: inframarginal job finders and the composition of jobs will be affected too, along with dynamic selection among the unemployed.[2] Our strategy, in contrast, does not rely on such an exclusion restriction by focusing on pre-reform, pre-separation attributes. Moreover, our focus on separations allows us to *directly observe* no-

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

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

longer-viable jobs' attributes in the pre-treatment cross section of active jobs. This resolves a basic measurement challenge: for the unemployed, only the average accepted job ends up being directly observed, whereas rejected offers require worker surveys[3] or strong parametric assumptions.[4] Our approach therefore provides new insights and a rich, sharp and easy-to-handle method that complements the lessons learned from the canonical focus on unemployed job seekers. Beyond allowing us to shed new light on elementary mechanisms of labor market adjustment, job separations as such are important factors in business cycles,[5] reallocation,[6] and households' exposure to aggregate fluctuations[7] and idiosyncratic lifetime earnings risk.[8]

We implement our strategy in the laboratory of unemployment insurance to identify a clean, quasi-experimental shift in job-specific surplus arising from workers' nonemployment value. We study a reform that increased maximum benefit duration from one year to four years in Austria, which was in place between 1988 and 1993. The system featured a replacement rate of around 40%, very similar to the U.S. rate, and no experience rating, as in most OECD countries but unlike in the United States.[9] Even quitters were eligible for UI and the extension, which ensures a reduction of surplus across *all* matches rather in those at risk of layoff. The quasi-experimental features of the reform allow for comparisons along three dimensions to rule out potential region-, time- or age-specific confounds. First, the policy was implemented in 1988 and ended in 1993. Second, the policy only applied to 28 out of 100 regions. Third, the policy applied only to workers aged 50 and above (no change for younger workers). In a difference-in-discontinuity design, we compare these older eligible workers to 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.[10] We use population administrative data with workers' daily earnings biographies and precise date of birth. 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.[11] The

[3]For worker survey evidence on reservation wages, see Krueger and Mueller (2016) and Le Barbanchon et al. (2017).

[4]See Heckman (1979) for discussion of the econometrics of censored reservation wages.

[5]The role of separations in aggregate employment fluctuations is explored in Elsby et al. (2013) for the cross section of OECD countries.

[6]See Acemoglu and Shimer (2000) for theoretical work on the effects of UI on optimal investments; Hombert et al. (2014) for self employment.

[7]Davis and von Wachter (2011) measure drop in lifecycle income following displacement and its cyclicality; Berger et al. (2016) draw the conclusions for monetary policy.

[8]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 unemployment.

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

[10]While Lalive et al. (2015) document local search spillovers between unemployed job seekers, these spillovers would be netted out across our age discontinuity.

[11]In the model of Prescott et al. (2009), extensive-margin Frisch elasticities play out at 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 Manoli and Weber (2016)), Gelber et al.

confluence of these factors provides a unique laboratory to document, trace and characterize the marginal matches and workers, and their response to surplus reduction.

We find sizable separation responses to the reform, which implies that a large mass of the treated employment relationships carried low surplus. This step is our first stage. We present visually transparent evidence of the causal effect of UI generosity on separations by largely plotting raw data on labor market outcomes by birth cohort, separately for eligible workers aged 46, 47,..., compared to eligible cohorts right above the sharp age discontinuities of eligibility aged 50, 51 during, after and before the reform. Among the initially active jobs at the onset of the reform, only 40% of jobs survive through the five-year policy period, compared to 60% in the control regions. By contrast, the slightly younger – ineligible – control cohorts see no differential separation behavior before, during or after the reform. Importantly, while Austria has no experience rating, these separations are not temporary layoffs (e.g. Feldstein 1977): instead, we find that the separators move into long-term nonemployment, almost entirely accounted for by the reduction of employment with the initial employer. Similarly, at the quarterly frequency, the probability of moving from employment into unemployment increased from 2 percentage points to 3 percentage points among treated workers.

Our reduced-form evidence for separation responses to shifts in workers' nonemployment value can be understood in a simple discrete-choice framework in which the employer and the employee maintain a match as long its internal value exceeds that of the two parties' outside options combined, and a bilaterally efficient wage is found that leaves each party to strictly prefer her share of the pie to her outside option. While we cast our conceptual framework in a quasi-static form in the main part of the paper, we develop a full dynamic equilibrium model in the Appendix that features labor demand, idiosyncratic shocks in matches, as well as long-term employment relationships with workers transitioning between unemployment and employment. 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 that positive surplus equal to the policy-induced increase of the workers' nonemployment value. The latter match becomes the new marginal match, now with zero surplus under the reform regime. Under these Coasean conditions, our treatment effects on separation quantified the mass of pre-reform matches between the old and the new marginal match. With frictions, inefficient separations may emerge among positive-surplus jobs, in form of one-sided layoffs or quits.

We next take the predictions from the model to the data to dissect the characteristics of marginal jobs and marginal workers. In order to do so, we non-parametrically characterize the pre-reform attributes of jobs that were resolve in response to the policy, i.e. the *compliers* whose job would have survived in the absence of an improved outside option. Specifically, we adopt the methodology from Imbens and Rubin (1997) and Abadie (2003) to identify complier

(2017) and Kyyrä and Wilke (2007) and Inderbitzin et al. (2016).

attributes and apply the methodology to attributes that directly enter the discrete choice problem determining employment. Beyond a characterization of marginal jobs and workers, we test whether the matches resolved by the policy carry markers of low surplus or whether the reform destroyed high surplus jobs.

Compared to workers whose job survives, we find that marginal workers have several markers of low surplus as indicated by lower wages and firm fixed effects and having worked in declining industries with high shares of sickness among older workers. Specifically, marginal workers earned, on average, 4% less than never-separators, about half of which can be attributed to lower firm fixed effects. The lower firm fixed effect is economically highly relevant as it indicates low value-added per worker (Card et al. (ming)) and thus low surplus. Turning to the jobs that these workers separated from, we find that marginal jobs are overwhelmingly (72%) manual, blue-collar jobs, while a smaller fraction (57%) of never-separators stems from that segment of the labor market. These results are mirrored by the industries from which marginal jobs stem, as the ones with the highest shares, mining and manufacturing, are characterized by manual labor intensity. Analyzing a proxy for changes in labor demand, we find that marginal jobs stem primarily from stagnating industries and, within those industries, from shrinking firms (67% compared to 51% among never-separators). We also investigate shifts in labor supply that reduce the surplus of employment by considering the incidence of sickness and disability among older workers in an interest. Our result provide strong evidence that marginal jobs were in industries where the incidence of sickness and disability of older workers was high at baseline, suggesting a high disutility of working or a low productivity of older workers. Both potential interpretations have in common that they indicate low levels of surplus among marginal workers.

We also trace the nature of the separations, and find that they combine both layoffs and quits.[12] We construct an administrative proxy for quits by exploiting the mandatory 28-day wait period for UI, after which even worker quits are eligible for full UI. We find a positive treatment effect on this quit proxy, which implies that the separations induced by the surplus reduction contained quits or that some inframarginal separations shifted towards quits.[13] Second, we document an increase in layoff proxies among the compliers, where we exploit an institutional feature in the severance payment structure upon layoffs to learn about the increase in layoffs among the treatment (reform) regions. Third, we draw from a worker level survey that retroactively asks workers to classify the type of separation of the worker's last job, and

[12]Our benchmark framework appeals to a Coasean set-up with surplus shared through bargaining serves. With frictions, positive-surplus jobs may be destroyed by one-sided layoffs or quits. Other variants point to feedback mechanisms from worker outside option onto firm surplus, such as efficiency wage theories (Shapiro and Stiglitz (1984), Akerlof and Yellen (1986), Katz (1986b), or firing costs arising from reputational or morale concerns on part of employers (Bewley (2002), Hall and Lazear (1984), Lazear (1981), Lazear (1979)). Finally, firing regulations, unions or formal wage contracts, recruitment or training costs for replacement hires should up on the firm's side.

[13]Since this variable is an *outcome* variable and always-separators might be affected by the treatment in their layoff/quit decision, we cannot characterize compliers by quit composition.

find an increase in quits and layoffs among the treated cohorts.

Importantly, our model clarifies that the compliers' pre-separation wages are reservation wages, an important but elusive measurement object in standard investigations on already-unemployed's job search decisions.[14] We find that compliers carry a wide spectrum of pre-separation wages. The compliers' wage distribution also corresponds to the net benefit of non-work for the marginal workers that drives the aggregate labor supply curve, and a key variable in the literature on labor market monopsony (Manning (2003)), which has estimated the elasticity of separations with respect to endogenous shifts in firm-specific wages.

We argue that the quantity and quality of existing jobs destroyed by UI can shed new light on the costs and benefits of unemployment insurance. The existing literature has assessed the benefits of unemployment insurance almost exclusively by investigating the quantity and quality of new jobs. The paradigm of exogenous separations has also guided normative studies of optimal insurance design.[15] Important exceptions have studied separations, although largely in the context of temporary layoffs and recalls (Feldstein (1976), Feldstein (1978), Topel (1983), Topel (1984), Card and Levine (1994), Katz (1986a)). and Lalive et al. (2011) find inflow effects in the Austrian UI context, using regression-based difference-in-difference methods with regards to a national reform during our sample period, and compare inflow and outflow effects in a steady-state analysis. Winter-Ebmer (2003) pioneers the study of inflow effects in the context our REBP reform, distinguishing two worker groups (all below 50 vs. 50-65) using a 2% sample of our population data. Neither paper uses the separation responses to study marginal matches. In Section 7, we revisit and extend that work in our population data and provide transparent graphical evidence for separation effects that includes parallel pre-trends. Those studies focus on the quantity of separations, not the quality, and do not study the effect in the context of marginal matches. 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). The job finding rate on its own would only account for around 30% of that increase. It will be interesting to study whether separations also play an important role in other UI reforms and for younger workers. We plan to examine different reforms in a follow-up study.

We start with a conceptual framework for the separation process and the characteristics of margin jobs in Section 2. In Section 3 we describe the reform we study, the institutional context as well as the administrative micro data sets. In Section 4 we provide causal evidence

[14] Absent frictions and specific investments, both decisions generate a threshold rule that returns a single, identical reservation wage. We reiterate this result in the full model in Appendix Section A.1.

[15] Moral hazard costs of UI arise from *total employment* effects (Baily (1978), Chetty (2006), Britto (2015)).

of UI on separations and job destruction. In Section 5, nonparametrically characterize the compliers along a rich set of worker-, firm- and job-level pre-separation attributes, and we explain our methodology. In Section 6 we categorize the separations into quits and layoffs. Finally, in Section 7, we develop additional implications for policy debates in public finance and macroeconomics: we assess the relative importance of the canonical job finding channel and the understudied separation channel in the costs of unemployment insurance, and discuss selection by job quality; we discuss the selection of unemployed workers by expected spell duration; we also discuss the reservation wages we identify.

2 An Instrumental Variable Strategy to Characterize the Attributes of Marginal Matches

Our conceptual framework aggregates bilateral discrete choices in individual job matches to the market level. It clarifies the treatment effect of the reform on labor market quantities through the separation channel. It will also underlie the econometric technique we adapt to characterize the *compliers* – those workers (jobs) that separated due to the policy but would otherwise have have stayed on the job (remained intact). Appendix Section A.1 presents a full equilibrium model with job finding and separation margins.

2.1 Bilateral Discrete Choice

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. The participation constraint demands that for each party for $i \in \{w, f\}$, the job value be at least her (separation) outside value:

$$V^i_{\text{Stay Matched}}(b, \mathbf{x}) \geq V^i_{\text{Separate}}(b, \mathbf{x}) \tag{1}$$

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, and unemployment insurance b.

Wages. Suppose that the parties can transfer utility (value) by means of "wage" w. 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:

$$V^w_{\text{Stay Matched}}(b,\mathbf{x}) + w \geq V^w_{\text{Separate}}(b,\mathbf{x}) \tag{2}$$
$$V^f_{\text{Stay Matched}}(b,\mathbf{x}) - w \geq V^f_{\text{Separate}}(b,\mathbf{x}) \tag{3}$$

Reservation wages. Reservation wages are the wage thresholds that barely keep the respective party prefer the match to the outside option:[16]

$$\underline{w}^w(b,\mathbf{x}) = V^w_{\text{Separate}}(b,\mathbf{x}) - V^w_{\text{Stay Matched}}(b,\mathbf{x}) \tag{4}$$
$$\overline{w}^f(b,\mathbf{x}) = V^f_{\text{Stay Matched}}(b,\mathbf{x}) - V^f_{\text{Separate}}(b,\mathbf{x}) \tag{5}$$

Bargaining set. Wages within the bargaining set $w \in [\underline{w}^w, \overline{w}^f]$ maintain the match.

Job surplus. Job surplus is the differennce 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:

$$S(b,\mathbf{x}) = V^w_{\text{Stay Matched}}(b,\mathbf{x}) + V^f_{\text{Stay Matched}}(b,\mathbf{x}) - V^w_{\text{Separate}}(b,\mathbf{x}) - V^f_{\text{Separate}}(b,\mathbf{x}) \tag{6}$$

Surplus, bargaining and bilateral efficiency. In our Coasean benchmark, the parties find a wage within the bargaining set $w \in [\underline{w}^w, \overline{w}^f]$ 5hat implements the *bilaterally efficient* allocation: forming and maintaining matches that carry non-negative surplus, i.e. where the firm's reservation (maximum) wage weakly exceeds the workers reservation (minimum) wage:[17]

$$V^f_{\text{Stay Matched}}(b,\mathbf{x}) - V^f_{\text{Separate}}(b,\mathbf{x}) = \boxed{\overline{w}^f(b,\mathbf{x}) \geq w \geq \underline{w}^w(b,\mathbf{x})} = V^w_{\text{Separate}}(b,\mathbf{x}) - V^w_{\text{Stay Matched}}(b,\mathbf{x})$$

Marginal jobs. Marginal jobs are barely viable jobs with *exactly zero surplus*:

$$\boxed{0 =}\ V^w_{\text{Stay Matched}}(b,\mathbf{x}) + V^f_{\text{Stay Matched}}(b,\mathbf{x}) - V^w_{\text{Separate}}(b,\mathbf{x}) - V^f_{\text{Separate}}(b,\mathbf{x}) \tag{9}$$

[16] Absent frictions in firing or specific investments, this reservation wage equals the one the marginal worker applies in her capacity as an *unemployed job seeker*. Analogous discrete choices are made at the rejection margin.

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

$$\max_w \left([V^w_{\text{Stay Matched}}(b,\mathbf{x}) + w] - V^w_{\text{Separate}}(b,\mathbf{x})\right)^{\beta} \cdot \left([V^f_{\text{Stay Matched}}(b,\mathbf{x}) - w] - V^f_{\text{Separate}}(b,\mathbf{x})\right)^{1-\beta} \tag{7}$$
$$\Rightarrow w^N = [V^w_{\text{Separate}}(b,\mathbf{x}) - V^w_{\text{Stay Matched}}(b,\mathbf{x})] + \beta \cdot S(b,\mathbf{x}) \tag{8}$$

While a range of wages maintains positive-surplus matches, only one wage fulfills both parties' participation constraint for zero-surplus matches, namely the reservation wages of each party:

$$V^f_{\text{Stay Matched}}(b,\mathbf{x}) - V^f_{\text{Separate}}(b,\mathbf{x}) = \boxed{\overline{w}^f(b,\mathbf{x}) = w = \underline{w}^w(b,\mathbf{x})} = V^w_{\text{Stay Matched}}(b,\mathbf{x}) - V^w_{\text{Separate}}(b,\mathbf{x})$$

That is, for the marginal match, the workers' reservation wage equals the firm's reservation wage, which in turn equals the net value of employment over nonemployment to the worker, and the productivity of the match (net of vacancy value, potential firing cost,..) to the firm. By moving across the extensive margin, marginal matches drive adjustment in aggregate employment.

Efficient separations. In the Coasean setup, separations occur if and only if surplus would turn negative, e.g. through shifts in $\mathbf{x}$ such as productivity, the worker's nonemployment value, expectations, or tastes. Here, no wage w falls within the bargaining set $w \in [\underline{w}^w(b,\mathbf{x}), \overline{w}^f(b,\mathbf{x})]$, i.e. if $\underline{w}^w(b,\mathbf{x}) > \overline{w}^f(b,\mathbf{x})$. Even at the firm's maximum wage, the worker prefers separating:

$$V^w_{\text{Stay Matched}}(b,\mathbf{x}) + \overline{w}^f(b,\mathbf{x}) < V^w_{\text{Separate}}(b,\mathbf{x}) \tag{10}$$

$$\Leftrightarrow V^w_{\text{Stay Matched}}(b,\mathbf{x}) + V^f_{\text{Stay Matched}}(b,\mathbf{x}) < V^w_{\text{Separate}}(b,\mathbf{x}) + V^f_{\text{Separate}}(b,\mathbf{x}) \tag{11}$$

and similarly for the firm: $V^f_{\text{Stay Matched}}(b,\mathbf{x}) - \underline{w}^w(b,\mathbf{x}) < V^f_{\text{Separate}}(b,\mathbf{x})$.

2.2 Moving From Model to Measurement of Marginal Matches

Which are the marginal jobs and workers? In our framework, we rephrase this question into more tangible terms: Which *attributes* $\mathbf{x}$ are associated with marginal jobs, and are match formation and separation efficient? Of course, inference from the set of viable, active matches, merely provides a moment inequality, since $S(\mathbf{x}', b) \geq 0$ for those separating matches. In this Section, we first clarify that separations as such also convey no sharp information about marginal attributes, since they reveal the complementary inequality of negative surplus $S(\mathbf{x}', b) < 0$. By contrast, however, we argue that an instrumental variables strategy that shifts the surplus allows us to nonparametrically back out the set of marginal workers, their attributes and their surplus proxies.

Heterogeneity. Real-world distributions of job, firm and worker characteristics may come in arbitrary combinations from a latent distribution $G(\mathbf{x})$ with arbitrary covariances between the individual characteristics. The realized and observed distribution of employment relationships is described by a distribution $F(\mathbf{x})$ of the $\mathbf{x}$-combinations of matches with non-negative surplus.

Law of motion for attributes x. Match attributes $\mathbf{x}$ may evolve, for matches to ever separate. We assume a Markov process $g(\mathbf{x}'|\mathbf{x})$, which denotes the stationary and homogeneous

law of motion of attributes: the probability of $\mathbf{x}'$ following $\mathbf{x}$.[18]

Separation rate. The probability of separation for a currently viable match $\mathbf{x}$ (i.e. $S(\mathbf{x}, b) \geq 0$) in environment b is the probability that its next-period surplus were to turn negative:

$$\delta(\mathbf{x}, b) = \text{Prob}\left[S(\mathbf{x}', b) < 0 | \mathbf{x}, b\right] = \int 1(S(\mathbf{x}', b) < 0) g(\mathbf{x}'|\mathbf{x}) d\mathbf{x}' \tag{12}$$

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

$$\overline{\delta}(b, F(\mathbf{x})) = \int \text{Prob}\left[S(\mathbf{x}', b) < 0 | \mathbf{x}, b\right] dF(\mathbf{x}) = \int \int 1(S(\mathbf{x}', b) < 0) g(\mathbf{x}'|\mathbf{x}) d\mathbf{x}' dF(\mathbf{x}) \tag{13}$$

Econometric challenges associated with measuring marginal workers from separations. The allocative attribute vector with respect to which the surplus is defined and thus the separation decision is made, is $\mathbf{x}'$, rather than $\mathbf{x}$.

(i) Censoring. Exactly for the separations, the match-related subset of attributes of $\mathbf{x}'$ becomes censored at the time of separation, for example wages. This challenge is analogous to the unobservability of unemployment job seeker's rejected job offers.

(ii) Endogeneity. Even those attributes that remain observable in $\mathbf{x}'$ despite separations, are endogenous for stayers. For example, health outcomes, consumption or time use are observed only in the separation state of separation and may be affected by the separation. They are not observed in the counterfactual state of work, which would have determined the job surplus.

(iii) Inframarginality. Could examining $\mathbf{x}$ instead of $\mathbf{x}'$ provide interesting information, at least for the subset of surplus-relevant attributes that are expected to remain stable even during separations? While pre-separation attributes $\mathbf{x}$ are observed and may be exogenous to the separation, they in general do not convey sharp iinformation about separation attributes $\mathbf{x}'$ of the *marginal match*, or the efficiency of separations or the distribution of surplus among resolved matches – without making strong assumptions about the law of motion of job attributes $g(\mathbf{x}'|\mathbf{x})$.[19] Specifically, separators' attributes are associated with surplus of $\in (-\infty, 0]$ and therefore represent largely inframarginal matches. The set of unconditional separation events

[18] 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 $p_t(\mathbf{x}'|\mathbf{x})$. Homogeneity between treatment and control groups is necessary.

[19] If $g(\mathbf{x}'|\mathbf{x})$ implied persistent attributes, then separations will always come from barely marginal jobs. However, separator attributes $\mathbf{x}'$ are not observed, exactly for the separation-relevant $g(\mathbf{x}'|\mathbf{x})$, the data are censored. Moreover, empirical evidence on labor market volatility suggests that $g(\mathbf{x}'|\mathbf{x})$ wide dispersion in attribute shifts period-by-period (e.g. Guvenen (2009)).

provides therefore provide as little information as stayers, who carry surplus $\in [0, \infty)$. Neither moment inequality provides sharp information about the marginal matches absent strong structural assumptions.

Strategy: binary surplus shift instrument to identify marginal workers. Our solution to these challenges is to employ a binary instrument that induces separations by reducing surplus. Recognizing that the instrumented distribution is an average (weighted by the number of incremental separations) of the uninstrumented distribution and the incremental marginal separations due to the policy, we can back out the marginal matches' characteristics. We do so by comparing the distribution of separators' pre-separation attributes $\mathbf{x}$ between the two regimes. This strategy draws from complier characterization, and the econometric method is described in the following Sections. This strategy overcomes the inframarginality challenge (iii).

Information content of marginal attributes. By zooming into the observations that in the treatment effects literature are known as "compliers", we can describe marginal matches by a rich set of characteristics, and can examine whether they indeed exhibit proxies for low surplus. Specifically, we characterize marginal matches along their pre-separation attributes. This strategy is particularly useful for the subset of surplus-relevant characteristics that are predictive, such as persistent attributes (e.g. education), or slow-moving trends (e.g. declining industries). Our strategy does not overcome challenges of (i) censoring and (ii) endogeneity of the $\mathbf{x}'$ vector.[20] We therefore identify either identify factors that directly lower surplus (such as sickness, low productivity, high outside options) or that are symptoms of those (hospital visits, low performance evaluations, late arrivals), or associated correlates. The $\mathbf{x}$ vector of attributes we obtain is therefore descriptive. However, obtaining observables associated with marginality is useful in a variety of ways, such as:

(i) Marginality markers. The attributes we identify can be used to predict marginality in other contexts, and it will be interesting to compare marginal attributes between different instruments, i.e. whether compliers that exhibited the most responsive behavior in response to UI policy also react to other shocks to the nonemployment value similarly.

(ii) Distributional incidence of shocks and policies. The attributes shed light on the distributional incidence of a particular policy. It will be interesting to know whether the policy destroyed high-wage or low-wage jobs, what the productivity might have been, and so forth.

[20] To the degree that a strong exclusion restriction holds (that the instrument affects matches only through surplus, but that the instrument does not affect any $\mathbf{x}'$ through any channel incl. the separation decision), we could even distill the on-the-job characteristics $\mathbf{x}'$. However, this exclusion restriction is almost certaintly violated in our case.

(iii) Reservation wages. Our discrete-choice model clarifies that the pre-separation wage is the net benefit of working and also the reservation wages of those marginal workers, also in their capacity as unemployed job seekers.

(iv) Surplus proxies. We are particularly curious about the subset of attributes that is highly related to surplus; we loosely refer to such attributes as "**surplus proxies**". First, the degree to which the compliers exhibit low surplus markers serves as a test of our benchmark model, by which the marginal match carries low surplus, and helps us adjudicate between this model and the competing, frictioneless model. Second, the efficiency costs of a policy are dramatically different if marginal matches may, due to frictions that entail inefficient separations in non-Coasean settings, carry positive surplus.

Our instrument: reduction in worker's nonemployment value Δb from unemployment insurance. We explore the effects of a quasi-experimental variation in workers' nonemployment value. Our particular laboratory is unemployment insurance generosity $b = b_0 + Z \times \Delta b$, which generates the quasi-experimental setting. Instrument Z increases generosity by $\Delta b > 0$, and therefore reduces surplus by increasing the worker's value of nonemployment:[21]

$$\begin{aligned}
\Delta S(b_0 + Z \times \Delta b, \mathbf{x}) &= S(b_0 + \Delta b, \mathbf{x}) - S(b_0, \mathbf{x}) && (16)\\
&= \Delta V^w_{\text{Stay Matched}}(b_0 + Z \times \Delta b, \mathbf{x}) - \Delta V^w_{\text{Separate}}(b_0 + Z \times \Delta b, \mathbf{x}) && (17)\\
&\quad + \Delta V^f_{\text{Stay Matched}}(b_0 + Z \times \Delta b, \mathbf{x}) - \Delta V^f_{\text{Separate}}(b_0 + Z \times \Delta b, \mathbf{x}) && (18)
\end{aligned}$$

The transmission channels of unemployment insurance into surplus. Since most plausible channel of surplus reduction from Δb works through the worker's nonemployment value. Indeed, in the context we consider, 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 (1986b)). Indirectly, surplus may fall if 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 nonemployment value.

[21] where

$$\begin{aligned}
\Delta V^w_{\text{Stay Matched}}(b_0 + Z \times \Delta b, \mathbf{x}) &= V^w_{\text{Stay Matched}}(b_0 + \Delta b, \mathbf{x}) - V^w_{\text{Stay Matched}}(b_0, \mathbf{x}) && (14)\\
\Delta V^w_{\text{Separate}}(b_0 + Z \times \Delta b, \mathbf{x}) &= V^w_{\text{Separate}}(b_0 + \Delta b, \mathbf{x}) - V^w_{\text{Separate}}(b_0, \mathbf{x}) && (15)
\end{aligned}$$

As in the Austrian context we examine, b is paid for quits and layoffs. b may even enter the firm's separation value because it may affect recruitment costs or the quality of replacement hires. Or, b may reduce layoffs costs in certain implicit contract models.

Effects of instrument on reservation values and separations. The instrument shifts the surplus distribution to the left. Which matches are resolved following the regime shift to $b_0 + \Delta b > b_0$ in the treatment group ($Z = 1$), compared to a control group ($Z = 0$) with stable $b = b_0$? First, the shift Δb destroys *marginal jobs* for whom the weak inequality just holds absent the reform:

$$\boxed{0 =} \; S(b_0, \mathbf{x}') = V^w_{\text{Stay Matched}}(b, \mathbf{x}') + V^f_{\text{Stay Matched}}(b, \mathbf{x}') - V^w_{\text{Separate}}(b, \mathbf{x}') - V^f_{\text{Separate}}(b, \mathbf{x}') \quad (19)$$

Second, Δb *sweeps up* near-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, post-reform marginal match by its *no-reform* counterfactual surplus:

$$S(b_0, \mathbf{x}') + \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') = 0 \quad (20)$$

$$\Leftrightarrow S(b_0, \mathbf{x}') = \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') \quad (21)$$

Δb therefore triggers separation of matches with surplus $[0, \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')$. $\Delta S(b_0 + Z \times \Delta b, \mathbf{x}')$ acts as an *upper bound* on the value of the matches destroyed in response to the policy.

Partitioning the set of separations. The separation rate in the control group ($Z = 0$) is driven by the *Always-Separators*: the subset of matches that already separate in the old regime $b = b_0$ and therefore would separate also in the new regime $b = b_0 + \Delta b$ for $\Delta b \geq 0$):

$$\overline{\delta}^{Z=0} = \overline{\delta}(b_0, F(\mathbf{x})) = \int \delta^{Z=0}(\mathbf{x}, b_0) dF(\mathbf{x}) \quad (22)$$

Treatment group ($Z = 1$) separators are those matches that separate under $b = b_0 + \Delta b$, and therefore comprise Always-Separators and Compliers:

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

Compliers are matches that separate under, but would continue absent, the instrument (reform). They drive the *first stage* of the instrument on separations. Their fraction is:

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

Never-Separators separate neither with nor without the instrument. They correspond to the stayers (non-separators) in the treatment region, remaining viable under b as well as $b + \Delta b$:

$$1 - \overline{\delta}^{Z=1} = \int \text{Prob}\left[S(\mathbf{x}', b) \geq \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') | \mathbf{x}, b_0\right] dF(\mathbf{x}) \tag{23}$$

2.3 The Pre-Separation Attributes of Marginal Matches

Compliers are the subset of the matches that are resolved under (in response to) the policy but would have survived otherwise. While a standard treatment effect on separations estimates the fraction of compliers, the attributes of compliers are not directly measured in the data. But we present a simple formulate for a given mean to back out the complier characteristics from the first stage and the combination of the partitioned jobs. The results of this strategy are presented in Section 5. Given the nonparametric identification of the entire distribution, we can trace out any moment. Here, we first illustrate this procedure at a concrete example of one moment of that distribution, the mean of an arbitrary attribute from vector $\mathbf{x}$. That attribute can be decision relevant (e.g. productivity) or irrelevant (e.g. last digit of social security number).

The *Complier* mean of that attribute x – which is not directly observed in the data – is:

$$\bar{\mathbf{x}}^{\text{C}} = \mathbb{E}\left[\mathbf{x} | \, 0 \leq S(\mathbf{x}', b_0) < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')\right] \tag{24}$$

Moments from the distribution of complier characteristics $\mathbf{x}$ identify the pre-separation attributes of matches that separate. 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 $(0, \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')]$. Complier characteristics are associated with those surplus.

Measuring the characteristics of marginal matches destroyed by the reform. Importantly, complier characteristics do not predict separation in general. Conversely, attributes of separators are usually not informative of marginality.

A clean illustration is the control group, i.e. the baseline environment with $b = b_0$. Here, we observe the Always-Separators' attributes $\bar{\mathbf{x}}^{\text{AS}}$. *Always-Separators* are those matches that separate even in the regime b and therefore also in regime $b + \Delta b$, and have surplus $\in (-\infty, 0]$.

$$\bar{\mathbf{x}}^{\text{AS}} = \mathbb{E}\left[\mathbf{x} | \, S(\mathbf{x}', b_0) < 0 < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')\right] \tag{25}$$

Never-Separators remain viable under b as well as $b + \Delta b$, carrying surplus $\in [0, \infty)$:

$$\bar{\mathbf{x}}^{\text{NS}} = \mathbb{E}\left[\mathbf{x} | \, S(\mathbf{x}', b_0) \geq \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') > 0\right] \tag{26}$$

In the treatment group, *Treatment Separators* are observed, who are the union of Always-Separators and Compliers, and thus their attributes are $\mathbf{x}^{\text{AS}\cup\text{C}}$:

$$\bar{\mathbf{x}}^{\text{AS}\cup\text{C}} = \mathbb{E}\left[\mathbf{x}\middle|\, S(\mathbf{x}', b_0) < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')\right] \tag{27}$$

$$= \frac{\bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1}} \times \bar{\mathbf{x}}^{\text{AS}} + \frac{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1}} \times \bar{\mathbf{x}}^{\text{C}} \tag{28}$$

As a union, these observations are "even less" informative about marginal workers as those matches are associated with surplus of $(-\infty, \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')]$, an even larger set than the Always-Separators $(-\infty, 0]$.

However, *combining* the sets of the Treatment-Separators and the Always-Separators allows us to distill the Compliers' attributes. Concretely, the observed average $\mathbf{x}$ is just the average of the 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:

$$\bar{\mathbf{x}}^{\text{C}} = \frac{\bar{\delta}^{Z=1}}{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}} \times \bar{\mathbf{x}}^{\text{AS}\cup\text{C}} - \frac{\bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}} \times \bar{\mathbf{x}}^{\text{AS}} \tag{29}$$

Compliers' no-reform counterfactual surplus is in $(0, \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')]$. For small changes in b, $\lim_{\Delta b \to 0} \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') = 0$, we approximate the marginal – i.e. zero-surplus – match absent the reform. This procedure for one particular moment – namely the mean – can be generalized to nonparametrically back out the full distribution of any complier characteristic,

No-Defiers assumption for complier characterization. Always-Separators may leave because of any idiosyncratic shocks to productivity, amenities, tastes, or firing costs, as captured by the various variables. The key assumption for identification is that the distribution of these shifts is unaffected by the policy. That is, absent the policy, the treatment area would have experienced the same distribution of shocks and therefore separations. Another version of this argument is that the policy did not lead individuals that would have separated without the reform to no longer separate under the reform. In econometric terms, the assumption is that there are no defiers. Potential sources of this concern are changes in the marginal product of marginal workers if the treatment may apply unequally. For example, high-wage workers whose nonemployment value is unaffected by Δb (for example because they plan on not taking up unemployment benefits or plan on finding a job quickly after the separation) may decide to stay employment in response to increases in their marginal product because of the separations by substiutable Δb-sensitive compliers.[22]

[22] That is, spillover effects (search externalities on the market level, production function spillovers in firm or in aggregate), information or peer effects, or taste heterogeneity such that $\xi_i \Delta b$ with $\xi_i < 0$ for some i, for example; or more generally, the partial effect of b on the value functions has the same sign for all workers.

2.4 Econometric Strategy: Identification Assumptions and Implementation

To characterize the marginal jobs resolved 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). 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. 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 $b + Z \cdot \Delta b$.

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

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

δ_0 and δ_1 denote the value that δ takes for $Z = 0$ and $Z = 1$, respectively.

Partitioning the set of pre-reform matches. Based on the discrete choice framework, we define:

$$\begin{aligned}
\delta_0 = \delta_1 = 1\text{: Always-Separators} &\quad \Leftrightarrow S(\mathbf{x}', b_0) < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') \\
\delta_0 = \delta_1 = 0\text{: Never-Separators} &\quad \Leftrightarrow S(\mathbf{x}', b_0) \geq \Delta S(b_0 + Z \times \Delta b, \mathbf{x}') \\
\delta_0 = 0, \delta_1 = 1\text{: Compliers (marginal matches)} &\quad \Leftrightarrow 0 \leq S(\mathbf{x}', b_0) < \Delta S(b_0 + Z \times \Delta b, \mathbf{x}')
\end{aligned}$$

Attributes. We define X to be attributes that we characterize for compliers and the other two groups.

Assumptions. For the following, we make a set assumptions similar to but distinct from the assumptions in Abadie (2002, 2003):

1. Independence: δ_1, δ_0 and X are independent of Z.
2. First stage: $0 < P(Z = 1) < 1$ and $P(\delta_1 = 1) > P(\delta_0 = 1)$
3. Monotonicity: $\delta_1 - \delta_0 \geq 0$

Note that the independence and the first stage assumption is *different* from Abadie (2003). Specifically, Abadie (2003) assumed that *conditional on* X, Y – not needed in our setup – and δ are independent of Z whereas we assume that X and δ are independent of Z. Moreover, the first stage assumption in Abadie (2003) needs to hold *conditional on* X. Under the assumption above, it does not need to hold conditional on X.

Given our assumptions, we can write the conditional expectation of X given $\delta_1 = 1$ as follows:

$$\begin{aligned} E[X|\delta_1 = 1] &= E[X|S_1 = 1, \delta_0 = 1] \cdot P(\delta_0 = 1|S\delta_1 = 1) \\ &\quad + E[X|\delta_1 = 1, \delta_0 = 0] \cdot P(\delta_0 = 0|\delta_1 = 1) \end{aligned} \tag{30}$$

Rearranging yields

$$E[X|S_1 = 1, \delta_0 = 0] = \frac{1}{P(\delta_0=0|\delta_1=1)} \cdot E[X|\delta_1 = 1] - \frac{P(\delta_0=1|\delta_1=1)}{P(\delta_0=0|\delta_1=1)} E[X|\delta_1 = 1, \delta_0 = 1] \tag{31}$$

Monotonicity ($\delta_1 - \delta_0 \geq 0$) implies $E[X|\delta_1 = 1, \delta_0 = 1] = E[X|\delta_0 = 1]$, which implies:

$$E[X|\delta_1 = 1, \delta_0 = 0] = \frac{1}{P(S_0=0|\delta_1=1)} \cdot E[X|\delta_1 = 1] - \frac{P(\delta_0=1|\delta_1=1)}{P(\delta_0=0|\delta_1=1)} E[X|\delta_0 = 1] \tag{32}$$

Due to independence, we can write $E[X|\delta_1 = 1] = E[X|\delta = 1, Z = 1]$ and $E[X|\delta_0 = 1] = E[X|\delta = 1, Z = 0]$.

We can further write $\pi_A = P(\delta_0 = 1)$ and $\pi_{NT} = P(\delta_1 = 0)$ for the probabilities of always-takers and never-takers and, by monotonicity ruling out defiers, have $\pi_C = 1 - \pi_A - \pi_C$.[23]

[23]For clarification, note that $P(\delta_0 = 1|\delta_1 = 1) = \frac{P(\delta_0=1,\delta_1=1)}{P(\delta_1=1)}$ and $P(\delta_0 = 0|\delta_1 = 1) = \frac{P(\delta_0=0,\delta_1=1)}{P(\delta_1=1)}$. We can further write the probability of always-takers $(\delta_0 = 1, \delta_1 = 1)$ as $\pi_A = P(\delta_0 = 1, \delta_1 = 1) = P(\delta_0 = 1, \delta_1 = 1)$ by monotonicity, the probability of never-takers $(\delta_0 = 0, \delta_1 = 0)$ as $\pi_N = P(\delta_0 = 0, \delta_1 = 0)$ by monotonicity, and of compliers $(\delta_0 = 0, \delta_1 = 1)$ as $\pi_C = P(\delta_0 = 0, \delta_1 = 1) = 1 - \pi_A - \pi_N$ as there are no defiers $(\delta_0 = 1, \delta_1 = 0)$, again by the monotonicity assumption.

We thus have the following expression for the complier mean:

$$E[X|\delta_1 = 1, \delta_0 = 0] = \frac{\pi_c + \pi_A}{\pi_c} E[X|S = 1, Z = 1] - \frac{\pi_A}{\pi_C} E[X|\delta = 1, Z = 0] \tag{33}$$

Each term on the right has a corresponding sample mean so the complier means are identified. Note that the characteristic X could be an indicator variable, e.g., $X = 1(H \leq c)$, so that we can in principle characterize distributions of complier characteristics.

Complier characteristics in difference-in-differences IV. We adjust (33) to account for the fact that we condition on region and age fixed effects in our econometric setup. Letting r and a denote region and age, respectively, we make the following, adjusted assumptions:

1. Independence: Conditional on region and age, we have that δ_1, δ_0 and X are independent of Z.

2. Regularity and First Stage Assumption:

   (a) The expectations $E[X|r, a, \delta = 1, Z = 1]$ and $E[X|r, a, \delta = 1, Z = 0]$ exist and are non-degenerate.

   (b) $P(\delta_1 = 1|r, a) > P(\delta_0 = 1|r, a)$

3. Monotonicity: $\delta_1 - \delta_0 \geq 0$

We can write the following:

$$\begin{aligned} E[X|S_1 = 1, r, a] \;=\; & E[X|r, a, \delta_1 = 1, \delta_0 = 1] \cdot P(\delta_0 = 1|r, a, \delta_1 = 1) \\ & + E[X|r, a, \delta_1 = 1, \delta_0 = 0] \cdot P(S_0 = 0|r, a, \delta_1 = 1) \end{aligned} \tag{34}$$

By monotonicity we have that $E[X|r, a, \delta_1 = 1, S_0 = 1] = E[X|r, a, \delta_0 = 1]$. Due to independence, we have that $E[X|r, a, \delta_1 = 1] = E[X|r, a, \delta = 1, Z = 1]$ and $E[X|r, a, \delta_0 = 1] = E[X|r, a, \delta = 1, Z = 0]$. These expectations exist and are non-degenerate by assumption 2.(a) above.

Applying the same steps as for the unconditional results, we thus have the following result:

$$\begin{aligned} E[X|r, a, \delta_1 = 1, \delta_0 = 0] = & \frac{\pi_c + \pi_A}{\pi_c} E[X|r, a, \delta = 1, Z = 1] \\ & - \frac{\pi_A}{\pi_C} E[X|r, a, \delta = 1, Z = 0] \end{aligned} \tag{35}$$

We can obtain the sample means for the never-taker, always-taker, and complier shares.

Formula (35) is the econometric analogue of model expression (29), although our expression allows for *conditional* expectations based on controls (r, a). In order to estimate the sample

analogue to $E[X|r, a, D, Z]$, we estimate a model that assumes additive separability in r and a:

$$X_{ira} = \alpha + \gamma_r + \lambda_a + \delta D_{ira} + \gamma Z_{ra} + \epsilon_{ira} \tag{36}$$

Finally, we construct confidence intervals through non-parametric bootstrapping.

3 Institutional Context, Reform, and Data

Our analysis builds on a large unemployment benefit extension that was introduced in 1988 in Austria, which increased the potential UI benefit duration from 1 year to 4 years for treated workers. The design of the reform allows comparisons among three dimensions: region, time, and age. This chapter describes the Austrian labor market at the relevant time, introduces the Regional Extended Benefit Program (REBP) and provides an overview of the data we use.

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 (see Figure 1), much like its Continental European peers such as Germany, Italy, or France. Unemployment insurance is funded through employer- and employee-borne contributions that are proportional to the employees' wages up to a social security earnings maximum. Importantly, the Austrian system does not feature experience rating as in the majority of OECD countries. During our study period until 1989, the replacement rate was around 41% for most employees and capped below and above at a minimum and maximum amount. 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.

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

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 laboratory 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 belong to a large holding company, the Oesterreichische Industrie AG, OeIAG, which was owned by the state. By the mid-1970s, the company suffered from 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 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,

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

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

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 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.

3.3 Administrative Data on the Population of Austrian Private-Sector Employees

Our data come from two different sources: (i) the Austrian Social Security Database (ASSD) and (ii) the Austrian unemployment register. 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

[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.

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

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). Based on this, we can determine individual entries and exits into a firm. The unemployment register contains information on relevant socio-economic characteristics and the community of residence of unemployed workers.

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 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.

ASSD. Our data come from two different sources: (i) the Austrian Social Security Database (ASSD) and (ii) the Austrian unemployment register. The ASSD gives detailed information on the worker?s labor market and earnings histories 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. 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 leverage this information to distinguish between firms in REBP- and Non-REBP-Regions as well as different industries. The unemployment register contains information on relevant socio-economic characteristics and the community of residence of unemployed workers.

We drop all individuals working in the steel sector because the reform was targeted on these workers, who presumably face worse labor market prospects. The reform affected all sectors.

4 The Effect of Benefit Generosity on Job Separations

We investigate the effect of UI extensions on job separation in two steps. First, we consider the sample of employed workers in 1988 for various age groups (birth cohorts), and ask which fraction of jobs separations anytime between the policy is active, from 1988 to 1993, comparing treatment and control regions, and eligible and ineligible cohorts. Second, we also investigate a complementary cut of the data: we sample workers at age 49 and ask which fraction of

employment relationship separates in the subsequent 5-year window.

4.1 Five-Year Retention With Pre-Eligibility Employer

Separation effect among 1988 job holders. In a first step, we test whether 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 documents 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 head-on 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 which was exposed to longer potential benefits by 300%, from one to four years, over a period 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

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.[29] 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

[29]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.

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.

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

5 Complier Characterization: Marginal Jobs and Workers

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 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 high shares of disability or sickness among older workers and lower firm effects. Table 1 provides an overview of means for compliers, always-separators, never-separators, and the whole sample based on pre-reform data from 1988, along with p-values for mean differences.

Wages. Relative to the whole sample (mean EUR 27,000, CPI-deflated to 2003), marginal matches have slightly lower average earnings at EUR 26,700 and fall in between the wages of always-separators (EUR 26,000) and never-separators (EUR 27,900). The order of mean wages for the different groups can be readily interpreted through the discrete choice framework in Section 2: low-surplus matches (always-separators) carry lower wages than high-surplus matches (never-separators) and marginal jobs carry surplus and wages that fall between those of the other two groups. We dissect the mean wages further in Figure 9, which shows the pdf of complier

wages.

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 marginal jobs are associated with, on average, lower firm effects. Specifically, 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. We extract worker and firm effects from the model and also calculate a residual. 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. The residual $r_{it} = \ln w_{it} - \alpha_i - \psi_{J(i,t)}$ represents the idiosyncratic component that is unexplained by either the worker or firm effect, and may capture phenomena like rents, match quality and other match-specific wage terms. As in the AKM literature, the terms are identified off movers between different employers, in the largest connected set.[30]

Figure 10 shows differences in the wage components of compliers relative to the whole sample. The figure reveals that firm and, in particular, worker effects are lower among marginal workers while residuals are slightly higher. The results document that marginal matches are among workers that have permanently lower earnings compared to their peers. Firm effects can be interpreted as a measure of employer profitability or rents in models of imperfectly competitive labor markets and correlate strongly with measures of value added per worker (see Card et al. (ming)). These results thus provide evidence consistent with the notion that marginal jobs have, on average, lower surplus.

Lifecycle earnings profiles. We also shed light on the lifecycle pattern of wages in Figure 11. As for wages in 1988, we consider wages in previous years as complier characteristic and trace out wage dynamics over time for the different groups. As the figure reveals, compliers had steeper wage profiles compared to always- and never-separators. This is consistent with predictions from Lazear (1979), Lazear (1981), which predicts that wage growth is particularly high among jobs that use backloaded compensation to incentivize workers earlier in the career and which posits that surplus at the end of the career is lower in jobs with steeper wage profiles.

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

[30] We follow the estimation procedure in Correia et al. (2016).

tenure in 1988 is 10.75 compared to 10.41 years for the whole sample and 8.75 and 11.58 years for always-separators and never-separators, respectively. As with wages, we find that characteristics of marginal jobs fall between those of always- and never-separators. We also implement a decomposition into worker and firm effects, analogous to the AKM decomposition for wages described above. Figure 13 reveals that compliers had lower worker effects but higher firm effects compared to the whole sample.

Occupational proxy. As a final worker-level characteristic, we investigate the occupational structure of marginal jobs based on a classification into blue- and white-collar occupations. Strikingly, more than 80% of marginal jobs are in blue-collar occupations while the share of 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 14 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.

Age gradient of sickness by industry. First, we investigate a labor supply-based interpretation by investigating the age patterns of sickness and disability across sectors in the pre-reform period. Figure 15 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. This suggests that either disutility of working increases or worker productivity decreases sharply with age in these manual labor-intensive sectors.

Finally, we show that the incidence of sickness among older workers (in the pre-period) in a given sector strongly predicts the share of jobs that are marginal in that sector (see scatter plot in Figure 16, which compares the complier share of the industry against the share sick at age 50). This strongly suggests that marginal jobs are more likely when surplus is low and/or shrinking, for instance when worker productivity is low or the disutility of working is high.

Age gradient of sickness: worker-level life cycle. We also provide an individual-level measure of sickness over time and assess the complier, always- and never-separator lifecycle trajectories (Panel ii.b Figure 11). The figure shows relatively stable patterns over time

for always-separators and never-separators, with always-separators having substantially higher probabilities of previous sickness or disability spells. Strikingly, the prevalence of sickness in marginal jobs, while noisy, increases over time.

Pre-separation employment growth: firm and industry level. In a second 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 stagnating industries which had a a growth rate of 0.5ppt in the time period while both always- and never-separators come from moderately growing industries with growth rates of around 2ppt 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 rate of -4.7ppt while always- and never-separators stem from firms with positive growth rates of 5.5 and 4.3ppt, respectively (see also Figure 18, which shows a histogram of firm growth rates in the three respective groups). We also consider the full time series of firms' employment in Figure 17, which reveals that marginal jobs stem from initially 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.

6 Tracing the Nature of Separations

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 a three ways. 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. Survey data, by contrast, would not allow us to conduct as rich an analysis. Still, given the limited knowledge about the nature and process of separations, let alone in response to shifts in the worker's nonemployment value, we attempt to shed light on this blackbox in three 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, we exploit an administrative wait period for UI eligibility for quits that we can trace in the administrative data. Third, we exploit an institutional feature in the severance payment structure upon layoffs to indirectly learn about the increase in layoffs among the REBP regions and potential shifts

in timing of separations.

6.1 Non-Coasean, Inefficient Separations: Quits and Layoffs

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 efficient 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:

$$w < \underline{w}^w(b, \mathbf{x}) \quad \wedge \quad \overline{w}^w(b, \mathbf{x}) < \overline{w}^f(b, \mathbf{x}) \tag{37}$$

$$\Leftrightarrow \quad V^w_{\text{Stay Matched}}(b, \mathbf{x}) + w < V^w_{\text{Separate}}(b, \mathbf{x}) \quad \wedge \quad V^f_{\text{Stay Matched}}(b, \mathbf{x}) - w \geq V^f_{\text{Separate}}(b, \mathbf{x}) \tag{38}$$

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:

$$w > \overline{w}^f(b, \mathbf{x}) \quad \wedge \quad \overline{w}^w(b, \mathbf{x}) < \overline{w}^f(b, \mathbf{x}) \tag{39}$$

$$\Leftrightarrow \quad V^f_{\text{Stay Matched}}(b, \mathbf{x}) - w < V^f_{\text{Separate}}(b, \mathbf{x}) \quad \wedge \quad V^w_{\text{Stay Matched}}(b, \mathbf{x}) + w \geq V^w_{\text{Separate}}(b, \mathbf{x}) \tag{40}$$

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 $\underline{w}^w(b, \mathbf{x})$:

$$w \boxed{=} \underline{w}^w(b, \mathbf{x}) \quad \wedge \quad \overline{w}^f(b, \mathbf{x}) > \overline{w}^f(b, \mathbf{x}) \tag{41}$$

$$\Leftrightarrow \quad V^w_{\text{Stay Matched}}(b, \mathbf{x}) + w \boxed{=} V^w_{\text{Separate}}(b, \mathbf{x}) \quad \wedge \quad V^f_{\text{Stay Matched}}(b, \mathbf{x}) - w > V^f_{\text{Separate}}(b, \mathbf{x}) \tag{42}$$

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 $\overline{w}^f(b, \mathbf{x})$:

$$w \boxed{=} \overline{w}^f(b, \mathbf{x}) \quad \wedge \quad \overline{w}^w(b, \mathbf{x}) < \overline{w}^f(b, \mathbf{x}) \tag{43}$$

$$\Leftrightarrow \quad V^f_{\text{Stay Matched}}(b, \mathbf{x}) - w \boxed{=} V^f_{\text{Separate}}(b, \mathbf{x}) \quad \wedge \quad V^w_{\text{Stay Matched}}(b, \mathbf{x}) + w \geq V^w_{\text{Separate}}(b, \mathbf{x}) \tag{44}$$

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 (nonemployment) value of the workers. Therefore, we now conjecture that if some matches resolve, 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 nonemployment (net of the amenity).

6.2 Retrospective Classification: Household Survey

In order to further disentangle the types of REBP-induced separations, we leverage rich micro-data 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. Additional evidence shows that the additional 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.[31] 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 19 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 region 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:

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

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

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 four 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,
3. a layoff due to economic reasons,
4. early retirement,
5. regular retirement, or
6. other reasons.[32]

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 20, 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 20.)

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

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

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 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 considering the incidental surplus reduction from our instrument.

6.3 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.[33]

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.

[33]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.

Treatment effect on the fraction of quits in total separations. We conduct a version of our difference-in-discontinuity specification in two ways. First, we replace the separation indicator by a quit indicator, estimating quarterly transition rates. In Figure 22, 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 nonemployment through a quit increases in response to the UI extension.

6.4 Administrative Layoff Proxy: Severance Payment Tenure Thresholds

[incomplete]

7 Implications of the Separation Channel for UI Studies

7.1 Separation vs. Job Finding: Quarterly Worker Transition Rates

Section 4 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.

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. 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.[34] 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 23 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

[34] 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.

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 24 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.

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 25 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:[35]

$$e_{b,t} = e_{b,t-1} - \rho^{EN}_{b,t-1} \cdot e_{b,t-1} + \rho^{NE}_{b,t-1} \cdot (1 - e_{b,t-1}) \tag{45}$$

[35]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 nonemployment includes both unemployment job search and labor force abstention.

To assess the role of each margin, we conduct counterfactuals with this law of motion.[36] 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:[37]

$$\tilde{e}^{EN}_{T,b,t} = \tilde{e}^{EN}_{T,b,t-1} - \rho^{EN}_{T,b,t-1} \cdot \tilde{e}^{EN}_{T,b,t-1} + \boxed{\rho^{NE}_{C,b,t-1}} \cdot \left(1 - \tilde{e}^{EN}_{T,b,t-1}\right) \tag{46}$$

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:

$$\tilde{e}^{NE}_{T,b,t} = \tilde{e}^{EN}_{T,b,t-1} - \boxed{\rho^{EN}_{C,b,t-1}} \cdot \tilde{e}^{NE}_{T,b,t-1} + \rho^{NE}_{T,b,t-1} \cdot \left(1 - \tilde{e}^{NE}_{T,b,t-1}\right) \tag{47}$$

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

$$\tilde{E}^{X}_{T,b\in(b_{\min},b_{\max}),t} = \sum_{b_{\min}}^{b_{\max}} \omega_{T,b,t} \cdot \tilde{e}^{X}_{T,b,t} \tag{48}$$

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 27 plots the underlying population-weighted average transition rate for the REBP and the control regions.

Figure 26 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

[36] 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.

[37] 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.

of the dramatic employment effects from the benefit extension in our analysis of the REBP effects. It will be interesting to study whether separations also play an important role in other UI reforms and for younger workers.[38] We plan to examine different reforms in a follow-up study.

7.2 Macro Studies

7.3 Baily Chetty

7.4 Selection of Compliers by Unemployment Spell Duration

7.5 Reservation Wage Interpretation

8 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).[39] 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 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. Yet, 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 have *directly* documented the link between job surplus and job destruction, by exploiting a laboratory with quasi-experimental shifts in workers' nonemployment value from a large extension of unemployment insurance benefits in Austria between 1988 and 1993. When workers' nonemployment value increases, job surplus shrinks, and marginal matches are resolved. While we provide new evidence on the effect of unemployment insurance on jobs separations, our main focus is on documenting the surplus proxies that characterize these marginal matches.

The empirical picture we paint of the marginal jobs and marginal workers is broadly consistent with the theoretical prediction of low surplus jobs being resolved first, in response to the increase in nonemployment value of the worker. However, we find wide dispersion in a variety of surplus-relevant attributes, which suggests that the UI-induced separations affected a broad cross-section of job types still.

[38] 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.

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

The link between job surplus and employment sensitivity is the core theory of economic models of the labor market. Low (average and homogeneous) job surplus is the fundamental ingredient that underlies successful models of the labor market (Ljungqvist and Sargent (2017)). It also is the core mechanism of equilibrium unemployment in steady state (Mortensen and Pissarides (1994)). Our attempt to link surplus and job viability provides direct micro evidence for this basic mechanism.

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9 Tables

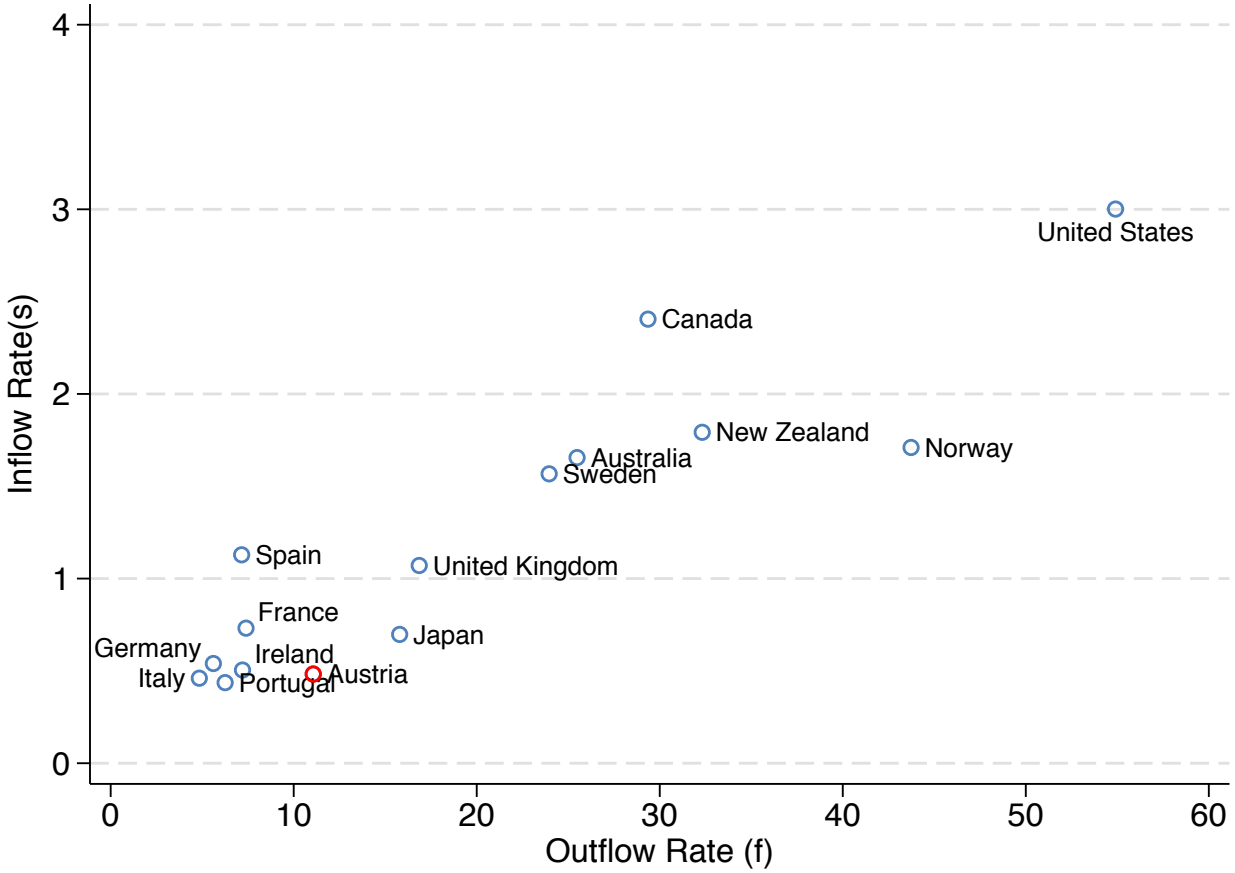
Table 1: Complier Characteristics

Attribute	Comp.	NevS	AlwS	Avg	p-Value of Difference:		
	.				C–N	C–A	C–Avg
Local Unemp. Rate	0.0601	0.0436	0.0435	0.0454	0	0	0
	(0.00111)	(0.00021)	(0.00015)	(0.00022)			
White Collar	0.280	0.426	0.338	0.375	0	0	0
	(0.01564)	(0.00285)	(0.00221)	(0.00276)			
Share Post Tenure	0.514	0.517	0.497	0.509	0	0	0
Cutoff Sep (NACE)	(0.00232)	(0.00039)	(0.00035)	(0.00038)			
Tenure Firm FE	-0.827	-0.841	-1.366	-1.044	0	0	0
	(0.11922)	(0.02275)	(0.01769)	(0.02220)			
Quit Share (Firm)	0.287	0.291	0.270	0.283	0	0.320	0.320
	(0.00438)	(0.00097)	(0.00079)	(0.00087)			
Emp. Growth Firm	-0.0473	0.0394	0.0538	0.0352	0	0	0
	(0.01094)	(0.00207)	(0.00206)	(0.00200)			
Firm Size	529.5	395.6	280.6	366.0	0	0	0
	(18.039)	(3.0549)	(2.8479)	(3.0595)			
Worker FE	0.0969	0.142	0.0893	0.116	0.520	0	0
	(0.00832)	(0.00167)	(0.00110)	(0.00159)			
Firm FE	-0.00910	0.0140	0.00458	0.00773	0.180	0.0600	0.0600
	(0.00932)	(0.00162)	(0.00149)	(0.00161)			
Earnings	27111	28150	26208	27275	0	0.400	0.400
	(236.76)	(44.451)	(33.211)	(44.249)			
Quit Share (Ind.)	0.237	0.244	0.233	0.239	0.100	0.400	0.400
	(0.00250)	(0.00048)	(0.00041)	(0.00045)			
Local URate (50+)	0.212	0.149	0.147	0.155	0	0	0
	(0.00394)	(0.00080)	(0.00060)	(0.00081)			
Emp. Growth Ind.	0.00717	0.0205	0.0173	0.0177	0	0	0
	(0.00200)	(0.00036)	(0.00037)	(0.00032)			
Share Post Tenure	0.552	0.538	0.506	0.527	0	0	0
Cutoff Sep (firm)	(0.00626)	(0.00115)	(0.00097)	(0.00110)			
Tenure	10.91	11.66	8.773	10.45	0	0	0
	(0.17105)	(0.03082)	(0.02845)	(0.03009)			
Tenure Worker FE	-0.208	0.443	-1.060	-0.217	0	0.840	0.840
	(0.08948)	(0.01914)	(0.01530)	(0.01902)			
Ln Earnings	4.273	4.314	4.237	4.279	0	0.340	0.340
	(0.00908)	(0.00170)	(0.00131)	(0.00169)			
1(Growing Firm)	0.325	0.487	0.387	0.430	0	0	0
	(0.01452)	(0.00279)	(0.00245)	(0.00259)			

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 2.4 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 2.4 for the methodology underlying the decomposition into the groups.

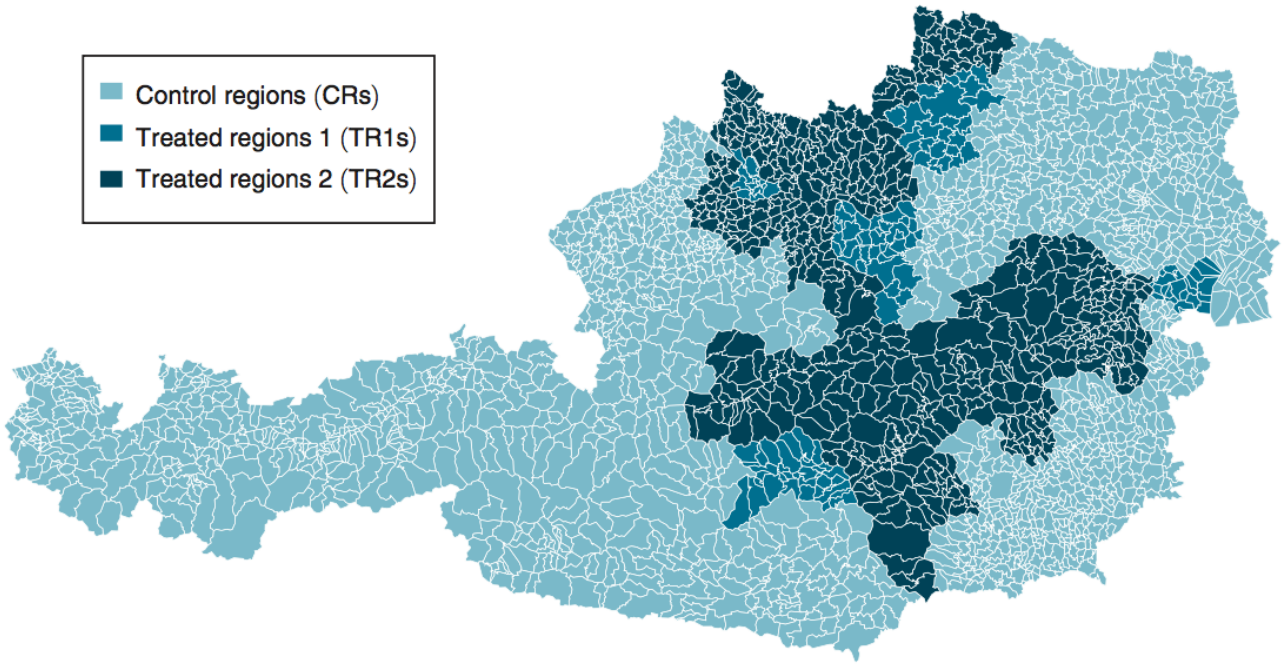
10 Figures

Figure 1: Inflow and Outflow Rates in Austria and other OECD Countries



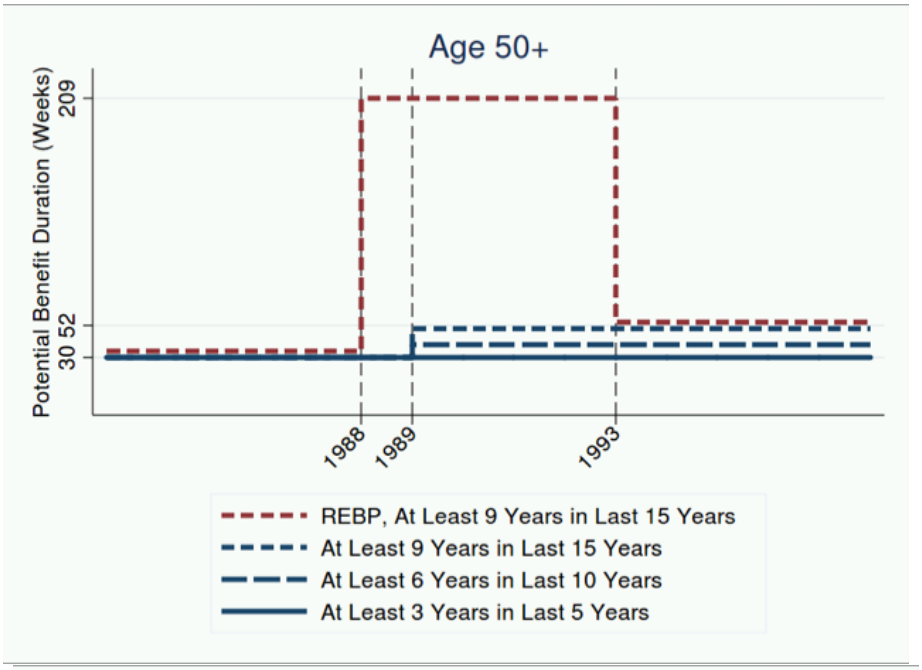
Note: The figure shows inflow and outflow rates into and out of unemployment in Austria and other OECD countries for the time period from 1994 to 2009. It extends the analysis in Elsby et al. (2013) to include Austria.

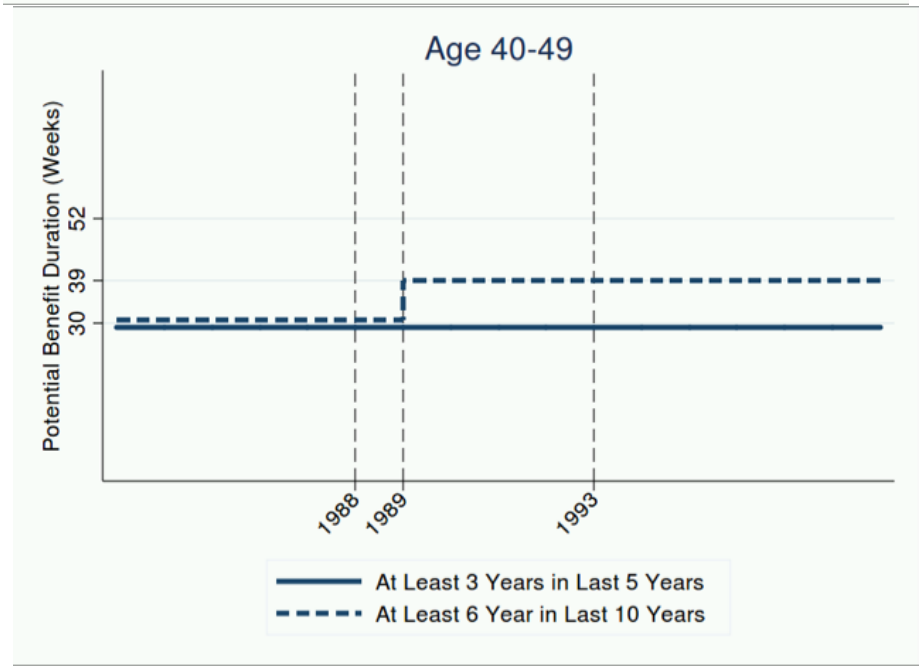
Figure 2: Map of REBP Treatment and Control Regions



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 Changes Due To REBP

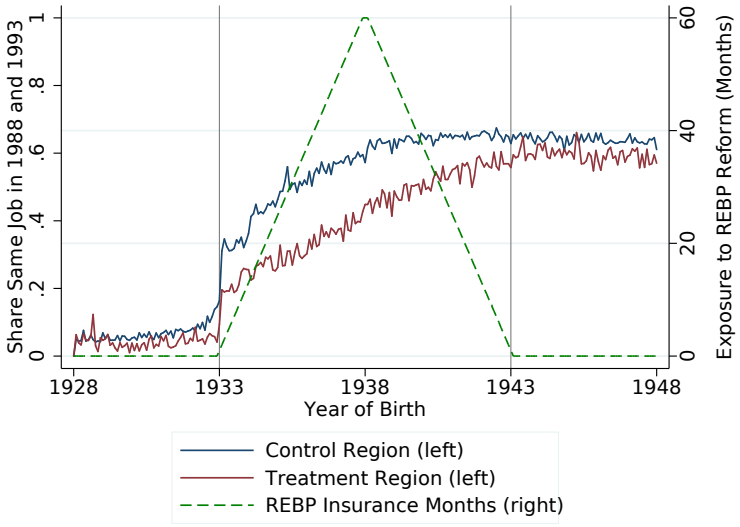




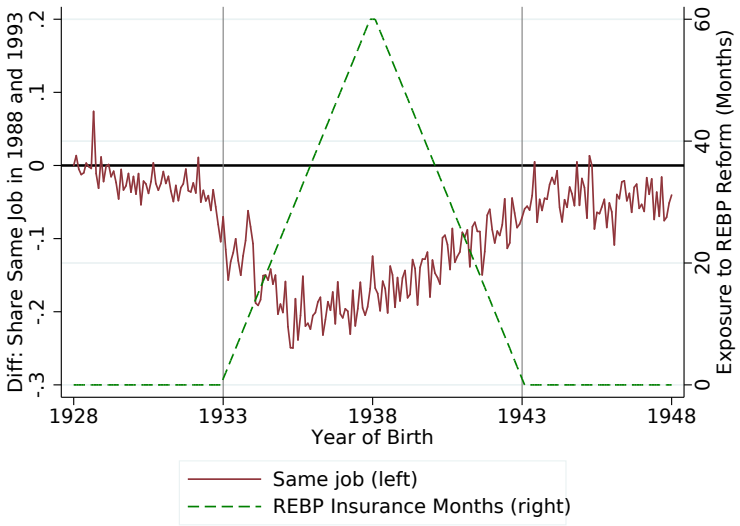
Note: The figure shows a timeline of reform changes in potential benefit duration for eligible workers in REBP and control regions.

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

(a) REBP and Control Region



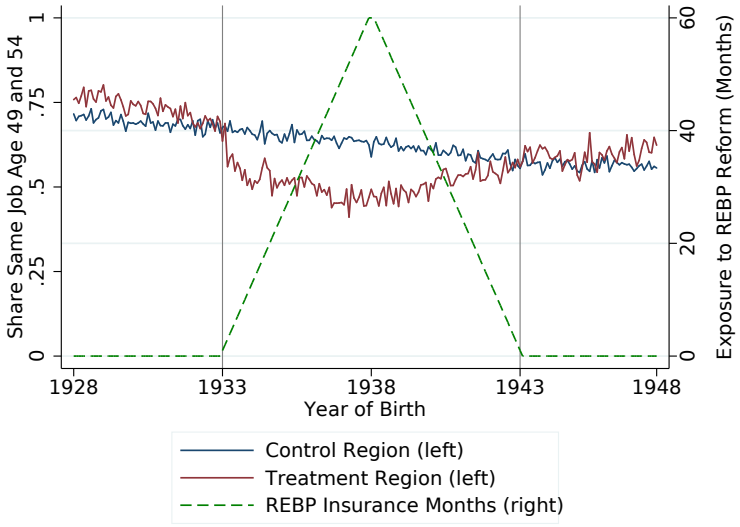
(b) Difference (REBP - Control Region)



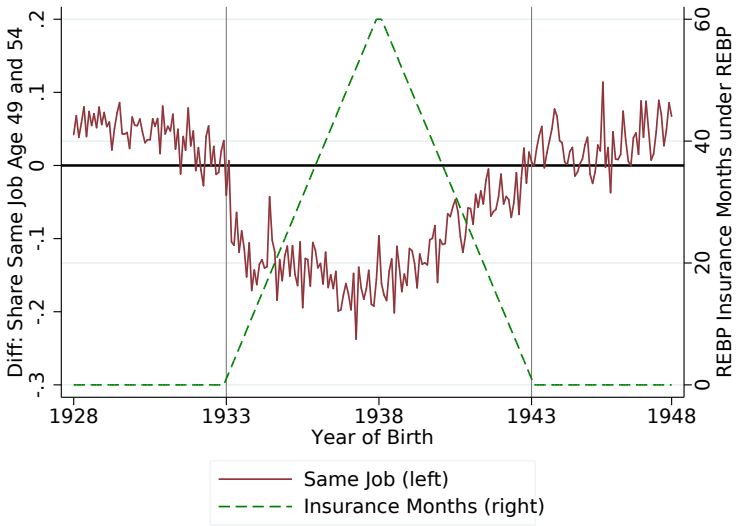
Note: The figures show the share of workers who hold the same job in Q.3 of 1993 as in Q.2 of 1988 across birth cohorts. The sample is confined to workers who are employed in Q.2 of 1988 ($N = 537,899$). Holding the same job is defined as being with the same employer. Panel (a) shows the share by month 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



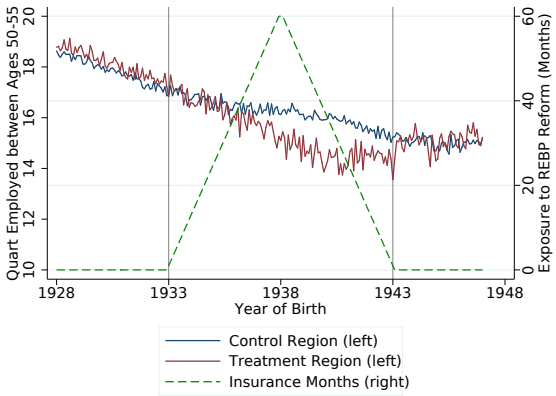
(b) Difference (REBP - Control Region)



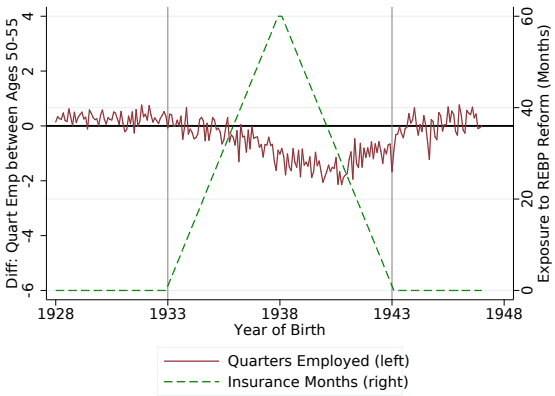
Note: The figures show the share of workers who hold the same job at age 49 as at age 54 across birth cohorts. The sample is confined to workers who are employed at age 49 ($N = 537,899$). Holding the same job is defined as being with the same employer. Panel (a) shows the share by month of birth in 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

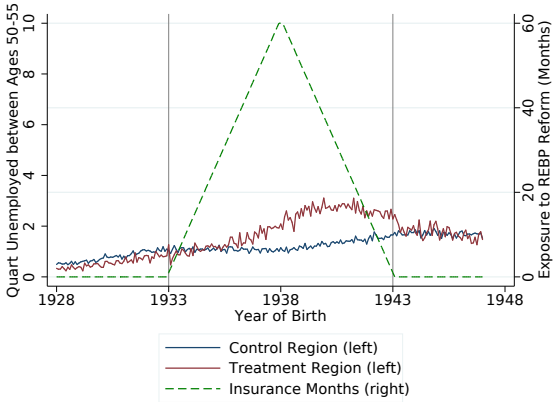
(a.i) Quarters Employed (Ages 50 to 55)



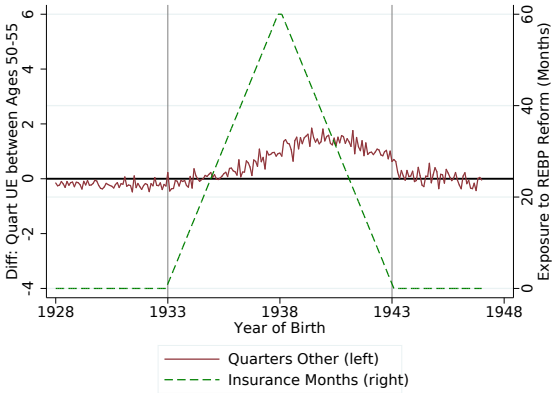
(a.ii) Difference (REBP - Control)



(b.i) Quarters Unemployed (Ages 50 to 55)



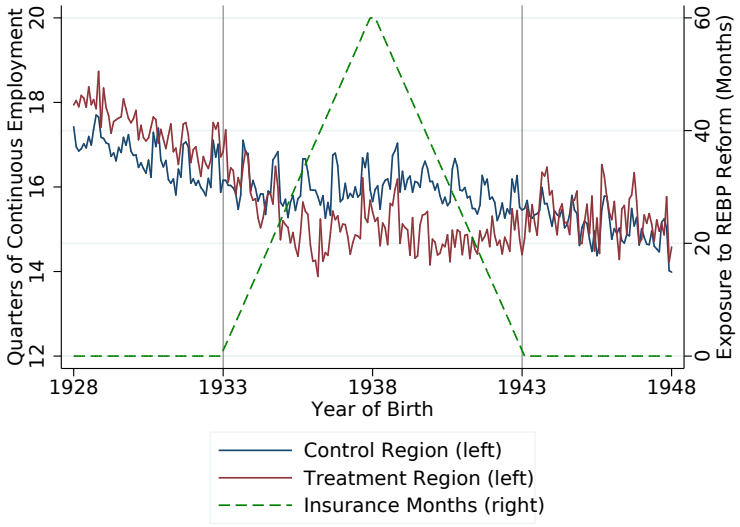
(b.ii) Difference (REBP - Control)



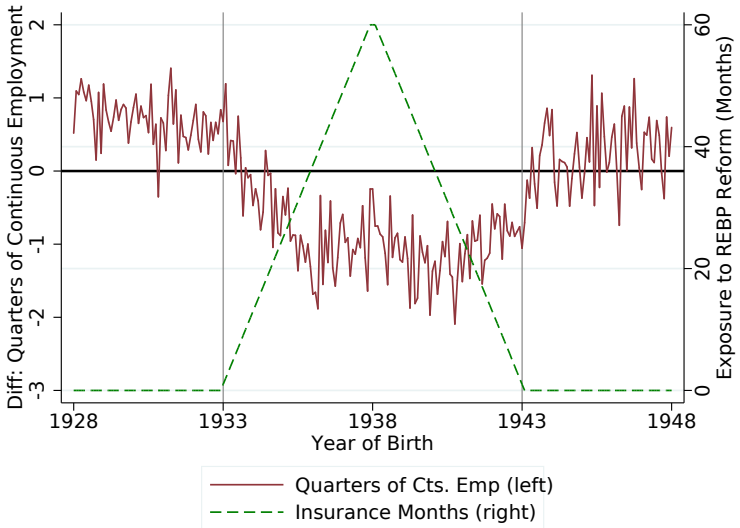
Note: The figures show the quarters between the ages of 50 and 55 in employment and unemployment across birth cohorts. The sample is confined to workers who are employed at age 49 ($N = 537,899$). The dashed green line, associated with the right y-axes, shows the number of months that REBP-region workers 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



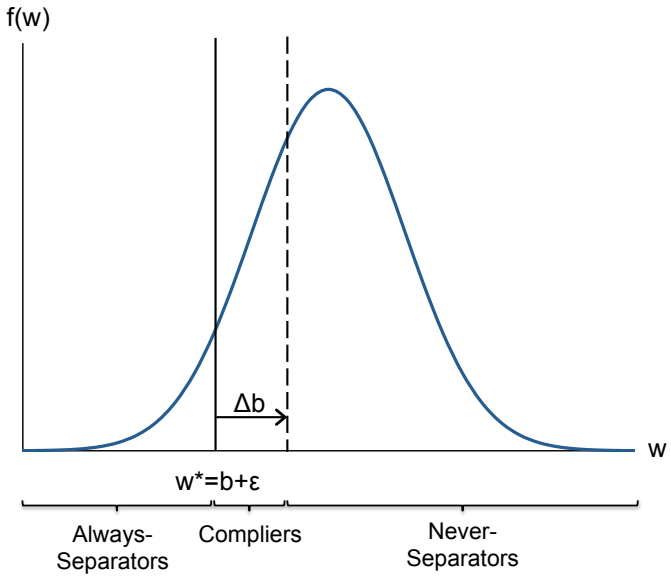
(b) Continuous Employment With Initial Employer (Difference REBP - Control Regions)



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: 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

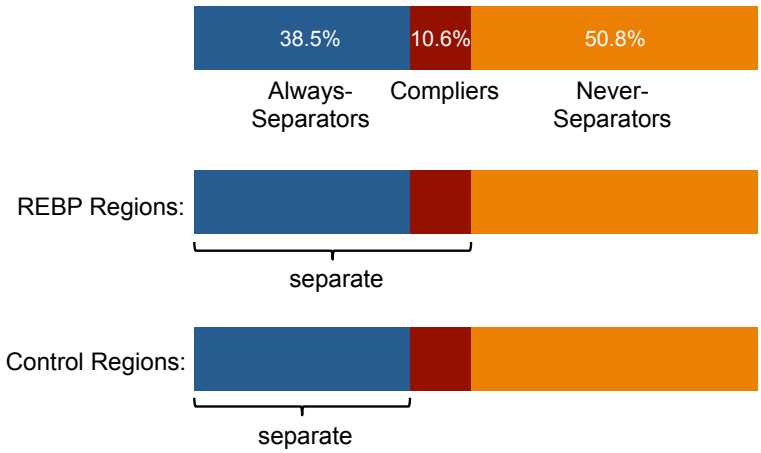
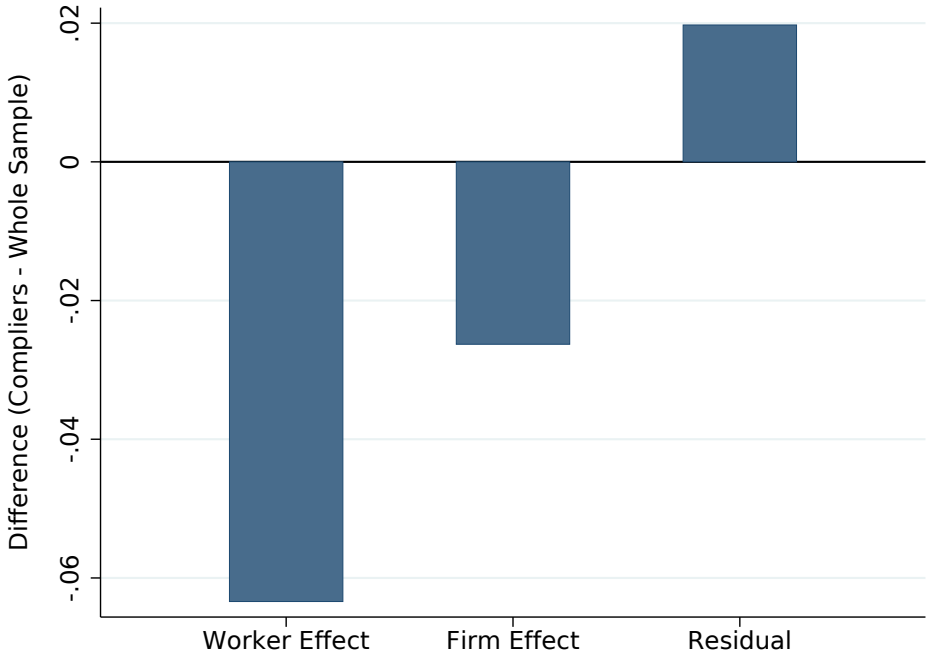


Figure 9: Wage Distribution in Marginal Jobs



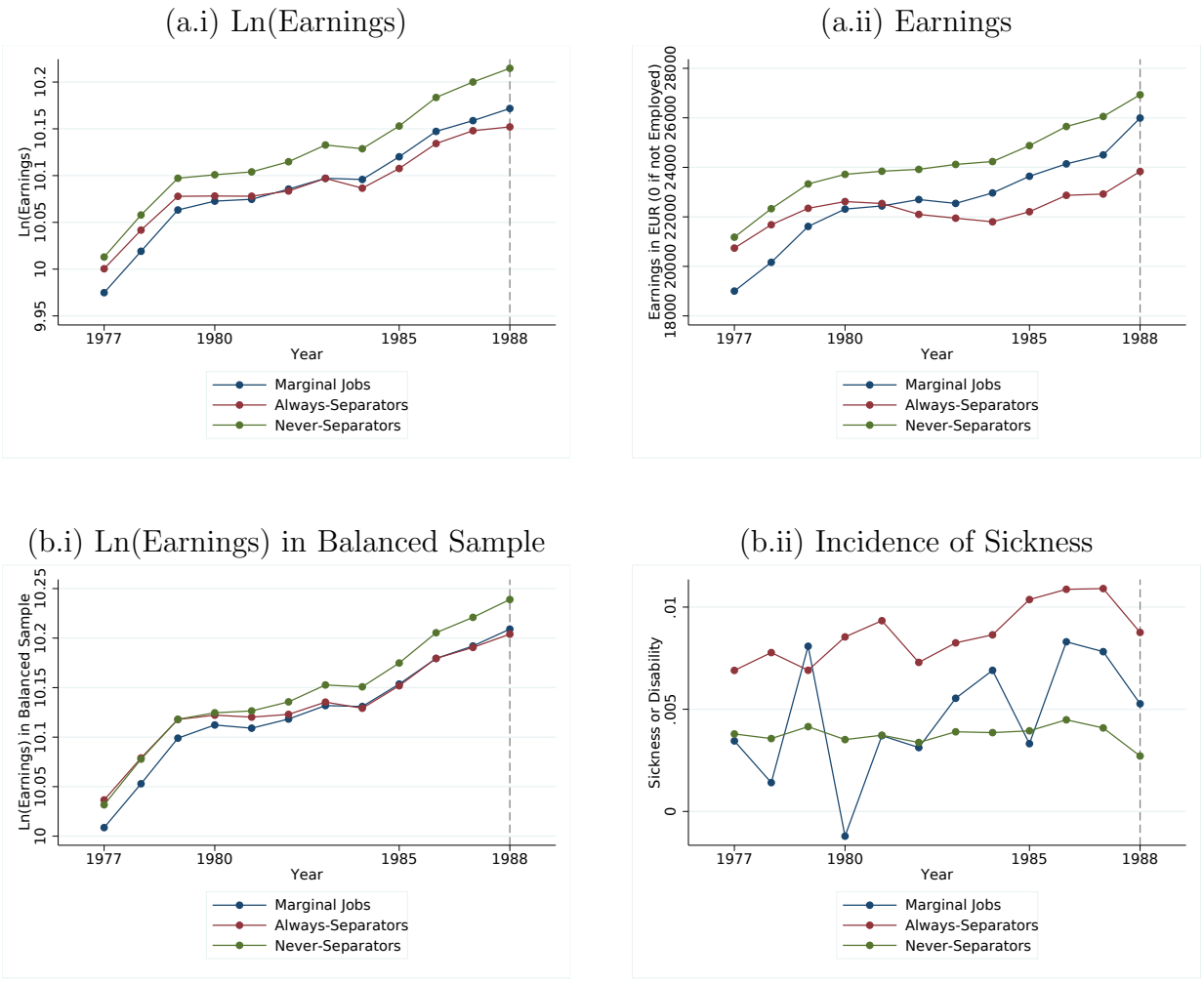
Note: The figure plots the probability mass function for 1988 earnings (plotted on the x-axis) in marginal jobs. Marginal jobs or compliers are defined as jobs that would survive between 1988 and 1993 in the absence of the REBP reform but resulted in a separation under REBP. 1988-earnings are a pre-attribute and the probability mass function is calculated based on the strength of the first stage relationship for binned groups of 1988 earnings drawing on the concepts in Imbens and Rubin (1996) and Abadie (2003).

Figure 10: AKM Effects: Wages of Compliers vs. Whole Sample in 1988



Note: See main text.

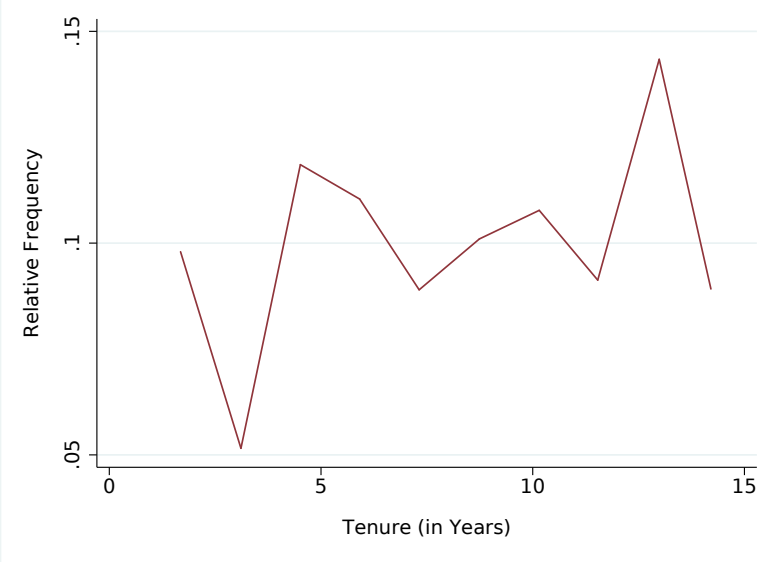
Figure 11: Lifecycle Profiles in Marginal Jobs



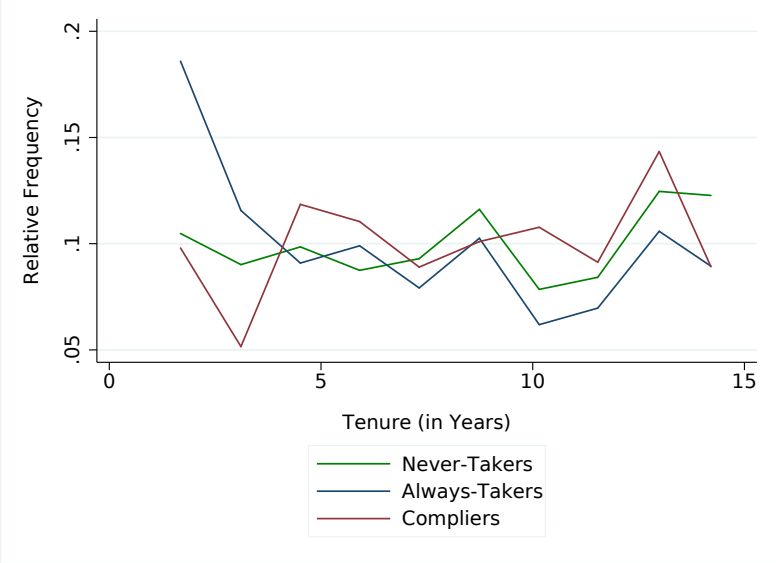
Note: The figures show trajectories of earnings, employment, and sickness status over time. For each year, we measure the respective attribute and then use the complier analysis described in section 2 to estimate conditional means for compliers, always-separators, and never-separators. Always-separators are defined as workers whose job would have led to a separation between 1988 and 1993 even in the absence of the reform, never-separators are defined as workers whose match survived even under the REBP reform. 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 12: Distribution of Tenure in Marginal Jobs

(a) Distribution of Tenure Among Compliers



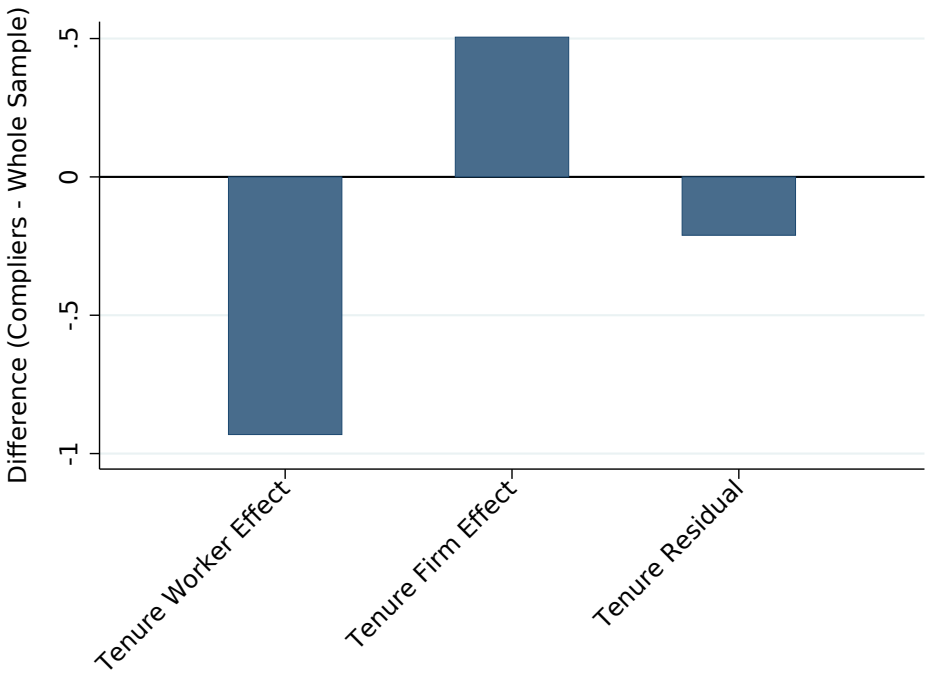
(b) Distribution of Tenure Among Compliers, Never-Separators, and Always-Separators



Note: The figure plots the probability mass function for 1988 tenure (plotted on the x-axis) for compliers, always-separators, and never-separators. 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, never-separators are defined as workers whose match survived even under the REBP reform. 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. See section 2 for

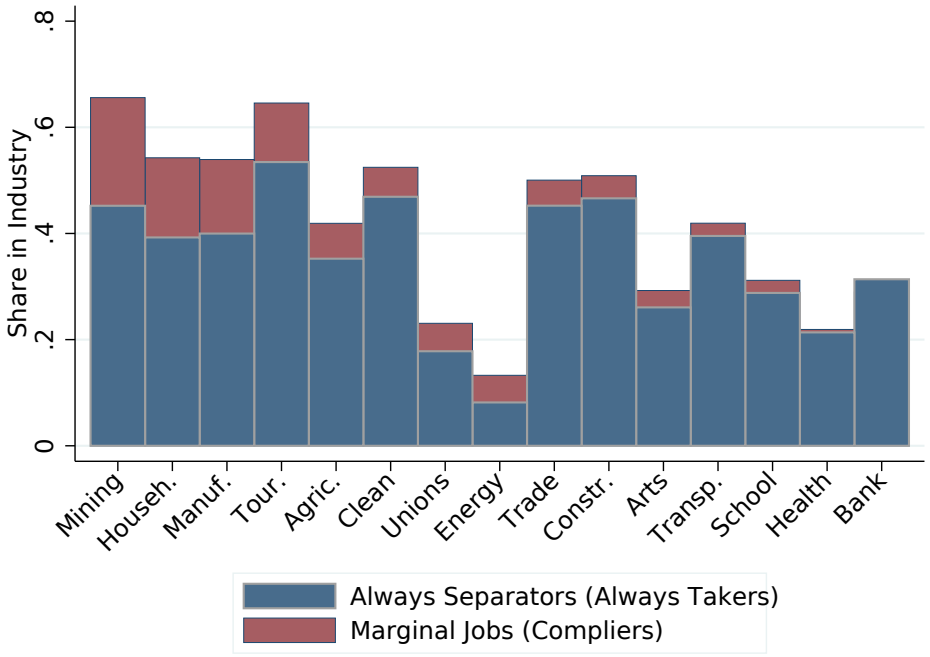
more details on the procedure. The sample is constrained to workers with less than 15 years of tenure as the dataset is left-censored 16 years before 1988.

Figure 13: AKM Effects: Tenure of Compliers vs. Whole Sample in 1988



Note: See main text.

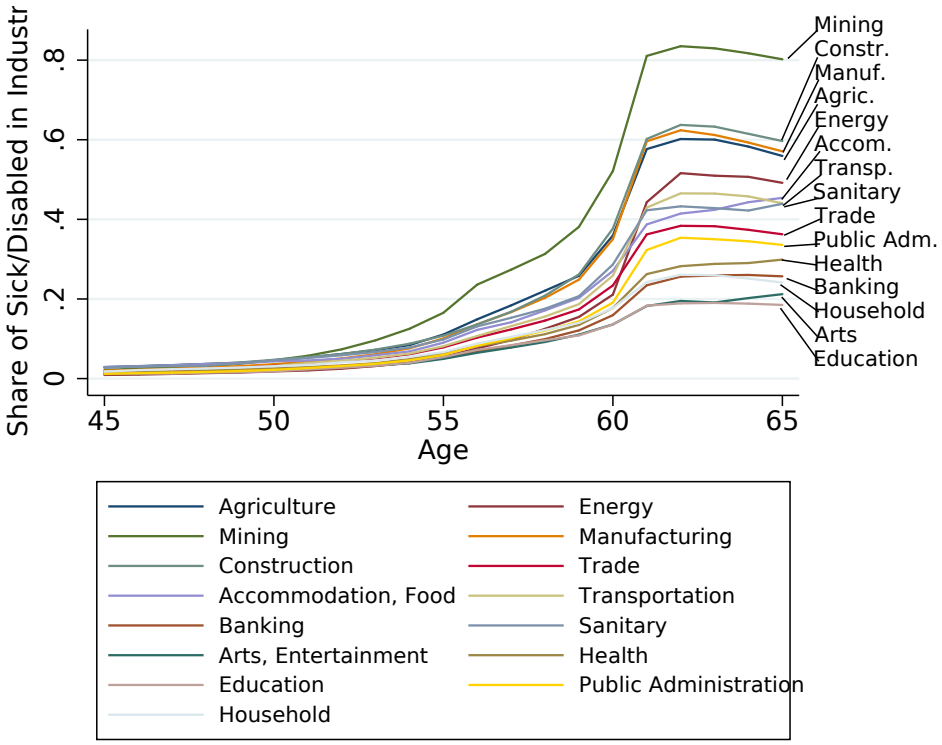
Figure 14: Share of Compliers By Industry



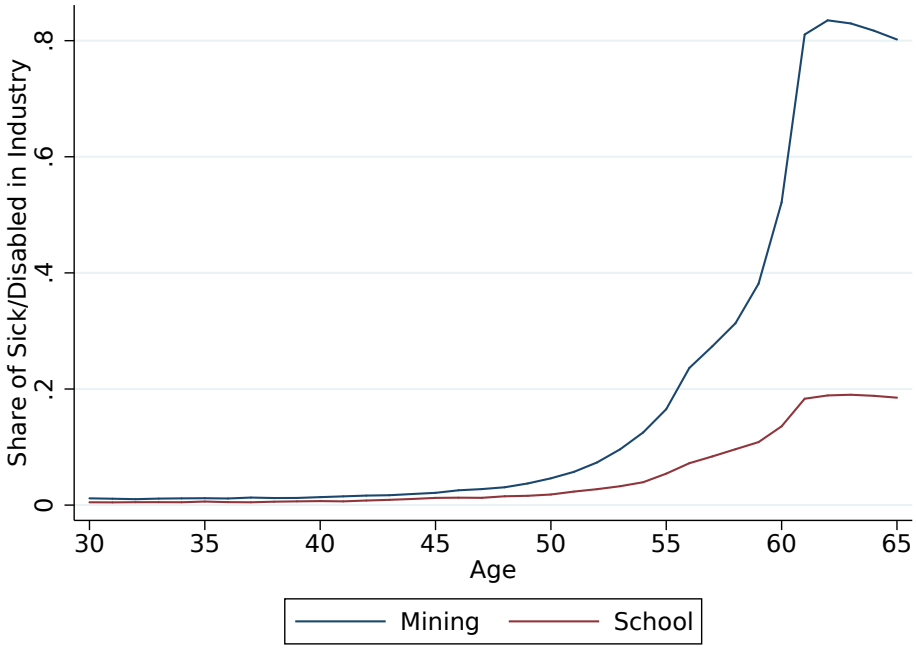
Note: The figure plots the share of always-separators and marginal jobs by industry. Always-separators are defined as workers whose job would have led to a separation between 1988 and 1993 even in the absence of the reform. Marginal jobs or compliers are defined as jobs that would survive between 1988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

Figure 15: Lifecycle Sickness Incidence By Industry

(a) All Industries

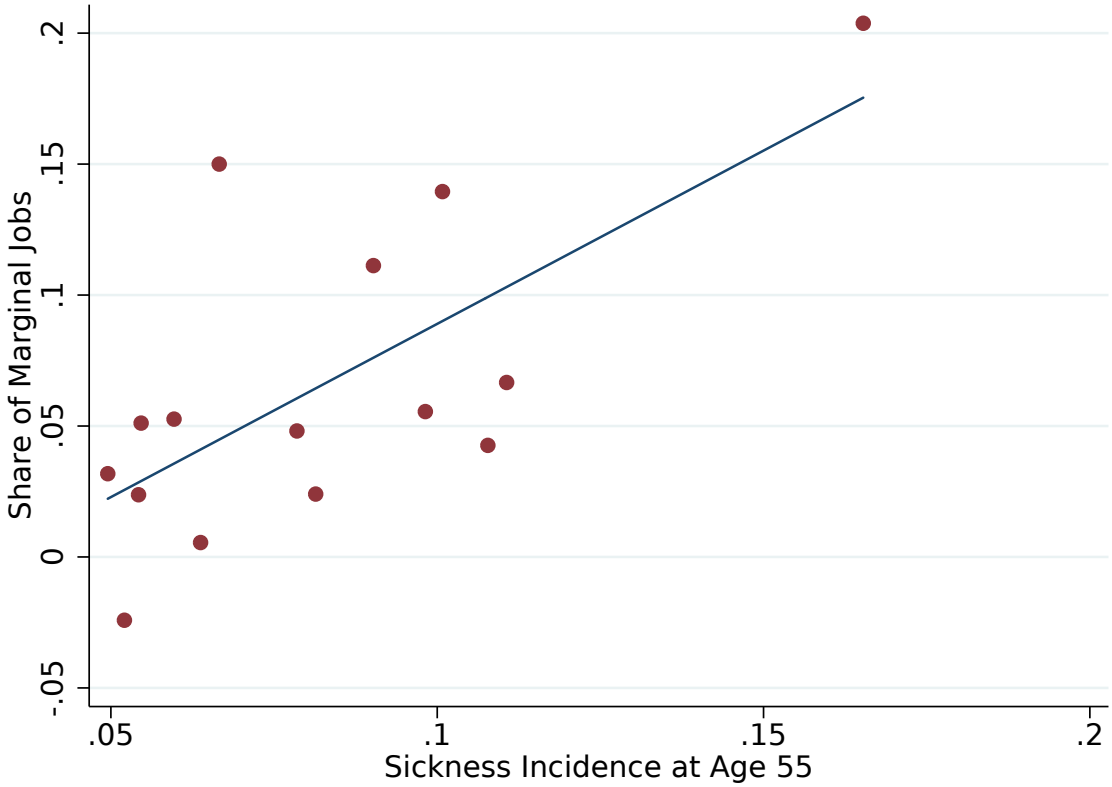


(b) Mining and School



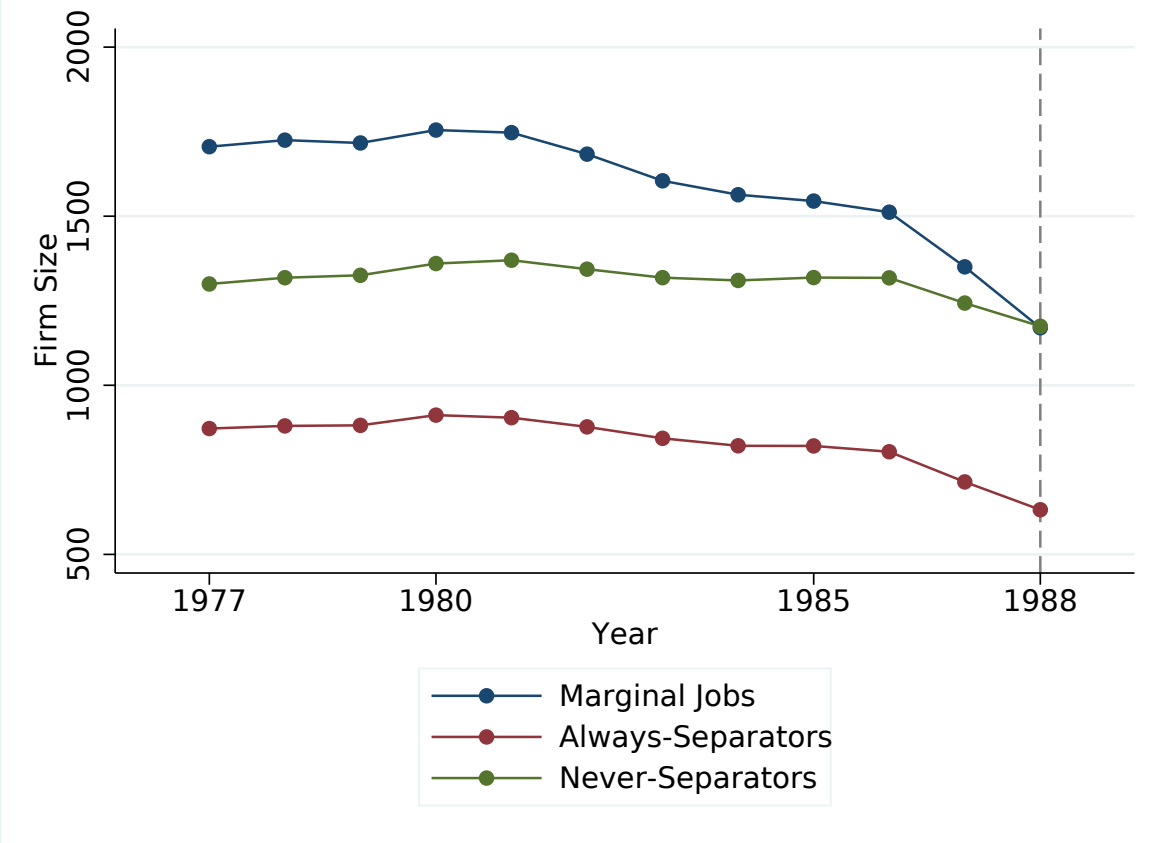
Note: The figures plot the incidence of sickness and disability spells over the lifecycle by industry based on quarterly data. Each sickness or disability spell gets assigned to the industry of a worker's last employer.

Figure 16: 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 17: Firm Size History



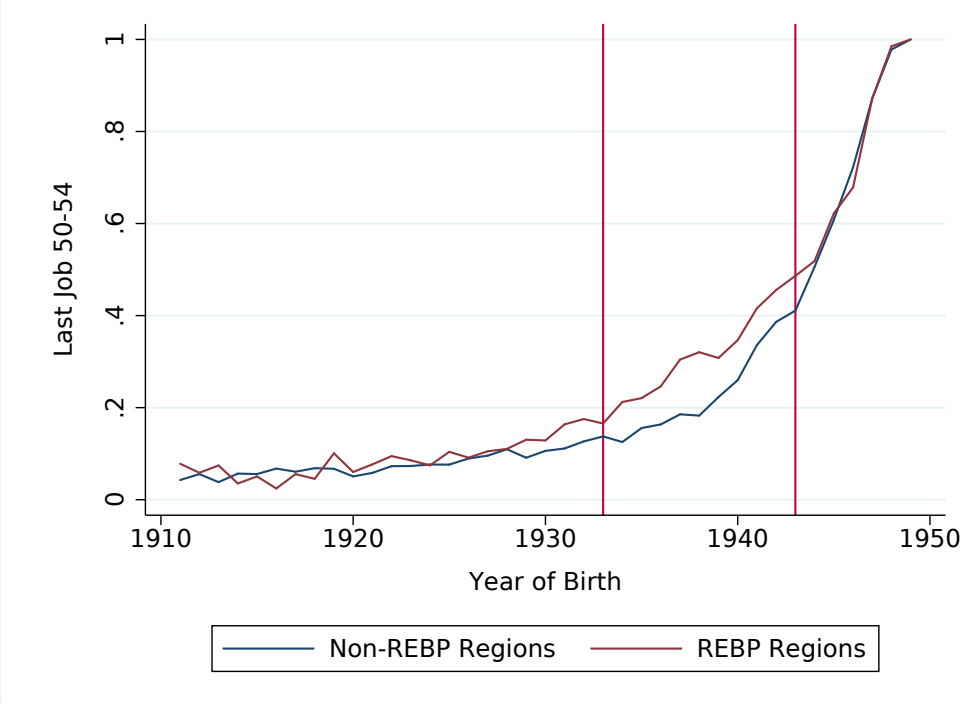
Note: The figures show trajectories of firm sizes over time. For each year, we retrospectively measure the historical size of the firm that a worker was employed at in 1988 and then use the complier analysis described in section 2 to estimate conditional means for compliers, always-separators, and never-separators. 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, never-separators are defined as workers whose match survived even under the REBP reform. 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 18: Firm Size History



Note: See main text

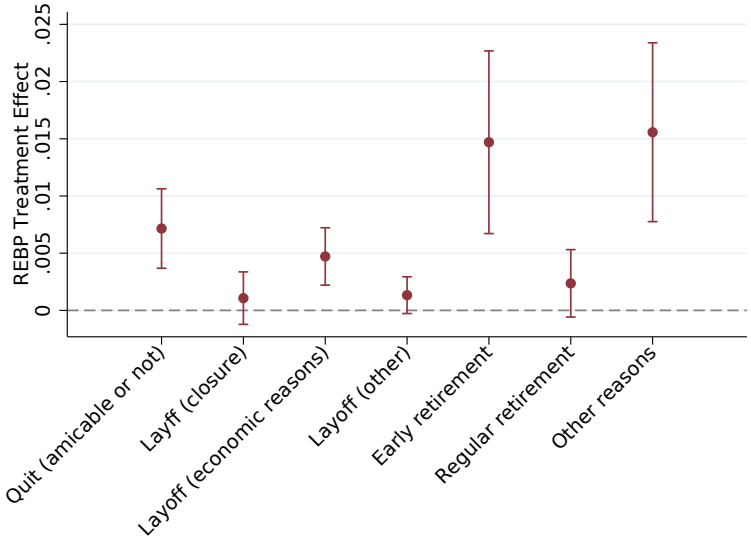
Figure 19: Microcensus: Effect of REBP on Separations between Age 50 and 54



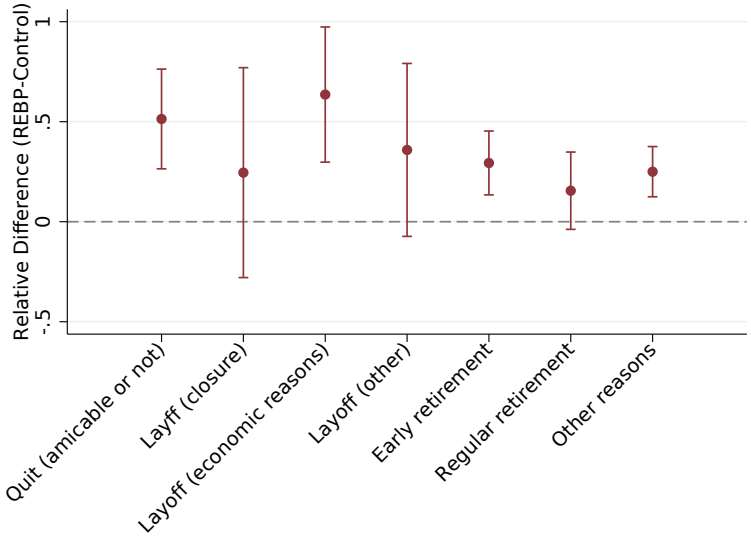
Note: The figure plots data based on the Austrian Microcensus. Across cohorts and regions, it plots an indicator variable for whether a respondent's last employment spell ended in the time when a respondent was between 50 and 54 years old. The two red vertical lines denote the oldest and youngest cohorts, 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 20: Microcensus: Effect of REBP on Type of Separations between Age 50 and 54.

(a) Absolute Effects (Percentage Points).

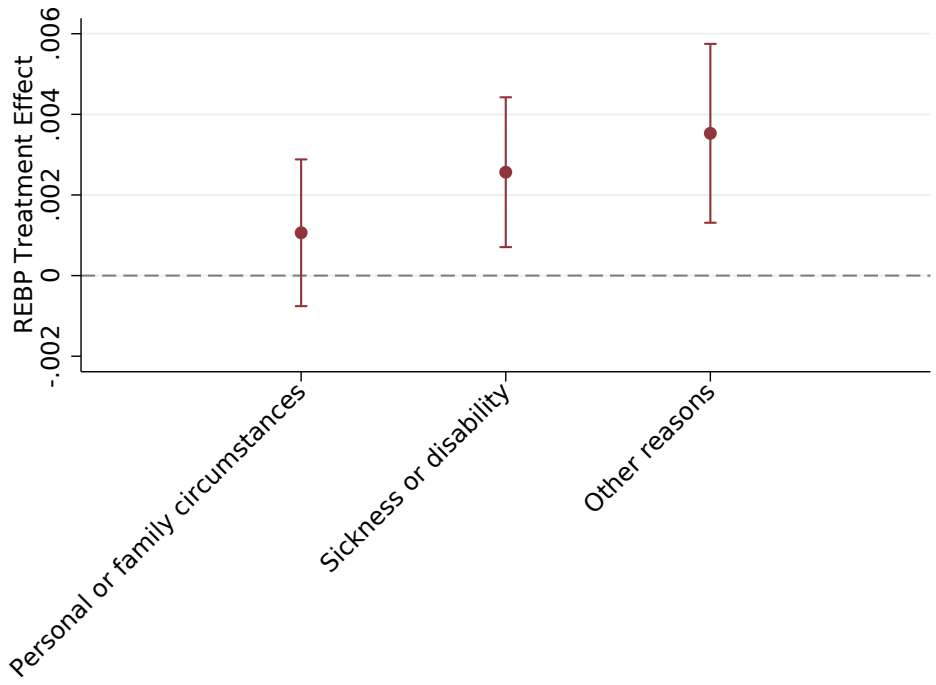


(b) Relative Effects (Percent) Relative to Control Mean.



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.

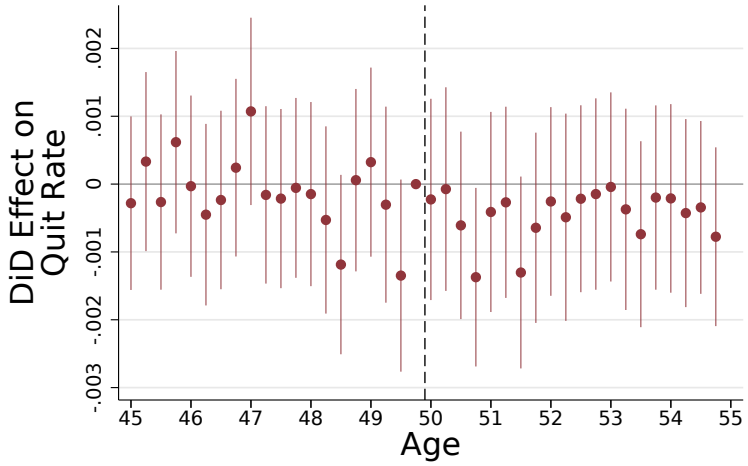
Figure 21: Microcensus: Effect of REBP on Types of Quits and Mutually Agreed-Upon Separations between Age 50 and 54



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.

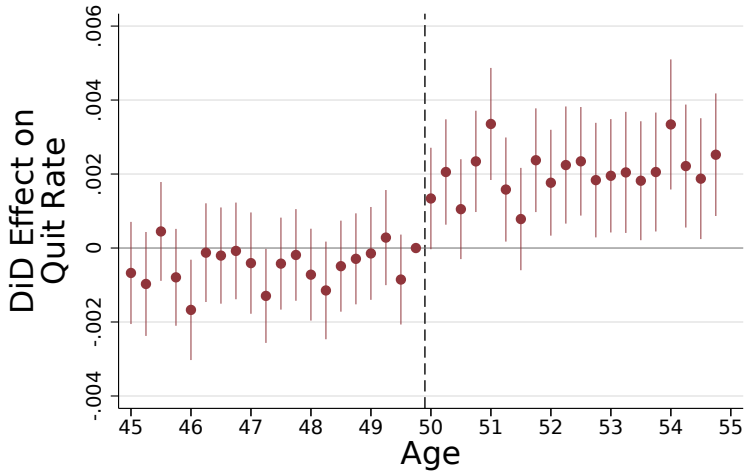
Figure 22: The Effect of Extensions on Non-Amicable Quits

(a) Pre-Period 1982–1987



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

(b) Reform Period 1988–1993

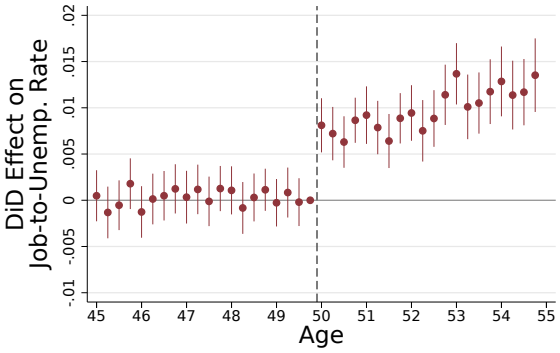


Control mean (for 50+): 0.0050
Sample: Non-steel, treatment period (1988-1992).

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.

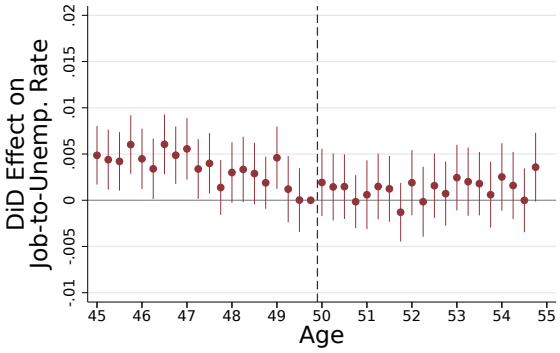
Figure 23: The Effect of Benefit Extensions on the Separation Rate

(a) Treatment Effect



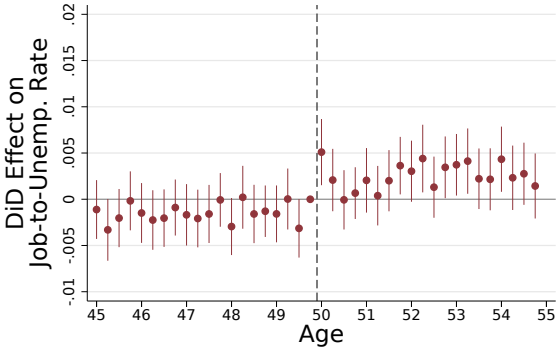
Control mean (for 50+): 0.0201
Sample: Non-steel, treatment period (1988-1992).

(b) Placebo I (Pre-Period)



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

(c) Placebo II (Post-Period I)



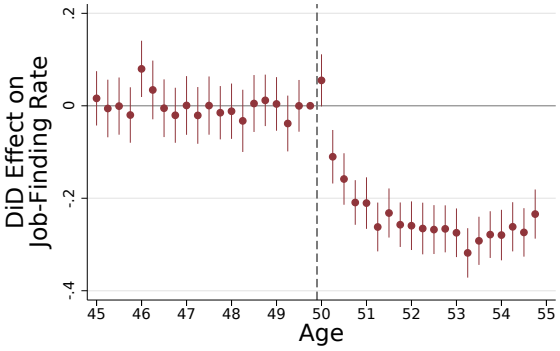
Control mean (for 50+): 0.0255
Sample: Non-steel, post-treatment period I (1993-1997).

(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.

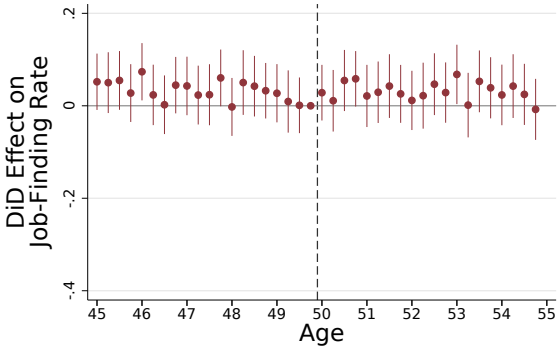
Figure 24: The Effect of Benefit Extensions on the Job Finding Rate

(a) Treatment Effect



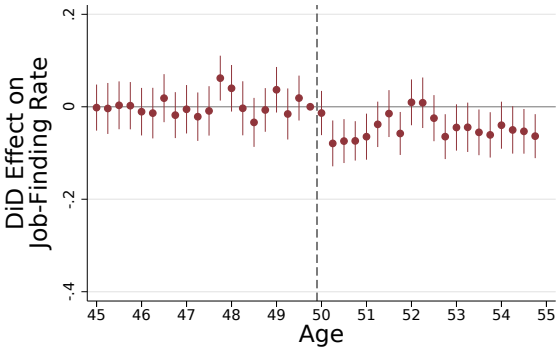
Control mean (for 50+): 0.4332
Sample: Non-steel, treatment period (1988-1992).

(b) Placebo I (Pre-Period)



Control mean (for 50+): 0.5218
Sample: Non-steel, pre-period (1983-1987).

(c) Placebo II (Post-Period II)

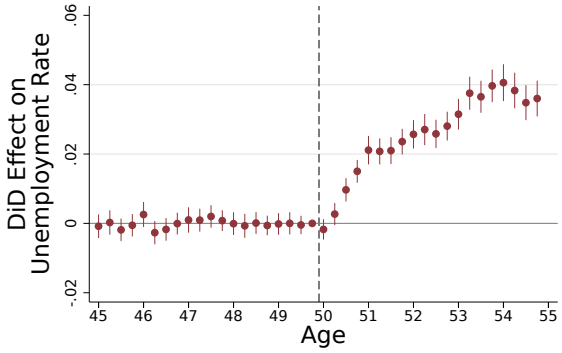


Control mean (for 50+): 0.3849
Sample: Non-steel, post-treatment period II (1997-2002).

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.

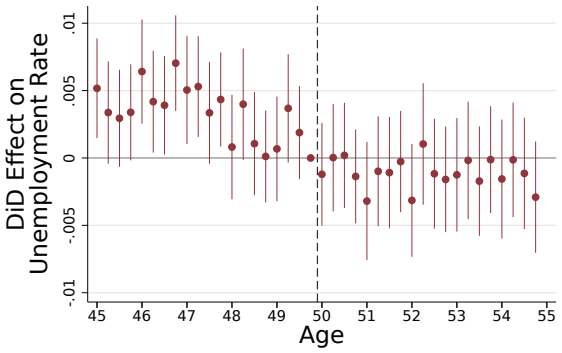
Figure 25: The Effect of Benefit Extensions on the Unemployment Rate

(a) Treatment Effect



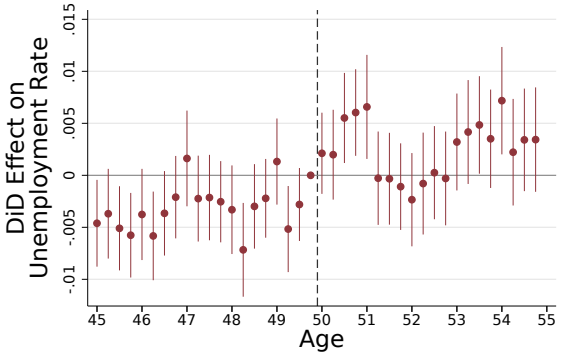
Control mean (for 50+): 0.0342
Sample: Non-steel, treatment period (1988-1992).

(b) Placebo I (Pre-Period)



Control mean (for 50+): 0.0377
Sample: Non-steel, pre-period (1983-1987).

(c) Placebo II (Post-Period II)

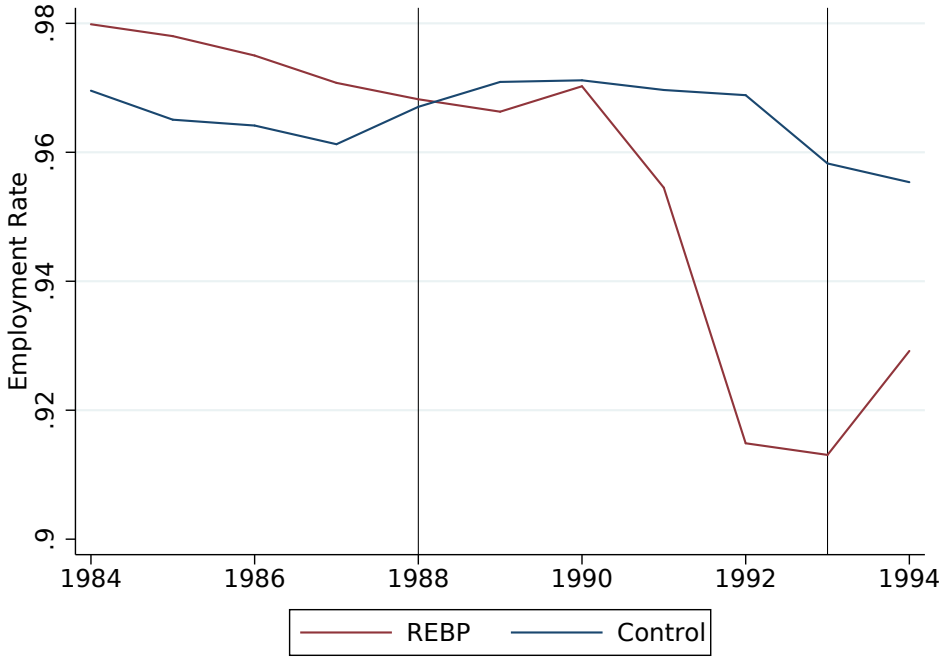


Control mean (for 50+): 0.0534
Sample: Non-steel, post-treatment period II (1997-2002).

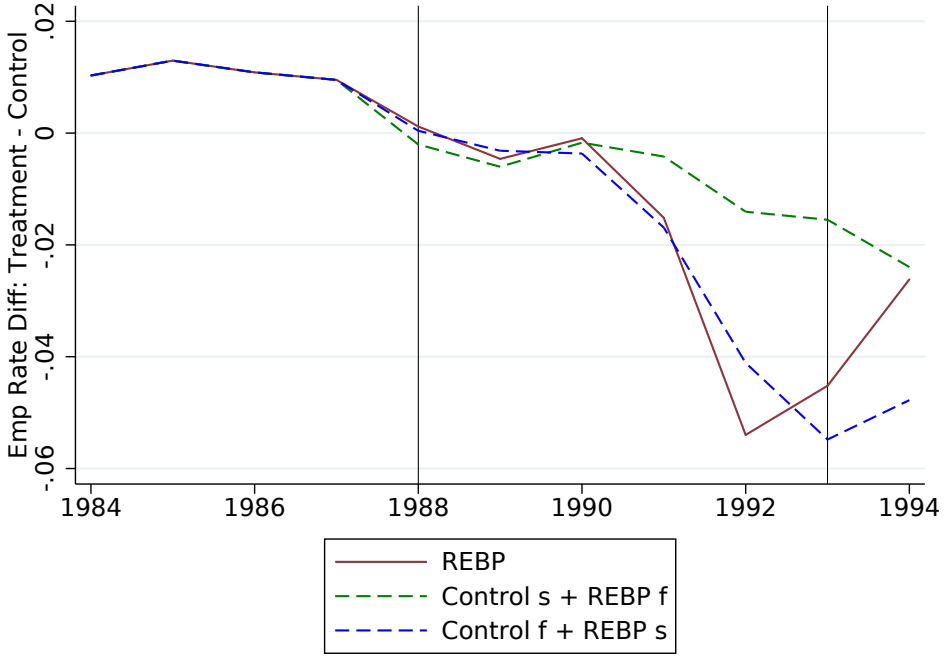
Note: The figures show difference-in-differences specifications for the interaction between age and REBP region in specifications controlling for the base effects of REBP and age. During the treatment period (Panel (a)), the benefit extension from 1 to 4 years affected eligible workers in REBP regions 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 26: Actual and Counterfactual Employment Rate Time Series

(a) Employment Rate Time Series

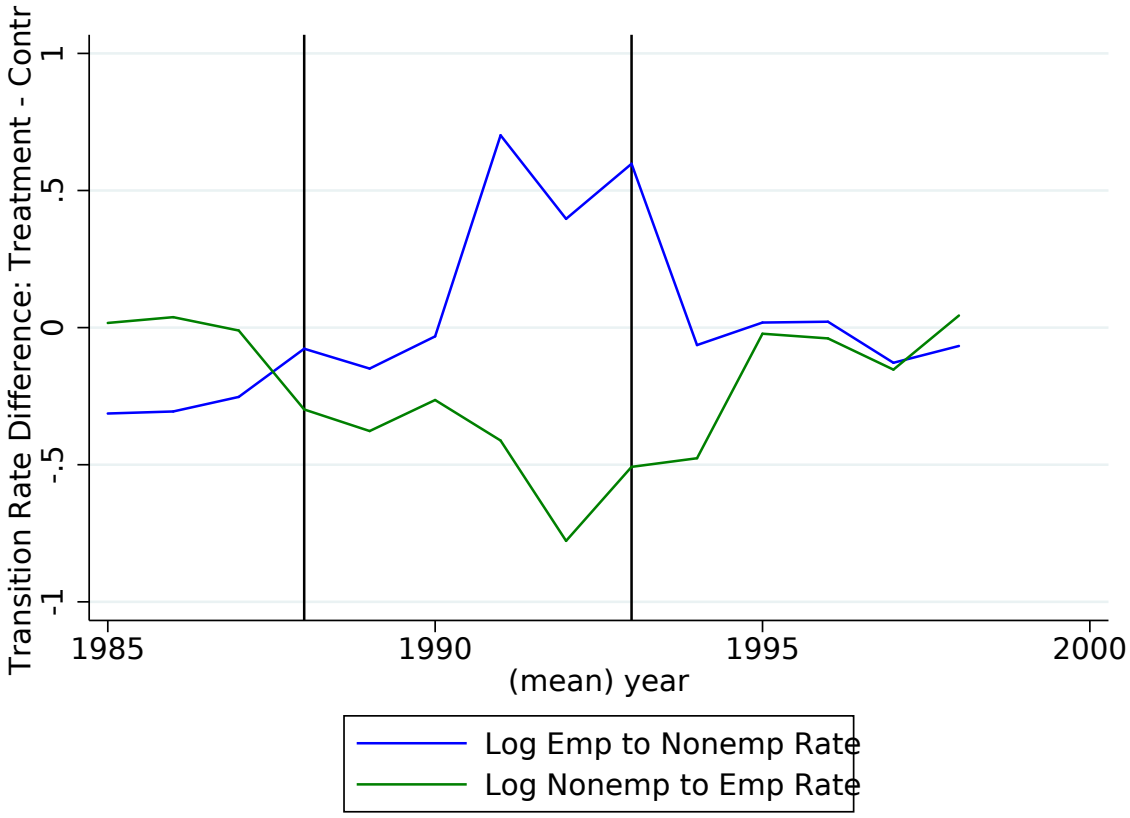


(b) Difference in Employment Rate (REBP - Control Region) and Counterfactual Employment Rate Time Series



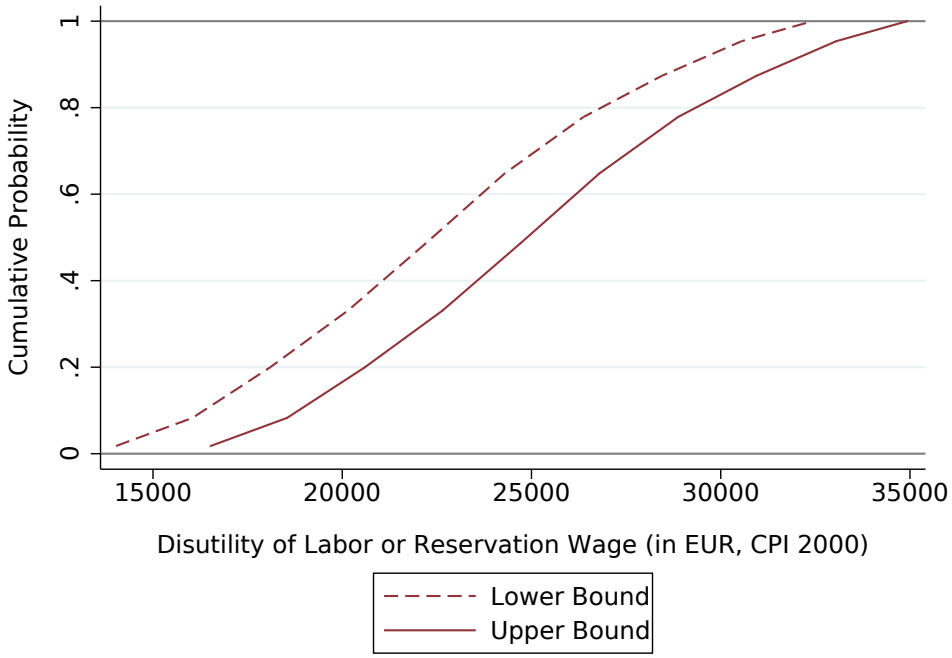
Note: See main text.

Figure 27: Transition Rate Differences: f vs. s



Note: See main text.

Figure 28: Labor Supply Curve or Reservation Wage Distribution for Marginal Jobs



Note: The figure plots the distribution of 1988 wages for marginal jobs (upper bound). For infinitesimal reforms and rigid wages, the upper bound identifies the reservation wage distribution. For non-infinitesimal reforms, the reservation wage is bounded below by a lower bound, which is derived from the upper bound by subtracting a measure for the monetary value of the REBP reform to workers. 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.

A Appendix

A.1 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 nonemployment (disutility of labor).[40]

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 repuational 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.

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):

$$U = b + \xi + \beta(1 - f)U + \beta f \cdot \int_{-\infty}^{\infty} \max\{W(a + w), U\} \mathrm{d}\tilde{G}(a + w) \tag{49}$$

$$= b + \xi + \beta U + \beta f \cdot \int_{\overline{a+w}}^{\infty} (W(a + w) - U)\, \mathrm{d}\tilde{G}(a + w) \tag{50}$$

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

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

$$= a + w + \beta(1 - \delta)(1 - \lambda)(W(a, w) - U) + \beta U \tag{52}$$

$$+ \beta(1 - \delta)\lambda \int_{\overline{a+w}}^{\infty} (W(a + w) - U)\, d\tilde{G}(a + w) \tag{53}$$

[40] 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.

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

$$W(a,w) - U = a + w - (b+\xi) + \beta(1-\delta)(1-\lambda)\left(W(a,w) - U\right) \tag{54}$$

$$+ \beta((1-\delta)\lambda - f) \cdot \int_{\overline{a+w}}^{\infty} \left(W(a+w) - U\right) d\tilde{G}(a+w) \tag{55}$$

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

$$\overline{a+w} = (b+\xi) + \beta(f - (1-\delta)\lambda) \cdot \int_{\overline{a+w}}^{\infty} \left(W(a+w) - U\right) d\tilde{G}(a+w) \tag{56}$$

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:

$$W(\overline{a+w}) = \frac{\overline{a+w} + \beta(1-\delta)\lambda \int_{\overline{a+w}}^{\infty} \left(W(a+w) - U\right) d\tilde{G}(a+w)}{1-\beta} = U \tag{57}$$

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 $J(p-w)$.

$$J(p,w) = p - w + \beta(1-\delta)(1-\lambda)J(p,w) + \beta(1-\delta)\lambda \int_{-\infty}^{\infty} \max\{J(p,w), V\} d\hat{G}(p,w) + \beta\delta V \tag{58}$$

Vacancy value is posting cost k plus filling payoff:

$$V = -k + \beta q \int_{-\infty}^{\infty} \max\{J(p,w), V\} d\hat{G}(p,w) + \beta(1-q)V \tag{59}$$

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

$$\Rightarrow \frac{k}{q} = \beta \int_{-\infty}^{\infty} \max\{J(p,w), 0\} d\hat{G}(p,w) = \beta \int_{0}^{\infty} J(p,w) d\hat{G}(p,w) \tag{60}$$

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

$$J(p,w) = p - w + \beta(1-\delta)(1-\lambda)J(p,w) + \beta(1-\delta)\lambda \int_0^\infty J(p,w)d\hat{G}(p,w) \tag{61}$$

$$\Leftrightarrow J(p,w) = \frac{p - w + (1-\delta)\lambda\frac{k}{q}}{1-\beta(1-\delta)(1-\lambda)} \tag{62}$$

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:

$$\max_w \left(W(a,w) - U\right)^\phi \cdot \left(J(p,w) - V\right)^{1-\phi} \tag{63}$$

$$\Rightarrow \phi(J(p,w) - V) = (1-\phi)(W(a,w) - U) \tag{64}$$

$$\Leftrightarrow w = \phi f \cdot \frac{k}{q} + \phi p + (1-\phi)((b+\xi) - a) \tag{65}$$

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

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

$$a + w = \phi(p + \theta k + a) + (1-\phi)((b+\xi)) = (b+\xi) + \phi(p + a - (b+\xi)) + \phi\theta k \tag{66}$$

$$p - w = p - \phi(p + \theta k) - (1-\phi)((b+\xi) - a) = (1-\phi)(p + a - (b+\xi)) - \phi\theta k \tag{67}$$

Workers' reservation value. Remember that worker's reservation value (56). Now consider the job types that generate this wage and amenity combination as a function of job fundamentals a and p and Nash bargaining:

$$\overline{a+w} = \phi(p + \theta k + a) + (1-\phi)((b+\xi)) = (b+\xi) + \phi(p + a - (b+\xi)) + \phi\theta k \tag{68}$$

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

$$\beta(f - (1-\delta)\lambda) \cdot \int_{\overline{a+w}}^\infty \left(W(a+w) - U\right) d\tilde{G}(a+w) = \phi(p + a - (b+\xi)) + \phi\theta k \tag{69}$$

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

Firms' reservation value.

$$0 = J(\overline{p-w}) \tag{71}$$

$$\Leftrightarrow 0 = \overline{p-w} + \beta(1-\delta)(1-\lambda)0 + \beta(1-\delta)\lambda \int_0^\infty J(p,w)d\hat{G}(p,w) \tag{72}$$

$$\Leftrightarrow \overline{p-w} = \boxed{-}(1-\delta)\lambda\frac{k}{q} \tag{73}$$

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 $p - w(p, a)$:

$$\overline{p-w} = (1-\phi)(p+a-(b+\xi)) - \phi\theta k = \boxed{-}(1-\delta)\lambda\frac{k}{q} \tag{74}$$

$$\Leftrightarrow p+a-(b+\xi) = \frac{\phi}{1-\phi}\theta k - \frac{1}{1-\phi}(1-\delta)\lambda\frac{k}{q} \tag{75}$$

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.

$$s^{AS} = \delta + (1-\delta)\lambda G(\overline{a+p}(b)) \tag{76}$$

Fraction of Never-Separators.

$$s^{NS} = 1 - [\delta + (1-\delta)\lambda G(\overline{a+p}(b))] \tag{77}$$

Fraction of Compliers.

$$s^{C} = (1-\delta)\cdot[\lambda G(\overline{a+p}(b+Z\times\Delta b)) - \lambda G(\overline{a+p}(b))] \tag{78}$$

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, wjp 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.