Marginal Jobs and Job Surplus:
A Test of the Efficiency of Separations*

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Abstract

We present a sharp test for the efficiency of job separations. First, we document a dramatic increase in the separation rate – 11.2ppt (28%) over five years – in response to a quasi-experimental extension of UI benefit duration for older workers. Second, *after the abolition* of the policy, the "job survivors" in the formerly treated group exhibit *exactly* the same separation behavior as the control group. Juxtaposed, these facts reject the "Coasean" prediction of efficient separations, whereby the UI extensions should have extracted marginal (low-surplus) jobs and thereby rendered the remaining (high-surplus) jobs more resilient after its abolition. Third, we show that a formal model of predicted efficient separations implies a piece-wise linear function of the actual control group separations beyond the missing mass of marginal matches. A structural estimation reveals point estimates of the share of efficient separations below 4%, with confidence intervals rejecting shares above 13%. Fourth, to characterize the marginal jobs in the data, we extend complier analysis to difference-in-difference settings such as ours. The UI-induced separators stemmed from declining firms, blue-collar jobs, with a high share of sick older workers, and firms more likely to have works councils – while their wages were similar to program survivors. The evidence is consistent with a "non-Coasean" framework building on wage frictions preventing efficient bargaining, and with formal or informal institutional constraints on selective separations.

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1 Introduction

By *Coasean* theories of jobs, the employer and the worker exploit all bilateral gains from trade.[1] Hence all job separations are mutually preferable and efficient, in contrast to the inefficient separations predicted by *frictional* ("non-Coasean") theories that put limits on contracting due to, e.g., wage rigidity or institutional constraints. While the Coasean benchmark provides a powerful and popular point of departure in theoretical models, the degree to which they provide accurate descriptions of real-world job formation and destruction remains an open question. An empirical test of the Coasean benchmark is challenging exactly because of the abstract allocative concepts underlying its strong efficiency implications in the first place: joint job surplus split by unrestricted transferable-utility compensation arrangements. For example, measured flow wages, even if appearing perfectly rigid, can be interpreted to be consistent with bilaterally efficient bargaining outcomes (Barro, 1977; Hall, 2005; Shimer, 2004; Hall and Milgrom, 2008), and categories such as quits and layoffs can be interpreted as mere nominal labels of efficient separations in the Coasean framework (McLaughlin, 1991). As a result, the Coasean hypothesis and its tractable implementation through efficient bargaining remains the benchmark model, for example, in search and matching models (Mortensen and Pissarides, 1994; Pissarides, 2000), even in the face of pervasive evidence for the dramatic costs from job displacement (see, e.g., Jacobson et al., 1993; Davis and von Wachter, 2011).

We provide a sharp and high-powered test of whether Coasean theories can account for job separations in a uniquely suited quasi-experiment: a large, temporary unemployment insurance extension of potential benefit duration from one to four years in Austria from 1988 through 1993, the Regional Extended Benefit Program (REBP). Our analysis uses population matched employer-employee data and implements a clean difference-in-differences design, since eligibility was determined by age cutoffs (age 50 and up) and was restricted to specific regions.

Our first empirical step documents that the program triggered an increase in separations of 11.2ppt (28%) over the five-year horizon the program was active: 50% of jobs in treatment group separated, compared to only 39% in the treatment group. We illustrate this finding by plotting raw data for the fivbe-year job separation rate by birth cohorts in the treatment and control regions. Cohorts too old or too young to be eligible move in lockstep, while the treated cohorts exhibit a clear increase in job separations. These UI-induced separations largely go into long-term unemployment, consistent with reductions in surplus arising from the workers' improved nonemployment outside option. We therefore also provide new nonparametric causal evidence that UI incentives can induce separations and generate large unemployment inflows, at least in our context of older workers with the ability to use program as a bridge into early retirement.[2]

[1] We use "Coasean" as a short-hand to describe *settings in which bilateral bargaining is unconstrained and parties can and hence will reach bilaterally efficient outcomes, including at the separation margin.* For example, just as the initial assignment of property rates is irrelevant by the Coase Theorem, the initial incidence of a worker or firm shock will be irrelevant in our setting. Importantly, our notion of efficiency is *bilateral* and at the separation margin, and hence does not fully characterize market efficiency, such as those that would emerge at the hiring margin (Hosios, 1990).

[2] We have found similar separation rate increases studying quarterly transition probabilities rather than five-year horizons. Winter-Ebmer (2003) studies inflow effects from REBP, between two broad groups (all below 50 vs. 50-65) using a 2% sample of our population data. Lalive et al. (2011) find small inflow effects of different, national reform in Austria, in a regression-based difference-in-difference model. Lalive et al. (2015) study the REBP program with a focus on search externalities from treated onto untreated unemployed job seekers' job finding rates. Hutchens (1999) discusses the

In a second step, we track the job survivors in both the treatment and controls groups *after* the program was abruptly abolished in 1993. We again plot raw data of separations by cohort, now among the survivors. The formerly treated cohorts – whose ranks are, on average, 20% smaller than their control peers – were just as likely to separate year by year as the peer control cohorts. This result holds for unconditional separations, and even in response to negative labor demand shocks (sharp establishment shrinkage events and negative industry growth). In short, despite the massive separations among the ranks of the formerly treated groups, the survivors do not appear more resilient to subsequent shocks after the program was abolished.

Juxtaposed, the first result – large separations due to a well-identified transitory surplus shock – and the second result – no subsequent resilience – are inconsistent with the Coasean benchmark. By the Coasean hypothesis, the firm and worker exploit all gains from trade and hence the allocative surplus is a one-dimensional concept of joint surplus, the sum of the worker's and firm's inside value of the job (e.g., productivity, amenities) minus the sum of the outside options (e.g., hiring an alternative worker, quitting into unemployment). In the Coasean setting, the extraction of low-surplus jobs should have generated a missing mass of marginal matches with low joint job surplus. Any source of subsequent (post-abolition) surplus drop should have swept up fewer jobs into separation among the selected survivors of the UI program.[3] By contrast, frictional, i.e. non-Coasean, theories detach worker and firm surplus, and permit an absence of resilience even after a high share of jobs have been destroyed by a temporary surplus reduction.

Third, to quantitatively assess the gap between predicted Coasean separations and the data, we construct and structurally estimate a model-derived benchmark of efficient job separations, and then ask which share of REBP separations was consistent with this benchmark. Intuitively, the model exploits separation rate differentials among *job survivors* between the treatment and control group *after the program had been abolished*, to retroactively classify the efficiency of the *initial* REBP separations. We translate the Coasean surplus-based "pecking order" into an empirically tractable piece-wise linear function of predicted *post*-REBP Coasean separations in the formerly treated group compared to *actual separations* in the former control group: zero up until a "kink" corresponding to the treatment effect of the initial program on separations, and a tight comovement thereafter. Due to the UI-induced separations, kink is far on the x-axis, and so the Coasean benchmark predicts substantial "resilience" among REBP survivors. We estimate this model structurally, using GMM estimation to select the weight on the Coasean prediction, using variation between industry-occupation cells in control separation rates and kink locations. Even in our most conservative specification, the point estimates for the share of Coasean separations are smaller than 4%, with the 95% confidence interval at 13%.

All efficient separations are alike; each frictional setting is inefficient in its own way, as with Tolstoy's

theoretical interaction of UI and retirement. Inderbitzin et al. (2016) study the interaction of UI and disability insurance system in Austria. Kyyrä and Wilke (2007) provide empirical evidence for that interaction in Finland. Hartung et al. (2018) study the macroeconomic consequences of a German labor market reform for job separations. Feldstein (1976) studies temporary layoffs and recalls, whereas we study permanent nonemployment. These studies focus on inflow vs. outflow effects of UI, and do not study the quality of marginal jobs, or the compositional consequences after the abolition.

[3] We also discuss, but ultimately quantitatively dismiss, a third interpretation that in principle could be at play: idiosyncratic shocks could immediately and completely "reshuffle" the surplus distribution and thereby mask the Coasean nature of separations.

unhappy families. While a menu of frictions may explain the documented stark deviation from the Coasean benchmark, we sketch a particularly plausible class of non-Coasean narratives building on wage setting frictions. By preventing the flexible (re-)bargaining underlying the Coasean result, such frictions shrink the set of viable jobs by complicating the "positive surplus" test from single-dimensional Coasean joint job surplus, to two surplus concepts: separations may occur because either worker-specific or firm-specific surplus turns negative. In this world, the original UI extension would have destroyed matches with low *worker* surplus. Indeed, we use a retrospective worker survey to document that a significant share of the original separations in response to the reform materialized as voluntary to the worker (quits). The fact that despite the dramatic depletion of their ranks the treated cohorts move in lock-step with the control cohorts, implies that after the reform, *firm* surplus is largely allocative for separations, and that post-abolition separations are driven by firms crossing their participation constraint. In the Coasean benchmark, this distinction of two unilateral surplus concepts would have been meaningless and summed into joint surplus, forging an iron link between the marginal job in terms of worker surplus and in terms of joint job surplus.

We also resolve an ostensible tension: why did the extraction of so many low *worker*-surplus jobs in response to the initial UI program not measurably lower separation rates of the survivors? Our resolution has two simple features. The first feature is high typical worker surplus, such that few workers are *usually* on the separation margin. Indeed, our sample consists of male, older, and high-tenured workers. Austria mandates multiple months of severance payments in the case of layoffs that are foregone for quitters, providing little incentive for workers to quit unilaterally and pushing up worker surplus under normal conditions.[4] The second feature is the exceptional size of the initial UI treatment – *four years* of full UI eligibility, potentially serving as a bridge into early retirement – likely played a role. We ballpark the cash value of the reform to workers to be around 71% of an annual salary. The initial outside option boost was therefore sufficiently large to sweep up even those workers that would otherwise remain inframarginal to the typical worker-side shocks, due to high worker surplus.[5] As a result, the absence of these matches had no noticable differential effect on separations after REBP.

For our sample of older workers, that exact implied joint distribution of high worker surplus and low firm surplus, turns out to be predicted by the long-standing hypotheses of implicit contract models, in form of backloading of compensation over the job spell (Lazear, 1979, 1981): in a period-by-period consideration, young (low-tenure) workers are "underpaid", while older (high-tenure) workers are "overpaid".[6] In fact, the Austrian institutional setting features an explicit role for works councils that are consulted in the separation process, providing formal support for such implicit contracts. Consis-

[4]In this world, baseline quits likely reflect large inframarginal shifts such as health shocks. See Manoli and Weber (2016), who provide evidence that workers can delay voluntary quits until reaching the minimum-tenure threshold for eligibility to high severance payments in Austria.

[5]A prediction of this view is that smaller shifts in outside options should not induce workers to separate. Indeed, in Jäger et al. (2018) we find that smaller and shorter-lived shifts in the benefit level did not entail separation effects even among older workers and even during the 1980s in Austria.

[6]Frimmel et al. (2018) shed light on the Lazear hypothesis in the Austrian context. Their evidence suggests that firms with steeper seniority-wage profiles have a higher incentive to renege on implicit contracts in the presence of exogenous shocks. Another class of models that rationalizes the joint distribution is a model of job ladders and negotiation capital by Cahuc et al. (2006), where workers use outside offers to ratchet up their wage and grasp surplus, with experience, tenure and age correlating with high worker surplus and small firm surplus. However, these models feature bilaterally efficient bargaining and separations, and hence cannot explain our full set of explanations that are inconsistent with the Coasean view.

tent with our surplus shifter having improved the nonemployment outside option, the REBP-induced separations went into permanent nonemployment, evidently by workers unlikely to find or seek reemployment. Our explanatory framework relies on limited correlation between the two surplus concepts, as would emerge under some degree of wage rigidity and cross-sectional wage compression.[7]

In a fourth and last step, the paper provides a methodological contribution, as we extend the complier analysis method (Imbens and Rubin, 1997; Abadie, 2003) to difference-in-differences settings such as ours, in order to characterize the pre-separation attributes of the REBP separators. The complier analysis reveals that REBP separators have low worker fixed effects and overwhelmingly come from manual, blue-collar jobs (80%), while a smaller fraction (53%) of survivors stems from that segment of the labor market. Moreover, a majority of dissolved matches stem from shrinking firms. Lastly, REBP separators exhibited lower predicted employability (in form of predicted duration of unemployment) and worked in industries with high rates of sickness and disability among older workers. Overall, for these types of jobs, firms and workers such implicit contracts or the formal constraints imposed by works councils (which are explicitly permitted to veto such layoffs due to "social hardship" clauses) may perhaps be particularly relevant, consistent with our non-Coasean narrative.

Section 2 presents an overview of the Austrian institutional context, the reform, and our data. In Section 3, we document the large separation effects entailed by the UI extension. In Section 4, we study the separation behavior of the treatment and control groups after the program was abolished. Section 5 presents a model that we estimate to formally infer the share of Coasean separations, and present a potential specific non-Coasean setting rationalizing our findings. To trace that narrative in the data, we use survey evidence and complier analysis to characterize the REBP separators in Section 6. Section 7 concludes.

2 Institutional Context, and Data

We review the UI system, the REBP reform, other relevant institutional context as well as our data below.

The Austrian UI System During the 1980s and 1990s Two crucial institutional features ensure that UI generosity cleanly shifts the nonemployment outside option of workers in our setting. First, *Austrian workers are fully eligible for UI benefits upon quitting*, after fulfilling a four-week waiting period. Second, similar to most other European countries, the Austrian system does not feature experience rating, as the Austrian UI system is funded through employer and employee payroll taxes (which were not affected by the reform).

During the 1980s and 1990s, the gross replacement rate was between 40% and 48% for most employees and capped below and above at a minimum and maximum amount.[8] The potential benefit duration (PBD) of UI benefits during the 1980s was 30 weeks, provided the worker had worked (and

[7]In Jäger et al. (2018) we have documented that Austrian wages appear unresponsive to UI benefit level shifts (studying reforms that did not entail separations). We cannot credibly study wage effects in the present context given the large attrition implied by the separation effects.

[8]UI benefits are not taxed. The net replacement ratio, UI benefits over the wage net of social security contributions and income taxes is around 55%, slightly higher than in the US. See Jäger et al. (2018) for details on replacement rates.

paid contributions to the UI system) for at least three out of the last five years, otherwise PBD was 20 weeks. After exhaustion of UI, the unemployed can apply for unemployment assistance (UA, "Notstandshilfe"). UA benefits are means-tested (on income of other household members) and granted for successive 39-week periods, but could in principle be extended forever if eligibility is maintained. UA benefits are capped at 92 percent of UI benefits; in 1990 the median paid UA was about 70 percent of the median UI benefit. 59 [26] percent of the unemployed receive UI [UA].

1988-93 Regional Extended Benefit Program (REBP) In 1988, the Austrian government enacted the Austrian Regional Extended Benefit Program (REBP), a large expansion of potential benefit duration for older (50+) workers in a subset of regions in the country. The policy motivation was to mitigate labor market consequences of a crisis in the iron-, steel- and other heavy industries ("steel sector" in the following). The state-owned company, the Oesterreichische Industrie AG (OeIAG), had suffered from low commodity prices, shrinking markets, and low productivity since the mid-1970s. In response, the new OeIAG management implemented a sequence of restructuring plans during the 1980s, leading to plant closures and downsizing.

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 sector, compared around 5% in the Non-REBP-regions. Before REBP, treated and non-treated regions did not differ in terms of the unemployment rate or the fraction of long-term unemployed.

REBP extended the maximum duration of UI benefits to 209 weeks. To become eligible, a job loser had to satisfy each of the following criteria at the beginning of his or her unemployment spell: (i) age 50 or older; (ii) a continuous work history (780 employment weeks during the last 25 years prior to the current unemployment spell); (iii) location of residence in one of 28 selected labor market districts for at least 6 months prior to the claim; and (iv) start of a new unemployment spell after June 1988 or spell in progress in June 1988. REBP did *not* impose any industry requirement. All unemployed who met criteria (i) to (iv) were eligible, irrespective of whether they previously worked in the steel sector or not. (To minimize UI policy endogeneity problems, our empirical analysis below excludes workers employed in the steel sector.)

Figure 1a visualizes the changes to potential benefit duration brought about by the REBP reform and by the economy-wide UI reform 1989. An additional reform changed potential benefit duration for different groups of workers in August 1989 based on age and experience.[9] Importantly, the economy-wide 1989 reform is orthogonal to the regional variation we analyze and so 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. That national 1989 reform raises PBD for workers aged 40 to 49 [50 and above] to 39 [52] weeks with an experience requirement of 312 [468] weeks of employment in the last 10 [15] years. Figure 1b provides a map of the affected regions.

Therefore, the reform induces variation in several dimensions: first, across age as we can compare workers aged 50 and above to their younger peers and, second, across regions comparing the REBP

[9] 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.

regions to regions not affected by the reform. The confluence of these factors along with the large expansion in potential benefit duration makes the REBP reform an ideal setting in which to study which workers and matches separate in response to improved outside options.

Abolition of the Program REBP was initially in effect until December 1991 before its was extended in January 1992.[10] REBP was then abolished (stopped accepting new entrants) on August 1, 1993, though job seekers who established eligibility to REBP before August 1993 continued to be covered. The abolition decision was formally announced as late as in June 1993, an implementation gap of only two months. The program end occurred in a relatively abrupt fashion: In fact, the Austrian government had come up with a plan in January 1993 to *expand* access to longer benefit duration to older unemployed workers in *all* Austrian regions from one to four years along with changes in the eligibility requirements.[11] In the following weeks and months, the government reversed course completely and abolished the REBP program.

Interaction of UI with Other Social Policies UI interacts with other welfare state programs. In particular, REBP could effectively serve as a bridge into permanent non-employment and hence changed the incentives for men aged 50 and older to leave the labor force. In the absence of the REBP, unemployed men could effectively retire early at age 58 by claiming unemployment benefits for one year and special income support for one other year,[12] before drawing a regular public pension at age 60 (the retirement age for male workers with at least 35 years of insurance contributions). Since the REBP extended the maximum duration of UI benefits by three years,[13] eligible workers could already permanently withdraw from the labor force at age 55.

Another important program was disability insurance (DI). During the study period, the Austrian system granted relaxed access to a DI pension from age 55 onward. DI applicants below age 55 get a DI pension when a health impairment reduces the work capacity by more than 50 percent in any occupation. In contrast, DI applicants above age 55 are considered as disabled if their work capacity is lower than 50 percent in the *same* occupation. In practice, this means that not only health- but also employability-criteria establish DI access after age 55. In the REBP context, relaxed DI-entry at age 55 allowed job losers in REBP regions to retire already as young as age 51 while being on some kind

[10]The 1992 extension enacted two changes for new spells. First, the benefit extension was abolished in 6 of the original 28 regions. We exclude from our analysis the set of treated regions that were excluded after the 1991-reform. Second, the 1992-extension tightened eligibility criteria for extended benefits: new beneficiaries had to be not only residents, but also previously employed in a treated region.

[11]We confirm this course of events in a newspaper content analysis. For instance, Der Standard (a major Austrian newspaper) reported in an article entitled "*Länger Geld für alle Altersarbeitslosen* (Longer benefits for all unemployed workers)" from January 9, 1993: "All older unemployed workers throughout Austria - and not only in [REBP regions] as in the past - will be eligible for unemployment benefits of four years instead of one. Minister of Social Affairs, Josef Hesoun, and the social partners have agreed in principle on this [...]." [translation by the authors].

[12]For men, our sample, special income support is a program available for unemployed workers during the last year before they can claim the regular public pension. For workers with long insurance durations, the statutory retirement age was 60 (unemployed women aged 59) during the study period. Special income support was therefore available at age 59. Special income support is equivalent to UI spell in legal terms, but with 25 percent higher benefits, paid for a period of at most 12 months).

[13]To be precise, the REBP extended UI duration by 3 years for job losers after August 1989 (when the maximum UI benefit duration was 52 weeks), but for 3.44 years (= 209 weeks – 30 weeks) before August 1989 (when the maximum UI benefit duration was still 30 weeks).

of benefit up until age 60 when the public pension could be claimed.

Advance Notice for Layoffs, Works Councils, and Severance Pay While employment protection is not as stringent as in many other (particularly Southern European) countries, an Austrian firm firing a worker has to obey a set of rules. At the time of the REBP, the firm had to give advance notice, which amounted to 5 [4, 3, 2, 1.5] months for workers with at least 25 [15, 5, 2] years of tenure. Workers, too, are obliged to give a (one-month) advance notice.

The Austrian labor law provides a role for works councils in the firing process. In firms with 5 or more employees, workers can organize in works councils. The firm has to inform and consult the works council when a layoff is planned and if the firms fails to do so, the layoff is void. If a layoff violates substantial interests of the worker, the firm has to prove that the layoff is economically necessary for the survival of the firm. The works council must also be consulted when choosing the particular worker to be fired. The works council ensures that potential hardships of layoffs candidates are taken into account, which provides some employment protection for older and longer-tenured workers. Mass layoffs in larger firms are subject to specific further rules. Firms with more than 100 employees that reduce employment by more than 5 percent (or more than 50 employees) within one month must give written notice to the regional employment agency, one month before the mass layoff is implemented, where failure to notify renders the mass layoff void.

In case of a layoff, the firm has to make a severance payment to the worker. The amount is a step function of worker tenure: 3 (5, 10, 15, 20, 25) years of tenure map into 2 (3, 4, 6, 9, 12) monthly salaries, and zero below three years. Severance payments are only due for the following separation types: layoffs (but not dismissals for cause), job terminations upon mutual agreement (between firm and worker), and after the end of a temporary contract. In contrast, worker-induced quits were exempt from the severance-pay rule, except for workers with more than 10 years of tenure.

Data and Sample The Austrian Social Security Database (ASSD) is a matched employer-employee data set covering the universe of private-sector and non-tenured public sector employees in Austria from 1972 onward (Zweimüller et al., 2009).[14] We additionally use the Austrian Labor Force Survey or Micro Census ("Mikrozensus") to trace the nature of separations by type (quits, layoffs, other reasons). The Austrian Micro Census samples representatively based on administrative population registries and follows a rotating panel of households.

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 covered by the REBP only until 1991. We also drop women for data and institutional reasons.[15] The majority of our sample fulfilled the experience requirement; since this sample restriction turned out to not effect our estimates, we present the unconditional results. We report summary statistics for our main

[14] We complement the ASSD with the Austrian Ministry of Social Affairs on employment histories before 1972, to determine whether or not a worker is eligible for REBP (which is based on work experience within the last 25 years). In practice, the experience test was conducted on the basis of similar data.

[15] First, whereas old age insurance rules allow men to retire at age 60, women can 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.

analyses in Table 1. In our empirical analysis of the direct effects of REBP during 1988 and 1993, we will often focus on workers up until 55, for whom the reform made the largest and cleanest difference due to the early-retirement programs described above.

3 Large Separation Effects from the REBP Unemployment Insurance Benefit Extension

In this section, we estimate that the differential benefit extension (from 52 to 209 weeks) increased job separations among eligible workers by 11.2ppt (28%) among initial matches over the five year program horizon, in the treated cohorts compared to their ineligible peers. Most of these excess separations were into long-term unemployment followed by early retirement, rather than to employment with other firms. We present visual evidence of raw data before turning to regression estimates, to assess the parallel trends assumptions underlying our research design.

Plotting Raw Data: Cohort Gradients of Separations We sort the population of 1988 job holders (the onset of the reform) by month-of-birth cohorts and into REBP and non-REBP regions, and then plot outcomes over the course of REBP in Figures 2 and 3. Each figure presents levels for each cohort by region, and the within-cohort, between-region difference. Younger cohorts born after 1943 turned 50 after the REBP was abolished in 1993 and therefore could never claim extended benefits under the program. Older cohorts born before 1933, while eligible for extended benefits, 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. Moreover, they had reached the male retirement age of 60 before the program's abolition in 1993 so that these older cohorts separated by 1993 even in the control region. 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.

We then plot raw data of the fraction of workers in a cohort-region group that separates from their 1988 (defined as a worker-establishment match) by the third quarter of 1993, the first quarter after the REBP had been abolished. We assign workers by the location of their 1988q2 establishment and leave out workers in the 6 regions eligible for the REBP only through 1991 (TR1 regions in Figure 1b).

The red and blue lines of Figure 2(a) show the share of workers in the REBP and control region who had separated from their 1988 employer by 1993. We start from the left, with the older cohorts, our first control group. There is a clear cohort gradient, indicating that older workers' 1988-93 separation share is naturally much higher. For example, the cohorts born before 1933 had mostly retired by the end of the REBP, and therefore hardly any workers in these birth cohorts were still employed with their 1988 employer by 1993.

Our young control group – ineligible cohorts born after 1943 – exhibits a separation rate of roughly 40 percent in *both* regions, and differences between REBP and control regions are flat. The flat pre- and post-trends – small level differences for control (younger and older) cohorts – validate the identification assumptions of our difference-in-differences design by suggesting that labor market conditions were comparable in REBP and control regions during the study period.

Separations are markedly higher for affected cohorts in REBP regions (but not in the non-REBP region), representing the treatment effect of REBP. At its peak, the difference in the share is about 20 percentage points relative to a control region share of about 50 percent.

Potential remaining confounders are shocks or unobservables in separation behavior that vary at the region-by-age level. For instance, pathways to retirement could differ between regions as a consequence of different industry structures. To address this concern, we switch to *age* instead of cohort, now studying workers' separations from their job at age 50 by age 55.[16] Figures 2(c) and (d) show that separations between the ages of 50 and 55 increased sizeably in cohorts exposed to the REBP relative to older and younger non-exposed cohorts, again compared to the gradients in the control region. The job separation probability falls steadily from around 30 percent in older cohorts to just above 50 percent in younger cohorts in the control region (blue line, Figure 2(c)). While the pattern for the pre-1933 and post-1943 cohorts is similar in the REBP regions with slightly higher shares throughout, separations rise sharply for the treated, intermediate cohorts born between 1933 and 1943. As Figure 2(d) shows, the magnitude of the increase at peak is around 20 percentage points.

Building on the lifecycle perspective, in Appendix Figure A.3, we provide a "flip-book" that describes the year-by-year dynamics of the five-year treatment effect. We plot variants of Figures 2(c) and (d) from the quarter before turning 50 to the quarter before turning 55, indicating via a vertical dashed line the cohort that became newly eligible at that age. The figure reveals a clear spike in separations when workers age into eligibility, or at the onset of REBP for the older workers who were born before 1938 and therefore become immediately eligible. This spike suggests the presence of "pent-up" marginal matches with persistent heterogeneity in surplus in our sample.

Finally, Figure 3 plots *quarters employed* (panels (a) and (b)) and unemployed (panels (c) and (d)) between 1988q2 and 1993q3, mirroring Figure 2.[17] 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, Figure 3 reveals an economically significant decrease of employment and a tantamount increase in unemployment of almost four and three quarters at peak, respectively. We find similar results when we consider the sample of employed workers at age 50 and track their employment outcomes through age 55 (see Appendix Figure A.2).

Regression Estimates of Treatment Effect We complement the graphical evidence with regression estimates on the average treatment effect in Table 2, in a difference-in-differences specification on the population of workers holding a job in 1988 before the onset of the reform with fixed effects for region r and cohort c:

$$D_{rci} = \beta + \phi_r + \psi_c + \nu \cdot \underbrace{\text{REBP}_r \times \text{Treated Cohort}_c}_{Z_{rc}} + \chi_{rci}, \tag{1}$$

[16]In practice, we select the job in the quarter before the 50th birthday (right before aging into REBP eligibility), and the separation outcome in quarter before turning 55 (as the disability and early retirement incentives changed at 55).

[17]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.

where D_{rci} are various outcomes described below, for worker i in region r born in cohort c. The coefficient of interest ν captures the effect of REBP eligibility Z_{rc}.[18] The model includes a region effect ϕ_r and cohort effects ψ_c. Our regression specification thus exploits within-region, within-cohort variation. In the main specifications we report, we cluster standard errors at the level of administrative regions (groups of districts, *Arbeitsamtsbezirke*) but have also assessed robustness for clustering at other levels. Our main table reports on the cohort-based design (1998-93 outcomes) in Table 2; we additionally report the age-based estimates (50-55) in Table A.2, finding similar results. Our sample is composed of workers born between July 1933 and July 1948; we do not use older cohorts because they had mostly retired by 1993. We keep the young control cohorts to a five-year range to isolate the workers most comparable to the older, treated cohorts in the labor market. As in the raw data plots, we ignore workers in the few municipalities where the REBP was abolished early.

Table 2 column (1) shows our main result: an 11.2 ppt increase in separations among initially employed workers. Relative to the control group mean of 41.0% this represents about a 28% increase in separations. The 95% confidence interval for the separation effect ranges from 2.4 to 20.0 ppt.

We then split the effects on separations into the two possible types: separations either into non-employment (E-N separations) or into new jobs (E-E separations). REBP-induced separations are entirely made up of the former. In column (2), we report a sizable increase in E-N separations of 16.2 ppt (se 5.2 ppt). Meanwhile, the REBP program *decreased* E-E separations, with a point estimate of -5.1 ppt (se 1.4 ppt) in column (3). However this effect is statistically insignificant when investigating the age horizon (age 50 to 55) rather than years (see Appendix Table A.2), suggesting that unobserved differences at the region-by-age level play a role in E-E separation behavior. Column (4) of Table 2 reports effects on quarters employed, finding a negative effect of 4.5 months (se 1.1). Column (5) reports that quarters unemployed increased by a similar order of magnitude (3.1 months, se 1.6). Column (6) shows that a large share of the decrease in employment can be accounted for by a reduction of 3.1 months (se 1.2) in employment with the initial employer, such that our setting does not represent the standard temporary layoffs mechanism (Feldstein, 1976).

Taken together, the evidence therefore cleanly shows that REBP benefit extensions triggered a large number of separations (11.2ppt or 28% increase), shorter employment with the initial employer, and a tantamount increase in unemployment. This effect comes in response to a treatment 209 weeks of potential benefit duration compared to 52 weeks in the older control group, i.e. a differential four-fold increase in potential duration of UI benefits.[19]

4 Puzzle to the Coasean Hypothesis: No Attenuated Separations Among REBP Survivors After its Surprise Abolition in 1993

Next, we exploit the surprise abolition of the reform in August 1993 (described in Section 2) to study whether REBP "survivors" – jobs that existed before the onset of the reform in 1988 and continued

[18]Workers are eligible if they reside in the REBP region, REBP_r, and are members of a treated cohort, i.e. such that they were aged between 50 and 55 at some point between the start of the program in 1988 and its end in 1993.

[19]Both groups had at most 30 weeks PBD in 1988, but a national reform in 1989 increased that level to 52 weeks, leading us to choose this benchmark given the program duration through 1993.

through its abolition in 1993 – subsequently exhibited lower separations unconditionally and in response to identified labor demand shocks.[20]

We find that, after the abolition of the reform, the "survivors" in the dramatically shrunk former treatment group exhibited exactly the same separation behavior as the control group – on average, and in response to industry and firm labor demand shifts. Together, the large quantity of REBP-induced separations before 1993, and the zero differential post-REBP behavior, present a clean and transparent, and largely model-free, test of a core prediction of the Coasean view of jobs. We will dedicate the rest of the paper to understanding this result.[21]

4.1 Background on Coasean Prediction: Missing Mass of Marginal Jobs in Formerly Treated Group

Here we lay out out the intuitions of our conceptual framework, which we present as a formal model in our structural estimation in Section 5.

The Coasean benchmark of jobs implies an ordered set of marginal jobs that drive separations in response to any surplus-relevant shock: small shocks will destroy jobs with small joint surplus, larger shocks will destroy jobs with larger joint surplus. Hence, the REBP shock – which reduced surplus by boosting worker's nonemployment outside option – ought to have shifted the composition of *surviving* employment relationships towards jobs with higher *initial* surplus, or *gross-of-REBP* surplus. If outside options are subsequently reduced again, the allocative surplus distribution of the surviving jobs would be shifted to the right and therefore left-truncated, setting the density of jobs below the truncation point to zero.

That missing mass of marginal matches would equal the mass of compliers: the treatment effect of REBP on separations, which we estimated in the previous section. Unlike in the previous analysis, we now naturally do not expect the sample to be similar: in fact, we *know* that the older cohorts in the REBP region underwent a dramatic policy that removed an extra 11.2ppt of workers from their ranks, increasing separations by around 28% (Table 2, Column (1)).

A testable prediction of the Coasean view of jobs is that after the REBP abolition, the overall group of REBP survivors should be more resilient – have higher surplus and hence lower separation rates – compared to control group, where these marginal, low-surplus jobs are still present.

By contrast, non-Coasean settings – spelled out in detail in Section 5 – do not feature a one-dimensional ordering of jobs by joint surplus. For example, suppose that wages were rigid or firing restrictions were active, so that REBP selected workers into separation based solely on *worker* surplus rather than *joint* job surplus. Then, that group may still be more sensitive to worker surplus, but not to shocks that primarily affect firm surplus (for a given wage). In other words, the matches that

[20] That is, we essentially test for a dynamic known as "harvesting effects" in demography (see, e.g., Schwartz, 2000; Basu, 2009), usually applied to transitory negative health shocks (e.g., heat waves) that induce low-health individuals into exit and hence reduce the subsequent average mortality rate of the survivors.

[21] A previous conference draft version of our research reported on high dimensional interaction effects and parametric reduced-form specifications testing for resilience. That specification turned out to suggest spurious resilience arising from the inclusion industry fixed effects with leave-out-mean industry employment shifts as the dependent variable, with hard-to-evaluate residual variation in a triple interaction. That result further was not robust to variations in the surplus shock variable to separation rather than employment growth, or to instrumenting for Austrian employment growth with German industry growth.

separated in response to REBP need not have been the matches with low firm surplus.[22]

4.2 Baseline Results

Plotting Raw Data: Post-REBP Separation Rate Gradients by Cohort In Figures 4 (levels) and 5 (differences) we plot the post-REBP separation gradient by cohort for "REBP survivors" in the treatment group and the control groups: the jobs already active right before the onset of REBP in 1988 that continued through its abolition in 1993. In practice, we allow for REBP spillovers due to layoff notices and explicit grandfathering that the law permitted for pre-scheduled layoffs (see Section 2).[23] Hence, our cutoff is 1994q1. Except for this sample restriction, the figures mirror the cohort gradients of separations in Figures 2 and 3. We explore the fraction of REBP survivors subsequently separating at various horizons.

Figures 4 and 5 reveal that there are no post-REBP separation differences whatsoever among surviving jobs exposed to the REBP compared to those those that did not.

Both figures feature a yellow dashed line, which represents the *predicted* gradient from the Coasean model, which we formally derive and discuss in Section 5, and for now note that the gap between the prediction and the Coasean benchmark is quantitatively large, confirming that our test has power.

Regression Evidence To gauge quantitative effects and to assess confidence intervals, we again estimate the difference-in-differences model in Equation (1) on the current sample (REBP stayers) and the post-REBP separation outcomes. We choose 1994–1996 as one example but report the full set of horizons in Appendix Section F. Specifically, we track the labor market status of "REBP survivors" from 1994q1 through 1996q1, following the same birth cohorts and regions but with the additional "survival" restriction of being observed in the same establishment in 1988q2 and 1994q1. Just as in the previous section, we ignore those working in the regions where REBP was abolished in January 1992. We estimate, for a series of indicator outcomes,

$$D_{rci} = \beta + \phi_r + \psi_c + \nu \cdot \underbrace{\text{REBP}_r \times \text{Formerly Treated Cohort}_c}_{Z_{rc}} + \chi_{rci}, \tag{2}$$

where ϕ_r and ψ_c are region and cohort fixed effects. As before, we cluster standard errors at the administrative region level. The coefficient of interest remains ν, the difference-in-differences between REBP-eligible and -ineligible cohorts and regions.

We report results in Table 3. This basic difference-in-differences analysis of post-abolition separation behavior suggests that, if anything, separation rates are slightly *higher* (1.3ppt off a base of 24.5%) and employment spells slightly *shorter* (a fifth of a month, off a base of 24 months) among the formerly treated group.

[22] A third view, which we lay out in the model in Section 5, permits for some convergence of the distributions, such that the Coasen view may be somewhat masked.

[23] This grandfathering likely played a role in the additional increased separation rate in the REBP region immediately after the program's abolition. Appendix Figure A.4 documents these additional separations by separating the within-cohort regional difference from 1993q3 to 1994q1 into quintiles of industry growth over the same time period. In Section 5.5, we clarify that a version of the non-Coasean model can rationalize these patterns even without grandfathering.

4.3 Labor Demand Shocks

We present a series of additional results below to illustrate the robustness of the absence of differential post-REBP separation behavior. We have conducted a long series of robustness checks, largely unreported, that confirmed the absence of differential post-REBP separation. Here, we test whether *negative labor demand shocks* may unmask the potentially underlying missing mass of marginal matches that, by the Coasean view, should render the formerly treated group less sensitive to shocks. Both at the industry and at the establishment level, we find that separation behaviors of the groups remain indistinguishable.

Heterogeneity by Industry Growth Since we study the *separation* margin, the Coasean benchmark would predict resilience to negative shocks in particular. We therefore plot the differential separation rates among jobs that survived REBP separately for the top, middle and bottom tercile of the industry growth distribution from 1994 onward in Figure 6. Even in declining industries (all in the 1st tercile), when joint surplus is arguably shrinking, REBP cohorts do not exhibit relative resilience compared to the control group.

Establishment-Level "Hockey Sticks" We construct proxies for separation-inducing establishment labor demand shocks by building on the "hockey stick" graphs (Davis et al., 2013): net establishment growth (on the x-axis) is associated with increasing hiring rates for positive growth, but exhibits a kink at zero. The negative growth region features a steeply negative slope of separation rates, implying resort to separations (layoffs) more and more for net negative growth. Exploiting the matched employer-employee dimension of our population data, we replicate these graphs in Figure 7(a), where we plot average annual separation rates (of men employed in Q1) by bins of annual establishment growth rates.[24]

Figure 7(b) plots *cohort-region-specific* hiring and separation rates through 1996, among the sample of jobs that survived the REBP period through 1994q1. We focus on cohorts from 1936 through 1948 to avoid retiring cohorts. The results are naturally noisier due to the shrinking sample but are informative because the Coasean view would predict that separations occur by a pecking order following the ranking of job surplus. We estimate linear slopes separately for shrinking and growing establishments and for four separate groups: by birth cohort eligibility (born July 1936- July 1943 vs. August 1943-July 1948) and regional eligibility (being a stayer in a REBP region or not).

The slopes for the old ineligible, non-REBP workers and the formerly treated and formerly eligible old workers in the REBP group lie almost on top of each other. This provides additional evidence that the massive extraction of potentially marginal jobs due to REBP does not seem to affect subsequent layoffs (or separators) at firms.[25]

[24]We focus on establishments with at least 25 employees and on years 1994 through 1998. We exclude cohorts born before 1933 since they are past the male retirement age of 60 in 1994, and establishment spin-offs, take-overs, and administrative changes to the administrative data using the procedure outlined in Fink et al. (2010).

[25]We also experimented with plotting the relationship for firm cells where the treatment effects were particularly pronounced, and the patterns remained robust.

Lastly, we report cohort-specific slopes, estimating for each birth-year cohort c and region r,

$$\begin{aligned}
\text{Sep}_{i,1994+t} = &\sum_c \sum_r \beta_1^{c,r} \cdot \Big(\text{EmpGrowth}_{e(i),1994+t} \times 1(\text{EmpGrowth}_{e(i),1994+t} < 0) \times \mathbb{1}_{c,r}\Big) \\
&+ \sum_c \sum_r \beta_2^{c,r} \cdot (\text{EmpGrowth}_{e(i),1994+t} \times \mathbb{1}_{c,r}) \\
&+ \sum_c \sum_r \beta_3^{c,r} \cdot (1(\text{EmpGrowth}_{e(i),1994+t} < 0) \times \mathbb{1}_{c,r}) \\
&+ \sum_c \sum_r \beta_4^{c,r} \cdot \mathbb{1}_{c,r} + \xi_i,
\end{aligned} \tag{3}$$

where $\text{Sep}_{i,1994+t}$ is an indicator of whether a worker i employed in 1994q1 is still employed with the same establishment e in Q1 of year $1994 + t$. Our sample on the left-hand side are again the 1988-94 job stayers. $\text{EmpGrowth}_{e(i),1994+t}$ is the change in *total* establishment employment between 1994q1 and Q1 of year $1994 + t$, in establishment e where individual i is employed in 1994. $\mathbb{1}_{c,r}$ is an indicator for being in cohort c and region r. The coefficient of interest is $\beta_1^{c,r} + \beta_2^{c,r}$, the sensitivity of separations to downsizing at the establishment level by year of birth c and region r.

Figure 7(c) plots the estimates from this regression for separations/growth rates from 1994 to 1996 ($t = 2$), with similar results for other years. In both the REBP and the non-REBP region, the 1988-94 job stayers (REBP survivors) exhibit a downward-sloping sensitivity gradient in birth date, indicating that older workers appear shielded from a given establishment shrinkage event, perhaps due to seniority rules (see Section 2), larger job values for human capital reasons, or lower outside options among these older workers (Oi, 1962). Yet, the lines lie on top of each other (in fact, the REBP cohort appears sligthly *more* exposed to firm shocks): REBP and non-REBP birth cohort exhibit the same sensitivity of separations to negative establishment labor demand shifts.

5 Model and Structural Estimation: Which Share of REBP Separations Were Coasean?

In this section, we provide a formal quantitative assessment about the share of jobs whose separation behavior was consistent with Coasean separations, by deriving and then structurally estimating a formal model of Coasean and frictional job separations.

5.1 Two Benchmark Models of Jobs and Separations: Coasean vs. Non-Coasean Separations

The framework formalizes how improvements in worker outside option affect separations and truncate the distribution of job surplus among the surviving jobs, generating a missing mass of marginal matches. In consequence in the Coasean setting, the formerly-treated group should be less sensitive to any subsequent negative shocks, in stark contrast to our empirical findings. We then also present one alternative non-Coasean setting that can rationalize our results.

Jobs and Surplus Generally, jobs carry worker surplus S^W and firm surplus S^F, each of which must be non-negative: each party $i \in \{W, F\}$'s inside job value V^i_{In} (amenities, productivity,...) plus/minus the wage, amounts to at least her (separation) outside value V^i_{Out}:

$$S^W(\mathbf{V}^W, w) = V^W_{\text{In}} + w - V^W_{\text{Out}} \geq 0, \tag{4}$$

$$S^F(\mathbf{V}^F, w) = V^F_{\text{In}} - w - V^F_{\text{Out}} \geq 0, \tag{5}$$

where $\mathbf{V} = \{V^i_a\}_{i \in \{W,F\}; a \in \{\text{In},\text{Out}\}}$, and sometimes $\mathbf{V}^i = \{V^i_a\}_{a \in \{\text{In},\text{Out}\}}$. Alternatively, we can define *joint* job surplus, which the wage splits between the worker and firm:

$$S(\mathbf{V}) = \overbrace{V^W_{\text{In}} + V^F_{\text{In}} - V^W_{\text{Out}} - V^F_{\text{Out}}}^{S^W(\mathbf{V}^W, w) + S^F(\mathbf{V}^W, w)}. \tag{6}$$

Figure 8 Panel (a) plots the two-dimensional job space. The x-axis denotes worker surplus, and the y-axis denotes firm surplus. The figure plots various case studies of jobs characterized by different surplus coordinates. The *solid* circles (•) denote *gross-of-wage surpluses*, i.e. $V^W_{\text{In}}(b) - V^W_{\text{Out}}(b)$ for the worker and $V^F_{\text{In}}(b) - V^F_{\text{Out}}(b)$ for the firm. This is the surplus combination these job "fundamentals" would trigger before wage setting, or equivalently in the scenario of a zero wage. The *empty* circles (∘) denote *net of wage surpluses*: for each gross job, we provide various examples of potential wages. Wages move net surpluses of the parties along the 135 degree line: the iso-joint-surplus line.

The figure also partitions jobs into four regions of viability: feasible jobs (north east, solid line), quits (north west, dashed line), layoffs (south east, dotted line) and mutual separations (south west, dot-dash-patterned line). For a job to be viable, it must be in the north east corner, providing positive surplus to both parties. Three natures of separations are represented by the three remaining corners. *Quits* emerge if the worker is in negative surplus territory, while the firm would prefer to continue. This case would emerge in job A, which is "born" such a potential quit case. Yet, thanks to redistribution in form of a positive wage, the job is moved into the viable quadrant, A_1. The wage can also "overshoot": job A_2 has too high a wage, playing the job firmly into positive territory from the worker's perspective, but pushing it into negative firm surplus territory, leading to a *layoff*, along with all jobs in the south east quadrant. By contrast, job B is "born" in the feasible region even without a wage, as one would imagine with, e.g., an internship or a high-amenity job, for which workers would work for free. Yet the figure plots two ways to have wages entail separations: B_1 has too positive a wage, leading to a layoff. B_2 has a too negative a wage, entailing a quit. Job C goes one step further, where a no-wage scenario would have the firm prefer a layoff, but too low (negative) a wage would entail a quit (C_1), whereas any positive wage would leave the job in layoff territory (C_2). By contrast, doomed jobs such as X are born in negative surplus territory for both parties, and so provide negative joint surplus such that no wage can be found to fulfill both participation constraints. Wages in the south west quadrant hence entail a *mutual separation* (otherwise a quit or layoff). Finally, M is a "marginal" job, carrying exactly zero joint surplus. That job is born in, e.g., quit territory, but *can* be rendered feasible with a unique wage that moves that job into the origin, where both parties enjoy exactly zero unilateral surplus. Any increase [decrease] from that wage will entail a layoff [quit].

Coasean Bargaining In the Coasean (i.e. efficient bargaining) benchmark, the two-dimensional condition for job viability, Equations (4) and (5), collapses to a one-dimensional, single condition. This is because the parties find a wage within the bargaining set of reservation wages $w \in [\underline{w}^W, \overline{w}^F]$, any of which implements the bilaterally efficient allocation: forming and maintaining matches that carry non-negative *joint* – rather than private – job surplus (i.e. whenever $\overline{w}^F \geq \underline{w}^W$).[26] The essence of the Coasean setup is that the parties find a wage to split the joint surplus to leave both participation constraints fulfilled. In the figure, this means that jobs move along the iso-joint-surplus curve, the 135-degree line. Efficient bargaining renders feasible all jobs along a positive iso-joint-surplus line (i.e. north east of the marginal-jobs frontier).

Non-Coasean Bargaining With frictions that prevent such efficient and flexible bargaining, e.g., wage rigidity, the Coasean allocation is not necessarily attainable. In our model depicted in Figure 8 Panel (a), such frictions prevent the parties from moving towards a wage in the feasible-jobs frontier even though the job carries positive joint surplus, thereby shrinking the set of feasible jobs in the set of jobs with values $\mathbf{V}$. Going forward, we think of wage w as one additional job attribute that can evolve or be fixed, such that jobs are now characterized by $(w, \mathbf{V})$, and unilateral worker and firm surpluses $S^W(w, \mathbf{V})$ and $S^F(w, \mathbf{V})$ are allocative.

Below, we formally explain why our evidence from unique empirical laboratory of the REBP reform rejects the Coasean hypothesis in favor of the non-Coasean view, and we derive our estimating equations.

The UI Extension (REBP) We think of the treatment, an increase in UI generosity (a binary variable $Z \in \{0, 1\}$ such that $b_Z = b_0 + Z \times \Delta b$, with $Z = 1$ for the treatment group and $Z = 0$ for the control group), as primarily improving the worker's outside option $V^W_{\text{Out}}(b)$, such that the worker surplus size of the shock is $\varepsilon^W_b = V^W_{\text{Out}}(b_0) - V^W_{\text{Out}}(b_0 + \Delta b) > 0$, where our convention is that a positive ε^W_b denotes a *negative* shock.[27] In our Austrian context, described in Section 2, even worker-sided quits receive full benefits (after a brief waiting period). There is no experience rating. UI take-up is high. Alternatively, due to moral hazard and efficiency-wage mechanisms, the worker's improved outside option may lower productivity (Shapiro and Stiglitz, 1984; Akerlof and Yellen, 1986; Katz, 1986) and thus the firm's inside value, $V^F_{\text{In}}(b_0)$. Or, surplus may fall if implicit firing costs fall when workers stand to lose less from a separation, in effect increasing $V^F_{\text{Out}}(b)$.[28] Still, these alternative mechanisms

[26]For example, by Nash bargaining, the worker (firm) receives their outside option [or reservation wage], plus fraction β (resp. $1 - \beta$), the party's bargaining power, of the surplus (the reservation wage difference):

$$\max_w \left([V^W_{\text{In}} + w] - V^W_{\text{Out}} \right)^\beta \cdot \left([V^F_{\text{In}} - w] - V^F_{\text{Out}} \right)^{1-\beta} \Rightarrow w^N = [V^W_{\text{Out}} - V^W_{\text{In}}] + \beta \cdot S = \underline{w}^W + \beta \cdot [\overline{w}^F - \underline{w}^W].$$

[27]For exposition, the surplus shock is modeled as homogeneous for all workers. With heterogeneous treatment effects, marginal jobs also comprise workers with particularly large shock valuation.

[28]For example, since firms may backload compensation due to agency concerns, firms' flow surplus and the continuation value from older workers is negative *gross of firing costs*, generated, e.g., by the erosion of the firm's reputation to honor such implicit contracts (Lazear, 1979, 1981; Hall and Lazear, 1984; Bewley, 2002). b may also enter the firm's separation value through shifts in recruitment costs or quality of replacement hires. However, our empirical design would net out this mechanism with a similar yet ineligible slightly younger control group presumably close substitutes to and in the same labor market as the slightly older colleagues.

ultimately arise from the worker's outside option. In Appendix A, we calculate the cash value of the reform as 71% of a worker's annual salary.

5.2 Effects of REBP in the Coasean Setting

Intuitions To study the group-level effects of REBP, we now switch gears from individual job case studies to the *distribution* of heterogeneous jobs. Figure 9 depicts, as our expositional example, the contour maps of the joint distribution of worker and firm surplus embedded in the four quadrants of Figure 8 Panel (a) and illustrates the evolution of jobs, the treatment effect of REBP, which jobs it destroyed, and the consequences for post-REBP job dynamics. The figure plots the Coasean (efficient bargaining, left panels) and non-Coasean (fixed-wage, right panels) settings. The figures plot contour maps of the density of the joint distribution of firm (y-axis) and worker (x-axis) surpluses; darker shades indicate higher densities, at the example of a bivariate normal distribution.

We start with the Coasean setting in Panels (a.C), (b.C), and (c.C). Panel (a.C) depicts how REBP lowers *joint* surplus by $\varepsilon_b^W = V_{\text{Out}}^W(b + \Delta b) - V_{\text{Out}}^W(b) > 0$. That is, REBP shifts all potential jobs to the west, along worker surplus, by ε_b^W and thereby extracts – pushes into separation – all matches with "gross-of-ε_b^W" surplus below ε_b^W. These jobs drive the treatment effect on separations documented in Section 3.

After REBP is abolished, depicted in Figure 9 Panels (b.C) and (c.C), all surviving matches' surplus is restored again to their peer in the control group, except that the abolition does not bring back to life the previously destroyed jobs – since we track survivors only. The former treatment group features a missing mass of marginal matches with respect to REBP. This gap is indicated by a parallel gap between the surviving matches and the zero-joint-surplus line. By contrast, these low-surplus jobs continue to be present in the former control group.

The testable prediction characterizing the Coasean view is that the former treated group should exhibit dramatic insensitivity to *any* post-REBP surplus shocks compared to the control group. Figure 9 Panels (b.C) and (c.C) illustrate this feature in form of shifts in the worker component of surplus to the west, which moves the job down the ranking of iso-joint-surplus lines. But as long as the subsequent shock is smaller than the REBP shocks, it will not entail *any* separations. Importantly, in the Coasean setting, the same resilience would emerge with an equally sized – southward – decline in firm surplus. Hence due to efficient (re-)bargaining and hence joint surplus serving as the allocative concept, the missing mass of low-joint-surplus matches emerging from REBP henceworth isolates the formerly treated group's REBP survivors' separations to subsequent surplus shocks of *either* kind.

Formal Model Formally and more generally, separations (during [after] REBP denoted by δ [Δ]) occur if S' were to turn negative, either aggregate shocks (e.g., ε_b^W from the shift in UI benefits) or idiosyncratic shocks (health, productivity, amenities,...) following Markov process $k(\mathbf{V}'|\mathbf{V})$, where, going forward, x' denotes the next-period value of x. We define $\tilde{S}(\mathbf{V}')$ as the short-hand for the surplus level gross of a given aggregate surplus shifter, such that, for an aggregate shock $-\varepsilon' < 0$, $\widetilde{S}(\mathbf{V}', \varepsilon' = 0) = S(\mathbf{V}', \varepsilon') - \varepsilon'$. For REBP, $\varepsilon' = \varepsilon_b^W$, and hence separations in the treatment [control] group $Z = 1[= 0]$ are:

$$\delta^Z = \int_{\mathbf{V}} \int_{\mathbf{V}'} \mathbb{1}(\widetilde{S}(\mathbf{V}') < Z \times \varepsilon_b^W) k(\mathbf{V}'|\mathbf{V}) f_{\text{pre}}^Z(\mathbf{V}) d\mathbf{V}' d\mathbf{V}. \tag{7}$$

where $f_{\text{pre}}(.)$ denotes the distribution prevailing at the onset of REBP, where we will assume that initial distributions are the same across groups $f_{\text{pre}}^0(.) = f_{\text{pre}}^1(.)$. In Appendix Table A.1, we confirm this condition for observable characteristics. By contrast, f^Z will denote *post*-REBP distributions that will naturally diverge due to REBP.

Separations are efficient: all gains from trade are exhausted in that no wage w can be found to fulfill both parties' participation constraints. The *marginal jobs* extracted by REBP make up set $M = \{\mathbf{V} : 0 \leq \widetilde{S}(\mathbf{V}) < \varepsilon_b^W\}$. The wider set in the control group is $J = \{\mathbf{V} : \widetilde{S}(\mathbf{V}) \geq 0\}$, hence prone to separations for even small negative shocks.

We discuss the role of idiosyncratic shocks in Section 5, but henceforth suppose that in the short run, convergence through idiosyncratic shocks $k(.|.)$ are of limited importance: the Markov process is the identity matrix I, such that job values remain constant, i.e. $\mathbf{V}' = \mathbf{V}, \forall \mathbf{V}$. In practice, the condition for resilience is *some* persistence in surplus in the very short run (one year in our case), so that separations simplify to the case illustrated in Figure 9:

$$\delta^Z = \int_{\mathbf{V}} \mathbb{1}(\widetilde{S}(\mathbf{V}') < Z \times \varepsilon_b^W) f_{\text{pre}}^Z(\mathbf{V}) d\mathbf{V}, \tag{8}$$

and the treatment effect estimated Section 3 simplifies to

$$\begin{aligned} \delta^1 - \delta^0 &= \int_{\mathbf{V}} \int_{\mathbf{V}'} \mathbb{1}(0 \leq S(\mathbf{V}') < \varepsilon_b^W) k(\mathbf{V}'|\mathbf{V}) d\mathbf{V}') f_{\text{pre}}^0(\mathbf{V}) d\mathbf{V} = \int_{\mathbf{V}} \mathbb{1}(0 \leq \widetilde{S}(\mathbf{V}) < \varepsilon_b^W) f_{\text{pre}}^0(\mathbf{V}) d\mathbf{V} \\ &= \int_{\mathbf{V} \in M} f^0(\mathbf{V}) d\mathbf{V}. \end{aligned} \tag{9}$$

where the last step follows from the absence of idiosyncratic shocks and our normalization that no aggregate shock occurred during REBP except for the treatment. (In practice, idiosyncratic shocks will have generated "always-separators".)

After the abolition of REBP, the program has truncated the treatment group's joint-surplus distribution below ε_b^W, such that density $f^1(\mathbf{V})$ is zero, while the inframarginal REBP survivors reflect the (conditional) distribution in the control group starting from truncation point ε_b^W:

$$f^1(\mathbf{V}) = \begin{cases} 0 & \text{if } \ \mathbf{V} \notin (J \setminus M) \Leftrightarrow \ S(\mathbf{V}) < \varepsilon_b^W \\ \frac{f^0(\mathbf{V})}{1 - \int_{\mathbf{V} \in M} f^0(\mathbf{V}) d\mathbf{V}} & \text{if } \ \mathbf{V} \in (J \setminus M) \Leftrightarrow \ S(\mathbf{V}) \geq \varepsilon_b^W. \end{cases} \tag{10}$$

The post-REBP resilience of the formerly treated group can be formalized by considering *aggregate* (common to both groups) surplus shocks $\varepsilon^{F\prime}$ and $\varepsilon^{W\prime}$:

$$\Delta^Z = \int_{\mathbf{V}} \mathbb{1}(\widetilde{S}(\mathbf{V}') < \varepsilon^{W\prime} + \varepsilon^{F\prime}) f^Z(\mathbf{V}) d\mathbf{V}. \tag{11}$$

We now derive the separation rate of the former treatment group by replacing its densities as truncated versions of the control group's, as following Equation (10):

$$
\begin{aligned}
\Delta^1 &= \frac{1}{1-\int_{\mathbf{V}\in M} f^0(\mathbf{V})d\mathbf{V}} \int_{\boxed{\mathbf{V}\setminus M}} \mathbb{1}(\widetilde{S}(\mathbf{V}) < \varepsilon^{W\prime} + \varepsilon^{F\prime}) f^{\boxed{0}}(\mathbf{V})d\mathbf{V} \\
&= \frac{1}{1-\int_{\mathbf{V}\in M} f^0(\mathbf{V})d\mathbf{V}} \int_{\boxed{\mathbf{V}}} \mathbb{1}(\widetilde{S}(\mathbf{V}) < \varepsilon^{W\prime} + \varepsilon^{F\prime}) f^{\boxed{0}}(\mathbf{V})d\mathbf{V} \\
&- \frac{1}{1-\int_{\mathbf{V}\in M} f^0(\mathbf{V})d\mathbf{V}} \int_{\boxed{\mathbf{V}\in M}} \mathbb{1}(\widetilde{S}(\mathbf{V}) < \varepsilon^{W\prime} + \varepsilon^{F\prime}) f^{\boxed{0}}(\mathbf{V})d\mathbf{V} \\
&= \frac{1}{1-\int_{\mathbf{V}\in M} f^0(\mathbf{V})d\mathbf{V}} \times \left[\boxed{\Delta^0} - \int_{\boxed{\mathbf{V}\in M}} \mathbb{1}(\widetilde{S}(\mathbf{V}) < \varepsilon^{W\prime} + \varepsilon^{F\prime}) f^{\boxed{0}}(\mathbf{V})d\mathbf{V} \right].
\end{aligned} \tag{12}
$$

Figure 8 Panels (b) and (c) depict treatment and control groups' post-REBP separations as a function of worker and firm surplus shock. The Coasean setting (approximated to no idiosyncratic shocks) predicts resilience: both types of shocks leave the formerly treated group's separations flat at zero up until the point hits the truncation point of surplus (the REBP surplus threshold), and from that point increases somewhat more steeply, eventually meeting the control group at 1 for very large surplus shocks.

Using the last step of equation (9), we can rewrite expression (12) in an empirically tractable form of realized *control group* post-REBP separation rates and the REBP treatment effect, as a sufficient statistic for the (impossible to measure) surplus concepts:

$$\Rightarrow \Delta^1_{\text{Coasean}} = \max\left\{0, \frac{1}{1-(\delta^1-\delta^0)}\left[\Delta^0 - (\delta^1-\delta^0)\right]\right\}. \tag{13}$$

Figure 8 Panel (d) plots a theoretical version of Equation (13) (red dashed line), post-REBP treatment group against control group separations. By the Coasean theory, no separations should occur in the treatment group *unless the control group separation rate* exceeds the truncation point in the surplus distribution (the share of marginal matches). Starting from that kink, the slope will be somewhat steeper, as both groups will have separation rates of 100% if all control jobs dissolve.

Empirical Evaluation: Cohort Graphs To gauge the gap between the data and a benchmark model, we compute and provide the *predicted* separations (yellow dashed line) following a strictly interpreted Coasean view as presented in Equation (13). Specifically, for each cohort, we collect the REBP separation rates building on the difference-in-difference results in Section 3 depicted in Figure 2 to proxy for δ^Z, and feed in post-REBP cohort-specific separation rates from the control group Δ^0 (Figure 4). The yellow dashed line in Figures 4 (levels) and 5 (differences) plots this predicted Coasean benchmark, along with the original realized post-REBP separation rates by cohort for the treatment and control groups. The figures confirm that the design has power: For example, within the first year (Figure 4(a)), the benchmark model would predict *zero* separations in the formerly treated group, whereas the control group's actual post-REBP separation rate hovers around 20% in these cohorts.[29] The empirical separation rate in the formerly treated group nearly perfectly coincides with the control

[29] Separations spike once a birth cohort turns 60 years old, the age of retirement for Austrian men in this period.

group, in stark contrast to the benchmark model. Moreover, Figures 4(b) through (d) clarify that over longer horizons, the design retains power but the differences shrink, because the control group separation increase, so that the difference between the two groups shrinks.

This benchmark clarifies that the absence of any attenuated separation responses whatsoever among the treated cohorts documented in Section 4 is quantitatively significant because the effect induced by the REBP on separations was so dramatic (Section 3 and Figures 2 and 3).

5.3 Effects of REBP in the Non-Coasean Setting

The evidence is therefore clearly inconsistent with the predictions from the Coasean model. We now sketch out a non-Coasean alternative that, in particular, can generate the perfect comovement between treatment and control groups.

Intuitions Building on the intuitions of job dynamics from the basic framework in Section 5.1 and Figure 9 Panels (a.NC), (b.NC), and (c.NC), we next present the narrative of REBP and its aftermath in the non-Coasean setting, again with the example of contour maps that now plot the density of the joint distribution of firm (y-axis) and worker (x-axis) *net of wage* surpluses. Figure 9 Panel (a.NC) illustrates the treatment effect documented in Section 3: REBP improved worker's outside option. In the non-Coasean setting with wages not following efficient (re-)bargaining, the incidence is therefore on worker surplus, hence the treatment group's jobs (joint distribution of worker and firm surplus) shifts to east. While worker surplus may be large initially (before REBP, as in the control group), REBP is large, and therefore pushes into separation marginal jobs with *worker* surplus. Rather than the Coasean diagonal line of zero joint surplus, the inequality conditions allocative for separations are now the unilateral participation constraints of positive worker surplus and positive firm surplus. As a result the truncation in response to a worker surplus shift such as REBP (UI generosity that boosts workers' outside option) selects *relatively* low worker surplus jobs.

After REBP, Figure 9 Panel (a.NC) illustrates that the former treatment group features a missing mass of marginal matches *with respect to worker surplus shocks*, but still contains just as many jobs with *low firm surplus.* Figure 9 Panels (b.NC) and (c.NC) depict the post-REBP behavior in response to firm and worker shocks, separately for the former treatment group (Panel (b.NC)) and control group (Panel (c.NC)). Unlike in the Coasean setting, the survivors of REBP need not exhibit resilience despite a large extraction of marginal (worker-surplus) jobs brought about by REBP. Post-REBP shocks to *firm* surplus shift the jobs southward in both groups. The treatment group and the control group still feature a similar share of marginal matches that now separate.

The explanation for why *worker* surplus shocks need not have a large differential effect is subtle but intuitive: if worker surplus is relatively high to begin with in the older population that we study, the truncation of worker surplus is quantitatively inconsequential for subsequent separation behavior since even the control group features hardly any separations to that shock.[30] The figure clarifies that REBP was a large shock that even reached deep into the worker surplus distribution to cause – otherwise inframarginal – workers to separate. In Section 5.5 below, we discuss why such a correlation may naturally arise in our sample of older workers.

[30]Alternatively, shocks to worker surplus may be smaller compared to firm shocks.

Formal Model Formally, the jobs destroyed by REBP had *worker* surplus between 0 and ε_b^W, and the non-Coasean set of matches marginal to REBP are $M^{NC} = \{(w', \mathbf{V}') : 0 \leq S^W(w', \mathbf{V}') < \varepsilon_b^W \wedge S^F(w', \mathbf{V}') \geq 0\}$. By contrast, the distribution of surpluses in the control group remains a larger set $J = \{(w', \mathbf{V}') : S^W(w', \mathbf{V}') \geq 0 \wedge S^F(w', \mathbf{V}') \geq 0\}$. After REBP was abolished, the former treatment group's post-REBP surplus distribution $f^Z(.)$ is now again truncated, but specifically with regards to *worker rather than joint* surplus:

$$f_1(w, \mathbf{V}) = \begin{cases} 0 & \text{if} \quad (w, \mathbf{V}) \notin (J \setminus M^{NC}) \Leftrightarrow S^W(w, \mathbf{V}) < \varepsilon_b^W \vee S^F(w, \mathbf{V}) < 0 \\ \frac{f^0(w, \mathbf{V})}{1 - \int_{(w,\mathbf{V}) \in M^{NC}} f^0(w, \mathbf{V}) d(w, \mathbf{V})} & \text{if} \quad (w, \mathbf{V}) \in (J \setminus M^{NC}) \Leftrightarrow S^W(w, \mathbf{V}) \geq \varepsilon_b^W \wedge S^F(w, \mathbf{V}) \geq 0 \end{cases} . \tag{14}$$

This truncation arises during REBP because jobs initially viable in 1988 are placed into quit or layoff (or both) territory during REBP, where again $f^Z_{\text{pre}}(.)$ denotes the pre-REBP initial surplus distribution:

$$\begin{aligned} \delta^Z = \int_{(w,\mathbf{V})} \int_{(w',\mathbf{V}')} \mathbb{1}\Big(& \underbrace{\widetilde{S}^W(w', \mathbf{V}') < Z \times \varepsilon_b^W}_{\text{Quit}} \overbrace{\vee}^{\text{Mutual Sep.: } \wedge} \underbrace{\widetilde{S}^F(w', \mathbf{V}') < 0}_{\text{Layoff}} \Big) \\ & k((w', \mathbf{V}')|(w, \mathbf{V})) f^Z_{\text{pre}}(w, \mathbf{V}) d(w', \mathbf{V}') d(w, \mathbf{V}). \end{aligned} \tag{15}$$

Post-abolition of REBP, we can replace the densitities in the treatment group with the truncated distribution (15), and derive separation rates. We present the simplified expression setting idiosyncratic shocks to zero in the very short run (e.g., one year):

$$\begin{aligned} \Delta^1 = & \frac{1}{1 - \int_{(w,\mathbf{V}) \in M^{NC}} f^0(\mathbf{V}) d(w, \mathbf{V})} \int_{(w,\mathbf{V}) \setminus M^{NC}} \mathbb{1}\Big(\widetilde{S}^W(w, \mathbf{V}) < \varepsilon^{W'} \;\vee\; \widetilde{S}^F(w, \mathbf{V}) < \varepsilon^{F'}\Big) \\ & f^0(w, \mathbf{V}) d(w, \mathbf{V}). \end{aligned} \tag{16}$$

As a result, the initial *incidence* of a given surplus shocks – on the firm or worker side – matters, unlike in the Coasean setting where joint surplus and hence the net effect of shocks on that allocative concept matter. Figure 8 Panels (b) and (c) illustrates this implication by plotting the relationship between separations in the former treatment and control groups, and the surplus shock after REBP abolition. For *worker* shocks, the line is flat up until the point hits the truncation point of surplus, and then increases somewhat more steeply. For firm shocks however, the separation rates are very similar, as depicted in the case study Figure 9 Panels (b.NC) and (c.NC).

In fact, we can derive the non-Coasean version of the predicted Coasean post-REBP separations (13):

$$\Delta^1 = \frac{1}{1 - \int_{(w,\mathbf{V}) \in M^{NC}} f^0(\mathbf{V}) d(w, \mathbf{V})} \times \left[\Delta^0 - \int_{(w,\mathbf{V}) \in M^{NC}} \mathbb{1}(\widetilde{S}(w, \mathbf{V}) < \varepsilon^{W'} + \varepsilon^{F'}) f^0(w, \mathbf{V}) d(w, \mathbf{V}) \right]. \tag{17}$$

And similarly to the Coasean case, we can rewrite expression (17) in an empirically tractable form of realized *control group* separation rates, before and after REBP – but only if most (all) post-REBP separations arise from *worker* shocks. In this case, the formerly treated group again exhibits resilience in form of a kinked comovement between treatment and control separations even in the non-Coasean setting:

$$\Rightarrow \Delta^1 = \max\left\{0, \frac{1}{1-(\delta^1-\delta^0)}\left[\Delta^0-(\delta^1-\delta^0)\right]\right\}. \tag{18}$$

Figure 8 Panels (d) plots Equation 18, post-REBP treatment group against control group separations for this case.

By contrast, if post-REBP shocks are largely due to *firm* shocks, then the non-Coasean model can rationalize very similar separation sensitivities between the treatment and control group REBP survivors – despite the large separations previously induced by REBP:

$$\Rightarrow \Delta^1_{\text{NonCoasean}} \approx \Delta^0. \tag{19}$$

This situation can arise for two reasons, following the intuitions of Figure 9 Panels (b.NC) and (c.NC). Either worker shocks are just few and small. Or even the control group has few low-worker-surplus jobs (yet REBP was a large worker surplus shock). The non-Coasean setting can then rationalize our findings of no post-REBP resilience whatsoever (which rejects the Coasean view). Below in Section 5.5 we discuss this particular non-Coasean constellation, specifically *high worker surplus and low firm surplus*, with limited correlation between the two, by which the non-Coasean setting rationalizes the empirical findings. We also discuss the concrete real-world sources potentially generating these conditions.

Empirical Evaluation: Reduced-Form Estimation of the Share of Coasean Separations in a "Mixed Model" Neither framework will fully describe empirical labor markets. To gauge the quantitative power of each view, we present a "mixed model" that permits us to ask *which share* of jobs (separations) are consistent with Coasean protocols. Let share κ of jobs be Coasean (or non-Coasean with worker shocks driving separations), and share $1-\kappa$ follow the specific non-Coasean setting that we can distinguish (where separations are largely driven by firm shocks due to rigid wages, such that they ultimately follow largely the control group separation, due to high average worker and low firm surplus and perhaps wage rigidity). This "mixed" setting implies:

$$\Delta^1_\kappa = \kappa\Delta^1_{\text{Coasean}} + (1-\kappa)\Delta^1_{\text{NonCoasean}}, \tag{20}$$

which if shocks are largely driven by firm shocks, our particular non-Coasean model can approximate as a mixture of the Coasean and non-Coasean (firm shock):

$$\approx \alpha^{\text{Coasean}}\Delta^1_{\text{Coasean}} + \alpha^{\text{Non-Coasean}}\Delta^0 + \varepsilon. \tag{21}$$

where, informally, α^{Coasean} now captures the degree to which the data aligns with the benchmark Coasean model for any shocks *and additionally* with the share of separations that are due to worker surplus shocks. By contrast, $\alpha^{\text{Non-Coasean}}$ now captures the separations due to firm shocks in the non-

Coasean environment. Hence the estimated α^{Coasean} would be an *upper bound* for κ, since we cannot directly observe the incidence of shocks driving the separations. ϵ captures errors related to, e.g., group-specific shocks.

In this section, we provide a direct measure of κ by estimating model (21) at the birth-year cohort level, comparing post-REBP separations by cohort between REBP and non-REBP region (i.e. we collapse the data to the cohort and region level). That is, we revisit the visually striking patterns in Figures 4 and 5, which plotted these cohort separation gradients. Table 4 reports the estimates for various horizons after REBP. For the short run, 1995, the estimates imply an essential unit weight on the non-Coasean model as $\alpha^{\text{Non-Coasean}} = 1.01$ and an (insignificantly) negative weight on the Coasean model $\alpha^{\text{Coasean}} = -0.032$. In later years, when the power of our prediction decreases, the weight α^{Coasean} becomes more negative and even statistically significant. That is, the Coasean model seems to have no additional predictive ability.

In the next Section, 5.4, we present a structural estimation of our model to estimate κ, the share of Coasean job separations, by estimating Equation (21) with variation at the industry-by-occupation level and accounting for the nonlinear nature of the specification (due to the max operator in equation (13)). Subsequently, in Section 5.5, we discuss the assumptions underlying the non-Coasean framework and its mapping into labor market features in the data.

5.4 Structural Estimation of the Model: Which Share of REBP Separations was Coasean?

Building on the model, we now present the formal econometric specification estimating the share of jobs that appears consistent with the fully Coasean benchmark. Specifically, we estimate the fraction κ of jobs destroyed by the REBP program whose separation behavior was consistent with the Coasean prediction. Our estimation strategy takes the structure of the economic model in equation (21), along with (13) and (18), and analyzes variation across industry-occupation cells in the post-abolition period separation behavior of surviving jobs. The estimation reveals point estimates $\hat{\kappa}$ that are close to zero or even negative. Even in our most conservative specification, the upper limit of the 95% confidence interval for κ is 0.127, indicating that fewer than 13% of separations were efficient. With small shares of efficient separations, the estimation of the structural model thus delivers parameters estimates that directly mirror the reduced-form evidence in Section 4, which – at odds with the Coasean framework – had shown that post-abolition behavior did not differ between treated and untreated matches.

For our analysis, we focus on post-REBP separation rates of the former treatment group, $\Delta_i^{Z=1}$ at the industry-by-occupation cell level i. Our methodology could be easily applied to other cell categorizations. κ again denotes the share of jobs dissolved by the REBP according to the Coasean protocol (again a lower bound, as it may also capture worker-shock-related separations in the non-Coasean setting, as explained in the previous section). Our economic model predicts the following separation rates in among the treated cohorts $Z = 1$ in the posts-REBP period for a given set of latent post-REBP firm or worker shocks $(\varepsilon_i^W, \varepsilon_i^F)$:

$$\Delta_i^1 = (1-\kappa) \times \underbrace{\Delta_i^0(\varepsilon_i^W, \varepsilon_i^F)}_{\substack{\text{Non-Coasean,}\\ \text{Firm Shocks}}} + \kappa \times \underbrace{\max\left\{0, \frac{N_i + C_i}{N_i}\Delta_i^0(\varepsilon_i^W, \varepsilon_i^F) - \frac{C_i}{N_i}\right\}}_{\substack{\text{Coasean, Any Shocks}\\ \text{Non-Coasean, Worker Shocks}}}, \tag{22}$$

C_i and N_i denote the estimated share of compliers and never-separators to the REBP reform at the industry-occupation cell level, which we calculate by running the difference-in-differences specification in (1) within each industry-occupation cell. These terms are the analogues of predicted separations in equations (13) and (18) in the presence of idiosyncratic shocks ("always-separators").[31]

Using the notation in Schennach (2012), we let $Y_i = \Delta_i^1$ denote the observed dependent variable and $X_i^* = \Delta_i^0(\varepsilon_i^W, \varepsilon_i^F)$ denote the separations arising from the (unobserved) surplus distribution and shocks. The empirical estimating equation that identifies the main parameter of interest κ can be written as:

$$Y_i = (1-\kappa)X_i^* + \kappa \max\left(0, \frac{N_i + C_i}{N_i}X_i^* - \frac{C_i}{N_i}\right) + \Delta Y_i \tag{23}$$

$$= g(\kappa, N_i, C_i, X_i^*) + \Delta Y_i, \tag{24}$$

where $g()$ is a known function that is nonlinear in X_i^*, $\kappa \in$ is the main parameter of interest, and N_i and C_i are incidental parameters. Estimation of κ can be set up from the regression equation relationship $E[\Delta Y_i | X_i^*] = 0$.

To proxy for X_i^*, we use separation rates among younger cohorts in the REBP region in the same industry-occupation cell who were not treated by the program and who thus still contain marginal jobs but who are exposed to similar industry-level surplus shocks. Concretely, we use separation rates among workers in the REBP region born between 1943 and 1948.

Reduced-Form Evidence on Inter-Cohort Comovement of Separation Rates Before providing results of the structural estimation, we first plot the reduced-form relationship between Y_i and X_i^* in Figure 10. The figure shows the relationship between separation rates at the industry-by-occupation cell level in formerly treated cohorts (born between 1938 and 1943) against separations rates in slightly younger cohorts (born between 1943 and 1948). The relationship is strong, positive, and appears linear. As a benchmark, we also plot as outcome variable the separation rate in even younger cohorts (born between 1948 and 1953). These have a lower level, as one would expect if surplus is on average higher, but the relationship is just as strong and positive, thus providing evidence that different cohorts within an industry-by-occupation cell are affected by similar shocks to surplus. As Figure 10 reveals, there is no apparent difference between the separation behavior of formerly treated cohorts and their slightly younger peers as the relationship is almost identical (beyond a level shift) compared to the relationship between separation rates of the two younger cohorts of workers.

[31] To see this, note that the multiplication factor $\frac{1}{1-(\delta_1-\delta_0)} = \frac{N+C}{N} \Leftrightarrow \delta_1 - \delta_0 = \frac{C}{N+C}$.

Results of Estimation Table 5 reports estimates of κ based on estimation of equation 22 with non-linear least squares. Across specifications, we find estimates of κ that are close to zero or even negative. Our largest point estimate is 0.033 with variation at the four-digit level and considering post-abolition separations by 1995. Even the most conservative 95% confidence interval rules out $\hat{\kappa} \geq 0.127$. Stated alternatively, fewer than 13% of separations induced by the REBP were efficient. The table further reveals more negative estimates at longer horizons, e.g., with the confidence intervals excluding zero when considering separations by 1998. Arguably, the assumption of absence of idiosyncratic shocks / absence of convergence is less likely to be fulfilled at longer horizons leading to model misspecification. However, even at the shortest horizon, considering separations by 1995 as outcome variable, we find that fewer than 13% of REBP-induced separations had been efficient.[32]

A General Method to Detect "Harvesting Effects" In principle our econometric method can be used for *any* setting in which a researcher wishes to test for and quantify "harvesting effects", or positive selection at the exit margin (see Section 5.5, "5th Ingredient"). The first requirement is quasi-experimental and transitory shocks in the payoff function that induce exit from a distribution (in our case: job separations) in the treatment group, but did not hit the control group. Second, the research requires panel data to track survivors of that shock and their peers in the control group, after the transitory payoff shifter is switched off again (in our case: the reform abolition).

5.5 Discussion: Mapping the Non-Coasean Framework to Concrete Labor Market Features

Below we discuss the five model ingredients featured in the non-Coasean framework that can rationalize our findings. Since there is a menu of potential frictional candidates generating these properties and hence explaining the rejection of the Coasesan hypothesis, we map these theoretical ingredients into plausible real-world labor market features.

1st Ingredient: High Worker Surplus and Low Firm Surplus The high initial worker surplus ensured that REBP – a massive policy shift – managed to extract some marginal matches that otherwise (as in the control group) would have remained inframarginal.[33] Hence, the selection by worker surplus did not generate post-REBP resilience – allowing the non-Coasean model to break the iron link between selective separations and subsequent resilience ("harvesting effects") that would be the strong prediction of core features of the Coasean setting – if most post-REBP separations are driven by firm surplus shifts. Indeed, our sample consists of male, older, and high-tenured workers, that exact constellation turns out to be predicted by the long-standing hypotheses of implicit contract models, in form of backloading of compensation over the job spell (Lazear (1979, 1981)): in a period-by-period consideration, young workers are "underpaid", while older workers are "overpaid". This backloading

[32] We have also explored strategies to account for to measurement error using methodology for nonlinear models developed in Li and Vuong (1998), Li (2002) and Schennach (2004), surveyed in Schennach (2012).

[33] Alternatively, small worker shocks rather than high baseline worker surplus would help explain why worker surplus is not a driver of most separations. The distinction between the two models would remain robust to this view.

is supported by implicit contracts or formal institutions.[34] Moreover, the types of the incremental REBP separations were into permanent nonemployment, evidently by workers unlikely to find or seek reemployment (Section 3, Table 2, Columns (4) and (6)). Consistent with that view, Section 4 revealed that a series of labor demand shocks indeed did not trigger differential firing for the REBP cohorts. On the institutional side, the Austrian setting features an explicit role for works councils that are consulted in the separation process, providing formal and informal support for such implicit contracts and the selectivity of layoffs (Bewley, 2002). Moreover, Austria mandates multiple months of severance payments in the case of layoffs that are foregone for quitters, providing little incentive for workers to quit unilaterally (despite full UI eligibility for quitters in Austria, as discussed in Section 2).[35]

2nd Ingredient: Large Worker Surplus Shift From REBP If worker surplus is high on average (to have firm shocks drive separations), then how did REBP extract so many workers through the worker surplus channel? The second ingredient is the exceptional size of the initial UI treatment – *four years* of full UI eligibility, hence serving as a bridge into early retirement – likely played a role. In Appendix Section A, we benchmark that, for the average worker, the cash value corresponds to 71% of annual earnings. In the treatment group, the initial outside option boost was therefore sufficiently large to sweep up even some workers that would otherwise remain inframarginal to the typical worker-side shocks. Finally, a cross-validation test is that smaller shifts in UI should not induce workers to separate. Indeed, in Jäger et al. (2018), where we document that wages are insulated from UI shifts, we find that smaller shifts in the benefit level (but lower benefit duration) did not entail separation effects even among older workers and even during the 1980s in Austria.

3rd Ingredient: Limited Correlation Between Initial Firm and Worker Surpluses In the non-Coasean setting, the low-*worker* surplus jobs REBP extracted need not have been marginal with respect to *firm* surplus, for example, if the unilateral surpluses are independently distributed. There are various ways to limit the correlation between the worker and firm surpluses. First, the firm and worker fundamentals $\mathbf{V}^F$ and $\mathbf{V}^W$ may be uncorrelated. Second, the *wage* setting process may exactly generate uncorrelated surplus, in form of wage compression, i.e. cross-sectional wage rigidity.[36] By contrast, in the Coasean setting, the correlation between *initial* worker and firm surpluses is irrelevant because of rebargaining whenever at a given wage bargain one party's unilateral surplus were to cross zero into negative territory, provided the match maintains positive joint surplus. While absence of resilience is therefore sufficient to reject the Coasean view of job separations in our context, the non-Coasean setting could even rationalize "anti-resilience", i.e. higher sensitivity among the REBP survivors to subsequent shocks. This could emerge under a "random" wage (not related to fundamentals) that can generate negative correlation between worker and firm surplus: REBP quitters would then be very underpaid – and hence particularly valuable to firms, and REBP would have removed low worker surplus jobs but also high firm surplus jobs. In this thought experiment, the non-Coasean model with such particular features could even rationalize *higher* separations among the former treatment group

[34] While models of job ladders and negotiation capital such as Cahuc et al. (2006) generate this joint distribution, these models feature bilaterally efficient (re-)bargaining and separations.

[35] In this world, baseline quits may also reflect large inframarginal shifts (e.g., health shocks).

[36] In Jäger et al. (2018), we document evidence that wages are rigid to shifts in worker's outside option.

in response to firm shocks.[37]

4th Ingredient: Persistence in Surplus Differences (Limited Reshuffling by Idiosyncratic Shocks) An extreme version of an alternative mechanism may *mask* Coasean separations, by virtue of idiosyncratic shocks: if the model featured no persistence in worker and firm surplus factors whatsoever, such that the idiosyncratic Markov process reshuffles the position of jobs into the same, stationary surplus distribution in each period, the economy would not feature a truncated distribution when REBP is abolished, and would be observationally equivalent to a non-Coasean setting.

We believe that the immediate-convergence assumption is unlikely to explain our full set of results. First and most obviously, it would require full convergence already at the one-year horizon. Second, in Section 4, we conduct a complier analysis and in fact trace out some persistent observable attributes associated with the incremental REBP separators that distinguish them from their non-separating peers. Third, in Appendix Figure A.3, already discussed in Section 3, we plot the dynamics, year by year, of the additional separations due to REBP: when workers age into eligibility, or at the onset of REBP for the older workers who were born before 1938 and therefore become immediately eligible, there is a pronounced one-time *spike* in separations. This spike provides ancillary evidence for some degree of persistence and hence against the quick-convergence view, revealing a "pent-up" mass of marginal matches that separate when outside options increase, rather than a persistent elevation in the annual separation rate (implied by cross-sectional surplus distributions to be reshuffled each period). Fourth, the reform was large such that the idiosyncratic shocks would need to be accordingly large to replenish the mass of marginal matches: REBP increased separations by about 28%; and as discussed above, REBP amounted to around 71% of the average worker's annual salary. Fifth, our sample contains older workers with high labor force attachment, whose baseline turnover is typically lower, suggesting that the amount of separation-relevant idiosyncratic shocks is, if anything, selected towards a stable and persistent surplus group.

5th Ingredient: Limited 'Scarring" Effects From REBP The demography literature distinguishes harvesting and scarring effects: harvesting effects imply that transitory negative shocks (e.g., heat waves) induce low-health workers into exit and thereby reduce the subsequent average mortality rate of the survivors. Scarring effects describe situations in which the original shock may permanently lower the cohorts' health going forward, limiting or even masking potential harvesting effects. Our double difference-in-difference design, specifically our slightly younger control cohorts in the REBP regions, helps rule out this explanation for our absence of harvesting effects. For example, if REBP (or shocks correlated with REBP) had lowered firm investment, labor demand, stigma, or otherwise have changed social norms, we would expect to see most of these to show up negatively among the slightly younger workers (presumably close substitutes and in the same labor market) within in the REBP region – whom our design's second difference-in-difference layer compares to the young in the

[37] Some of our evidence may suggest such short-run "anti-resilience". Appendix Figure A.4 documents additional separations in the formerly treated group between 1993q3 to 1994q1 for the bottom quintile of industry growth cells. Figure 7 documents slightly more sensitive slopes in the "hockey stick" for formerly treated cohorts. However, any such short run "phantom treatment effects" may arise from the institutional fact of grandfathering in of pre-scheduled layoffs due to advance notifications as required by law, of multiple months, as we discuss in the previous section.

non-REBP regions.[38]

6 Worker Survey and Complier Analysis: Who Separated Due to REBP, How, and From Which Jobs?

To complement our model-driven test that sorted workers by the economic concept of job surplus, we provide evidence on classifications of REBP separations into subjective categories reported by workers themselves. Second, we extend the method of complier analysis to difference-in-differences settings such as our setting, to shed direct light on the pre-separation attributes of the REBP separators.

6.1 Classifying REBP Separations: Survey and Administrative Data

We now study micro-level survey data and an administrative measure to study the nature of the separations induced by the outside option shift. We find that workers largely perceived these separations to be amicable quits and early retirement.

Survey: Micro Census The Austrian Micro Census is the largest continuous survey of the Austrian population. It contains information on the reasons for the ends of nonemployed worker's last job, starting in 1995. For jobs that ended within the last eight years, respondents also report the reason for the separation:

1. a one-sided or amicable quit,
2. a layoff due to establishment closure,
3. a layoff due to economic reasons,
4. a layoff due to other reasons,
5. early retirement, incl. limited employability and health,[39]
6. regular retirement, or
7. other reasons (incl. expiration of fixed term contracts, civil service, and a residual, unclassified category).

As a basis for this analysis, we replicate the REBP treatment effect on separations in that the micro census sample, described in Appendix Section B.

[38] Wage effects of the workers' outside option boost could, in combination with wage rigidity, perhaps lead to subsequently higher separations than would be warranted under the Coasean view, but this route already departs from the Coasean view. We cannot credibly study wage effects in the present context given the attrition (separation effects). Yet in Jäger et al. (2018), we document that in Austria, wages of stayers are insensitive to UI-induced boosts in workers' outside option, even for older workers such as our sample here, and during the 1980s. We also do not focus on potential labor market spillovers through replacement hiring (Jäger, 2016; Mercan and Schoefer, 2018) or through search externalities (Lalive et al., 2011).

[39] The questionnaire and survey manual clarify that "early retirement" is supposed to encompass these additional categories.

Composition Shifts in Separation Types We focus on the sample of respondents whose job ended when they were between 50 and 54 years old. We estimate a difference-in-differences model with cohort and region effects from Equation (1), with indicator variables for last job ending between the ages of 50 and 54 in a respective separation type.

Figure 11(a) plots the difference-in-difference treatment effects of the REBP program on these seven types of separation indicators and documents that the largest categories of separation types are classified as early retirement and retirement for other reasons as well as quits, including amicable quits. Specifically, we find a statistically significant, positive effect of the REBP program on quits among older workers of about 0.7 ppt and effects of about 1.5 ppt for both early retirement and retirement for other reasons. Figure 11(a) also provides some evidence for a small and positive effect on layoffs due to economic reasons with an effect size of about 0.5 ppt, about a tenth of the overall increase in separations. The effects on layoffs due to closure or for other reasons are much smaller, and statistically not significant. In a next step, we dissect the reasons for the additional *quits* into (i) personal or family circumstances, (ii) sickness or disability, or (iii) other reasons (Figure 11(b)). Strikingly, almost half were due to sickness or disability.

Measuring Unilateral Quits in Administrative Data Next, we exploit an administrative waiting period for UI eligibility for quitters that we can trace in the administrative data. Specifically, worker-initiated quits come with a 28-day waiting period between registration with the government employment agency and eligibility for UI receipt, both of which our administrative data provides with daily accuracy. Hence we classify a separation as a worker-initiated quit if the delay in UI receipt is at least 28 days.[40] In Figure 11(c), we sort quits between ages 50 and 55 by month of birth and region akin to Figure 2(c) and to the validation exercise using the Microcensus in Appendix Section B. We confirm the observed increase in quits using our DiD strategy in column 7 of Table 2 and Appendix Table A.2, where we document a 2.1- or 2.3-ppt effect on the share of workers who take up unemployment benefits after a longer-than-28-day waiting period. Quits therefore accounted for about 20% of the overall increase in separations. This result does not mean that the remaining 80% of separations were layoffs but likely reflect mutually agreed-upon separations, as Austria does not feature experience rating.

6.2 Complier Characterization of the REBP Separators

We next dissect the *characteristics* of REBP separators. Our conceptual framework would predict that these jobs should have carried low initial worker surplus (but not necessarily different firm surplus). In this section, we examine characteristics of REBP separators and also assess whether REBP had particularly large separation effects among groups with proxies for that surplus constellation.[41]

[40] This is a lower bound on the share of quitters, since there could be quitters who become re-employed in less than 28 days or decide never to receive UI (false negatives). We are not concerned about non-quitters delaying UI take-up (false positives), because workers have a strong incentive to register immediately 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 Jäger et al. (2018), we find that most separators register with the employment agency within little more than a month of the end of an employment spell.

[41] Our simple conceptual framework posits that the surplus shift due to the treatment was homogeneous and triggered differential separation behavior because of dispersion in the initial worker surplus. An alternative interpretation of the

All characteristics we consider are measured before the start of the REBP program in 1988 and by construction unaffected by the reform.

These jobs are *compliers*: they would have survived in the absence of the treatment (hence still present in the control group) but did not survive when exposed to it (treatment group). We therefore extend the complier analysis method (Imbens and Rubin, 1997; Abadie, 2003) to difference-in-difference settings such as ours.

Our complier analysis reveals that marginal jobs indeed exhibited some observables consistent with low surplus: marginal jobs originate from blue-collar occupations in industries with a high incidence of sickness and disability among older workers. Compared to surviving jobs, marginal jobs had a higher risk of long unemployment duration and were more prevalent in shrinking industries and firms. They also had similar wages to their peers, potentially in line with our non-Coasean candidate that appeals to wage compression and wage rigidity.

But the analysis also reveals that the REBP separators stemmed from many pockets of the Austrian labor market and would therefore not be definitely identified off their pre-separation attributes. Moreover, our discussion below highlights how each given attribute may carry multiple potential interpretations, and hence illustrates the difficulty to trace surplus concepts on the basis of observables – a limitation that necessitated our sharp and robust test in the previous sections in the first place.

Intuition of Complier Analysis We provide the basic intuitions of complier analysis (Imbens and Rubin, 1997; Abadie, 2003) before moving to our difference-in-differences strategy. We would like to distill information about the attributes carried by marginal jobs M, for example the mean $\bar{x}$ of some scalar variable x (but the methodology can be applied to non-parametrically back out any moment of the distribution).[42] Empirically, we observe the attributes as well as amount of treatment-group separators and control-group separators. On their own, treatment and control separators convey largely *inframarginal* surplus ranges, of surplus at least or at most 0. Yet, we can back out the attributes of marginal matches (those with close to 0 surplus and hence swept up by REBP) by rearranging the expression for treatment separators, which are the union of control separators and marginal matches, such that $x^{Z=1}$ is just the average of $\bar{x}^{Z=0}$ and $\bar{x}^{M}$, weighted by their share in total separations:

$$\bar{x}^{Z=1} = \frac{\bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1}} \times \bar{x}^{Z=0} + \frac{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1}} \times \bar{x}^{M}, \tag{25}$$

$$\Leftrightarrow \bar{x}^{M} = \frac{\bar{\delta}^{Z=1}}{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}} \times \bar{x}^{Z=1} - \frac{\bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}} \times \bar{x}^{Z=0} \tag{26}$$

The weights are measured as group-level separation rates, along with treatment effect $\bar{\delta}^{Z=1} - \bar{\delta}^{Z=0}$. Below we extend complier analysis to difference-in-difference settings such as ours; to our knowledge, the formal derivation is new.

heterogeneity is that the surplus shifts the treatment entailed had some heterogeneous sizes as well.

[42]Specifically, the density of nearly marginal matches carrying x, backed out from treatment and control separators, is: $s^M(x) = \frac{\bar{\delta}^{Z=1}}{\bar{\delta}^{Z=1}-\bar{\delta}^{Z=0}} \times s(x)^{Z=1} - \frac{\bar{\delta}^{Z=0}}{\bar{\delta}^{Z=1}-\bar{\delta}^{Z=0}} \times s(x)^{Z=0}$. Alternatively, $s(x)$ is the product of the relative treatment effect on separations among type-x jobs compared to the average effect, times the share of type-x job in the initial sample, $f(x)$: $s^M(x) = \frac{\delta^{Z=1}(x)-\delta^{Z=0}(x)}{\int_x \left[\delta^{Z=1}(x)-\delta^{Z=0}(x)\right] dF(x)} \cdot f(x)$.

6.2.1 Methodology: Complier Analysis in Difference-in-Differences Settings

We formally characterize sufficient conditions for complier analysis (Abadie, 2003) in difference-in-differences contexts and summarize the framework, building on an additive separability assumption for attributes.[43]

Identification of Marginal Matches We set up a potential outcomes framework to describe the assumptions under which the estimated coefficient $\hat{\nu}$ from the difference-in-differences model (1) identifies the mass of marginal matches in the model from the discrete choice problem from Section 5. There, we estimated the effect of the REBP programs on separations in a difference-in-differences specification on the population of workers holding a job in 1988 before the onset of the reform with fixed effects for region r and cohort c.

Notation and Definitions A binary variable Z captures whether workers are eligible for the REBP benefit extension:

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

Interpreted through the lens of the discrete choice problem, Z shifts a component of the worker's outside option $b + Z \cdot \Delta b$.

$D \in \{0, 1\}$ indicates whether a given worker separates from their initial, pre-reform job by the end of the reform period (whether she was treated or not). We let D_0 and D_1 denote the potential values that D takes for $Z = 0$ and $Z = 1$, respectively ($D = ZD_1 + (1 - Z)D_0$). That is, each worker is characterized by ($D_0 = 0, D_1 = 1$) with regards to his potential outcomes that would emerge for either treatment status $Z \in \{0, 1\}$:

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

Economically, a separation reveals that the job's surplus fell beneath the threshold and rendered the match non-viable.

The outside option shift varied by region $r \in \{r_0, r_1\}$ and cohort $c \in \{c_1, c_0\}$. We define r_0 to be the control region and r_1 to be the REBP region. Analogously, we define c_0 to be ineligible cohorts and c_1 to be eligible cohorts. We thus have that $Z = 1$ for (r_1, c_1) only, and $Z = 0$ for other combinations (r_1, c_0), (r_0, c_0) and (r_0, c_0).

Partitioning the Set of Pre-Reform Matches We characterize pre-reform attributes $\mathbf{x}$ of compliers, never-separators and always-separators. Compliers denote workers that separate when exposed to the REBP program but who would not have separated from their job otherwise. Never-separators

[43] Our additive separability assumption for attributes mirrors analogous assumptions for parallel trends in potential outcomes in recent work extending the instrumental variables setup to difference-in-differences settings (De Chaisemartin and D'Haultfoeuille (forthcoming), Hudson et al. (2017)). In Appendix C.1, we provide a detailed, stand-alone methodological guide.

are workers in matches that survive regardless of whether they are exposed to the REBP program while always-separators are workers in those matches that dissolve even in the absence of REBP. Formally, we define the following three groups:

AS: **Always-Separators** have potential outcomes $(D_0 = 1, D_1 = 1)$ and share π^{AS}.

NS: **Never-Separators** have potential outcomes $(D_0 = 1, D_1 = 1)$ and share π^{NS}.

C: **Compliers** – **Marginal Jobs** – have potential outcomes $(D_0 = 0, D_1 = 1)$ and share π^{C}.

Assumptions for Identification of Complier Characteristics in Difference-in-Differences Settings We make the following four assumptions to estimate the treatment effect in the difference-in-differences strategy. The assumptions are mostly common ones in an instrumental variables setup. For extending complier analysis to difference-in-differences settings, we *additionally* make an independence assumption for characteristics and assume additive separability for attributes.

A.1: First Stage For all $r \in (r_0, r_1)$ and $c \in (c_0, c_1)$, $P(D_1 = 1|r, c) > P(D_0 = 1|r, c)$.

Intuitively, Assumption 1 implies that more separations take place under the reform, as documented in the previous section, and ensures the existence of compliers.

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

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

A.3: Independence $(D_0, D_1, x) \perp Z|(r, c)$.

The independence assumption posits that conditional on r and c, the instrument Z is orthogonal to potential outcomes D_0, and D_1 and, extending the usual assumptions in instrumental variables settings, also to attributes x.

A.4: Additive Separability For all $d, d' \in \{0, 1\}$,
$P(D_0 = d, D_1 = d'|c_1, r) - P(D_0 = d, D_1 = d'|c_0, r)$ and $\mathbb{E}[x|c_1, r, D_0 = d, D_1 = d'] - \mathbb{E}[x|c_0, r, D_0 = d, D_1 = d']$ do not depend on r.

The additive separability assumption for characteristics x is analogous to assumptions for parallel trends or additive separability in recent work extending the instrumental variables setup to difference-in-differences settings (De Chaisemartin and D'Haultfoeuille (forthcoming), Hudson et al. (2017)). A testable implication of Assumption 4 that we bring to the data is that $\mathbb{E}[x|c_1, r] - \mathbb{E}[x|c_0, r]$ does not depend on r. This is equivalent to saying that the between-cohort differences in the job attribute distribution are the same across regions, specifically among marginal jobs.

Identification and Estimation of Mass of Marginal Matches (Compliers) ν Under the independence and additive separability assumption, we can express the conditional probabilities of each type as follows:

$$\begin{aligned}
\pi_{rc}^{A} = P(D_0 = 1, D_1 = 1|r_1, c_1) &= P(D_0 = 1|r_1, c_1) \\
&= P(D_0 = 1|r_1, c_0) + P(D_0 = 1|r_0, c_1) - P(D_0 = 1|r_0, c_0) \quad (27)\\
&= E(D|Z = 0, r_1, c_0) + E(D|Z = 0, r_0, c_1) - E(D|Z = 0, r_0, c_0),\\
\pi_{rc}^{N} = P(D_0 = 0, D_1 = 0|r_1, c_1) &= P(D_1 = 0|r_1, c_1)\\
&= P(D = 0|Z = 1, r_1, c_1) \quad (28)\\
&= 1 - E(D|Z = 1, r_1, c_1),\\
\pi_{rc}^{C} = P(D_0 = 0, D_1 = 1|r_1, c_1) &= 1 - \pi_{rc}^{A} - \pi_{rc}^{N}, \quad (29)
\end{aligned}$$

where π_{rc}^{A} denotes the share of always-separators, $\pi_{rc}^{N} = P(D_0 = 0, D_1 = 0|r_1, c_1)$ the share of never-separators, and π_{rc}^{C} the share of compliers. The sample estimators for these shares from regression (1) are then $\pi_{rc}^{A} = \widehat{\beta} + \widehat{\phi}_r + \hat{\psi}_c$, $\pi_{rc}^{N} = 1 - \widehat{\beta} - \widehat{\phi}_r - \widehat{\psi}_c - \widehat{\nu}$, and $\pi_{rc}^{C} = 1 - \pi_{rc}^{N} - \pi_{rc}^{A} = \hat{\nu}$. That is, $\widehat{\nu}$ identifies the size of the set of marginal matches. It is our empirical estimate of the mass of marginal matches, estimated in regression model as ν in Equation (1).

Identification and Estimation of Complier Means Under the first stage and monotonicity assumptions above, we can express the characteristics of compliers, $\mathbb{E}[x|r_1, c_1, D_1 = 1, D_0 = 0]$, in terms of estimable quantities:

$$\mathbb{E}[x|r_1, c_1, D_1 = 1, D_0 = 0] = \tfrac{\pi_{rc}^{C} + \pi_{rc}^{A}}{\pi_{rc}^{C}} \cdot \mathbb{E}[x|r_1, c_1, D_1 = 1] - \tfrac{\pi_{rc}^{A}}{\pi_{rc}^{C}} \mathbb{E}[x|r_1, c_1, D_0 = 1]. \quad (30)$$

Appendix C.1 documents the proof building on arguments in Abadie (2003).

Complier characteristics are thus identified in a difference-in-difference IV setting as we can construct sample analogues to each of the terms on the right-hand side as follows. By independence, we have that:

$$\mathbb{E}[x|r_1, c_1, D_1 = 1] = \mathbb{E}[x|r_1, c_1, D = 1, Z = 1]. \quad (31)$$

By independence and additive separability in x, we have:

$$\begin{aligned}
\mathbb{E}[x|r_1, c_1, D_0 = 1] =& \mathbb{E}[x|r_1, c_0, D_0 = 1] + (\mathbb{E}[x|r_0, c_1, D_0 = 1] - \mathbb{E}[x|r_0, c_0, D_0 = 1])\\
=& \mathbb{E}[x|r_1, c_0, D = 1, Z = 0] + (\mathbb{E}[x|r_0, c_1, D = 1, Z = 0] - \mathbb{E}[x|r_0, c_0, D = 1, Z = 0]).
\end{aligned} \quad (32)$$

Sample analogues exist for each of the right-hand side terms in 31 and 32:

$$\mathbb{E}[x|r, c, D = 1, Z = 0] = \frac{1}{N_{r,c}} \sum_{i \in (r,c)} x_i D_i \ , \quad (33)$$

where $N_{r,c}$ is the number of observations in the given (r, c) cell.

Thus, under the assumptions above, our framework allows us to directly estimate any complier characteristic x (including the full distribution) in a difference-in-differences IV setup. In Appendix C.1, we also present an equivalent one-step regression estimation of complier attributes in difference-in-differences settings. For inference, we use the non-parametric bootstrap to arrive at a sampling distribution of (30).

6.2.2 Results

At a broad level, our results suggest that marginal jobs are characterized by low and declining surplus. In particular, we find that, compared to jobs surviving the treatment, compliers are primarily blue-collar workers in declining establishments from manual labor-intensive industries with higher shares of disability or sickness among older workers. Remarkably, we find no evidence for wage differences between compliers and never-separators – perhaps consistent with wage compression or wage rigidity. That is, the results could be consistent with low joint surplus but also with lower worker surplus, compared to never-separators. While the complier characteristics overall point to lower surplus markers, the analysis is not as clear-cut an adjudication between Coasean and non-Coasean theories of job separations as our sharp test in Section 5.[44]

Table 6 provides an overview of means for compliers, never-separators, and always-separators, based on pre-reform data from 1988, along with p-values for mean differences between compliers and the other groups.[45] We also provide an overview of the differences between compliers and never-separators in Figure 12 (c), where we normalize the difference by the standard deviation for each variable to facilitate the comparison across variables. The sample for Table 6 is the same on which we estimate Equation (1): employed men in May 1988 working in regions where REBP was never in place or was in place until 1993.

To complement our analysis of complier characteristics, we also present estimates of treatment effect heterogeneity of the REBP program on separations, largely mirroring the results of the complier analysis.[46] Figure 12(b) and the last column of Table 6 present the estimates along several dimensions. For each covariate, we separately estimate equation (1) while including the interaction effect of the covariate with REBP treatment. The categories are binary characteristics (e.g., white collar) or above/below median for continuous values (e.g., earnings).

Appendix D provides a detailed description of each variable we analyze below.

[44] A case in point is that, in some cases, compliers exhibit lower surplus markers compared to always-separators. What type of model would rationalize these findings? One explanation are large idiosyncratic shocks such as severe health shocks, as captured in our model in Section 5 in form of Markov process $k(.|.)$ and as discussed further in Section 5.5. Such shocks could push into separation very high-surplus jobs compared to compliers (REBP separators) and may strike independently of the initial surplus level. As a result, jobs with initially high-surplus markers would be dissolved, to a degree that always-separators compare favorably to compliers.

[45] We focus on workers in the cohorts born after 1933 for the complier analysis. We provide a test of an implication of the additive separability assumption and report results in Table A.1. While we find statistically significant differences for some variables we consider, the magnitudes of the respective differences are small.

[46] For Bernoulli-distributed covariates Angrist and Pischke (2008) and Angrist and Fernandez-Val (2013) show that the relative likelihood of a complier having a particular Bernoulli-distributed attribute is given by the ratio of the first stage effect for that group compared to the average first stage. In our setting, this would correspond to the ratio of the separation effect for that group compared to the average separation effect.

Blue vs. White Collar Occupations An analysis of the occupational structure of marginal jobs (for data reasons limited to a classification into blue- and white-collar occupations) reveals that 80% of marginal jobs are in blue-collar occupations while the share of blue-collar workers among both always- and never-separators are lower at 68% and 53%, respectively. When analyzing treatment effect heterogeneity, we also find stronger effects of the REBP on separations among blue-collar workers.

We also in unreported results considered the share of marginal jobs across industries and found a concentration in manual labor-intensive sectors such as mining or manufacturing. Virtually no marginal jobs exist in high-skilled, white-collar sectors such as health or banking.

Worker Codetermination, Works Councils, and Establishment Size We then investigate whether compliers are particularly likely to originate from establishments with stronger worker codetermination through the works council. To do so, we explore the size cutoffs at 5, 20, 100, and 1,000 employees. At each threshold, the codetermination rights of workers are strengthened.[47] As described in Section 2, workers can form a works council starting at establishment with five or more employees. In establishments with more than 20 employees, employer-works council agreements can be formed to take older workers' interests into consideration. In establishments with more than 100 employees, the local employment agency needs to be notified before sizable layoffs of workers. Finally, the works council can appeal to an external arbitration committee, e.g., in case of layoffs, when employment crosses a 200-employee threshold.

Our results show clear differences between compliers and never-separators across establishment size thresholds. Specifically, marginal jobs are more likely than never-separators to come from very large establishments where works councils can appeal to external arbitration committees (72.7% vs. 50.8%). This pattern is consistent with stronger codetermination through works councils lending formal institutional support to implicit contracts as described in Lazear (1979, 1981) and Bewley (2002) and potentially consistent with our alternative non-Coasean framework described in Section 5.

Employment Growth: Industry and Establishment Level Marginal jobs stem from industries or establishments with stagnating or declining labor demand. This complier attribute represents industry and establishment employment growth rates in the pre-period from 1982 to 1987.[48] The analysis reveals that marginal jobs stem from declining industries which had a *negative* growth rate of -1.4ppt in the pre-period while both always- and never-separators come from moderately growing industries with positive growth rates of around 0.8 and 2.9ppt in the same time frame. The pattern is more pronounced at the establishment level, where we find that compliers stem from establishments with a negative average growth of -16.5ppt while never-separators stem from establishments with positive growth of 7.7ppt. Relatedly, only 14% of marginal jobs stem from establishments with positive employment growth in the pre-period while the shares of both always- and never-separators in growing establishments are substantially higher at 35.5% and 53.4%, respectively. Mirroring the complier characteristics, we also find that employment growth at the industry and establishment level correlates

[47] Of course, other attributes also vary with establishment size so our analysis of worker codetermination and establishment size does not definitively pin down *only* variation in codetermination.

[48] This analysis is therefore restricted to those establishments we observe in the pre-REBP period.

negatively with the treatment effect on separations, documenting that the REBP led to more separations when industries or firms were in decline. Overall, the evidence documents robustly that marginal jobs occurred in sectors and establishments that were declining, consistent with a low or decreasing joint surplus, worker surplus (lower expected continuation value), or firm surplus (at a given wage). It is consistent with a narrative in which the REBP program managed to buffer and perhaps accelerate labor market adjustment to some degree (since the compliers are, by construction, still present in the peer employers in the control regions and cohorts).

Wages Wages of compliers and never-separators are statistically indistinguishable (EUR 27,800 vs EUR 28,200, CPI-adjusted to 2003). Similarly, the earnings of compliers and always-separators are also not distinguishable at the 5% level. Relatedly, we do not find statistically significant treatment effect heterogeneity by earnings, conditional on the other covariates, in our estimation of treatment effect heterogeneity.

We further analyze AKM-specifications (Abowd et al., 1999) in the pre-period before 1988 and find that compliers and never-separators have similar firm and worker effects, with the point evidence perhaps pointing towards lower worker effects. We estimate the following AKM 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 establishment fixed effects. Control vector X_{it} comprises a third-order polynomial in age. Worker effects α_i can be interpreted as the permanent component of wages workers command irrespective of the particular employer. Establishment effects $\psi_{J(i,t)}$ capture the wage premium or discount a given employer pays to a worker controlling for the worker effect. The fixed effects are identified through worker moves across employers, in the largest connected set.[49]

Our analysis reveals that the point estimates for worker effects are 6.3% smaller among marginal workers compared to never-separators, although the difference is not statistically significant. The results are consistent with marginal matches having permanently lower earnings compared to their peers, although the imprecision of the estimate prohibits a strong interpretation. Marginal jobs and never-separators have virtually indistinguishable establishment effects, with the point estimate for the difference being 0.1% smaller among marginal jobs. The treatment effect heterogeneity analysis also reveals no statistically significant differences in treatment effects between matches with different establishment or worker effects. One interpretation is that the excess REBP separations stem from matches with similar firm surplus, since establishment fixed effects have been shown to correlate positively with measures of value added per worker (see Card et al., 2017). A caveat to this interpretation builds on the aforementioned compliers' similarity of raw earnings and negative labor demand proxies (employment growth).

Tenure The point estimate for tenure of workers in marginal jobs is higher compared to the whole sample, although the difference is not statistically significant (same for treatment effect heterogeneity). The complier mean for tenure in 1988 is 12.6 years compared to 11.6 years for never-separators. Tenure

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

is a particularly ambiguous attribute as it may be associated with distinctly negative firm surplus but positive worker surplus (Lazear, 1979, 1981; Frimmel et al., 2018), but alternatively positive joint surplus due to ex ante investments (Oi, 1962) or match quality due to selection mechanism (Cahuc et al., 2006; Hagedorn and Manovskii, 2013; Bagger et al., 2014).

Sickness and Disability Measures of worker sickness and disability are interesting complier attributes because they may capture disutiltiy of labor or lower productivity. Since we analyze a sample of workers employed at the onset of the reform, we cannot focus on sick leave or disability spells at that time. Instead, we focus on sick leave and disability rates at the 4-digit industry level in the pre-reform period. We exploit the ASSD administrative registration of these spells. For both indicators, sick leave and disability, as well as for the combined rate of the two, we find that compliers come from industries with higher indicators of morbidity among older workers. For example, compliers' industries have a 0.8 ppt higher share of workers on sick leave or disability compared to never-separators, about 50% of a standard deviation.[50] Similarly, these industries exhibited stronger treatment effects. Overall, these results suggest that REBP dissolved matches in which the disutility of working increases or worker productivity decreases with age, consistent with low and shrinking (worker) surplus.[51]

Long-Term Unemployment Risk Finally, we consider an indicator for risk of long-term unemployment. We do so by regressing an indicator for experiencing an unemployment spell longer than one year on a set of predictors among the sample of workers in the pre-reform period from 1982 to 1987. The pre-separation variables we include for the prediction are fixed effects for 15 large industries, an indicator for being in a white-collar occupation, the local unemployment rate, and third-degree polynomials of tenure in current job and experience over the last 25 years. We then analyze the predicted risk of a long-term UI spell as a complier attribute for our sample of pre-REBP 1988 jobs.

Compared to the never-separators, compliers have a 2.6 ppt higher risk of experiencing an unemployment spell longer than one year based on our prediction. The difference is statistically significant. The point estimates for treatment effect heterogeneity along this dimension point in a similar direction although are less precisely estimated. Overall, the evidence suggests that the REBP reform selected those workers who had a higher risk of experiencing long-term unemployment after a separation even in the absence of the reform. This perspective is consistent with our finding that most of the excess REBP separators went into long-term nonemployment as documented in Section 3. Moreover, the workers that separated from their 1988 job in response to REBP evidently were unlikely to find, or seek, reemployment, consistent with the workers having low surplus with respect to nonemployment, as in our framework in Section 5.

[50] These proxies are powerful because for workers younger than 55, a disability spell indicates severe health problems as disability insurance formally requires medical impairment to reduce the capacity to work by at least 50 percent in *any* occupation. The highest incidence of sickness and disability can be found in mining, construction, and manufacturing, sectors dominated by blue-collar labor, which also showed a high share of marginal jobs.

[51] A caveat to this interpretation arises from the potential positive selection implied by the fact that our sample considers workers employed at that age in these industries at the onset of the reform.

7 Conclusion

When labor market quantities adjust, they largely do so along the employment (extensive) margin rather than the hours (intensive) margin.[52] At the micro level, such extensive-margin adjustments play out as discrete choice problems within employment relationships of individual workers and firms that are destroyed, maintained or formed, with a basic condition: each party be better off inside the job than with her respective outside option, hence enjoying surplus from the job. By this theory, the distribution of heterogeneous job surpluses determines the level of employment, and its adjustment.[53]

An open question is which theories of job formation and separation guide these interactions driving extensive-margin employment adjustment. According to the popular approach that we have labelled "Coasean", the parties exploit all gains from trade by using arbitrarily complicated contractual arrangements to transfer utility. This powerful and theoretically tractable modeling approach therefore appeals to abstract allocative wage and surplus concepts that do not directly map into empirical analogues. On the basis of wage behavior or typical observables among separators, this view is therefore difficult to definitely distinguish from "non-Coasean" models, in which frictions such as in wage setting or due to formal or informal institutional constraints, potentially permit inefficient separations that are costly to the firm and the worker.

Which of these two perspectives provides a better description of real-world job dynamics, naturally determines the welfare properties of employment adjustment and hence the potential scope for policy interventions, such as short-term work arrangements (Giupponi and Landais, 2018), unemployment insurance (Chetty, 2006), minimum wages (Lee and Saez, 2012), payroll taxes (Saez et al. forthcoming), seniority layoff rules (Sorensen, 2018), retirement policies (Lazear, 1979) and monetary policy (Berger et al., 2015).

We have presented a sharp empirical test of whether Coasean theories of jobs can account for empirical separation behavior. We studied a quasi-experimental extension of UI benefits for older Austrian workers in the late 1980s and early 1990s. By the Coasean view, this policy should have increased workers' non-employment value, reduced job surplus, and and led to separations of marginal matches – namely those with low joint job surplus. Indeed, we document large – 11.2ppt (28%) – separation increases among the treated cohorts, compared to their slightly younger ineligible colleagues and also compared to their older peers in control regions.

The key prediction distinguishing Coasean job theories, positing efficiency of bargaining and hence implying efficient separations, was not borne out. Studying survivors of the reform period, we exploited the transitory nature of the quasi-experiment program to generate two distinct labor markets: one in which the reform extracted marginal matches, and one with those marginal matches still present. In stark contradiction to the Coasean prediction – in which a notion of joint job surplus would have isolated the selected survivors to *any* kind of subsequent shock –, the formerly treated and thereby potentially advantageously selected jobs subsequently exhibited *exactly the same* separation behavior after the

[52]For business cycle adjustment, see Hansen (1985). For cross-country facts, see Prescott et al. (2009) and Bick et al. (2014). Mui and Schoefer (2018) study the labor supply side of extensive-margin adjustment with potential rationing.

[53]See, e.g., Pissarides (2000) for the DMP theory of employment as a function of job surplus arising from recruitment costs, and Ljungqvist and Sargent (2017) proposes "fundamental surplus" as the key determinant of the amplitude of cyclical DMP fluctuations in employment.

outside options are reduced again. Our structural model estimates that at most 4% (upper confidence interval: 13%) of job separations in our study conformed to the canonical Coasean conditions.

We present one promising non-Coasean alternative that can coherently rationalize our full set of findings. At the core, this narrative appeals to wage frictions or other – formal, behavioral or social – institutional constraints in the spirit of Bewley (2002).[54] Such frictions, as would emerge under some degree of wage rigidity and cross-sectional wage compression, curb the potentially very complicated-to-implement compensation schemes necessary to transfer utility in the efficient bargaining inherent to the Coasean setting.[55] Putting limits on such bilateral trades detaches worker and firm surplus, and hence permits inefficient, one-sided separations. The particular joint distribution that can rationalize the results is high worker surplus and low firm surplus. For our sample of older workers, that exact constellation turns out to be predicted by the long-standing hypotheses of implicit contract models, in form of backloading of compensation (Lazear, 1979, 1981): in a period-by-period perspective, young workers are "underpaid", while older workers are "overpaid".[56] Besides potential implicit contracts, the Austrian setting of our study features formal institutional support for these dynamics. Works councils are required to be consulted in the separation process, and moreover separations are subject to firing regulations including severance payments. Consistent with the view of low firm surplus and rents accruing to these older workers, the types of separations induced by the UI program are into permanent nonemployment, evidently by workers unlikely to find or seek reemployment, many of which represented quits. Finally, our complier analysis revealed that UI-induced separations stemmed from a diverse set of jobs, consistent with the challenge that observable attributes of jobs would have made it hard to assess the underlying allocational concepts of the policy-induced separations.

We conclude that the separation dynamics we studied provided a sharp empirical rejection of Coasean theories of efficient bargaining and efficient separations. While we focused on a uniquely suited quasi-experiment arising in the Austrian labor market among workers in their 50s in the early 1990s, the full rejection in our particular setting implies that much of the results likely carries over to other labor market contexts. While we have attempted to trace out the potential concrete frictions underlying our findings, our paper raises, and provides a methodological toolkit to further study, a natural set of follow-up questions: under which conditions do real-world labor markets approximate the Coasean benchmark, and hence limit inefficient separations?

[54] An alternative explanation we discuss but ultimately deem implausible to account for the full extent of the similarity is that idiosyncratic shocks could in principle lead the two groups to converge within a single year to the same surplus distribution.

[55] In Jäger et al. (2018) we have documented that Austrian wages appear unresponsive to UI benefit level shifts (which did not entail separations). We cannot credibly study wage effects in the present context given the large attrition implied by the separation effects.

[56] Models of job ladders and negotiation capital by Cahuc et al. (2006) predict that over time, workers raise their wage and grasp job surplus. While these models feature bilaterally efficient bargaining and separations, perhaps incorporating some notion of ex-post wage rigidity could accommodate our results.

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Tables

Table 1: Summary Statistics

	1988 Job Holders		Age 50 Job Holders	
	No REBP	REBP	No REBP	REBP
	(1)	(2)	(3)	(4)
Age	37.338	36.840	49.750	49.751
	(10.872)	(10.625)	(0.068)	(0.068)
White Collar	0.423	0.354	0.466	0.380
	(0.494)	(0.478)	(0.499)	(0.485)
Experience	15.108	15.840	20.796	21.883
	(7.658)	(7.515)	(6.364)	(5.728)
Tenure	7.053	7.944	9.301	9.898
	(5.730)	(5.907)	(8.194)	(8.292)
Earnings	2.5e+04	2.5e+04	2.8e+04	2.9e+04
	(8124.116)	(7502.007)	(1.0e+04)	(9328.159)
Ln Earnings	10.055	10.073	10.174	10.204
	(0.386)	(0.338)	(0.433)	(0.382)
Local Unemp. Rate	0.026	0.040	0.027	0.042
	(0.026)	(0.033)	(0.029)	(0.036)
Local Unemp. Rate (50+)	0.045	0.133	0.047	0.142
	(0.043)	(0.127)	(0.048)	(0.135)
Firm Size	386.638	1360.413	369.443	965.193
	(994.056)	(3537.014)	(952.405)	(2115.902)
Emp. Growth at Establishment	0.391	0.469	0.283	0.355
	(3.202)	(5.577)	(2.889)	(5.382)
Emp. Growth Industry	0.038	0.017	0.045	0.020
	(0.081)	(0.076)	(0.085)	(0.080)
1(Growing Establishment)	0.498	0.461	0.336	0.303
	(0.500)	(0.498)	(0.472)	(0.460)

Note: The table reports results summary statistics for a sample of workers employed at the onset of the reform (1988 Q.2) in columns (1) and (2) and for a sample of workers employed in the quarter before turning 50 in columns (3) and (4).

Table 2: Difference-in-Differences Effects of the REBP on Outcomes of Initially Employed Workers (1988 to 1993)

	(1) Separation	(2) Separation Into Nonemployment	(3) Separation Into New Job	(4) Employment (Months)	(5) Unemployment (Months)	(6) Cont. Empl. (Months)	(7) Quit (Indicator)
REBP Eligibility	0.112**	0.162***	-0.051***	-4.454***	3.055*	-3.133**	0.024*
	(0.045)	(0.052)	(0.014)	(1.045)	(1.618)	(1.218)	(0.014)
REBP Region	-0.003	-0.017	0.014	0.941	-0.396	0.101	-0.021
	(0.047)	(0.022)	(0.026)	(0.886)	(0.580)	(2.155)	(0.023)
Constant	0.400***	0.197***	0.203***	58.907***	2.498*	47.786***	0.072
	(0.085)	(0.044)	(0.041)	(1.962)	(1.502)	(4.952)	(0.045)
Observations	349196	349196	349196	349196	349196	349196	349196
Adjusted R^2	0.032	0.121	0.025	0.071	0.034	0.010	0.001
No of Clusters	100	100	100	100	100	100	100
Mean Dep Var, Control Region	0.410	0.237	0.172	58.121	2.657	48.054	0.066
Sd of Dep Var, Control Region	0.492	0.425	0.378	14.722	6.998	23.346	0.248

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

Table 3: Difference-in-Differences Effects of the REBP on Outcomes of Survivors (1994 Through 1996)

	(1) Separation	(2) Separation Into Nonemployment	(3) Separation Into New Job	(4) Employment (Months)	(5) Unemployment (Months)	(6) Cont. Empl. (Months)	(7) Quit (Indicator)
REBP Eligibility	0.013*	0.012**	0.002	-0.130***	-0.214*	-0.292***	-0.002
	(0.007)	(0.006)	(0.005)	(0.045)	(0.118)	(0.078)	(0.002)
REBP Region	-0.003	0.006	-0.010	-0.029	0.001	0.274	-0.003
	(0.020)	(0.014)	(0.007)	(0.187)	(0.134)	(0.290)	(0.007)
Constant	0.245***	0.176***	0.058***	24.527***	0.734	23.494***	0.022
	(0.051)	(0.042)	(0.009)	(0.514)	(0.471)	(0.744)	(0.017)
Observations	183141	183141	183141	183141	183141	183141	183141
Adjusted R^2	0.147	0.217	0.007	0.225	0.034	0.132	0.001
No of Clusters	99	99	99	99	99	99	99
Mean Dep Var, Control Region	0.303	0.241	0.046	23.602	0.922	22.735	0.022
Sd of Dep Var, Control Region	0.459	0.428	0.211	6.632	3.222	7.438	0.146

Note: The table reports results of the specification in 2. *REBP* captures the effect of REBP-eligibility on the outcomes listed in columns (1) through (7) on a sample of workers employed at the same establishment in May 1988 and February 1994. The regression specification includes region and cohort effects. *Separation* denotes an indicator function that is 1 if a worker is not employed by their employer from February 1994 (and May 1988) in February 1996. *Separation into Nonemployment* denotes an indicator for *Separation* from the initial employer interacted with an indicator for not being employed in February 1996. *Employment (Indicator)* denotes whether a worker is employed in February 1996. *Employment (Months)* , *Unemployment (Months)* and *Continuous Employment (Months)* denote the months of employment, unemployment, and continuous employment with the initial employer between February 1994 and 1996. Standard errors clustered at the administrative region level are reported in parentheses. Levels of significance: * 10%, ** 5%, and *** 1%.

Table 4: Reduced-Form Evidence on Share of Coasean vs. Non-Coasean Separations: Cohort-Region Cells

	1995	1996	1997	1998
$\alpha^{\text{Non-Coasean}}$	1.012	1.098	1.277	1.355
	(0.034)	(0.041)	(0.056)	(0.066)
α^{Coasean}	-0.032	-0.085	-0.248	-0.299
	(0.034)	(0.041)	(0.056)	(0.065)
R^2	0.930	0.960	0.971	0.977

Note: The table reports estimates of the coefficients in Equation (21). We regress the post-REBP separation rate from February 1994 to February of each year among REBP stayers in the REBP region, by month of birth, on both the separation rate among stayers in the non-REBP region and the predicted separation rate based on a Coasean model the REBP period. We weight the observations of the month of birth by the number of employed workers born in that month and report standard errors clustered at the administrative region level.

Table 5: Structural Estimation of Share κ of Efficient Separations Induced by REBP: Industry-Occupation Cells

	2-Digit Industry × Occupation Cells				4-Digit Industry × Occupation Cells			
	1995	1996	1997	1998	1995	1996	1997	1998
$\hat{\kappa}$	-0.0464	-0.123	-0.184	-0.302	0.033	-0.0367	-0.074	-0.168
	(0.087)	(0.077)	(0.088)	(0.081)	0.046	(0.055)	(0.063)	(0.064)
95% CI (Upper Limit)	0.127	0.029	-0.010	-0.141	0.124	0.072	0.050	-0.041
N	109	109	109	109	275	275	275	275

Note: The table reports estimates of κ based on estimation of Equation 22 with non-linear least squares and allowing for an intercept shift. We collapse the data at the industry by occupation (blue/white collar) level and weight each observation by the number of workers in the cell, dropping cells with fewer than ten workers who survived the REBP program. The outcome variable is the separation rate from February 1994 to February of each year among REBP survivors in the REBP region. The model includes the corresponding separation rate among younger workers (cohorts born between 1943 and 1948) as the main regressor along with its transformations according to Equation 22.

Table 6: Complier Characteristics and Heterogeneous Treatment Effects: Results

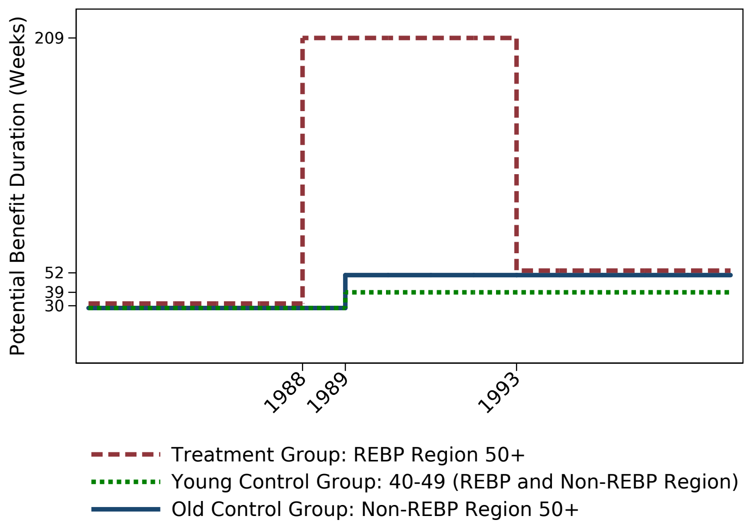
	Complier	Never-Sep	Diff. (C-N)	Always-Sep	Diff. (C-A)	SD	Interaction w Treatment
Blue Collar	0.801	0.528	0.273	0.680	0.121	0.499	0.0700
	(0.064)	(0.055)	(0.055)	(0.034)	(0.059)		(0.023)
Emp. Growth Industry	-0.00100	0.0420	-0.0430	0.0240	-0.0260	0.0670	-0.0970
	(0.013)	(0.007)	(0.012)	(0.005)	(0.014)		(0.042)
Emp. Growth at Establishment	-0.174	0.0930	-0.267	0.00100	-0.175	0.290	-0.0930
	(0.072)	(0.014)	(0.065)	(0.031)	(0.078)		(0.041)
1(Growing Establishment)	0.0970	0.551	-0.454	0.376	-0.279	0.499	-0.104
	(0.104)	(0.032)	(0.103)	(0.030)	(0.108)		(0.041)
Earnings	27961	28250	-288.6	26785	1176	7412	0.0180
	(1114.991)	(851.958)	(868.875)	(748.608)	(1051.335)		(0.022)
Establishment FE	0.0230	0.0130	0.0100	0.0390	-0.0160	0.224	-0.0200
	(0.039)	(0.010)	(0.041)	(0.011)	(0.042)		(0.030)
Worker FE	0.0630	0.128	-0.0650	0.0490	0.0140	0.271	-0.0180
	(0.056)	(0.024)	(0.046)	(0.017)	(0.051)		(0.021)
Tenure	13.04	11.53	1.509	10.80	2.231	5.845	0.0350
	(1.523)	(0.573)	(1.634)	(0.756)	(1.876)		(0.041)
Share on Sick Leave in Industry	0.00900	0.00700	0.00200	0.00800	0.00100	0.00200	0.0960
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)		(0.066)
Share on Disability in Industry	0.0650	0.0560	0.00900	0.0720	-0.00700	0.0170	0.0570
	(0.006)	(0.002)	(0.005)	(0.002)	(0.006)		(0.028)
Share on Sick Leave/Disability in Industry	0.0740	0.0630	0.0110	0.0800	-0.00600	0.0180	0.0820
	(0.006)	(0.002)	(0.005)	(0.002)	(0.006)		(0.030)
Long Spell Duration Risk	0.160	0.118	0.0420	0.130	0.0300	0.0540	0.0650
	(0.015)	(0.002)	(0.015)	(0.004)	(0.017)		(0.046)
Establishment Size: 5 or Less	0.0550	0.0600	-0.00500	0.0330	0.0220	0.257	-0.0570
	(0.029)	(0.003)	(0.029)	(0.005)	(0.030)		(0.035)
Establishment Size: 6 and 20	0.0500	0.0710	-0.0210	0.0440	0.00600	0.269	-0.0230
	(0.027)	(0.008)	(0.026)	(0.006)	(0.027)		(0.031)
Establishment Size: 21 and 100	0.109	0.183	-0.0750	0.0990	0.0100	0.368	-0.0530
	(0.051)	(0.012)	(0.045)	(0.013)	(0.050)		(0.031)
Establishment Size: 101 and 200	0.0600	0.178	-0.118	0.0890	-0.0290	0.361	-0.0510
	(0.042)	(0.015)	(0.039)	(0.011)	(0.042)		(0.030)
Establishment Size: 201 or Greater	0.727	0.508	0.219	0.736	-0.0100	0.499	0.0590
	(0.117)	(0.017)	(0.107)	(0.026)	(0.113)		(0.035)

Note: This table reports characteristics of compliers, never-separators, always-separators as well as difference between the groups based on the methodology in Section 6.2.1. 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 1,000 bootstrap replications blocked at the administrative region level. See Section 6 and Appendix D for more details on how the variables are constructed. See Section 6.2.1 for the methodology underlying the decomposition into the groups.

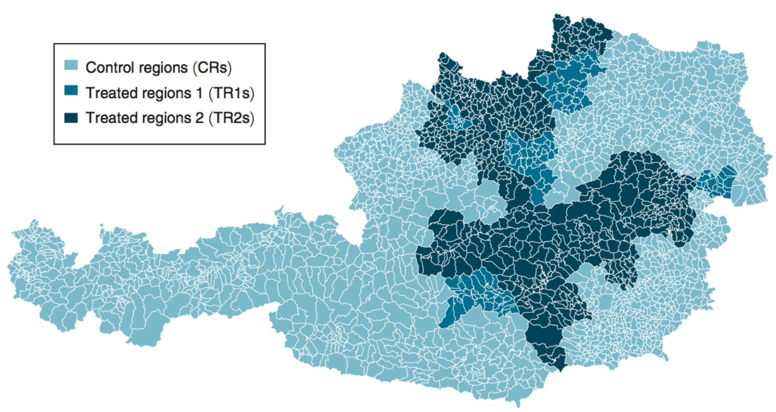
Figures

Figure 1: The Regional Extended Benefit Program (REBP)

(a) Timeline of Potential Benefit Duration During REBP

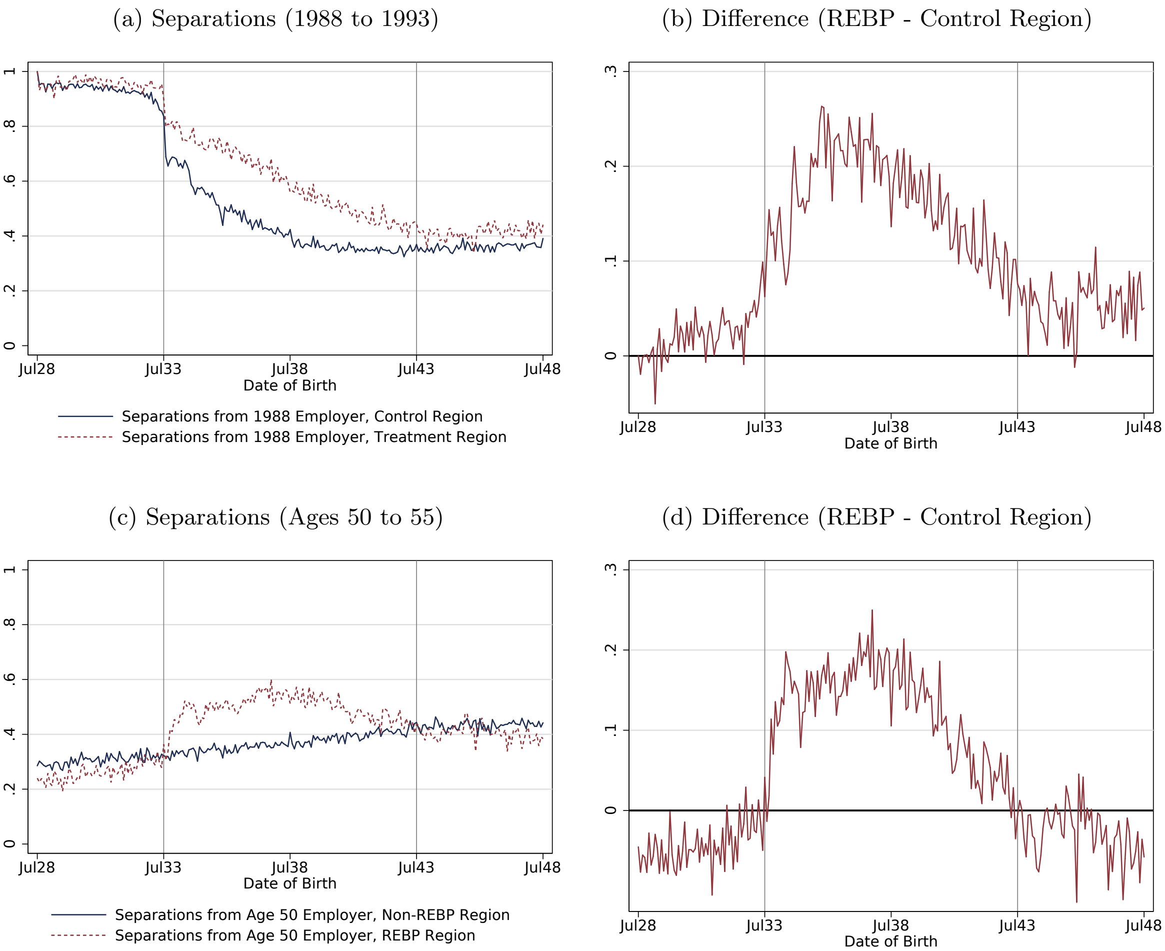


(b) Map of REBP Treatment and Control Regions



Note: Panel (a) shows the timeline of reform changes in potential benefit duration for eligible workers in REBP and control regions. It shows the maximum length of benefits for individuals aged 50 or older in the highest experience category (at least 9 years during the past 15 years), which increased from 30 to 209 weeks starting July 1988. Individuals who have worked less have a lower maximum benefit duration: If they have worked at least 6 years during the past 10 years, they experience an increase from 30 to 39 weeks in August 1989; if they have worked at least 3 years during the past 5 years, their maximum length of unemployment stays constant at 30 weeks over the whole time period. It also shows the maximum length of unemployment insurance for individuals aged 40-49 who fall into the highest experience category: individuals in this category have worked at least 6 years during the past 10 years. In August 1989, maximum benefit duration increased from 30 to 39 weeks. (The maximum length of unemployment stays constant at 30 weeks for individuals who have worked at least 3 years during the past 5 years.) Panel (b) depicts a map of Austrian municipalities showing 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. Source for map: Inderbitzin et al. (2016), Figure 1.

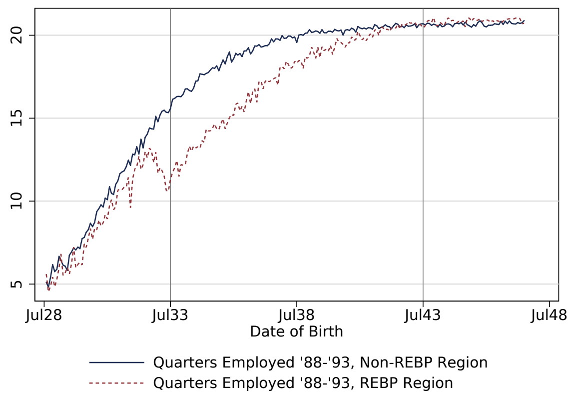
Figure 2: Benefit Extensions and Separations



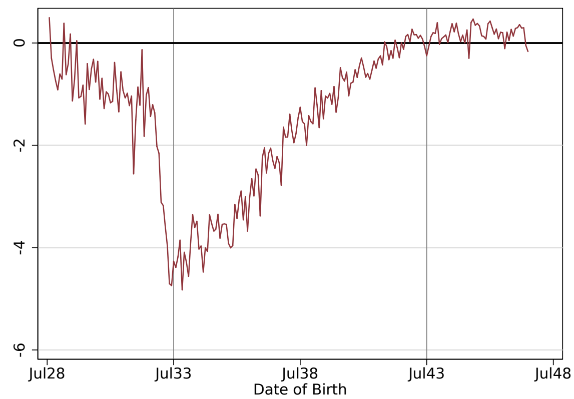
Note: Panel (a) shows the share of workers who separated from their 1988.Q2-employer (right before the reform) by 1993.Q3 (when reform had just ended). Panel (c) shows the share of workers who have separated from their employer in the quarter before turning 50 by the quarter before turning 55, i.e. the age range where REBP extended benefits for eligible workers. Both plot rates by month of birth and within the REBP (red, short dashes) and non-REBP (blue, solid) regions. Panels (b) and (d) show the difference between the REBP and the control region by cohort. Cohorts born after 1943 were not covered by the policy as they turned 50 after the program was abolished 1993.

Figure 3: Benefit Extensions and Employment Outcomes

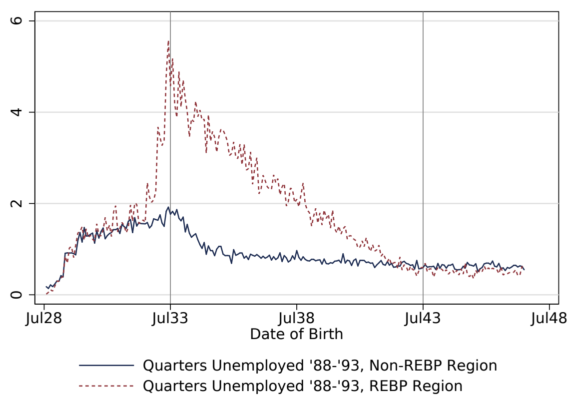
(a) Quarters Employed (1988 to 1993)



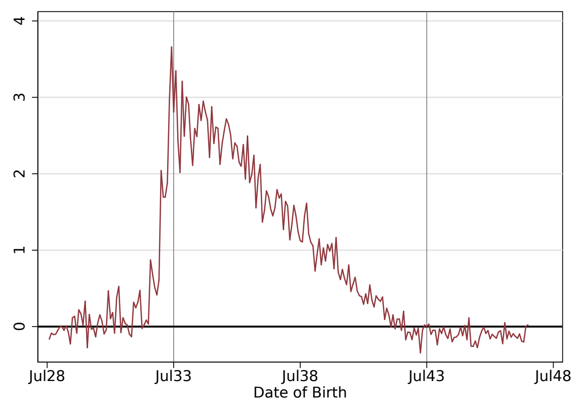
(a) Difference (REBP - Control)



(b) Quarters Unemployed (1988 to 1993)



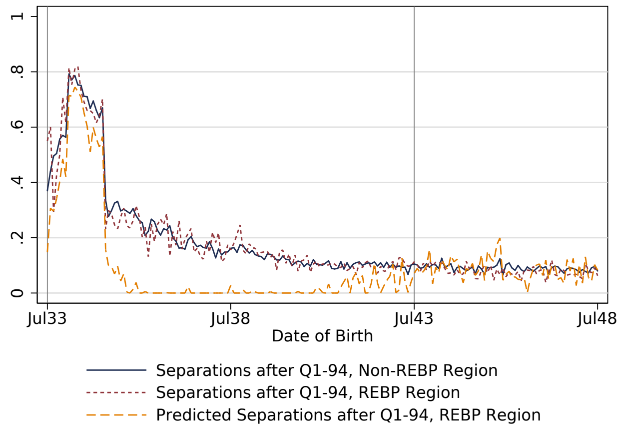
(b) Difference (REBP - Control)



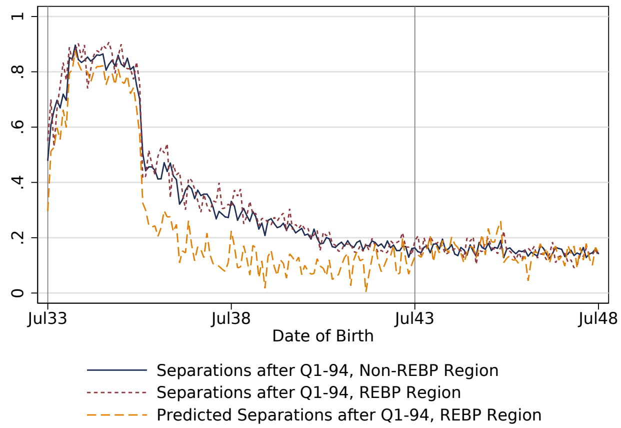
Note: Panels (a) and (c) show the average number of quarters that the workers are employed and on UI, respectively, until the quarter after the end of the REBP (1993q3), among those employed in the quarter before the start of the REBP (1988q2). Both plot rates by month of birth and within the REBP (red, short dashes) and non-REBP (blue, solid) regions. Panels (b) and (d) show the difference between the REBP and the control region by cohort. Cohorts born after 1943 were not covered by the policy as they turned 50 after the program was abolished in 1993.

Figure 4: Separation Rate of "REBP Survivors" from 1994 Onward

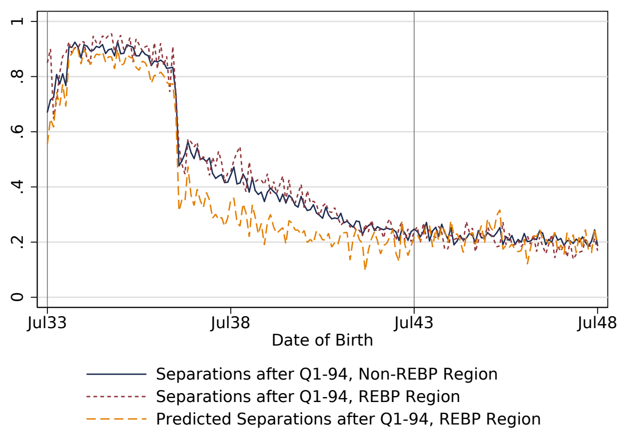
(a) Through 1995



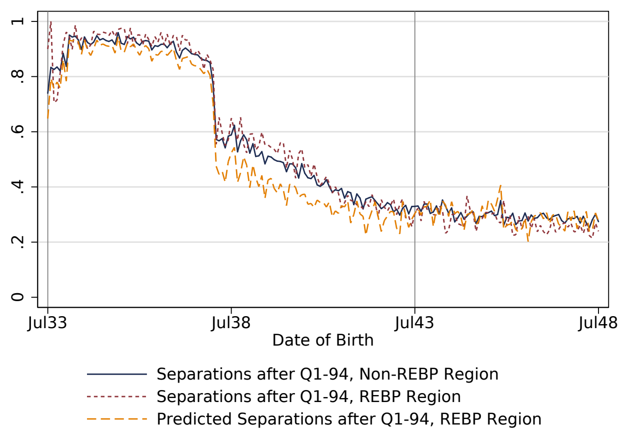
(b) Through 1996



(c) Through 1997



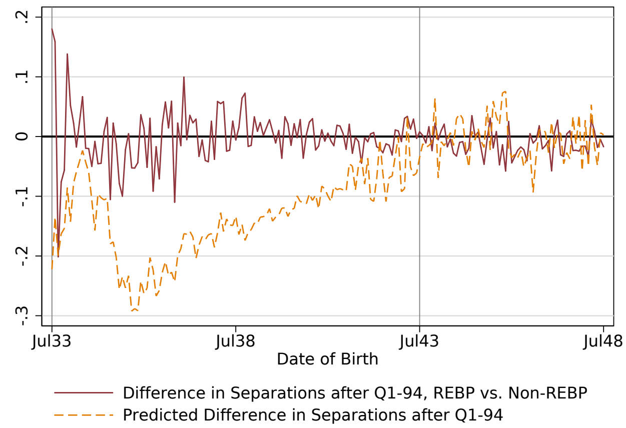
(d) Through 1998



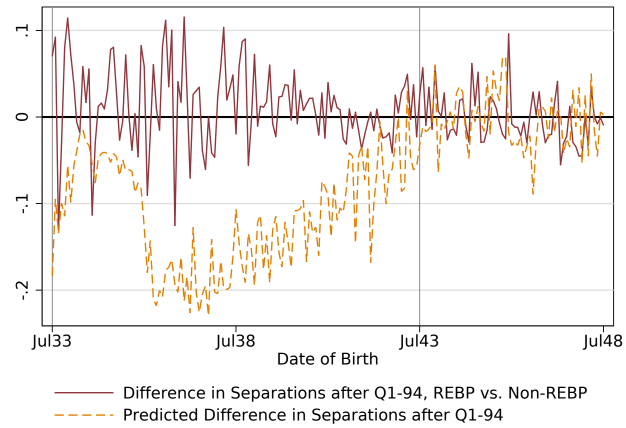
Note: The figure shows, by month of birth, the share of workers observed in the same establishment between 1988.Q2 and 1994.Q1 who separate from that employer by Q1 of each subsequent year. The sample is split into REBP (red, short dashes) and non-REBP (blue, solid) regions. The yellow dashed line plots the predicted Coasean separation rate using Equation (13). The retirement age for Austrian men was 60 years old in this period, which explains the spike in separations among older cohorts.

Figure 5: Difference, REBP vs. Non-REBP Region: Separation Rate of “REBP Survivors” from 1994 Onward

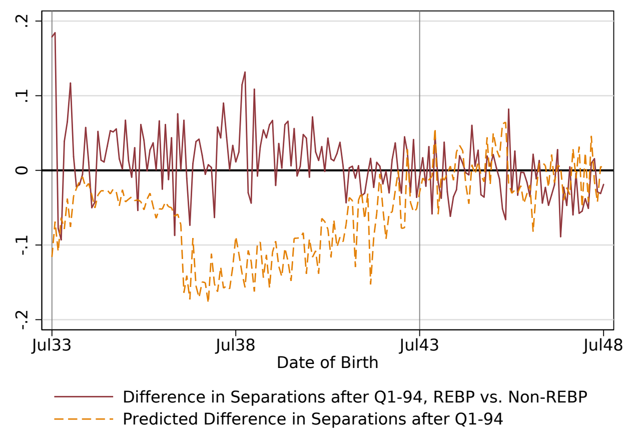
(a) Through 1995



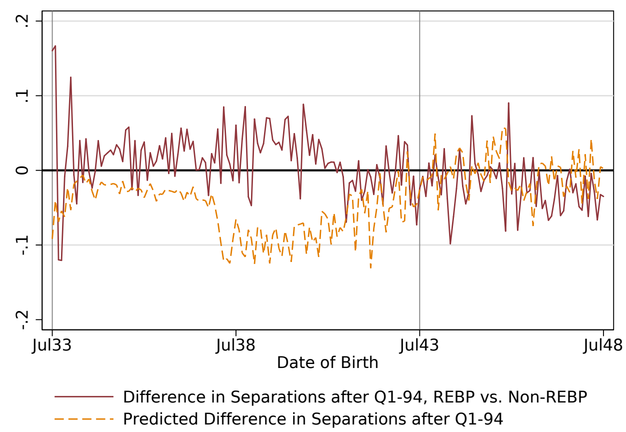
(b) Through 1996



(c) Through 1997



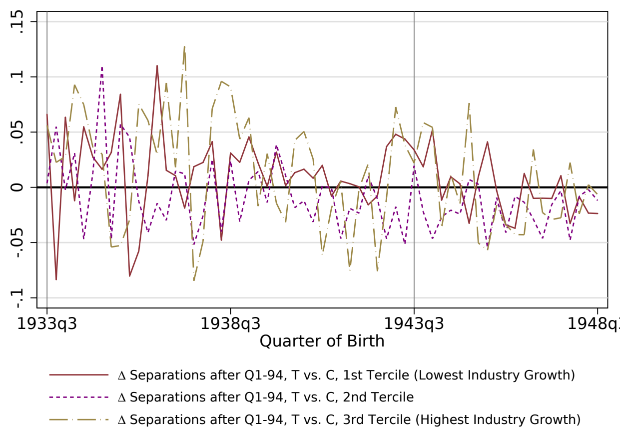
(d) Through 1998



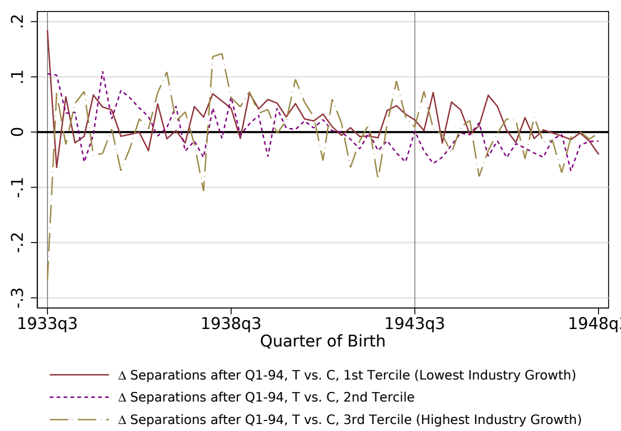
Note: The figure shows, by month of birth, the difference in separation rates from Figure 4 between the REBP and non-REBP regions (red, solid). The yellow dashed line plots the predicted Coasean separation rate using Equation (13).

Figure 6: Difference by Industry Growth, REBP vs. Non-REBP Region: Separation Rate of "REBP Survivors" from 1994 Onward

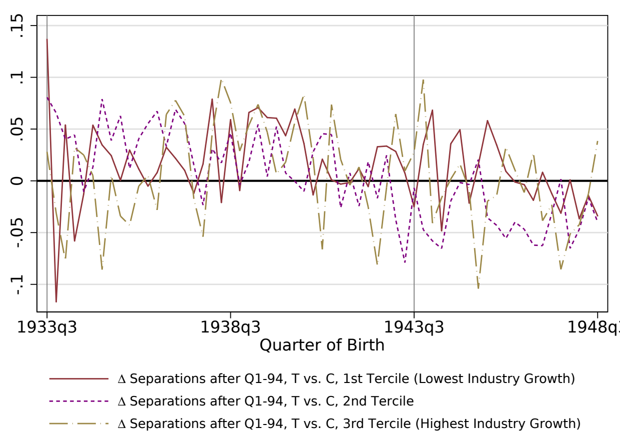
(a) Through 1995



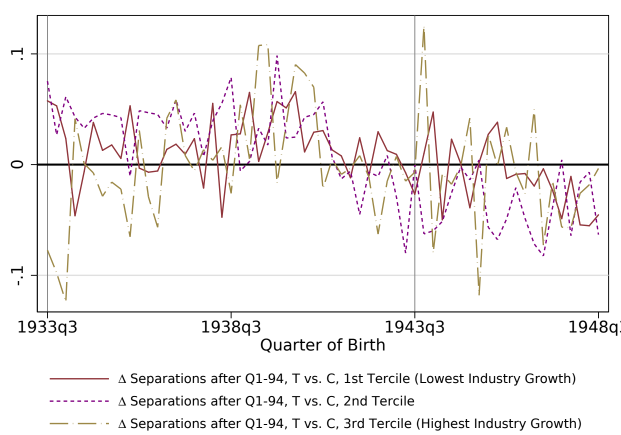
(b) Through 1996



(c) Through 1997



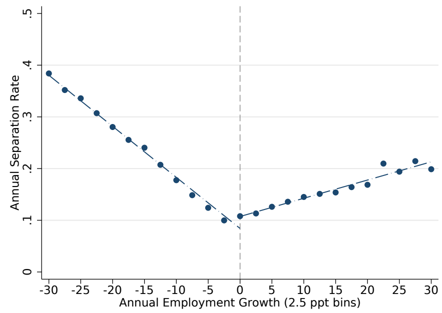
(d) Through 1998



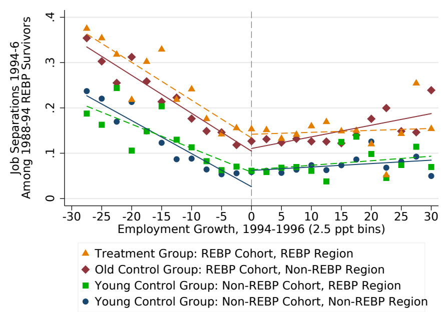
Note: These figures split the by-cohort regional difference from Figure 5 into terciles of industry growth, with the first tercile denoting the lowest and the third tercile denoting the highest industry growth. Specifically, we calculate employment growth between February 1994 and the given year for each industry, among all workers (not just stayers) born 1938 or later to avoid those who are retiring. We then assign REBP stayers to the industries of their REBP-period establishments. We calculate growth rates for industries at the two-digit NACE level.

Figure 7: Establishment-Level “Hockey-Sticks”

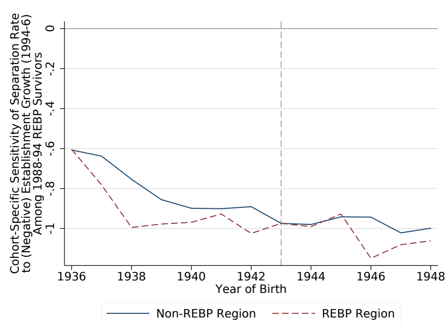
(a) Separations vs. Annual Establishment Growth, 1994-1998



(b) Survivor Separations by Cohort and Region (1994 to 1996)

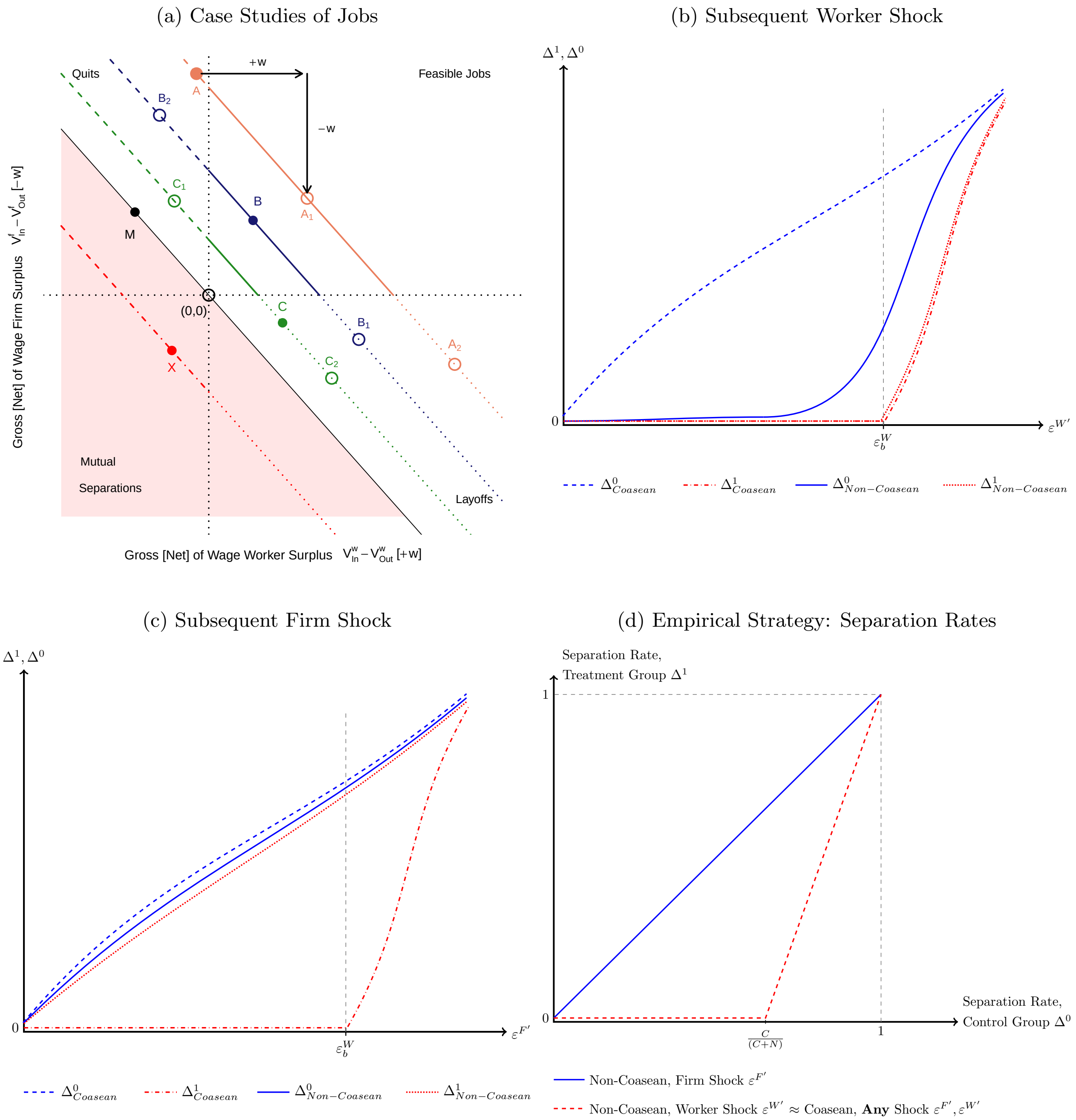


(c) Birth Cohort-Specific Slopes



Note: These figures plot the results of an analysis focusing on labor demand shifts within establishments. In an attempt to confirm the “hockey-stick” relationship between separations and employment growth at the establishment level (Davis et al., 2013) in the Austrian setting, Panel (a) plots annual separation rates for all workers employed in a given year by bins of annual establishment employment growth rates (first quarter of every year), pooling years 1994 through 1998. Panel (b) focuses on separations among the four relevant groups tracked throughout the paper: REBP-eligible and -ineligible birth cohorts and regions, while still plotting against *total* establishment employment growth. We ignore the cohorts born before 1936 since they have reached retirement age in 1996. Panel (c) plots the slope of the cohort-specific relationship between separations and establishment growth (1994-1996) among shrinking establishments for each year of birth and split between the REBP and non-REBP regions. We adjust throughout for spurious layoffs due to mergers, take-overs, and administrative changes to the ASSD using the procedure outlined in Fink et al. (2010).

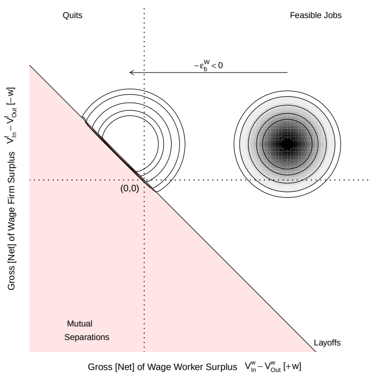
Figure 8: Conceptual Framework: Separations and Shocks



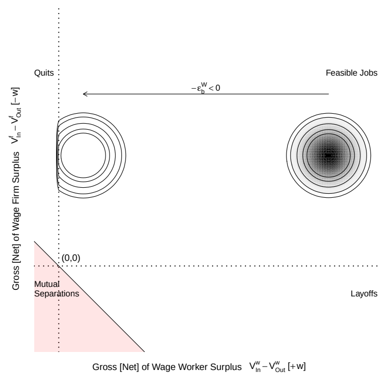
Note: The figures plot the dynamics of job separations in the model, in the Coasean (efficient bargaining) and non-Coasean (fixed-wage) settings. Panel (a) plots job case studies in the two-dimensional space of worker and firm components of joint job surplus, and net of wage surpluses. Panels (a) and (b) plot the relationship between separations in the former treatment group (FT) and former control group (FC) in response to firm (a) and worker (b) side surplus shocks, after REBP, for the Coasean and non-Coasean settings. Panel (d) plots the relationship between treatment group and control group separation rates, after REBP, for the Coasean and non-Coasean setting. Section 5 describes the model and the exercises in detail.

Figure 9: Conceptual Framework: Distribution of Job Surplus and Separations

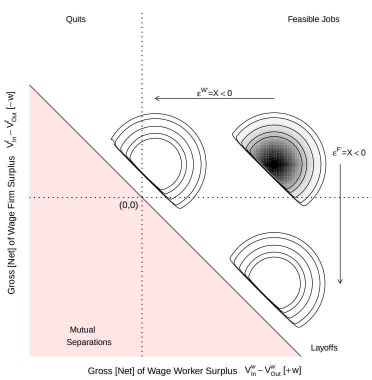
(a.C) Coasean, UI benefit increase



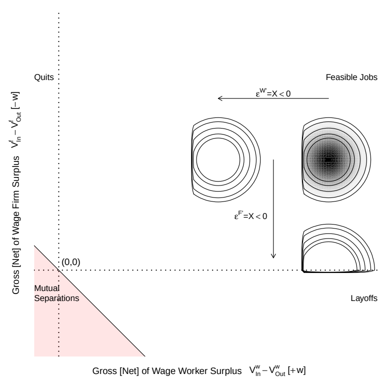
(a.NC) Non-Coasean, UI benefit increase



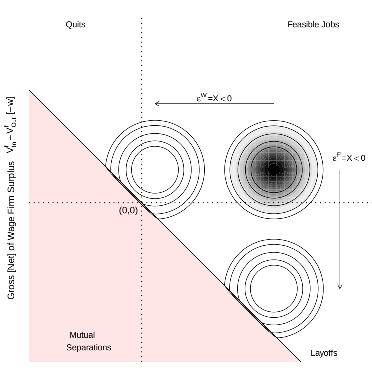
(b.C) Coasean, Post-REBP, Treated



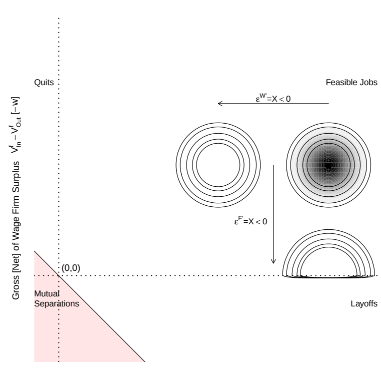
(b.NC) Non-Coasean, Post-REBP, Treated



(c.C) Coasean, Post-REBP, Control

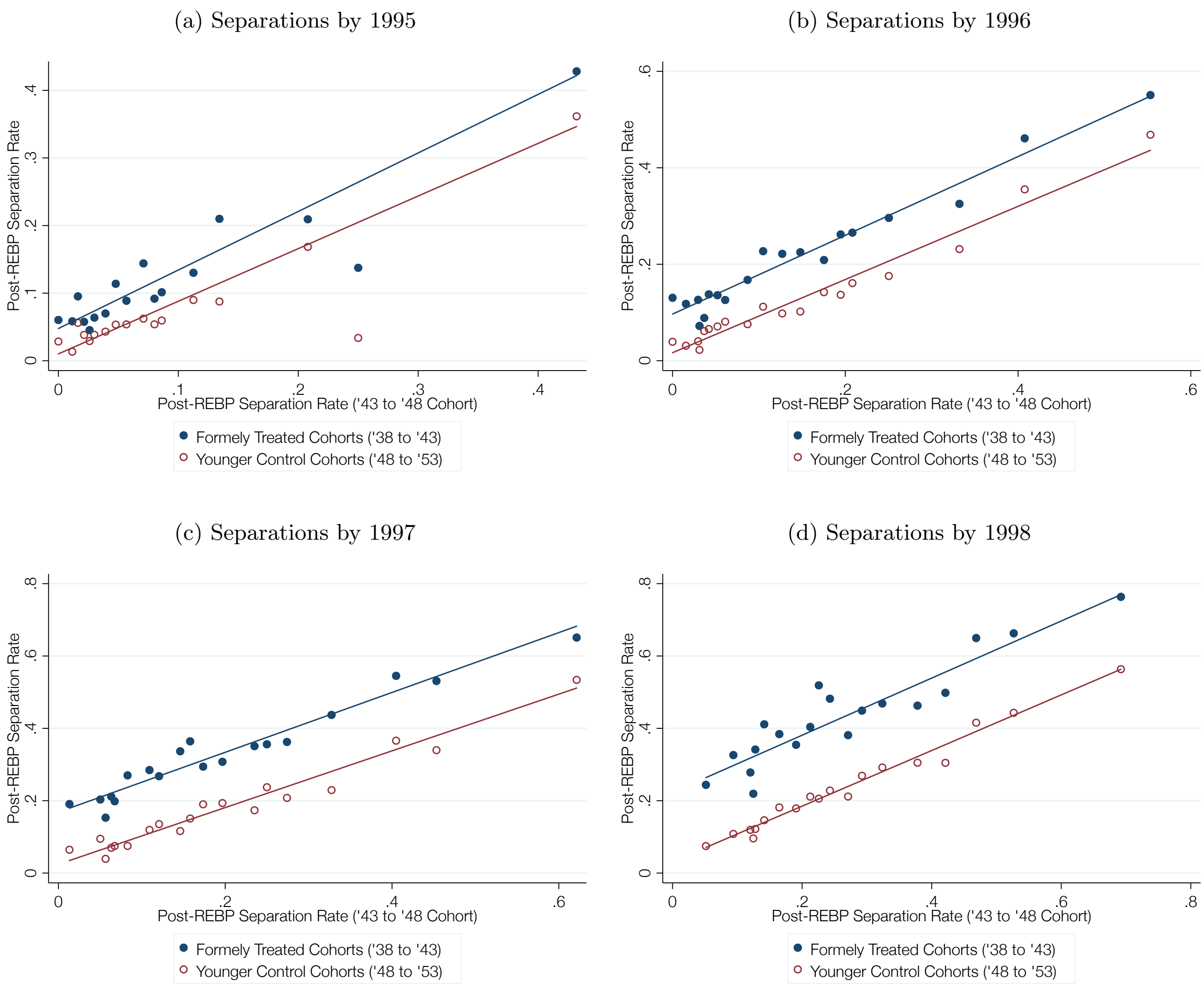


(c.NC) Non-Coasean, Post-REBP, Control



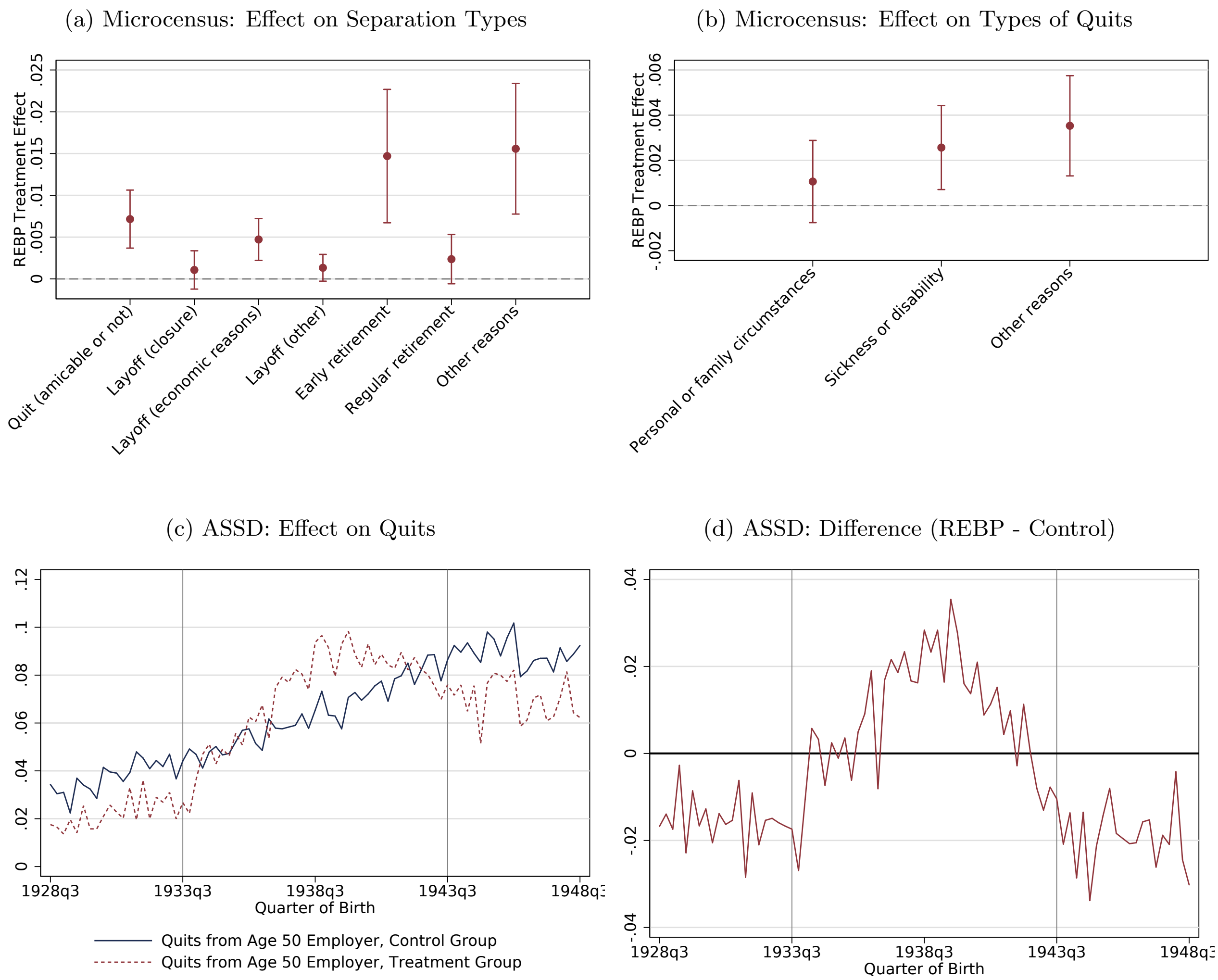
Note: The figure illustrates the model dynamics of job separations, described in Section 5, in the Coasean (efficient bargaining, left panels) and non-Coasean (fixed-wage, right panels) settings. The figures plot contour maps of the density of the joint distribution of firm (y-axis) and worker (x-axis) surpluses; darker shades indicate higher densities, drawn from a bivariate normal distribution. The [non-]Coasean surplus notions are gross[net]-of-wage fundamentals. Panels (a.C) and (a.NC) plot the selection of jobs surviving REBP in the treatment [control] group in the left [right] contour map within each panel a.C and a.NC. Panels (b.C) and (b.NC) [(c.C) and (c.NC)] plot post-REBP sensitivity of separations separately among the former treatment [control] group to post-REBP shocks to worker and firm surplus.

Figure 10: Separation Rates in Formerly Treated and Younger Control Cohorts (REBP Region)



Note: The figures plot binned scatter plots of the separation rates at the industry-by-occupation cell level in formerly treated cohorts (born between 1938 and 1943) against separations rates in slightly younger cohorts (born between 1943 and 1948). As a benchmark, we also plot as outcome variable the separation rate in even younger cohorts (born between 1948 and 1953). Observations are weighted by the number of REBP survivors. Industries are categorized at the four-digit level.

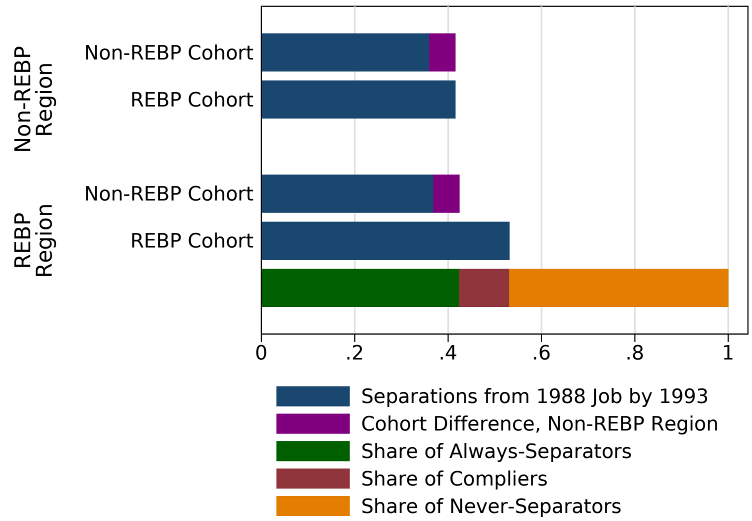
Figure 11: Effect of REBP on Quits and Mutual Separations between Age 50 and 54



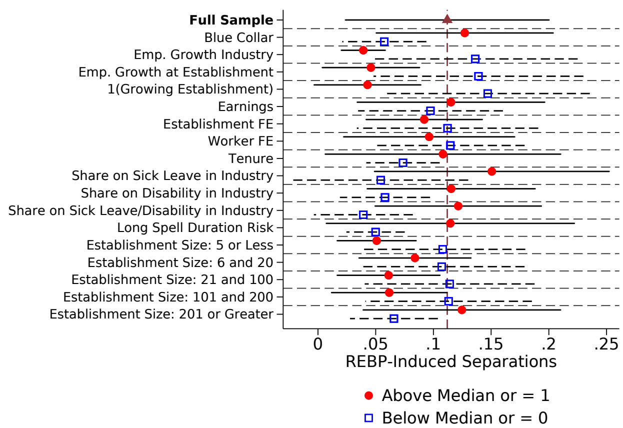
Note: The figures plot data on quits from two sources. Panels (a) and (b) plots outcomes based on the Austrian Microcensus and show treatment effects for the REBP program in a difference-in-difference specification controlling for region and cohort fixed effects on types of separation (a) and, more specifically, types of quits (b). The seven outcome variables that we consider in (a) 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. The three outcome variables that we consider in (b) 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. As a baseline for (a), the values in the control region (among workers from REBP-eligible cohorts) are Quit (amicable or not): 0.00697; Layoff (closure): 0.00346; Layoff (economic reason): 0.0036; Layoff (other): 0.00275; Early Retirement: 0.0350; Regular Retirement: 0.0126; Other: 0.0383. For (b), control region values are Personal or family circumstances: 0.00182; Sickness or disability: 0.00389; Other reasons: 0.00126. The red vertical lines indicate 95% confidence intervals based on standard errors clustered at the administrative region level. Using the Austrian Social Security Dataset, panels (c) and (d) plots an indicator for those who do not take up UI until at least 28 days (the waiting period for quitters to receive UI) after the end of *any* of their employment spells between the ages of 50 and 55. Note that there could be quitters who decide never to receive UI (see Section 6.1) or who find re-employment before the end of the 28-day window.

Figure 12: Complier Characteristics and Treatment Effect Heterogeneity

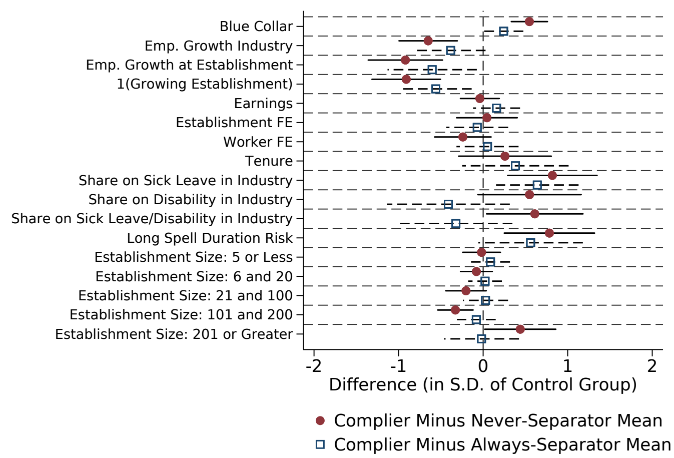
(a) Visualizing the Approach



(b) Treatment Effect Heterogeneity



(c) Difference in Characteristics



Note: Panel (a) outlines how we create the groups of Always-Separators, Compliers, and Never-Separators using the procedure outlined in Section 6.2.1. It first shows the separation rate among employed workers in 1988.Q2 by 1993.Q3, when REBP was abolished, by region and cohort eligibility. It then shows how the share of Always-Separators is made up of the separations among the ineligible age cohorts in the REBP region adjusted for the difference between age cohorts using values from the control region. Any separations in excess of this value are the REBP compliers. Then the share of never-separators are the stayers in the REBP region among the eligible cohort. Panel (b) shows the difference between the averages for compliers and always-separators (C-A) as well as for compliers and never-separators (C-N) that are reported in Table 6. Confidence intervals are based on standard errors from 1,000 bootstrap replications blocked at the administrative region level. See Sections 6 and D for more details on how the variables are constructed. See Section 6.2.1 for the methodology underlying the decomposition into the groups of compliers, never-separators, and always-separators. Panel (b) shows the heterogeneity in the treatment effect of REBP across different characteristics. For binary characteristics (e.g., Blue Collar), we report the treatment effect for each group (1 in red circles, 0 in blue squares). For continuous variables (e.g., earnings), we report the treatment effect on the sample above and below the median (red circles and blue squares respectively). The estimated treatment effect on the full sample is reported at the top with a maroon triangle and the vertical dashed line.

Online Appendix of:

Marginal Jobs and Job Surplus: A Test of the Efficiency of Separations

Simon Jäger, Benjamin Schoefer, and Josef Zweimüller

A Quantifying Worker's Value of the REBP UI Extension

We calculate the cash value of extended benefits following the approach in Card et al. (2007) and complement it with new data on unemployment assistance (UA, "Notstandshilfe" in German). Our estimate for the average cash value of the reform corresponds to about eight to nine months of pay or 71% of a worker's annual salary.

The REBP changed potential UI benefit duration from 30 or 52 weeks to 209 weeks for older workers (see Figure 1a).[57] To provide a conservative estimate of the value of the reform, we take 52 weeks as the alternative potential benefit duration. Under this assumption, REBP changed benefits by 157 weeks or 36.1315 months.

When benefits run out, many workers are eligible for lower UA benefits. UA benefits are means-tested and depend on other sources of income as well as the number of dependents. They are capped at 0.92 of the workers UI benefits, according to the formula

$$\text{UA} = \min(0.92 \times \text{UI}, \max(0,\ 0.95 \cdot\ \text{UI} \quad \text{Spousal Earnings} + \text{Dependent Allowances})). \tag{A1}$$

To impute counterfactual UA payments, we draw on data from the AMS, the Austrian employment agency, on unemployment benefit and UA receipt. This allows us to observe actually paid out UI and UA benefits. We draw on data from a period when both UI and UA payments are observed in the AMS data (2001-2009), and zoom in on workers whose UI benefits ran out and who did not take up employment in the subsequent 60 days. We then calculate the average ratio of UA to UI benefits. We assign everyone zero UA benefits if they do not receive UA benefits in the 60 days after UI benefits ran out, even though they may have been eligible for non-zero UA benefits but did not take them up. In our sample, we find that the average UA benefit corresponds to 50.5% of previous UI benefits.

The average replacement rate between 1988 and 1993 was 40.0%. We calculate that the average replacement rate for worker in eligible cohorts in the REBP region by simply assigning replacement rates to workers based on their earnings and averaging over workers from 1988 to 1993.

As a final input into our calculation, we account for the fact that benefits are not taxed. The average tax rate for personal income in Austria was 11.2% after a 1989 tax reform (OECD, 1990). In addition, employee-borne payroll taxes of about 18% were levied on wages.[58] We thus scale up UI and UA benefits relative to gross income by $1/((1-\tau_{\text{average}})(1-\tau^{\text{Soc. Sec.}}_{\text{average}}))$ to account for non-taxation of benefits.

We can then calculate the cash value of the reform to the average worker according to the formula:

$$\underbrace{31.1315}_{\text{Additional UI months}} \times \underbrace{0.400}_{\text{UI RR}} \times \underbrace{(1-0.505)}_{(\frac{\text{UI RR - UA RR}}{\text{UI RR}})} \times \underbrace{((1-0.115)(1-0.18))^{-1}}_{((1-\tau_{\text{average}})(1-\tau^{\text{Soc. Sec.}}_{\text{average}}))^{-1}} \times w \simeq 8.494 \cdot w, \tag{A2}$$

[57] For most of the treatment period, since 1989, the potential benefit duration for older workers was 52 weeks. Until 1989, the potential benefit duration was 30 weeks.

[58] Specifically, the total payroll taxes contribution rates for workers and firms were, in sum, 34.5% for blue- and 38.6% for white-collar workers (OECD, 1990). In our sample, about 35.4% of workers among 1988 job holders were white-collar workers so that the average social security contribution rate is $0.345 \cdot (1-0.354) + 0.386 \cdot 0.354 \simeq 0.36$, leading to a worker contribution rate of 18%.

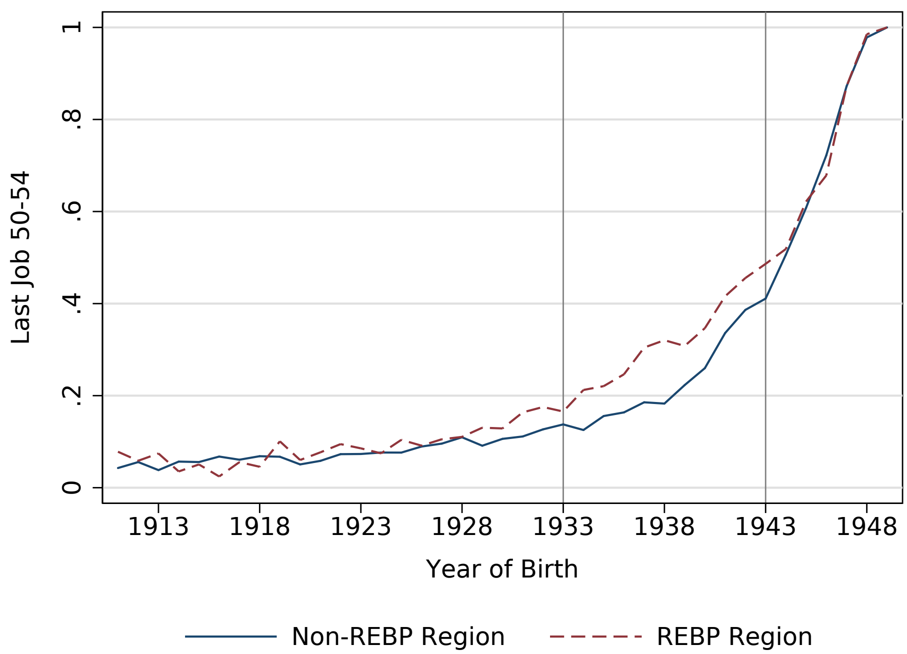
where w denotes the average worker's monthly gross wage and RR denotes replacement rates. According to this calculation, the average cash value of the REBP reform to workers was about eight to nine months of salary or 71% of an annual salary.[59]

[59] Wages in Austria are paid based in 14, rather than 12, installments. The additional two installments are incorporated in the calculation of UI benefits. The monthly wage we mention above corresponds to an average wage corresponding to the annual salary divided by 12.

B Validation of Micro Census: Replicating the Treatment Effect of REBP on Separations

We validate whether the REBP led to excess separations in our micro census sample. Figure A.1 plots an indicator for whether the last employment spell of a non-employed respondent ended when the respondent was between 50 and 54 years old, i.e. the effective age range when extended benefits were available under the REBP program. The Figure shows averages of the indicator by cohort and region. As in the analysis based on administrative data, the analysis based on the Microcensus survey data clearly documents excess separations in the REBP regions for cohorts affected by the reform, i.e. those born between 1933 and 1943. The lines of the REBP and the control regions are parallel and in fact almost lie on top of one another outside of the treatment cohorts.

Figure A.1: 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.

C Complier Characteristics in Difference-in-Differences IV Settings

C.1 Methodology

Here, we provide the detailed, stand-alone methodological guide deriving and formalizing complier analysis in difference-in-differences contexts. To our knowledge, ours is the first characterization of sufficient conditions for complier analysis in difference-in-differences contexts. We complement recent advances to extend instrumental variable approaches to the difference-in-differences framework (De Chaisemartin and D'Haultfoeuille (ming), Hudson et al. (2017)).

Setting and notation. We develop the methodology in the context of a two-dimensional group setup: cohorts $c \in \{c_0, c_1\}$ and regions $r \in \{r_0, r_1\}$. In other contexts, cohorts may describe time (pre- and post-reform, for example), and regions may define treatment and control groups. Importantly, the data set contains observations that span the group space: (r_1, c_1), (r_1, c_0), (r_0, c_1) and (r_0, c_0). In the expressions below, let (r_1, c_1) represent a region and cohort combination such that $Z = 1$ (the treated groups). For all other groups, $Z = 0$ (control groups), i.e. for other region and cohort combinations (i.e. $Z = 0$ for$(r_0, c_1), (r_1, c_0)$ or (r_0, c_0)).

Assumptions. We show how complier characteristics are identified in difference-in-differences settings under the following assumptions:

> **Assumption 1.** First stage: For all $r \in \{r_0, r_1\}$ and $c \in \{c_0, c_1\}$, $P(D_1 = 1|r, c) > P(D_0 = 1|r, c)$.

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

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

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

> **Assumption 3.** Independence: $(x, D_0, D_1) \perp Z|(r, c)$.

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

> **Assumption 4.** Additive separability: (a) For all $c \in \{c_0, c_1\}$ and $d, d' \in \{0, 1\}$, $\mathbb{E}[x|c, r, D_0 = d, D_1 = d'] - \mathbb{E}[x|c', r, D_0 = d, D_1 = d']$ does not depend on r.
> (b) For all $c \in \{c_0, c_1\}$ and $d, d' \in \{0, 1\}$, $P(D_0 = d, D_1 = d'|c, r)$-$P(D_0 = d, D_1 = d'|c', r)$ does not depend on r.

Instrument. Since Z does not vary conditional on region and age, we only observe either $Z = 0$, or $Z = 1$, for a given region and cohort cell. Under the first stage and monotonicity assumption, the expected value of complier characteristics can be represented as a function of observable quantities for all region/cohort combinations.

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

$$\begin{aligned} \mathbb{E}[x|D_1 = 1, r, c] \quad = \quad & \mathbb{E}[x|r, c, D_1 = 1, D_0 = 1] \cdot P(D_0 = 1|r, c, D_1 = 1) \\ & + \mathbb{E}[x|r, c, D_1 = 1, D_0 = 0] \cdot P(D_0 = 0|r, c, D_1 = 1). \end{aligned} \tag{A3}$$

Rearranging yields

$$
\begin{aligned}
\mathbb{E}[x|r,c,D_1=1,D_0=0] \quad = \quad & \tfrac{1}{P(D_0=0|r,c,D_1=1)}\cdot\mathbb{E}[x|r,c,D_1=1] \\
& -\tfrac{P(D_0=1|r,c,D_1=1)}{P(D_0=0|r,c,D_1=1)}\mathbb{E}[x|r,c,D_1=1,D_0=1].
\end{aligned} \tag{A4}
$$

By monotonicity ($D_1 - D_0 \geq 0$), we have that $\mathbb{E}[x|r,c,D_1=1,D_0=1]=\mathbb{E}[x|r,c,D_0=1]$, which implies:

$$
\begin{aligned}
\mathbb{E}[x|r,c,D_1=1,D_0=0] \quad = \quad & \tfrac{1}{P(D_0=0|r,c,D_1=1)}\cdot\mathbb{E}[x|r,c,D_1=1] \\
& -\tfrac{P(D_0=1|r,c,D_1=1)}{P(D_0=0|r,c,D_1=1)}\mathbb{E}[x|r,c,D_0=1].
\end{aligned} \tag{A5}
$$

Using the definition of conditional probabilities, $P(D_0=1|r,c,D_1=1)=\frac{P(D_0=1,D_1=1|r,c)}{P(D=1|r,c)}$ and $P(D_0=0|r,c,D_1=1)=\frac{P(D=0,D_1=1|r,c)}{P(D_1=1|r,c)}$. Define the (conditional on region and cohort) probability of always-separators as $\pi^A_{rc}=P(D_0=1,D_1=1|r,c)$, of never-separators as $\pi^N_{rc}=P(D_0=0,D_1=0|r,c)$ and by monotonicity of compliers as $\pi^C_{rc}=P(D_0=0,D_1=1|r,c)=1-\pi^A_{rc}-\pi^N_{rc}$. The conditional expectation term above can then be expressed as follows:

$$
\begin{aligned}
\mathbb{E}[x|r,c,D_1=1,D_0=0] \quad = \quad & \tfrac{\pi^C_{rc}+\pi^A_{rc}}{\pi^C_{rc}}\cdot\mathbb{E}[x|r,c,D_1=1] \\
& -\tfrac{\pi^A_{rc}}{\pi^C_{rc}}\mathbb{E}[x|r,c,D_0=1].
\end{aligned} \tag{A6}
$$

Equation A6 shows that complier characteristics are identified in a difference-in-difference IV setting. We can construct sample analogues to each of the terms on the right-hand side as follows by drawing on the independence and additive separability assumption. The following exposition follows the case of calculating characteristics conditional on $(r,c)=(r_1,c_1)$. By independence, we have that:

$$
\mathbb{E}[x|r_1,c_1,D_1=1]=\mathbb{E}[x|r_1,c_1,D=1,Z=1]. \tag{A7}
$$

By independence and additive separability in x, we have:

$$
\begin{aligned}
\mathbb{E}[x|r_1,c_1,D_0=1] &= \mathbb{E}[x|r_1,c_0,D_0=1]+(\mathbb{E}[x|r_0,c_1,D_0=1]-\mathbb{E}[x|r_0,c_0,D_0=1]) \\
&= \mathbb{E}[x|r_1,c_0,D=1,Z=0] \qquad \text{(A8)} \\
&\quad + \mathbb{E}[x|r_0,c_1,D=1,Z=0] \\
&\quad - \mathbb{E}[x|r_0,c_0,D=1,Z=0]. \qquad \text{(A9)}
\end{aligned}
$$

Sample analogues exist for each of the right-hand side terms in A7 and A8:

$$
\mathbb{E}[x|r_1,c_1,D=1,Z=1]=\frac{1}{N_{r_1,c_1}}\sum_{i\in(r_1,c_1)}x_iD_i, \tag{A10}
$$

$$
\mathbb{E}[x|r,c_0,D=1,Z=0]=\frac{1}{N_{r_1,c_0}}\sum_{i\in(r_1,c_0)}x_iD_i, \tag{A11}
$$

$$
\mathbb{E}[x|r_0,c_1,D=1,Z=0]=\frac{1}{N_{r_0,c_1}}\sum_{i\in(r_0,c_1)}x_iD_i, \tag{A12}
$$

$$
\mathbb{E}[x|r_0,c_0,D=1,Z=0]=\frac{1}{N_{r_0,c_0}}\sum_{i\in(r_0,c_0)}x_iD_i, \tag{A13}
$$

where N_{r_1,c_1} is the number of observations in (r_1,c_1) and so forth.

For the conditional probabilities in A6 note that (using independence and the additive separability in D assumption):

$$\begin{aligned}
\pi_{rc}^{A} = P(D_0 = 1, D_1 = 1|r_1, c_1) &= P(D_0 = 1|r_1, c_1) \\
&= P(D_0 = 1|r_1, c_0) + P(D_0 = 1|r_0, c_1) - P(D_0 = 1|r_0, c_0) \qquad \text{(A14)} \\
&= E(D|Z = 0, r_1, c_0) + E(D|Z = 0, r_0, c_1) - E(D|Z = 0, r_0, c_0) \\
\pi_{rc}^{N} = P(D_0 = 0, D_1 = 0|r_1, c_1) &= P(D_1 = 0|r_1, c_1) \\
&= P(D = 0|Z = 1, r_1, c_1) \qquad \text{(A15)} \\
&= 1 - E(D|Z = 1, r_1, c_1).
\end{aligned}$$

These quantities can be estimated in the regression:

$$D_{irc} = \beta + \phi_r + \psi_c + \nu Z_{rc} + \chi_{irc}. \qquad \text{(A16)}$$

The sample estimators are then given by $\pi_{rc}^{A} = \hat{\beta} + \hat{\phi}_r + \hat{\psi}_c$, $\pi_{rc}^{N} = 1 - \hat{\beta} - \hat{\phi}_r - \hat{\psi}_c - \hat{\nu}$ and $\pi_{rc}^{C} = 1 - \pi_{rc}^{N} - \pi_{rc}^{A} = \hat{\nu}$. All objects on the right-hand side of A6 thus have estimable sample counterparts.

Extensions. Under additional assumptions, we can alternatively estimate the conditional expectations in A6 in a regression framework. Specifically, if trends in x are the same for always-separators, always-separators and compliers, never-separators, and never-separators and compliers, the conditional expectations of characteristics can be estimated from the regression below:

$$x_{irc} = \alpha + \kappa_r + \lambda_c + \omega D_{irc} + \gamma Z_{rc} + \varphi D_{irc} \times Z_{rc} + \epsilon_{irc}. \qquad \text{(A17)}$$

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

Under slightly weaker assumptions, not requiring us to assume parallel trends for never-separators and compliers, we can use the following regression to estimate the required conditional expectations in equation 30 by interacting the trend variables with D so that we can estimate separate trends for (1) always-separators and (2) never-separators and compliers:

$$x_{irc} = \alpha + \kappa_r + \lambda_c + \omega D_{irc} + \tilde{\kappa_r} \times D_{irc} + \tilde{\lambda_c} \times D_{irc} + \gamma Z_{rc} + \varphi D_{irc} \times Z_{rc} + \epsilon_{irc} \qquad \text{(A18)}$$

We then have $\mathbb{E}[x|r, c, D_0 = 1] = \alpha + \kappa_r + \lambda_c + \omega + \tilde{\kappa_r} + \tilde{\lambda_c}$ and $\mathbb{E}[x|r, c, D_1 = 1] = \alpha + \kappa_r + \lambda_c + \omega + \tilde{\kappa_r} + \tilde{\lambda_c} + \gamma + \varphi$. Our approach can also be extended to estimate complier characteristics in the $Z = 0$ cells if one of the following assumptions holds:

> **Assumption 5 (a).** Age and region trends are the same for always-separators and compliers, or
>
> **Assumption 5 (b).** Age and region trends are the same for always-separators and never-separators and either the proportion of compliers or never-separators is constant across age and region.

C.2 Estimation Procedure for Complier Characteristics

We describe the non-parametric bootstrapping procedure for inference on the methodology described in Section 6.2.1.

We use bootstrap samples of all employed men in 1988.Q2 working in either the non-REBP region or the region where REBP was abolished in 1993. As in the DiD specifications (Tables 2 through

3), we take samples by random clusters of administrative region.[60] Specifically, we take blocks of the NUTS 3 designations for Austria, which consist of groups of districts (*Bezirke*) within the Austrian states (*Bundesländer*).

For each of the 1,000 random samplings of administrative regions, we

1. Estimate the proportion of always-separators, never-separators, and compliers using Equations 27, 28, and 29, respectively.

2. Estimate the average of each outcome among compliers, always-separators, and never-separators by calculating the relevant conditional means and using Equation 32. Namely, we use the sample analogues in Equation 33. We also take differences between the complier average and the other two averages.

3. Estimate regional and cohort averages to investigate parallel trends (see Table A.1). Specifically, for each outcome Y_i, we run the DiD specification

$$Y_i = \beta + \phi_r + \psi_c + \nu Z_{rc} + \chi_{irc} \tag{A19}$$

   where every individual i is in region r (REBP vs. non-REBP) and cohort c (REBP-eligible vs. ineligible). Then,

   - For column (1) in Table A.1, the difference between the eligible and ineligible cohorts in the non-REBP region is $\hat{\psi}_c$.
   - For column (2) in Table A.1, the cohort difference in the REBP region is $\hat{\psi}_c + \hat{\nu}$.
   - For column (3) in Table A.1, the difference-in-differences is $\hat{\nu}$.

4. Investigate treatment effect heterogeneity.

   - Specifically, we create indicators for whether the individual is above or below the median value of each characteristic for continuous variables like earnings and an indicator for each value for binary characteristics like being in a blue-collar occupation.
   - We run the DiD specification in Equation 1 separately for each group.[61]
   - We keep the two estimates of $\hat{\nu}$ and also take differences for the final column of Table 6.

The final output is 1,000 estimates of each value of interest, one from each random sample of districts. The reported standard errors are the standard deviation of these 1,000 estimates. The reported point estimates are the same specifications run on the full sample.

[60] In practice, we use the `bsample` command in Stata with the `cluster` option.
[61] Equivalently, we fully interact the DiD specification with the indicator.

D Variable Construction

We describe the construction of each outcome variable presented in the paper. In the descriptions below, `status` refers to a variable in the ASSD aggregating hundreds of administrative designations into 12 labor market statuses (Zweimüller et al., 2009).

1. **Separation**
   - Create an indicator if, between two periods (e.g., 1988q2 and 1993q3), the worker is observed in the same establishment.
   - If not, the worker is separated
2. **Separation into Nonemployment**
   - Create an indicator if, between two periods (e.g., 1988q2 and 1993q3), the worker is observed as employed ($\texttt{status} = 3$) in the current period and not employed ($\texttt{status} \neq 3$).
3. **Separation into New Job**
   - Create an indicator if, between two periods (e.g., 1988q2 and 1993q2), the worker is observed as employed ($\texttt{status} = 3$) in the current period and in the next but observed in different establishments.
4. **Unemployment (Months)**
   - Between two periods (e.g., 1988q2 and 1993q3), count the number of quarters where the worker is observed on UI ($\texttt{status} = 1$).
   - Multiply the quarter count by 3 to get a monthly count for tractability.
5. **Continuous Employment (Months)**
   - Between two periods (e.g., 1988q2 and 1993q3), count the number of quarters where the worker is employed in the same establishment as the quarter before.
   - Stop counting when the worker is observed either employed in a new establishment or with another labor market status.
6. **Quit (Indicator)**
   - Out of the original labor market spell data from the ASSD, count the number of days between the *end* of an employment spell ($\texttt{status} = 3$) and the *beginning* of an unemployment spell ($\texttt{status} = 1$).
   - For the yearly analysis, isolate employment spells that end during the REBP period (1988q2 through 1993q3).
   - For the age analysis, isolate employment spells that end while the worker is between 50 and 55 years old.
   - If the beginning of the unemployment spell occurs 28 days or more after the end of *any* employment spell during this range, the worker is considered to have quit.
7. **Local Unemployment Rate**
   - Take the snapshot of men in the ASSD from 1988q2 (May 15, 1988).
   - Assign each worker a municipality (*Gemeinde*) by the location of the establishment at which they work.

- Count the number on UI (`status` = 1) in each municipality.
- Count the number of workers not on any pension, i.e. neither on disability (e.g., *Berufsunfähigkeitspension*) nor retirement pensions.

8. **Local Unemp. Rate (50+)**
   - Do the same as for unemployment rates, but restrict to the sample of workers who are 50 years old or older in 1988q2.
9. **Establishment Size**
   - For every establishment, count the total number of men of any age employed in 1988q2.
10. **Employment Growth at Establishment**
    - In Table 1 and the complier analysis, this counts the percent difference between the number of men (no age restriction) employed in the establishment in 1983q2 and 1988q2.
    - In Figure 7, this is the percent difference between the number of men born in 1933 or later (to avoid retirements) and employed in each establishment in 1994q1 and Q1 of each following year.
11. **1(Growing Establishment)**
    - In all cases, this is an indicator for whether the establishment employment growth variable calculated above is positive.
12. **Employment Growth at Industry**
    - This counts the percent difference between the number of men (no age restriction) employed in one of 15 industries in 1983q2 and 1988q2.
13. **Establishment Fixed Effects**
    - Begin with the sample of all workers in May 1977 through 1987.
    - The earnings value is the total annual earnings in 2002 EUR from the establishment at which the worker is employed on June 15 of each year.
    - Winsorize the earnings value to the 5th and 95th percentile by year and take the logarithm.
    - Regress log earnings on a third-degree polynomial of age and fixed effects for each worker and establishment using the procedure to estimate high-dimensional fixed effects outlined in Correia et al. (2016).
    - Assign to each worker the establishment fixed effect from the establishment at which they work in 1988q2.
14. **Worker Fixed Effects**
    - Use the worker fixed effects estimated in the proecedure outlined to estimate establishment fixed effects.
15. **Share on Sick Leave/Disability in Industry**
    - Begin with the sample of men age 50 to 55 in any quarter of years 1977 through 1988.
    - If workers are non-employed in a given quarter, assign the establishment identifier of their last employe, i.e. the establishment identifier of the last establishment where `status` = 3.

- These assigned establishments are used to assign industry.
- By four-digit NACE industry level, calculate the share of workers on sick leave (`status` = 2) and receiving disability payments (`status` = 5) as a fraction of all workers assigned to that industry across all periods.

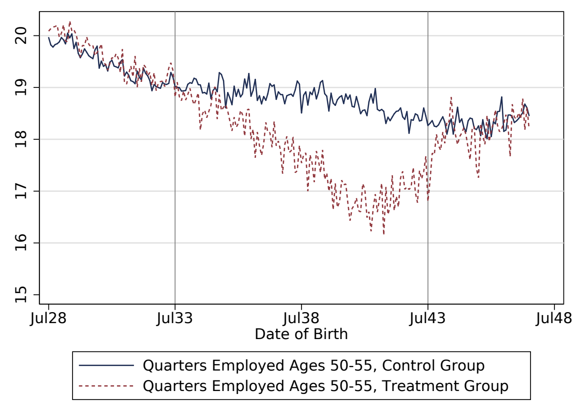
16. **Long Spell Duration Risk**

- Begin with the sample of all men and quarters from 1982 through 1987.
- Count the length of each spell on UI (`status` = 1) by quarter.
- Isolate the maximum length of a UI spell for each worker.
- Create an indicator for whether the maximum length of the UI spell for each worker is greater than 1 year (4 quarters).
- Regress this indicator on fixed effects for 15 large industries, an indicator for being in a white-collar occupation, the local unemployment rate (see above), and third-degree polynomials of tenure in current job and experience over the last 25 years.
- Predict the outcome, i.e. risk of long UI duration, for every worker in the REBP sample using these estimates.

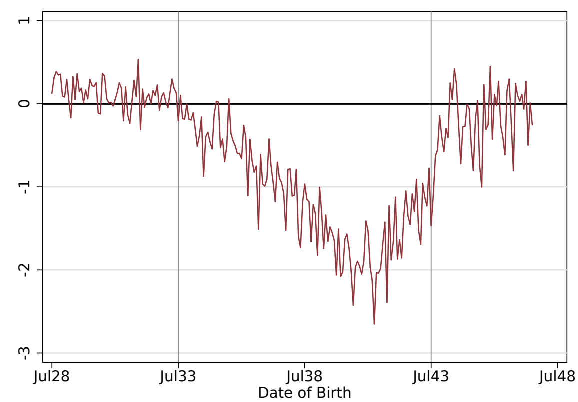
E Additional Figures

Figure A.2: Benefit Extensions and Employment Outcomes by Age

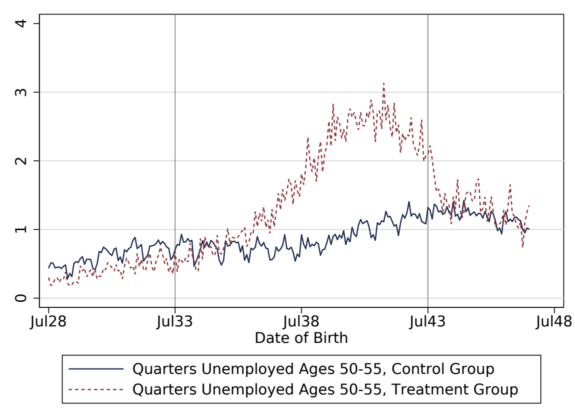
(a) Quarters Employed (Age 50 to 55)



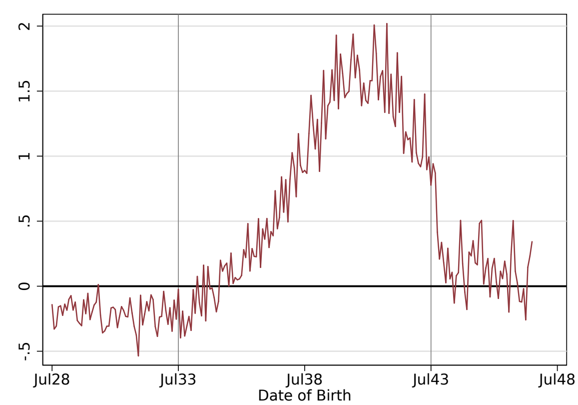
(a) Difference (REBP - Control)



(b) Quarters Unemployed (Age 50 to 55)

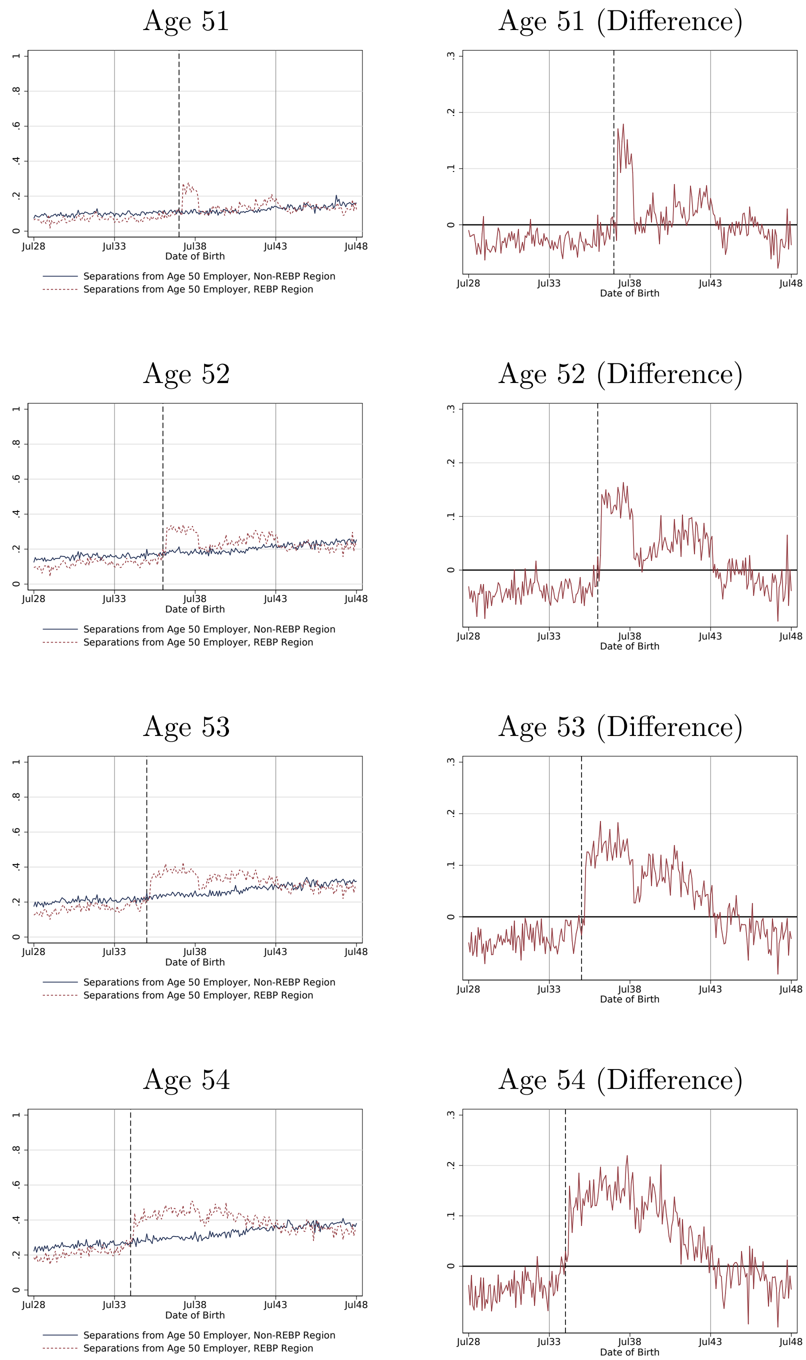


(b) Difference (REBP - Control)



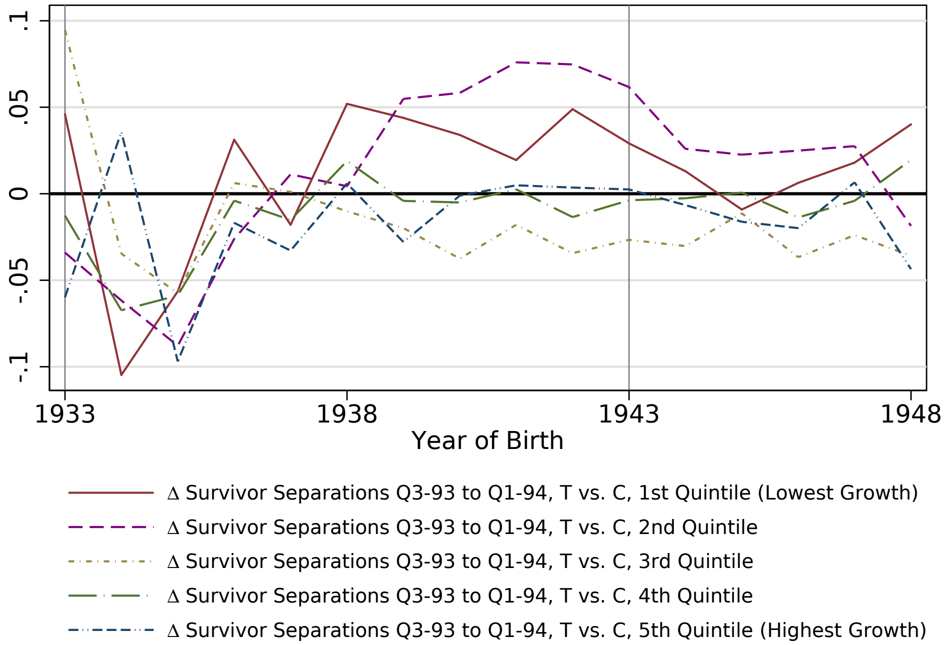
Note: Panels (a) and (c) show the average number of quarters that the workers are employed and on UI, respectively, until the quarter before they turn 55, among those employed in the quarter before they turn 50. Both plot rates by month of birth and within the REBP (red, short dashes) and non-REBP (blue, solid) regions. Panels (b) and (d) show the difference between the REBP and the control region by cohort. Cohorts born after 1943 were not covered by the policy as they turned 50 after the program was abolished 1993.

Figure A.3: REBP Effects by Age



Note: This figure shows the separation rate from jobs held in the quarter before workers turn 50 through the quarter before their birthday. See Figures 2(c) and (d) for the full effect of the REBP from ages 50 through 54. The horizontal dashed line shows the oldest cohort that had been exposed to the REBP at a given age. For example, at age 51, only those born between 1937 and 1943 had been exposed to the REBP because older cohorts turned 51 before the REBP began, i.e. before 1988. Younger cohorts, meanwhile, turned 50 after the REBP was abolished and thus were never eligible. Cohorts 1938-1943 turned 50 during the REBP. The thin, solid horizontal lines bookend the treated cohorts that were the focus of our analysis, i.e. cohorts that were ever eligible for REBP benefits but had not reached the retirement age by the program's abolition in 1993.

Figure A.4: "Phantom" Treatment Effects



Note: This figure gives a sense of possible post-abolition separation effects by REBP. It plots, by year of birth, the separations from 1993q3, the first quarter after the abolition of the REBP, to 1994q1, among the workers who remain employed in their 1988q2 establishment in 1993q3, i.e. the REBP "survivors." It breaks the separations into quintiles of industry growth rates (not worker-weighted) to emphasize where the additional separations occur. This, along with grandfathering clauses in the law abolishing the REBP (see Section 2), motivates our decision to analyze possible resilience effects by looking at jobs that survived through 1994q1, rather than those surviving only to the abolition of the reform.

F Additional Tables

Table A.1: Complier Characteristics: Balance Check

	Cohort Difference, REBP vs. Non-REBP		
	Non-REBP Region	REBP Region	Difference
Blue Collar	0.0180	0.0260	0.00800
	(0.006)	(0.005)	(0.008)
Emp. Growth Industry	-0.00500	-0.00700	-0.00200
	(0.001)	(0.001)	(0.001)
Emp. Growth at Establishment	-0.0300	-0.0530	-0.0230
	(0.002)	(0.006)	(0.007)
1(Growing Establishment)	-0.0220	-0.0500	-0.0280
	(0.004)	(0.010)	(0.010)
Earnings	233.7	300.8	67.06
	(51.122)	(101.396)	(109.150)
Establishment FE	0.0140	0.0110	-0.00300
	(0.002)	(0.003)	(0.003)
Worker FE	-0.0310	-0.0330	-0.00200
	(0.003)	(0.005)	(0.005)
Tenure	1.276	1.497	0.221
	(0.081)	(0.091)	(0.105)
Share on Sick Leave in Industry × 100	-0.00200	0.00700	0.00900
	(0.003)	(0.002)	(0.003)
Share on Disability in Industry × 100	0.0680	0.172	0.104
	(0.029)	(0.038)	(0.049)
Share on Sick Leave/Disability in Industry × 100	0.0660	0.179	0.113
	(0.031)	(0.039)	(0.051)
Long Spell Duration Risk	0.0220	0.0240	0.00300
	(0.001)	(0.001)	(0.001)
Establishment Size: 5 or Less	-0.0110	-0.00900	0.00200
	(0.001)	(0.002)	(0.002)
Establishment Size: 6 and 20	-0.00700	-0.0100	-0.00400
	(0.001)	(0.003)	(0.003)
Establishment Size: 21 and 100	-0.00500	0	0.00500
	(0.002)	(0.004)	(0.005)
Establishment Size: 101 and 200	0.00400	0	-0.00400
	(0.003)	(0.004)	(0.005)
Establishment Size: 201 or Greater	0.0180	0.0190	0.00100
	(0.004)	(0.008)	(0.009)

Note: This table reports the mean difference for the characteristics reported in the left column between REBP-eligible and -ineligible cohorts in the non-REBP Region (column 1) vs. the REBP Region (column 2). Column (3) reports the difference between column (2) and (1). For an overview of the methodology see Section 6. For each of the variables and groups, the table reports means as well as standard errors (in parentheses) based on 1,000 bootstrap replications blocked at the administrative region level. Given the small percentage-point differences, the industry share variables are multiplied by 100 in order to be legible. See Section 6 and Appendix D for more details on how the variables are constructed. See Section 6.2.1 for the methodology underlying the decomposition into the groups.

Table A.2: Difference-in-Differences Effects of the REBP on Outcomes of Initially Employed Workers (Age 50 to 55)

	(1) Separation	(2) Separation Into Nonemployment	(3) Separation Into New Job	(4) Employment (Months)	(5) Unemployment (Months)	(6) Cont. Empl. (Months)	(7) Quit (Indicator)
REBP Eligibility	0.110**	0.125**	-0.016	-3.216**	2.690	-2.561**	0.019***
	(0.042)	(0.062)	(0.020)	(1.597)	(1.949)	(1.084)	(0.005)
REBP Region	-0.029	0.006	-0.036***	-0.107	-0.056	1.596	-0.047
	(0.025)	(0.017)	(0.009)	(0.575)	(0.459)	(1.041)	(0.030)
Constant	0.371***	0.185***	0.185***	56.298***	2.923**	46.349***	0.175**
	(0.060)	(0.037)	(0.023)	(1.636)	(1.223)	(3.260)	(0.069)
Observations	462396	462396	462396	462396	462396	462396	462396
Adjusted R^2	0.003	0.011	0.003	0.006	0.010	0.002	0.002
No of Clusters	100	100	100	100	100	100	100
Mean Dep Var, Control Region	0.360	0.187	0.173	56.505	2.822	47.111	0.174
Sd of Dep Var, Control Region	0.480	0.390	0.379	13.157	7.566	22.144	0.379

Note: The table reports results of the econometric specification in 1. $REBP$ captures the effect of REBP-eligibility on the outcomes listed in columns (1) through (7) on a sample of workers employed in the quarter before turning 50. The regression specification includes region and cohort effects. $Separation$ denotes an indicator function that is 1 if a worker is not employed by their employer at age 49.75 by the quarter before they turn 55. $Separation\, into\, Nonemployment$ denotes an indicator for $Separation$ from the initial employer interacted with an indicator for not taking up employment with another employer by the quarter before turning 55. $Employment\,(Indicator)$ denotes whether a worker is employed in the quarter before turning 55. $Employment\,(Months)$, $Unemployment\,(Months)$ and $Continuous\,Employment\,(Months)$ denote the months of employment, unemployment, and continuous employment with the initial employer between age 50 and age 55. Standard errors clustered at the administrative region level are reported in parentheses. Levels of significance: * 10%, ** 5%, and *** 1%.

Table A.3: Difference-in-Differences Effects of the REBP on Outcomes of Survivors (1994 Through 1995)

	(1) Separation	(2) Separation Into Nonemployment	(3) Separation Into New Job	(4) Employment (Months)	(5) Unemployment (Months)	(6) Cont. Empl. (Months)	(7) Quit (Indicator)
REBP Eligibility	0.012***	0.006***	0.006	-0.027	-0.070*	-0.100***	-0.002
	(0.004)	(0.002)	(0.004)	(0.019)	(0.038)	(0.025)	(0.002)
REBP Region	-0.011	0.001	-0.011***	-0.006	0.003	0.138	-0.003
	(0.012)	(0.008)	(0.004)	(0.060)	(0.044)	(0.087)	(0.007)
Constant	0.137***	0.093***	0.039***	14.270***	0.243	13.982***	0.022
	(0.030)	(0.025)	(0.006)	(0.168)	(0.155)	(0.220)	(0.017)
Observations	183141	183141	183141	183141	183141	183141	183141
Adjusted R^2	0.098	0.149	0.002	0.153	0.029	0.100	0.001
No of Clusters	99	99	99	99	99	99	99
Mean Dep Var, Control Region	0.166	0.125	0.034	14.018	0.306	13.769	0.022
Sd of Dep Var, Control Region	0.372	0.330	0.181	2.728	1.471	3.073	0.146

Note: See the note for Table 3 for details.

Table A.4: Difference-in-Differences Effects of the REBP on Outcomes of Survivors (1994 Through 1997)

	(1) Separation	(2) Separation Into Nonemployment	(3) Separation Into New Job	(4) Employment (Months)	(5) Unemployment (Months)	(6) Cont. Empl. (Months)	(7) Quit (Indicator)
REBP Eligibility	0.032***	0.026***	0.006	-0.405***	-0.362	-0.613***	-0.002
	(0.008)	(0.006)	(0.006)	(0.080)	(0.219)	(0.160)	(0.002)
REBP Region	-0.016	0.003	-0.020**	-0.057	-0.000	0.408	-0.003
	(0.028)	(0.019)	(0.009)	(0.385)	(0.270)	(0.601)	(0.007)
Constant	0.348***	0.250***	0.079***	33.781***	1.336	31.673***	0.022
	(0.067)	(0.054)	(0.012)	(1.030)	(0.894)	(1.526)	(0.017)
Observations	183141	183141	183141	183141	183141	183141	183141
Adjusted R^2	0.169	0.254	0.013	0.274	0.023	0.157	0.001
No of Clusters	99	99	99	99	99	99	99
Mean Dep Var, Control Region	0.430	0.347	0.061	31.768	1.626	30.062	0.022
Sd of Dep Var, Control Region	0.495	0.476	0.238	11.115	4.863	12.204	0.146

Note: See the note for Table 3 for details.

Table A.5: Difference-in-Differences Effects of the REBP on Outcomes of Survivors (1994 Through 1998)

	(1) Separation	(2) Separation Into Nonemployment	(3) Separation Into New Job	(4) Employment (Months)	(5) Unemployment (Months)	(6) Cont. Empl. (Months)	(7) Quit (Indicator)
REBP Eligibility	0.037***	0.027***	0.009	-0.737***	-0.533	-1.056***	-0.002
	(0.009)	(0.006)	(0.008)	(0.136)	(0.324)	(0.262)	(0.002)
REBP Region	-0.024	0.000	-0.026***	-0.085	0.001	0.655	-0.003
	(0.032)	(0.024)	(0.010)	(0.631)	(0.439)	(0.978)	(0.007)
Constant	0.451***	0.323***	0.093***	42.087***	2.002	38.657***	0.022
	(0.076)	(0.066)	(0.011)	(1.695)	(1.385)	(2.468)	(0.017)
Observations	183141	183141	183141	183141	183141	183141	183141
Adjusted R^2	0.176	0.270	0.022	0.309	0.013	0.174	0.001
No of Clusters	99	99	99	99	99	99	99
Mean Dep Var, Control Region	0.549	0.439	0.066	38.643	2.358	35.968	0.022
Sd of Dep Var, Control Region	0.498	0.496	0.248	15.792	6.424	16.957	0.146

Note: See the note for Table 3 for details.